Life of Sir William Tennant Gairdner K.B.B., M.D., L.L.D., F.R.S. regius professor of practice of medicine in the University of Glasgow : with a selection of papers on general and medical subjects / by George Alexander Gibson.

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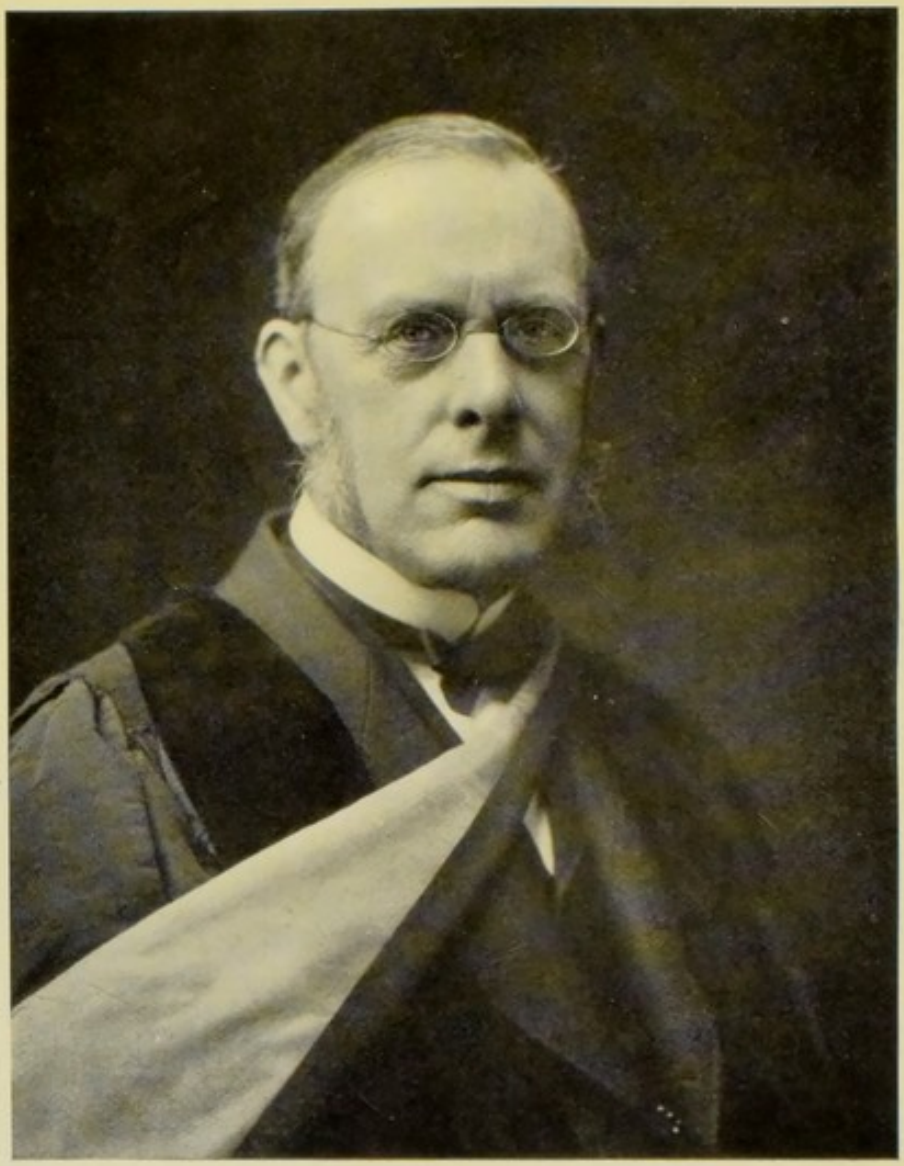

SIR WILLIAM TENNANT GAIRDNER, K.C.B.

From a Photograph taken during the later years of his life in Glasgow, by Messrs. T. & R. Annan & Sons.

Frontispiece

LIFE OF
SIR WILLIAM TENNANT GAIRDNER

K.C.B., M.D., LL.D., F.R.S.

REGIUS PROFESSOR OF PRACTICE OF MEDICINE IN THE UNIVERSITY OF GLASGOW

BY

GEORGE ALEXANDER GIBSON

M.D., Sc.D., LL.D.

SENIOR PHYSICIAN TO THE ROYAL INFIRMARY, EDINBURGH

WITH A SELECTION OF PAPERS ON GENERAL AND MEDICAL SUBJECTS

GLASGOW

JAMES MACLEHOSE AND SONS

PUBLISHERS TO THE UNIVERSITY

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PREFACE.

On Christmas Day, 1908, a letter reached me from Dr. Francis Gairdner, on behalf of Lady Gairdner and her family, requesting me to select some of his father's papers for publication, and to prepare a biographical sketch. Having long cherished a sincere affection for the late Sir William Gairdner, this request was equivalent to a command, and the duties of editor were at once entered upon. Some time elapsed before the literary material could be collected, and just as this preliminary part of the task was nearing its accomplishment, delay occurred through an unfortunate illness, which practically cost me the entire year 1910. The trust reposed in me was therefore only begun in earnest last year, and it has occupied all the scanty leisure of a somewhat busy life since that date. It has been, from first to last, a labour of love, and, on its completion, it need only be said that my estimate of my old friend in every relation has been, if that be possible, enhanced.

My warm thanks are due to a large number of admirers of the late Professor who have kindly assisted me in the production of this volume. Each has been mentioned in the appropriate connection in the following pages; my special obligations must, however, be publicly given to the members of Sir William Gairdner's family, who have rendered every assistance in their power. Mr. James Gairdner has laid me

under a particular debt of gratitude, for which my sincere acknowledgments are warmly rendered. Dr. W. T. Ritchie has greatly assisted me in the revision and criticism of the scientific papers, and it is a pleasure to return my hearty thanks for his kindness. Dr. G. A. Middleton and Dr. R. O. Adamson were so good as to offer the use of the cartoon by the late Dr. Smith, and of the early photograph by the late Dr. Adamson; these pictures add much to the value of the Biography, and both suggestions aroused feelings of warm obligation. The Librarians of the Royal College of Physicians of Edinburgh and the Royal Faculty of Physicians and Surgeons of Glasgow have greatly helped me, and my indebtedness must be expressed to them for much bibliographical assistance. To my Secretary it is a pleasing duty to extend a cordial expression of my thanks for constant and untiring assistance.

3 Drumsheugh Gardens,
2nd September, 1912.

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BIOGRAPHICAL SKETCH

DESCENT AND PARENTAGE

THE forefather to whom the families of Gairdner, Tennant, Fullarton, Shaw, Whiteside and Maclagan have looked up with a quite unusual degree of reverence was William Dalrymple. To the mind of any member of those families, that name is invested with an almost legendary light, though, as matter of fact, he was an eminently historical character—he was, indeed, the Rev. William Dalrymple, D.D., "Minister of the First Charge" in the Burgh of Ayr for 68 years. It is curious that all the posterity of the Dalrymple family came by the spindle side. As far as name is concerned, the family died out; yet it is to a Dalrymple, and not to any eponymous ancestor, that these families look as their head and origin.

The old minister was a notable character in Ayr. Although a "Moderate," his Christianity could hardly have been criticised, even by Hugh Miller himself, so profound was his benevolence of character, and so great his purity of life. There were many anecdotes told, illustrative of these features—among them, that he once gave his coat to a ragged man on a very cold day, and walked back to Mount Charles in his shirt sleeves. In this respect he somewhat resembled George Thomson, the original of Scott's famous character, "Dominie Sampson." His greatest title

to fame, and one of his descendants' chief sources of pride, is the fact that, during his ministry, Robert Burns was born, in a little cottage on the Alloway Road, within the confines of his parish. Sir Douglas Maclagan used frequently to refer to the fact that the poet had been baptised by Dr. Dalrymple, in the early days of his incumbency, while he himself had likewise undergone the rite at his hands in the later years of his tenure. Burns, who denounced so many of the ministers of the Kirk in those days, evidently looked up to his own as a saint indeed. The verse in which he gave him immortality in "The Kirk's Alarm," shows that the doctor's theological tendencies were considered by the rigorists to be damnably faulty, but offers one of the finest compliments ever given to a Christian minister by a genius—

> D'rymple mild, D'rymple mild, tho' your heart's like a child,
> And your life like the new driven snaw,
> Yet that winna save ye, auld Satan must have ye,
> For preaching that three's ane and twa.

Dr. Dalrymple was the proprietor of Mount Charles, a small estate, at a lovely bend of the Doon, on the right bank of the stream—just above the last bridge before the river reaches the sea. He must have spent much time and thought on the property; scattered widely over the grounds, for example, there are little inscriptions and mottoes from the classics, which he loved, and in which he, like all clergymen and ministers of that time, was much better versed than his brethren of to-day.

The connexion of the Gairdners with the Dalrymples began when Robert Gairdner, a captain in the Bengal Artillery, married Miss Macrae Smith, a niece of Dr. Dalrymple, in 1789. Further back than the grandfather of this gallant captain, the family records cease; the Gairdners

become merged (very properly, considering their name) with all the descendants of their first ancestor.[1] All that can be said is that they were an Ayrshire family through and through. The captain's grandfather, also Robert Gairdner, (who married Anna M'Fadzen), was a "merchant" in Ayr —whatever that elastic term may mean—at the end of the seventeenth century and the beginning of the eighteenth. His boyhood saw the Union, and he and his family grew up in the reign of Queen Anne. The Jacobite Risings left them totally unaffected, we may be sure, for the Gairdners have been Whigs to the marrow. Sir William used to complain that he was the last of the Whigs—an honour for which there must be a good deal of competition. About the captain's father, John Gairdner, nothing certain is known, except that he married Marion Smith. Even the filial piety of his grandson John could unearth no detail of his life—save that he was born in 1720; it has even been ascertained that the month of that birth was September. But the exactitude of this detail is counterbalanced by the uncertain date of his decease. From all these facts, it may be concluded that this Ayrshire family produced worthy and "douce" (if not distinguished) men. The Bengal captain must have caused immense surprise to his unwarlike family; and indeed, in him, the military ardour of the house appears to have worked itself out.

The introduction of the name Macrae into the Gairdner family deserves at least a passing reference. James Macrae was born in Ayrshire, of parents in straitened circumstances, in 1674, and was in his early years employed by Hew

[1] It is hardly necessary to observe that the usual spelling is merely a frank rendering of the Scottish, or rather Anglo-Saxon, way of pronouncing Gardener; cf. Ger. "Gärtner." As regards the way in which it is pronounced to-day, there are differences of usage, even in the family itself.

Macguire, a carpenter in Ayr. Macguire seems to have assisted the boy in many ways, not only by providing some means of education, but by getting him a post in one of the East India ships. He prospered in the service, and came under the direct notice of the Court of Directors. In command of the company's ship "Cassandra" in 1720, he greatly distinguished himself by his gallant conduct and successful defence against two powerful pirate vessels.[1] He became Deputy Governor of Fort St. David in 1725, but never entered upon his duties, since he was nominated immediately afterwards as Second Member of Council at Fort St. George. In 1726, he was appointed Governor of Madras. He retired from the service four years later, and lived for two years at Blackheath, to be near the headquarters of the Honourable East India Company, and then returned to Ayrshire, which he never again left. Being a wealthy man, he, like most "gangrel Scots" returned from abroad, acquired landed property. He purchased the estate of Monkton, afterwards called Orangefield, where he resided until his death, and he afterwards bought Ochiltree, Alva, and Drumdow—all, like Orangefield, in the county of Ayr—and Houston, in Renfrewshire.

Hew Macguire, son of his old friend, married his sister, and had two sons and four daughters; three of the daughters married. Elizabeth, the eldest, became the wife of William Cunningham, who, in 1768, succeeded his brother as 13th Earl of Glencairn, and was father of the patron of Robert Burns. The second daughter, Margaret, married James Erskine, of Barjarg, who was afterwards a Senator of the College of Justice. On becoming a judge he assumed, as usual in Scotland, a territorial title, becoming Lord Alva,

[1] See Chambers' *Domestic Annals of Scotland*, Edinburgh, 1859-61, vol. iii. p. 585.

from the property which Macrae had presented to his niece. Jacobina, the third daughter, did not marry. The fourth daughter, Macrae, was the wife of Charles Dalrymple, of Langlands, Sheriff-Clerk of Ayrshire. The elder son, James, received the estate of Houston, and assumed the name Macrae in addition to his own. Drumdow was given to the younger son, Hugh. The property of Orangefield, the original and principal portion of the estates acquired by the Governor, was presented to his youngest niece, Macrae Macguire, wife of Charles Dalrymple, who was a brother of Rev. Dr. Dalrymple of Ayr.

It may be added that Governor Macrae showed his political bias by presenting the City of Glasgow with the statue of William III., which stands in the open space where the Trongate and Gallowgate cross the Saltmarket and High Street. It was no doubt as another mark of his attachment to the new order that he gave the name of Orangefield to the property of Monkton.

The families of Smith, M'Fadzen and Fergusson, into which the Gairdners married, were all middle-class Ayrshire folk. We find, for example, two Ayrshire "shipmasters" among them—a term almost as wide as that of "merchant." They seem to have steadily improved their position every generation, by personal merit, and when a Gairdner obtained a commission in the army and married a niece of the Laird of Mount Charles, the status of the family may be said to have become assured. After this union, the Gairdners were, with almost unfailing regularity, either medical men or men of business. They have also gone far afield, for, in addition to several families in Ayrshire and the west of Scotland, there are others in England and Ireland, and even more distant lands—such as Egypt, South Africa, Australia, New Zealand and Java. None of Dr. Dalrymple's descendants

bear his name, as has already been mentioned, but there may be Dalrymples in Ireland and elsewhere descended from his brother, Charles Dalrymple of Orangefield. Of his sisters, Sara was the mother of Burns' friend, Robert Aiken, and the grandmother of Andrew Aiken—the young friend to whom the poet gave such good advice. Another sister, Marion, became the ancestress of several families of Shaw, some of whom live in Ayr to this day. Gairdners, too, have come back to the district and have, for many years, been citizens of the Old Burgh. Another sister, Catherine, was the progenitor of several families of Tennant, including that of Gairdner's maternal grandfather. Only one other of Dr. Dalrymple's daughters had any posterity that survived —Margaret, who married a Whiteside. Two of their daughters espoused respectively a Fullarton and a Maclagan. David Maclagan, the well-known Edinburgh doctor, and his wife were the parents of seven sons :—

Andrew Douglas, M.D., Professor of Medical Jurisprudence and Clinical Medicine in the University of Edinburgh, President first of the Royal College of Surgeons, and afterwards of the Royal College of Physicians of Edinburgh, knighted in 1886 ; Philip Whiteside, M.D., who practised his profession for many years in Berwick-on-Tweed ; Robert, R.E., who became a General Officer, and head of the Public Works Department of the Punjaub ; David, for many years Manager of the Edinburgh Life Assurance Company ; William Dalrymple, originally in the army, and afterwards a clergyman of the Church of England, successively Vicar of Kensington, Bishop of Lichfield, and Archbishop of York ; John Thomson, Accountant in Edinburgh, for many years in charge of the Widows' Fund of the Church of Scotland ; and James M'Grigor, M.D., who practised at Hexham in Northumberland.

As has already been mentioned, Captain Robert Gairdner married Miss Macrae Smith. Their wedding took place in 1787. Within five years they had a family of five sons, when the captain was killed in the most tragic manner by the kick of a horse. A daughter was born shortly after his death. His eldest son, John, was born 18th September, 1790—we shall return to him by and bye.

The second son, Thomas, was born on the 22nd October, 1791, and became a Writer to the Signet in Edinburgh. He married his cousin, Margaret Dalrymple Montgomerie, and retired from business at a comparatively early period. He lived for a time at Greenknowe, in Stirlingshire; then at Larchgrove, near Balerno; and, in 1846, he acquired the estate of Craigend, near Stirling. Thomas Gairdner died in 1860; he and his wife had no family.

Robert was born 3rd December, 1792. He went out to India, and made a considerable fortune as an indigo planter; but, returning to this country, he unfortunately lost most of it. He afterwards went out to Canada, where he was occupied as a farmer. While in India, he married Macquorn, eldest daughter of John M'Haffie. In Canada, he lost his wife and one child, and threw up his occupation to return home, with one son and one daughter. His daughter survived him many years, and his son Robert, who is now almost eighty, lives in New Zealand.

William, born 11th November, 1793, like his eldest brother, became a medical man. He settled at 12 Bolton Street, Piccadilly, London, and was the author of an excellent work on Gout, which went through several editions. He married Cecilia Bordier, daughter of a banker near Geneva. They had one daughter, Clara, who died last year. She married Julius Bordier, and had three sons and three daughters. William Gairdner died 28th April, 1867. Mr.

James Gairdner informs me that his uncle William was very unlike his brothers, both physically and mentally. He was a little man, with very white hair in middle age; of sturdy and independent mind, but rather dogmatic and doctrinaire.

Charles Dalrymple was born 11th December, 1794. He became a banker in Kilmarnock, and was afterwards appointed Commissioner to the Earl of Eglinton, on which he gave up his post as Manager of the Union Bank in Kilmarnock to reside at Auchans, near Dundonald. He left a family of five sons and four daughters, several descendants of whom live in Ayrshire. Two of his sons and a grandson have succeeded him as Managers of the Bank in Kilmarnock; and one son and a grandson have been successively Managers of the Union Bank in Glasgow. Charles Dalrymple Gairdner died twelve days after his brother William. Of his family, none are left but three unmarried daughters, who live at Ayr.

Marion Smith, born 23rd March, 1897, married her own cousin, William Tennant, of the Customs. She and her four children died before the middle of 1825. It may be added that William Tennant was twice married afterwards—to Nicolas Johnstone, and to his cousin, Ramsay Whiteside. His second wife had one son, who was admirably brought up by his third wife, and is the father of a large family.

After the death of Captain Gairdner, the family removed to Edinburgh; but Mount Charles was retained until the year 1817, when John Gairdner, about to be married, sold the property. It may be added that Sir William on one occasion took some of his children, when they visited Ayr during a holiday season, to show them "the pit out of which they were digged," as he liked to phrase it. Mrs. Gairdner was almost worshipped by her children, and indeed

she lived her widowed life most heroically for them. Her portrait is a charming one, with a sweet face beneath the old-fashioned widow's " mutch." There are several replicas of this picture, which to-day hang in different houses of her posterity as a still cherished possession.

John Gairdner, the eldest son of Captain and Mrs. Gairdner, was born in 1790. He studied medicine in Edinburgh, took his degree in 1811, and obtained the Fellowship of the Royal College of Surgeons in 1813. He became one of the most distinguished men of his adopted city, and occupied the Chair of his College in 1830 and 1831. Possessed of a fine historical and literary sense, he spent many of his leisure hours in antiquarian researches. Although he took an active interest in many matters outside of his particular calling, his favourite subject of study was the history of his own profession. His papers upon the history of the College of Surgeons and of the Medical School of Edinburgh are still of the greatest value. The old combination of the Chirurgeons and Barbers of Edinburgh had for him some interesting, not to say comic, aspects ; and in his time, the President of the Royal College of Surgeons still occupied a seat in the unreformed Town Council of Edinburgh, as " Deacon of the Chirurgeon-Barbers."

John Gairdner always looked back upon Mount Charles, the home of his infancy, with peculiar delight. Having been born there, in the very heart of the land of Burns, his love for the great national poet was augmented by much personal and local knowledge. This feeling caused him to resent not a little some statements of Burns' biographers and critics, which he at length answered anonymously, in letters to the *Scotsman,* which were afterwards collected by his family in a privately printed tract, entitled *Burns and the Ayrshire Moderates.* His eldest daughter, after his

death, feeling strongly that the matter should not be left there, followed it up by a little booklet of her own, entitled *Robert Burns, by a Scotswoman.* From a feeling of false modesty, she was very unwilling to disclose herself; but, in a re-issue of the book, consented to do so.

John Gairdner, in 1817, married Susanna Tennant, whose mother was Wilhelmina, daughter of Dr. Dalrymple; she therefore was his second cousin; William Tennant Gairdner was thus directly descended from, as well as collaterally connected with, the good doctor. The Tennant connection with the Dalrymples was by means of the marriage of William Tennant to Wilhelmina Dalrymple. The two were first cousins, for William Tennant was the son of Dr. Dalrymple's sister, Catherine. It may be added that Susanna Tennant had a brother, James Tennant, who commanded the artillery at Chillianwallah and Gujerat; he was created a K.C.B. for his distinguished services in India.

Descended from the Tennant family are the various members of the great chemical manufacturing house of St. Rollox, of whom the head is now Lord Glenconner. The name is taken from a farm near Ochiltree, where John Tennant was born in the eighteenth century.

Through the M'Fadzens, one of whom, as we have seen, was the grandmother of Captain Robert Gairdner, the family was connected with John Thomson of Duddingston, the celebrated artist and minister, and Sir Gilbert Blane, the distinguished naval surgeon, who, along with James Lind, introduced the modern treatment of scurvy, and stamped it out of the Navy.

Many of the connections of the family of Dalrymple have already been mentioned; but it should be added that Marion Dalrymple, sister of the minister of Ayr, married, as previously mentioned, the Rev. Dr. David Shaw, minister

of Coylton. Her son Charles had a large family, of whom Barbara married George Joseph Bell, Professor of Scots Law in the University of Edinburgh; and Marion married his brother, Sir Charles Bell, the eminent physiologist, Professor of Surgery in Edinburgh.

If it be true that "the first requisite of happiness is to be born in a famous city"—a remark contained in the encomium upon Alcibiades, usually, according to Plutarch, attributed to Euripides, Gairdner was fortunate in his birth-place, inasmuch as he was a native of the romantic Scottish capital. Dr. and Mrs. John Gairdner spent their early married years at 18 Hill Street, where all their children were born. They were seven:—

Macrae Smith was born on 31st August, 1818. She was named after her paternal grandmother, to whom, when she was left a widow, all her sons felt that they owed a very peculiar debt. The name descended from an adopted child of Governor Macrae, whose story has already been told. Miss Macrae Smith Gairdner had something more than a mere sisterly influence over her brothers, not only from being their senior, but on account of her well-balanced judgment. We have already seen how she was ready to enter the lists on behalf of the National Bard.

Robert, the eldest boy, was born 11th August, 1820, and died 3rd June, 1825.

William Tennant Gairdner was the second son. The date of his birth was 8th November, 1824.

John Smith Gairdner, born 11th May, 1826, was the earliest to leave the paternal roof-tree. He was appointed as a clerk in the Legacy Duty Office in Somerset House, and, after being there for a year or two, was transferred to the Dublin branch of the same service. In 1854, he married a Dublin lady, and lived very happily with her till she died

at Rome, in 1884. They had no children. In his later years his health became indifferent, and he was obliged to spend the winter in Rome every year. He died in 1889, at Scarborough, and was buried at Edinburgh.

Of Gairdner's brothers, the best known is the sole survivor—James Gairdner, the accomplished historian, born 22nd March, 1828. As he informs me, he has always been somewhat deaf, so that he was considered to be as ill-qualified for a man of business as for a doctor. He, therefore, in 1846, entered the Public Record Office, to which he still in a manner belongs, although long since released from regular attendance. He married, in 1867, Annie, daughter of Mr. Joseph Sayer of Carisbrooke, and has one daughter. He is author of *England*, in the series of " Early Chroniclers of Europe " ; a *Life of Richard III.* ; *The English Church in the Sixteenth Century* ; *Studies in English History* ; *Lollardy and the Reformation in England*. He published a delightful edition of the Paston Letters ; and he is editor of one of the most monumental works in existence—*The Calendar of the State Papers of the Reign of Henry VIII*. He was created C.B. in 1900, and received the Honorary Degree of LL.D. from Edinburgh in 1897, and Glasgow in 1909 ; while the University of Oxford conferred the Honorary Degree of D.Litt. upon him, also in the latter year.

Marion was born 11th January, 1831, and, with her elder sister, lived with their father until his death, after which they resided together in Edinburgh. Her death occurred in 1882.

Thomas Robert Gairdner was born 21st August, 1833, and died, at the age of eighteen, from chest disease.

Mr. James Gairdner has kindly furnished me with a few recollections of his brothers :

" The death of ' Tom Bob,' as we familiarly called him,

was the first break in the family that either William or I could remember, and we all felt it much. He showed great interest in mechanics, and also had aspirations, like William, after a medical career, which our father discouraged. As to William, the first symptom I remember of his future tendencies was when, at our father's table, we had rabbits one day for dinner, he preserved the head of a rabbit for dissection afterwards. A year or so later, I remember going an excursion into the Highlands with him and my second brother, John. I am inclined to date this excursion in the year 1842, when I was fourteen and William was in his eighteenth year. We took steamer from Glasgow to Dumbarton and sailed up Loch Lomond to Rowardennan Inn, ascended Ben Lomond and came down upon the other side, by the boggy sources of the Forth, to Aberfoyle.

"I am sure we were all a happy family, both while we lived together under the paternal roof and afterwards. There never was anything approaching to a family quarrel, though differences of opinion at times were most freely discussed. But even differences of opinion were never vital."

It cannot, indeed, for a moment be questioned that Gairdner's home life was characterised by loyalty and happiness. It is not to be supposed, however, that the members of the family were of those who wear their hearts on their sleeves. With all their affection they preserved an air of reticence in the expression of their feelings, and sometimes showed them in writing rather than by speaking. Take the following note, for instance, from his father :—

"Friday.

My dear William,

I daresay you think this an absurd way of thanking you for your kind present of Shelley ; but I am sure you do not suppose my thanks less sincere, because put upon

paper, instead of being given verbally; therefore dear W. allow me *here* to return you my sincere thanks for the book which you have been so kind as to present to me. I shall in all probability make good use of it, as what I have seen of his life, and writings, tends to make me esteem him as a man, and honour him as a poet.

Believe me, dear W.,

Ever yours affectionately,

JOHN GAIRDNER.

18 Hill Street,
31st March, 1843."

Mr. James Gairdner informs me that it is more than doubtful whether the old gentleman ever realised his anticipation of applying his mind seriously to Shelley.

The death of Gairdner's last surviving sister, in 1895, brought a very sympathetic letter from one of his old American friends. Some references contained in the letter will be clear, when the record of his visit to the United States is given in the sequel.

"Nutwood, Nov. 22nd, 1895.

My dear Dr. Gairdner,

I have two letters to thank you for, and I cannot let the next steamer sail without its carrying to you some words of true sympathy in the loss you have sustained. A sister who has been with one from earliest childhood holds ever a peculiar place, and the wrench of separation is hard to bear. Do you know that your sister was born the same year in which my dearest mother first saw the light—1818? What a tender and comprehensive sympathy she would have given you! Thank you very much for writing to me of this. I have sent your letter to my uncle William. I can fancy how interesting it must be to talk to so old a lady as your aunt—93 is a great age to attain. I can fancy also that you must dwell much on the thankfulness, that your dear sister is at rest, after so much physical suffering as you describe. The sense of peace for our dear ones is

such a help, when we suffer at their departure. I think it was strange my Uncle Charles never spoke to you of his home relations, which were always so peculiarly tender and intimate. But he was very reserved about those who were closest to his heart. I hope I may be able to get some of these letters copied. Evidently mamma's influence was intensely strong upon him and all for the wisest, highest, noblest purposes and ideals. One trouble is that I never can tell what my aunt, Mrs. Charles Perkins, may do or say about these letters : she is very peculiar, as you know, and I feel that I have to walk with circumspection. I found them, fastened in a book and wrapped up in paper, addressed to my dear mother in my aunt's handwriting. Whether, as they were among mamma's possessions and were written to her brother before his marriage, they belong to me as her heir, I do not know. I will ask a friend of mine, who is a lawyer, and then I shall know what right I have to get some of them copied or not.

I was away for four weeks, and made visits to friends, among them to Uncle Charles' eldest son Edward and his wife. He is getting on famously in his profession, and has a pleasant home at Plainfield, New-Jersey, where he and his wife, his three boys and two little girls live a very domestic life, with relatives and friends about them. There is an Irish clergyman and a graduate of Trinity College, Dublin, a clever man, keen in the classics, who has a school for 20 boys, to which Edward, Max, and Charley go. Ned likes Dr. Johnston very much, and I think he will have an excellent influence on the little boys. So I felt that mamma and Uncle Charley would have been pleased, and that gives me comfort. Uncle Edward is much the same, though more hysterical and nervous. We are settled into the winter life, and my friend Miss Foote is with me. My cousin, Bishop Doane's daughter, Mrs. Gardiner, was to sail for Genoa with her five children on Nov. 6th, but a week before, Margot was seized with a fever, with typhoid symptoms. Fortunately it was very light, and she is quite recovered ; and they sail in the Columbia on Saturday, Nov. 24th. The delay has

been very annoying. Affairs in the East look very threatening. I read my English papers with great interest. Dear Dr. Gairdner, I wish I had my dearest mother's power to express in fitting words my true appreciation and sympathy in your present sorrow. You will believe that I do feel it in my heart, and that I am always

Your affectionate and grateful

LILLY CLEVELAND."

SCHOOL AND COLLEGE

The boys, after the usual primary training at a Dame's School, were sent to the Edinburgh Institution, then at 3 and 5 Hill Street—the Rev. John Little, afterwards minister of Manor Parish, being at the time Headmaster. Here again, Mr. James Gairdner has laid me under obligations. " My brother," he writes, " said in after years, that really it was a drawback to be sent to a school so near at hand; boys are the better of plenty of exercise. The Institution is now in Queen Street, and no doubt is managed on different lines, to some extent, for the methods of education vary from one generation to another. But I daresay it still preserves the same leading ideas, which were somewhat novel in our days. It was thought that at the High School too much time was devoted to Latin and Greek, and too little to matters of more practical value for most people. The parents or guardians of a boy were accordingly not tied to any regular curriculum, but allowed to select for their charge an hour of Latin teaching, an hour of Arithmetic, another of English composition, and so on. As a schoolboy—and after—I thought the principle was a right one, but justice was scarcely done to it; for with less time given to the classics, the teaching ought to have been made all the more effective. And it was really a great mistake to

get as teachers young men from the University, who, though well enough up in grammar, had not the art of managing a class, and had no great notion of inculcating a taste for classical literature. I think this is the point in which Scottish schools are behind English ones. The trained English scholar has a pleasure in the literature of which he has been given the command. Not so, for the most part, the Scottish scholar, who spends more time over the rules of grammar, and less over the author. Now my brother William was, I think, an exceptional scholar at our school, for he did take an interest in the author, which I am sorry I did not, and though others perhaps did rather more than myself, I do not know that any of them made annotations on the authors they read for their own pleasure as he did. Surely short-sightedness favours book study, and long-sightedness the contrary. Dr. Johnson was short-sighted, besides other illustrious examples that might be quoted. My brother William was short-sighted, and obliged to wear spectacles even at school. I was long-sighted. No one could have beat me, even thirty years ago, in reading posters or church clocks at a distance, and I think that the eye which loves to rest on distant objects, and not to be focussed on near ones, must be inimical to study. Indeed, I think I have become a student simply as a matter of duty, and it has somewhat impaired my vision. I was driven to the use of spectacles thirty or forty years ago—but only to read by. Whereas my brother, when he looked into a book, took off his ! "

The last medical survivor of Gairdner's school-fellows at the Edinburgh Institution was the late Dr. John Smith, who invariably spoke with admiration and affection of his old contemporary ; two of his comrades are still with us—Mr. Walter Macdowal Hardie and Mr. James Williamson.

To the former of these gentlemen, who, by the way, is the father of the present Headmaster of the Institution, my thanks are due for his kindness in giving me some interesting information about the school when Gairdner was a pupil in it.

We often hear, in these later days, from the survivors of the first half of last century, that but little learning was carried away by the members of the large classes of that time. This was not the case, however, with Gairdner, who, on proceeding to the University of Edinburgh, in 1840, was possessed of excellent classical attainments, as well as a good general education.

It is worth our while to linger for a few minutes over the names of those who then occupied the Chairs with which he was personally concerned. Robert Graham not only held the Chair of Botany from 1820 to 1845, and the office of Conservator of the Royal Botanic Garden, but he was also—for most of this period—Physician to the Royal Infirmary and Clinical Professor; in the present epoch, such a double rôle would be difficult to play. Robert Jameson was Professor of Zoology from 1804 to 1854, and, as might be expected from his own natural inclination, gave his teaching a strong geological tendency. Chemistry and Clinical Medicine were united in the person of Thomas Charles Hope, who occupied the double Chair from 1798 till 1844. William Gregory, on succeeding him in the former office, was relieved from clinical teaching. The third Monro continued, from 1798 until 1846, to deliver his grandfather's lectures on Anatomy, and, even during Gairdner's undergraduate days, startled his audience now and then (if we are to believe popular rumour) by inadvertently reading aloud such an amazing statement as: "When I was a student at Leyden, in the year 1718"—

beyond which words his sentence became eclipsed. Although a highly gifted man in many directions, his strong point was not the subject which he was specially called upon to teach. The Chair of Physiology was filled by the great and good William Pulteney Alison (1821-1842), and afterwards by Allen Thomson (1842-48), famous in later years as the Glasgow Professor and doyen of British anatomists. In Pathology, John Thomson, "the chair maker," was professor from 1831-1842, in which later year he was succeeded by William Henderson—universally known (for some occult reason) as "Boiler"—who reigned in that department till 1869. Henderson was, at first, one of the clinical professors, but, having given his adhesion to a system of Therapeutics as dogmatic as unscientific, he was removed from this position. Robert Christison, great in every way, was Professor of Materia Medica from 1832 to 1877, and, during many years, was one of the Professors of Clinical Medicine. The Chair of Forensic Medicine was filled by Thomas Stewart Traill, from 1832 until 1862, and he also was called on to take his turn in teaching Clinical Medicine. The versatile James Young Simpson was Professor of Midwifery from 1840 to 1870, and during Gairdner's early days was in the thick of his anaesthetic investigations. Charles Bell, the illustrious physiologist, who was, as we have seen, a connexion of the Gairdners, was occupant of the Chair of Surgery from 1836 to 1842, when he was followed by the eloquent James Miller, who retained it until 1864. The Chair of Clinical Surgery was filled by James Syme from 1833 until 1869, seeing that the episode of his election to University College, London, his differences with the ruling powers there, and his indignant resignation, formed but a brief interlude in his tenure of office. The Chair of Medicine, which had been held by James Home since 1821, was filled

in 1842 by Alison, transferred thither from that of Physiology.

These names form in truth a brilliant galaxy in the intellectual firmament. As was often said by Gairdner in later days: "They were giants in these days; but they were very quarrelsome giants." It would serve no good purpose to retail the stories still extant, proving how some of the greatest of these great men, like Christians, "loved one another."

Into this famous scientific group we may picture the entrance of the tall, slender youth, whose grave and thoughtful aspect was heightened by the use of spectacles. It is probable that, even in boyhood, the tendency to become absorbed in whatever was forward (afterwards so marked a characteristic) may have been manifest. Some of his own contemporaries have indeed related anecdotes of these youthful days, proving the early existence of a singular absent-mindedness—almost as great as in the case of "Rabbi" Duncan, or Provost Salmond.

During the five years when he was an undergraduate, the Physicians to the Royal Infirmary of Edinburgh were Thomas Spens, Thomas Shortt, David Craigie, G. A. Borthwick, J. H. Peebles, William Henderson, William Thomson, Robert Spittal, George Patterson, John Rose Cormack, and Alexander Halliday Douglas. The Surgeons were Sir George Ballingall, John Campbell, Sir Charles Bell, James Syme, Alexander Watson, William Fergusson, James Argyll Robertson, P. D. Handyside, James Miller, James Duncan, Douglas Maclagan and James Dunsmure. The celebrated John Reid, famous for his researches on the cranial nerves, was Superintendent and Pathologist at first; he was succeeded in 1842 by Thomas Beavill Peacock, who in turn gave way to John Hughes Bennett in 1844.

In addition to his attendance at the systematic instruction in the University and in the Extra-Mural School, Gairdner was more particularly brought into contact with the clinical teachers in the Royal Infirmary. During the years 1842 and 1843, he was attached to the clinique of Dr. Handyside, who afterwards renounced Surgery, in order to devote himself to the study and teaching of Anatomy. In my own time—thirty years later—Dr. Handyside was an interesting character; but during those earlier years, when Gairdner was attached to him, and when he was flourishing the enormous knife, designed by him for operations on the hip joint, about which so many serio-comic legends are still extant, he must have been memorable indeed. During 1843-44 Gairdner was a clerk with Professor Syme, and appears also to have been turning his attention to Clinical Medicine, under the direction of Dr. Graham. His last year as an undergraduate (1844-45) was chiefly spent under the guidance of Dr. Alison and Dr. Christison. That he impressed his teachers very favourably is proved by the fact that his father received, a short time before his graduation, the following interesting letter from Professor Graham :—

"Edinburgh, 17th July, 1845.

My dear Sir,

I do not in the least doubt that my friend, your son, will with great credit to himself take his degree as Doctor of Medicine in this University at the ensuing graduation. While acting as my clerk in the Clinical Wards of the Infirmary here, I had ample opportunity of observing his character and conduct. He is active, zealous, humane and most persevering in acquiring practical professional information, and I feel confident that he will accept of no situation in which he will not conduct himself with approbation.

You are well aware that to an intelligent mind there are few greater advantages for obtaining practical medical

knowledge than that of an officiating Clinical Clerk in the Hospital here, and of these advantages your son fully availed himself. His mild general demeanour gave me a truly friendly interest in him, and I shall be most happy to find it inspire others with a similar sentiment.

Believe me,

Very truly yours,

ROBERT GRAHAM.

Dr. Gairdner,
18 Hill Street."

During his undergraduate days, the great fight for freedom of teaching was waged between the Senatus Academicus of the University of Edinburgh and the Lecturers in the more ancient School of Medicine belonging to the Royal Colleges of Physicians and Surgeons. The teaching, which had been, for something like three hundred years, carried on, in the first place, in the absence of any University, and, in the second place, outside of the University, was absolutely unrecognised by the academic authorities; and in the days of the "third Monro," anyone who desired to learn Anatomy found it necessary to pursue his studies beyond the walls; he must, nevertheless, attend the prelections of the Professor—or rather, if all tales are true, of the Professor's grandfather—and thus pay fees twice over. So absurd did this unjust regulation become, that a strong agitation began, and ended successfully, in the decision of the Town Council (then Patrons of the University) to issue statutes in 1840 allowing undergraduates to take a proportion of their classes outside. These statutes were strongly opposed by the professorial oligarchy, and an appeal to Caesar was only settled in the name of freedom by the House of Lords in 1847. It is therefore clear that Gairdner's undergraduate days were passed during the period of storm and stress, which led up to the present freedom in teaching.

Amongst his contemporaries in the undergraduate world, many afterwards rose to eminence, and it is pleasant to pause for a time over the names and achievements of those men. Taking them in order of seniority (at least in the order of their graduation) from 1841 to 1848, and therefore including the whole generation with which he, before graduation, came in contact, we find George Skene Keith, one of the most eminent practitioners in Edinburgh, whose *Plea for a Simpler Life* charmed everyone, and whose *Plea for a Simpler Faith* came as a bomb shell amongst the rigidly orthodox; Edward Henry Sieveking, afterwards the distinguished Physician to St. Mary's Hospital in London, and Physician to Her Majesty Queen Victoria, who was knighted in 1886; Robert James Mackenzie, the brilliant surgeon, who died in the performance of his duties in the Crimea; Charles Tupper, famous in Canada as the Right Honourable Sir Charles Tupper, Bart., at one time High Commissioner for Canada in England, and afterwards Prime Minister of the Dominion; John Byrom Bramwell, afterwards an eminent practitioner in Northumberland, and father of the present distinguished Edinburgh physician; Alexander Fleming, celebrated in after years, by reason of his investigations upon aconite; John Struthers, afterwards Assistant Surgeon to the Royal Infirmary, and later Professor of Anatomy in Aberdeen, who, after his retirement from the northern University, was knighted when President of the Royal College of Surgeons of Edinburgh; James Donaldson Gillespie, who became one of the Surgeons to the Royal Infirmary; George Hugh Kidd, the future Master of the Coombe Hospital in Dublin; George Johnson, a famous London physician, well-known in later years as Sir George Johnson; Robert Halliday Gunning, distinguished for his labours in Brazil, in which, during the palmy days of

the Empire, he became a member of the nobility; he afterwards founded the Gunning Scholarships at the University of Edinburgh; William Murray Dobie, the leading physician in Chester, and the county of which it is the capital; Alfred Scott Donkin, who practised for years in Sunderland, and was Lecturer on Forensic Medicine in the University of Durham—an authority on the British Diatoms, and famous for his contributions on the skim-milk treatment of disease; William Rutherford Sanders, afterwards Physician to the Royal Infirmary, and in later years, Professor of Pathology in the University of Edinburgh; James Warburton Begbie, one of the best loved men and most brilliant teachers of clinical medicine ever possessed by Edinburgh; Henry Duncan Littlejohn, in after years celebrated over the length and breadth of the world as a pioneer in improving the public health of the people, and as the most dramatic teacher of Medical Jurisprudence which these islands have ever seen—knighted in 1895; Daniel Rutherford Haldane, who in due time became Physician to the Royal Infirmary, and spent many of his great gifts in the service of the medical profession, by labouring in the cause of medical reform; Thomas Pretious Heslop, the distinguished consulting physician in Birmingham; Thomas Keith, one of the fathers of modern gynaecology; and William Aitken, who distinguished himself at a subsequent period in the Army Medical School at Netley. In addition to these men, all of whom took the degree of Doctor of Medicine in the University of Edinburgh, two other eminent contemporaries studied along with Gairdner, but graduated at St. Andrews—they were Alexander Keiller, afterwards Gynaecologist to the Royal Infirmary, and George William Balfour, the distinguished and learned physician.

Of all these gifted men, the three to whom Gairdner was

most closely bound by the ties of affection were James Warburton Begbie, Henry Duncan Littlejohn, and William Rutherford Sanders. With Sanders he afterwards became linked through marriage, as Lady Gairdner and Mrs. Sanders were cousins. But one of Gairdner's contemporaries as an undergraduate is still with us—the veteran Sir Henry Littlejohn, and my grateful thanks are due to him for some interesting remarks, which may be introduced here :—

" I regret that my friendship with Sir William did not commence in school life. He was not a pupil of the High School. He was educated at the Institution, which still exists, in Queen Street. I regret that our mutual friend, Dr. John Smith, is beyond our reach, else, as he was a schoolfellow with Sir William Gairdner, he could have supplied some interesting details. By the time he came under my ken, Gairdner was entered for the University, where I met him in the Anatomy, and especially in the Physiology, Class—the latter taught by Allen Thomson, with whom I remember Gairdner at the close of each lecture had animated discussions. Gairdner was a close reasoner, and the Professor had enough to do to hold his own. In fact, Gairdner had a highly cultivated mind, and at once took his place as ' facile princeps ' of his fellows. At the same time he had a courteous, engaging manner, was the soul of honour, and had a great and good influence in all our disputes.

" I had the opportunity of observing him closely during our residence in the Infirmary. We were not members of the Royal Medical Society, but were fellow-members of the Hunterian Society, where he was a good debater. Subsequently, of course, we also met at the Medico-Chirurgical Society, where I was a close attender, as I had to report for the *British Medical Journal*. There Gairdner was sometimes, when quite a young man, prompted to intervene too early in important debates. I, for one, knew the simplicity of Gairdner's character, and that he had acted on the

impulse of the moment, when a sudden opening had presented itself to his eager mind.

"He was at an early period remarkably facile with his pen, and often gave me, as editor of the *Journal*, valuable assistance in the way of reviewing. To sum up, he was a most lovable man, and spent a most useful life."

In addition to his distinction in the ordinary walks of an undergraduate life, he had, it is clear, distinct leanings in the direction of literature and art. It is evident that he had early developed a pleasant style of writing. He had not only a great love for poetry, but he himself wrote it, as well as read it.

A letter from his friend William Smith may be introduced here, as it not merely tells us of the literary pursuits of the two youths, but it, at the same time, gives some lines by Gairdner himself :—

"Millburn Cottage, Sat., Apr. 30/42.

My dear Gairdner,

Much as I have been gratified by the perusal of your poetical pieces, I have yet to thank you for a prior source of pleasure, I mean the confidence which committed them to my care. Men have no more valuable treasure to bestow on each other than their best thoughts and feelings, and therefore, I suppose, there is none which is so seldom given. I have often pictured to myself the advantages, and still more the happiness, of such mutual confidence; but my lot has been too much cast among those with whom I had no sympathy in thought, to enable me often to enjoy it, or indeed to enjoy it at all, except in one instance now long gone by. For fresher and purer feelings than have visited me since boyhood, I am indebted to you, not now alone, but throughout an intercourse which will ever remain dear to me, and which I trust will never end.

You rightly judged that I would find the papers you sent me interesting—very interesting! Everything must be so that helps me to the better understanding of a mind which

I love. I now comprehend your enthusiastic admiration of Shelley better than I did before. Whether any of the pieces I have just read were composed under his influence, I know not, but in many of them it is not difficult to find the germs of kindred feeling, the same love of nature, and the same blending of her influences with the feelings which we refer to ourselves, the same informing of the dead elements of nature with the living spirit of love to all things pure and beautiful which exists alone in the beholder, and only in such beholders as he was—this, which was the music of his being, likewise appears, imperfectly of course, but not obscurely, in these fragments. This harmony of the necessary part of our nature with that which lies within our will, is the Spring of all that is brilliant and enthusiastic in youth; its interruption by the upward progress of reason or the downward progress of passion, the cause of all the inconsistencies and contradictions which we have so often to deplore. Happy the man who can keep it unbroken through life; it is the lot of few, perhaps of none. Happier still, but yet more difficult his task, who can reconcile these contradictions, in whom thought and feeling, reason and impulse, flow in a reunited stream.

Having felt what is shadowed forth in these poems, you could not choose but love Shelley. His dreams must have been realities to you, expressions of what you yourself had imperfectly felt before, claiming kindred with your thoughts. It would be presumptuous in me to attempt any remarks on a species of composition which lies beyond my reach, and therefore I cannot give you a reason for my preference; but, without forgetting the merits of the other pieces, my favourites are "Music" and "Beauty." The conception of the first is excellent, and the words as musical as the theme. "Beauty," though a less aspiring strain, I like nearly as well as the former; the fourth line of the second stanza is a gem itself.

> "How beauteous is Evening, when the Sun,
> With his long lines of softened light, appears
> More glorious than when Day has just begun,—
> As though his beams were dimmed with ocean's tears."

What on earth could lead you to entertain so utterly absurd a notion as that I profess any similar productions? I assure you I never penned a line of aught but sober prose, for a good reason, I was never capable of anything else. Did such things exist they would surely be open to your eye. But indeed you know the prosaic character of my mind better than to harbour the idea except in jest.

I have been struck by nothing more forcibly, in thinking over these pieces of yours, viewed in connexion with the age at which you wrote them, than the immense value of that home education in youth, which trains the feelings and consequently forms the character of the man. I suppose you must have received such training in a very high degree. I have been very unfortunate, or very unwise in this respect.

I have not sent back the poems now. I wish to keep them a little longer, but I will return them into your own hands on Monday evening, if I see you then.

Yours faithfully,

W. SMITH."

It must not be supposed that these literary occupations in any way militated against an earnest devotion to his medical studies. During the winter session 1842-3, he obtained the first place in the Class of Physiology, and on that occasion he received the following letter from his cousin, John Gairdner :—

" Kilmarnock, April 30, 1843.

My dear William,

Of course, the first head in my discourse must be to congratulate you upon your success in the Physiology ; great success indeed, for the first prize, I am sure, is more than you expected, although certainly not more than you deserved, as I can testify, having more than once wakened you out of a comfortable snooze after your hard work with your essay. It must be very comfortable to the parents, to see that their child has learned his A B C to such advantage.

I am also going to be very much obliged to you to do a little piece of work for me, viz. the buying of a few books; for the present, until I make out a list of those I would like to have, the only one that you may be looking for is *Byron*, which you must get; I was also intending to get *Burns*, but, as there is a copy of him here, which I shall get, I won't require to buy it.

I remain, dear W.,

Ever yours,

JOHN GAIRDNER."

Some of Sir William's interests during his undergraduate existence are referred to in a letter from another friend, Mr. W. L. Nicholson, which is now introduced :—

"Middlesborough-on-Tees, Oct. 5, 1843.

My dear William,

Many thanks to you for your kind letter, upon which, after being long looked for, I was at length privileged to cast mine eyes. I do assure you that your letters, written in the midst of pressing business, or, as you say, as "peace offerings" to your anxious expectants after business has somewhat relaxed in urgency, are opened by me with more heartfelt gratitude than those epistles indited by friends, who, honest souls, say they make it the chief business of their evenings to keep up their correspondence. I was exceedingly glad to hear that you had managed to get a short visit to Ayrshire, and part of the Highlands, and I have no doubt that you and your brothers enjoyed it very much; as I am given to understand did also the Perigals[1] and their fair friends, the latter of whom being enchanted with the scenery, as also with the company whom they accidentally met, one of the ladies declaring, if I am to credit the testimony of my informant, that she had never met before with so romantic a young man as Mr. Wm. Gairdner. This comes of walking among the fairy regions of the Trossachs by moonlight.

[1] The family of the distinguished landscape painter, Arthur Perigal, R.S.A., of Edinburgh.

You seem to have had very severe duty at the Hospital, and no doubt will be congratulating yourself that November is fast approaching. You mention Dr. Handyside as the operator. A surgeon and apothecary here, a Scotsman from the neighbourhood of Lanark, and formerly at Doune, with good practice there, and with whom I sometimes have a chat, tells me that he and Handyside were fellow-students.

Another doctor is a queer stick from Cumberland, a very good mathematician, who talks of his acquaintance with Sedgewick, Whewell and Wordsworth. Talking of the latter, I forget whether or not I told you that he honoured this town with a visit. I saw his back and his cap and umbrella.

Has Professor Forbes been over to the continent this summer? But I suppose that he has more gallantly been attending to his bride instead of exploring so boldly the "crevasses" of the glaciers, or ascending the far-famed precipices of the Jungfrau. But if you recollect the conduct of Sir Astley Cooper at a similar period of his life, you will remember that a marriage jaunt may still be made subservient to carrying out professional researches, although, to be sure, there is a material difference between the sunny Boulevards and the dreary expanse of the Mer de glace, however the enthusiastic eyes of the savant may disregard its more forbidding features. Have Agassiz and his household yet flitted from the "Hôtel des Neuchatelois"? I should like very much to get hold of Forbes' book on his Alpine travels.

I shall be very glad to hear your account of the Musical Festival, which I hope will answer your fullest expectations. Braham, the celebrated singer, gave a concert last night at Stockton, but I did not go. I hear not very favourable reports of it. He must now be far from what he once was.

I shall be very glad to hear from you soon, and with my best respects to your father, mother, sisters and brothers,

I am,

Your sincere friend,

WALTER L. NICHOLSON."

In a letter from Mr. W. L. Nicholson, dated 21st January, 1843, he says :—

"I shall be glad to hear from you what you care about, and how Galen, Hippocrates, Schiller and Goethe are pleasing you. I sometimes see in the Supplement to the *North British Advertiser* some extracts about these gentlemen, which I suppose you and your friend, Mr. Smith, have had some hand in putting in."

The firm hold which literature had upon his mind in these early days is proved by a long letter to Mr. Niven of the Edinburgh Institution, on the management of the Composition Class, which had long been a feature of the school. The letter is characterised by originality of view and strength of opinion, and as it bears upon his future tendencies, part of it may be given here :—

"18 Hill Street, 5th October, 1843.

Sir,

Taking advantage of the liberty with which you have favoured me, I wish to make you acquainted with a few of the ideas in regard to the teaching of the Composition Class, which the instructions of two successive masters suggested to me. Mr. Nairne and Mr. Little pursued very different methods, and by observing closely the adaptation of each system to the wants of my own individual mind, I have formed some views upon the subject, which I hope may not be altogether useless to you.

The Composition Class has always appeared to me the most important in the Institution plan of education. It is the last and highest class in the school, and stands intermediate—with some, between school and college education —with others, between school-discipline and the harsher discipline of the world. You receive boys ; you are to send forth, in as far as you can, men. You receive your pupils from all the other classes indiscriminately, a multitude, with destinations as various as their characters ; you are to give such instruction as may speed each individual upon

his after way, and be as little as possible superfluous to any.

Your pupils have come from many classes, and each one has his peculiar faculty; but it is the memory and the attention that have been principally addressed; the judgment, the intellect proper (although not *neglected*, if the teachers have been true to their duty) has not yet been systematically cultivated; the time had not yet come for doing so. Many treasures have been amassed, many facts stored, several languages, each opening up a world in itself, have been more or less completely learned, but now something must be done with those treasures, if all hitherto learned is not to go for nothing. The annals of history and the details of geography must combine to furnish important views of the state and progress of man upon earth; mythology, if it is to be interesting, must unite with history to exhibit the childhood of humanity in these its wonderful creations; classical literature must elevate itself out of the region of parsing and construction, and assume its place in the mind as philosophy, history, poetry. So, too, grammar must cease to be associated merely with the learning of parts of speech and rules of syntax, and must become subservient to the lucid disposition and arrangements of ideas, and increased facility of communication between man and man: and the habits of generalization and of rigid logical accuracy, which the study of mathematics tends so eminently to form, must be carried into other fields of thought, and be employed on other, perhaps yet higher, objects.

You are not expected to do all this. That would be an Herculean task, indeed an impossible one, as each one must do it for himself; but an institution like the present has not done with its pupils till it has endeavoured by some systematic method to set them on the road towards this desirable end. Boys generally enter your class at a period of life when the development of the manly intellect has just begun; it is an eventful period, and much depends on the golden opportunity being seized. The intellect, still undeveloped, must be brought forth; the boy must be *taught*

to think, led to self-cultivation; for unless the reflective faculties are quickened into energy, there is too much danger that they may be hereafter quenched in the spiritless routine of daily business.

It appears to me, therefore, that the objects of the class in question are threefold: first, to accustom the pupils to free and original thought, and thus draw forth the intellect and encourage the genius of *each* individual; secondly, to exercise them in the arrangement of ideas, and the lucid and accurate expression of thought, whether original or not; and thirdly, the study of the best English authors, and of the history of English literature, as a special means towards the attainment of both the preceding ends.

The next question is, how are the objects to be carried out? The difficulty here arises partly from the nature of the studies, which require much more the earnest co-operation of the pupil than the earlier studies; but still more from the diversity of character of the pupils, which is beginning at this age to be much more definite, and which renders necessary a plan, which shall adapt itself to the wants of each. These difficulties, however, appear to me by no means insurmountable.

Leaving the details of hours, I have now a few words to say on the giving out and preparation of these essays. This part of the business you probably know pretty well, but I have one or two suggestions to make. I have before said that in this class you must trust more to the boys themselves than in former ones; you must work more by confidence, and less by restraint. Under a hard, inflexible system of restraint the development of free individual genius (which is one of the special objects of this class) becomes nearly impossible, and it is clear, therefore, that your policy is to remove certain restrictions, in order that the willing and well-disposed may be led to results which no system of compulsion could produce. You will have in your class many orders of talent; all these must have suitable work provided for them; those who manifested the dawning of a certain amount of creative or original genius must have it

in their power to work towards the development of this; while with those of a mechanical turn of mind, who are incapable of this higher exertion, your object must be mostly confined to improving them in the art of correct composition. The way in which this originality may be best met appears to me the following:

I would retain the old distinction of essays and exercises, the former term being applied to compositions wherein the student was permitted the arrangement and treatment of the subject in his own fashion, the latter meaning simply a series of written answers to questions propounded by the teacher. As you have Mr. Little's manuscript, in which the plan of the exercises is so well given, I need say nothing on this score; but I have long had my own ideas as to the essays, which are evidently the part of the business destined principally to accomplish the higher ends of the class. My first recommendation is in conformity with an opinion I have before expressed; and it is to leave the students perfectly free in the choice of subjects. This I conceive to be of the highest importance. You demand an essay at a given period, but impose no set subject. For the benefit, however, of those who would like to have subjects hinted to them, a list should be made out of a considerable number of subjects of various kinds; the adoption of any of which, however, should be entirely optional. In doing this, I would carefully point out to the boys a distinction in the grade of subjects; that some are such as to require a certain amount of originality, while others may be taken from books; that therefore, *cæteris paribus*, the former class of subjects is to be rated at a higher level than the latter. For the sake of distinctness, I would also arrange the list according to these two orders.

Believe me,

Yours very truly,

W. T. GAIRDNER."

His success in his medical studies is rendered clear by the following letter from his aunt Cecilia :—

"13th July, 1843, London.

It is a long time indeed, my dear William, since I have given myself the pleasure to write to you, and it was more than kind of you in the middle of all your heavy occupations to find time to devote to me, who deserve it so little. But be assured at least that neglect or indifference had nothing to do with my apparent silence, and that, on the contrary, it has been often in regard to you that I have abstained from writing when out of my scanty stock of information I could not spy a single subject of interest to you.

I have been told of your Herculean labours of this winter, and of their gratifying reward in the shape of those prizes so hardly earned and well-deserved, but I hope I shall not hear of such *tours de force* again, and that you only wait for the arrival of our Jack[1] to give yourself the leisure that after such exertions you must require so much. From his mother's letter of yesterday, he has now decided to go on the 22nd, and dined last night at Mr. Perigal's to make arrangements about his intended fair *compagnons de voyage*, from whom he received an invitation to dine to-day, that, in personal interview, they should effect that object better. I hope he will have good weather during that happy month, and that you will be able to take some agreeable little jaunts together, for really he has set his heart and soul on it, and only grudges that he cannot prolong his leave by all the solitary holidays that occur in the course of the year. His heart is at home even more now than when he left it for London, which had at first the attraction of novelty, but has now lost it; he sees it now in all its dreariness, and the loss also of his only companion, in whose society he should have sought for compensation, makes him revert with double force to that dear home where he possesses so many dearer still. But in your anxiety to give him a hearty welcome, pray have in view not to make him feel poor London odious afterwards! I shall depend on his eloquence to persuade his uncle and aunt to come and make our next winter a

[1] Gairdner's brother, John Smith Gairdner.

happy one, though I scarcely trust myself to hope anything on the subject. My German has not progressed, or I would have tried the effect of a petition in that powerful language. I think that if Clara had taken to it, it would have given me encouragement, and my homely occupations have often served me as an *oreiller de paresse*, when my conscience reproved me for my total neglect of it also.

Friday, 14th July. I had intended adding a good deal to-day to this letter begun yesterday, but the postman has come before I could accomplish my intention, and rather than make an old affair of it, I will send it as it is. Take the intention for the deed, my dear William, and once more excuse une lettre qui ne dit rien—sinon que je ne cesse de penser et d'être au milieu de vous, et que je vous prie de me rappeller de la manière la plus affectionnée à toute la famille dont étant *le plus grand*, vous ne manquerez pas de prendre la plus grosse part. Badinage à part, dearest William, believe me, your most attached aunt.

C. G.

Clara had this morning Macrae's letter, to which she will not be long, I suppose, in replying, and Jack had one from Bob, giving no news. He (Jack) had been delighted with his dinner-party of yesterday, and promising himself all sort of pleasure of his intended trip with his fair charges ! ! ! "

Engrossed both with his professional studies and his private pursuits, he undoubtedly showed tendencies to burn the candle at both ends, and indeed, on several occasions, manifested a liability to break down altogether. His family were quite well aware that he was labouring rather too strenuously, and the affectionate aunt already mentioned writes to him as follows :—

" What will be the astonishment of my dutiful nephew when, instead of the bursts of gratitude he, no doubt, expects in return for all the Galbanum he has lavished on his old aunt, and this so delicately, so lovingly too, as like a gentle shower in April to have penetrated easily to her

heart, and there given growth to a host of vanities and self-conceits—what will be his surprise to receive but a hearty scold!—a scold, a thundering, I should say. When I find that whilst one and all in the family here equally condemned his ill-practices, I am the only one bold enough to come forward and put a stop to them. How do you dare, sir, to dispose of, and to take such liberties with yourself as you have allowed yourself to do lately? and of what avail, pray, are all those important studies if, when they are completed, you are finished too?—*fusé, consumé*, fallen to ashes like a bit of tinder before it has been of long service. Right glad I am that once it happened you fell three times asleep, if it can but teach you not to overstep again the modesty of nature! Why, a laudable thirst for information in our bosoms is all very well, but if drenched when only it should be quenched, to the grave we go, instead of reaching the summit of our ambition. Thus you see that with all my love for books, I have taken special care not to let it work to my detriment; only follow my example, and you are perfectly safe, I assure you. Of it you can judge yourself, when I say that I have not the smallest report to make of any book or study begun since I last wrote to you!

Adieu, my dear William.

Believe me in joke, as well as in earnest,

Your affectionate aunt,

CECILIA GAIRDNER."

During the last year of his undergraduate life, Gairdner, in the exercise of his hospital duties, contracted typhus fever, and, apparently, the attack was a serious one. Among the letters which have been preserved of that date, there is one from Mr. William Tennant, the uncle after whom Sir William was named, which shows how anxious the family were during his illness:—

"Saturday evening, 26th Oct., 44.

My dear Macrae,

Tho' relieved by your letter of yesterday on the whole, yet we regret to learn that poor William suffers so

much from the distressing state of mind which the fever has thrown him into. We would endeavour, however, to trust in the assurance your father and the other medical gentlemen have given, that there is nothing in the case that is not the usual concomitant of the complaint, and that it is not one of an aggravated nature. I do hope and pray that a few days more will relieve him and you and us all by symptoms of abatement in the violence of the fever. When you write again, send the letter, as you did the last, by that evening's despatch, which reaches us the next forenoon —I got it at the Custom House at 12. Half a dozen lines are all we ask for, just to keep us informed of the progress of the complaint every 30 or 40 hours.

I have nothing further to add, but send love from aunt N. This is her preaching week—and she has been at church both yesterday and to-day. Mrs. Mitchell went with her, and means to go again to-morrow. She leaves us on Tuesday or Wednesday.

Believe me, dear Macrae,

Yours sincerely,

WM. TENNANT."

The same anxiety is also shown by the following letter from Gairdner's cousin, Clara :—

"Bowden, 31st Oct., 1844.

My dearest Macrae,

What news has this morning's post conveyed? Oh my poor dear sister, how fully do I enter into your distress! How unlooked for these sad tidings of your poor brother. God grant that he may yet overcome this terrible fever. I shall pray with you, and for you and him, my dear sister, that God in His mercy will spare a life so dear, so precious. My poor uncle and aunt, my heart bleeds for them, for you, for all in this agonizing moment. Thank Uncle John for his kind considerate letters—we got all at once this morning. Pray repeat them frequently—as often as you can, but one line to say how he progresses. I had meant to write yesterday, ashamed to have delayed so long.

I am glad I did not, but I shall say no more now, all else is absorbed in the all-agonizing idea of your misery and his sufferings. God grant they may quickly be over. I can but repeat the same thing over and over again, that you have our deepest, fullest sympathy. Our hearts and thoughts are all with you—would we were there too. Tho' poor comforters we should be. All are pretty well here—mamma much better.

Adieu! my own dear, dear sister. God support you in this trial. I can say no more—these are not words of course.

Fervently does my heart join with you in prayer that the impending calamity be averted.

Your loving

CLARA."

Fortunately, in due course, a complete recovery ensued, and Gairdner was able to resume his studies and complete his course by taking his degree in 1845.

It was considered advisable that, after graduation, Gairdner should have a long rest, and an excellent opportunity offered itself, when he was asked to accompany Lord and Lady Beverley (afterwards Duke and Duchess of Northumberland) to Rome, as their medical attendant. His residence in Rome had a powerful influence over his mind—an influence which was exercised to the end of his life. Some of his impressions of "The Eternal City" are recorded in the following letter to his brother James :—

"Rome, November 21st, 1845.

My dear Giacomo,

I have to thank you very much for your last letter, as well as the one before, which Macrae finished and sent off. Your last is so immensely improved in the handwriting that no person could possibly suppose it to be the same as the former, from which I see clearly that business habits are beginning to take effect upon you in this respect.

You will soon throw my handwriting, which has hitherto been a model to all Hill Street, into the shade !

I am sorry to begin my letter with a calamity ; but although it is of more importance to myself than anyone else, I tell you it, because it happens to be uppermost in my mind. The little sketch book, which I carried about with me, and which contained all that I had done on the road in the way of drawing, except two sketches, is gone, having fallen into the hands of a professor of art—that art, namely, so well known in London, and which requires the lightest fingers of any. Could anyone have suspected that in Catholic Italy pick-pockets should ply their unholy trade within the precincts of St. Peter's ? but so it is, and though abundantly provoked and annoyed, I was left to console myself by thinking that he had got nothing that would be of any use to him. I felt him nibbling, just in time to save my handkerchief, but Chambery, and Genoa, and Spezia, and all my French head-dresses and foot-dresses are gone for ever.

Well, misfortunes will come, though they put one's philosophy a little to the proof. The occasion of this one was the celebration of the anniversary of the dedication of St. Peter's, by vespers performed before the cardinals with the peculiarly intoning effect produced by 40 or 50 of the best singers in Rome, and two organs ; in short, what Mr. Murray would call ' the whole strength of the company, and an orchestra considerably augmented for the occasion.' Vespers are performed daily in St. Peter's, in the Cappella del Coro ; but generally with only one organ, and about 9 or 10 good voices. This chapel is separated from the rest of the church by 3 grated iron doors, which are thrown open at the commencement of the service. The public, however, have to stand outside, or crowd about the doors, for the interior is appropriated to the officials and the chapter of St. Peter's, who are a congregation of themselves. The cardinals, when present, occupy the highest seats, and are clothed in purple and scarlet robes, with a little scarlet cap, which covers the shaven crown. The canons have the

shoulders white, the body purple, and the cap black, and all of silk. Others all white, but of coarser material. The officiating bishops have stiff white silk or satin robes, spangled all over with gold and silver, and so made as to fall to the feet without folds; this is surmounted by the mitre of similar materials. To complete the display of costume, there were on this occasion about a dozen of the Swiss Guards to keep back the crowd, and make good with their halberds an entrance for the procession of priests. They are a set of stout fellows, with red plumes in their black hats, and a dress striped from the neck to the heels with up and down alternate stripes of red and yellow, and they wear their hats the whole time, though everybody else is uncovered in the church. The bishops on entering are preceded by a number of crosses and other insignia, and by about 20 boys and young men in priestly dresses, beginning with the size of Kilmarnock Davie, and gradually mounting up to the full height of a man, and they are followed by a large number of inferior officials. In regard to the service, I say nothing about it, because I don't understand it, having not yet got my missal, which is on the way from Paris to make a Catholic of me. After the conclusion of this service, there was an exhibition of relics, in which St. Peter's is very rich. It has 3 pillars from the temple at Jerusalem. Two of them serve to adorn an altar, but one is kept apart and locked up as being that against which Christ leaned when disputing with the doctors. All the altars were lighted up, and the 120 lamps, which burn night and day round the shrine of St. Peter, were reinforced by additional lights. As the daylight began to fail, lights appeared at the balcony, where the most sacred relics are kept, bells were rung, and priests and people knelt while they were displayed. They keep here a piece of the cross, cased in a framework of precious stones, a piece of the lance which pierced the side of the Saviour, and the handkerchief of St. Veronica, which is said to have been impressed by his features. All this may appear very extraordinary to you, but it is nothing at all to a Roman who has had his

faith properly disciplined; for a man may believe anything if he only makes up his mind, as a first step, to believe whatever he is told. If a Protestant, in common charity to his brethren, is disposed to believe that some of those things may be true, then there are plenty more remaining behind for him, all of them resting on the same evidence, tradition, and on the same authority, popes and canonicals. Thus there are abundance of Madonnas all over Italy painted by St. Luke—there is a crucifix at Lucca fashioned by Nicodemus, and an emerald vase at Genoa presented by the Queen of Sheba to Solomon, and which turned out, when brought to daylight at the time of the French, to be glass. Besides these, for which the Pope may possibly not be responsible, there are plenty in Rome itself; at the Lateran is a piece of the table of the Last Supper—close by are the stairs of Pontius Pilate's house; at S. Prassede is the column to which Jesus was bound, and a well in which the saint collected the blood of I don't know how many martyrs; at the Ara-Coeli is a wax-work infant Christ, or *bambino miracoloso,* which was left in the court by an angel who flew back to heaven immediately after; and at San Pietro, in Vincoli, where I was yesterday, you may hear how the Empress Eudoxia brought from Jerusalem the chains that had bound St. Peter, and gave them to Leo X, and how Leo, who was already in possession of the chain which had served the same purpose in Rome, brought the two together, whereupon they were miraculously united, and remain to this day one chain. What refuge can be found from such stories, and the host of miracles done by saints of all ages, and given as undoubted facts in the Breviary? Without indulging in rather uncharitable suppositions, I am at a loss to conceive at present, but no Catholic has any right to doubt these things, unless perhaps it be the sacred emerald, or some of St. Luke's paintings, which are not so well authenticated.

What a delightful thing it is in the midst of such monstrous confusion of fact and fable as meet you here at every turn, to find out the real materials of history—the true and

undoubted relics, which prejudice and interest have been unable to pervert, or have not dared to touch : to turn from unstable tradition to marble busts and bas-reliefs—or to monumental inscriptions, where the thoughts and feelings, common life, and actual character of bygone nations and individuals are recorded in such a way as to leave no room for scepticism. The reality of these things has an impressiveness around the tinsel and humbug of modern Rome, which cannot be imagined from a description. If a man does not contract a little antiquarian rust here, there is no hope for him.

There is a corridor in the Vatican, which serves as an entrance to the museum—it contains only a few sarcophagi and mutilated statues, but the walls are rich in this kind of marble history. There are four hundred feet of wall, on each side crowded from top to bottom with stones inserted into the plaster, and bearing monumental inscriptions. On the one side is the ' Dis Manibus ' of the Romans, on the other the ' Requiescat in pace ' of the early Christians. Many of them are mere names, but every now and then comes a designation or a name of a trade, a bas-relief, a sentiment, or a symbol of faith, something which carries you far back into the ages, and gives the material for serious reflection. Apart, too, from the grave interest excited by such things, there is an inexpressible comfort in being assured with your own eyes, that those immaculate ancient Romans were so far human as to spell very ill, and write grammar quite as bad as any that we used to lose places for at school. This gallery has a kind of fascination for me, and I have stood reading in it till the closing of the doors, quite dead to the sense of anything beyond. And yet beyond are furlongs of Greek sculpture—the glorious frescoes of Raphael, and the finest oil-paintings in the world. How unspeakably rich is the Pope with his paltry income of £30,000 a year.

I have seen the inside of more churches since coming to Italy than before in my life. Rome has a prodigious number, which are remarkable in some way or other—some

for rich marbles, some for beautiful paintings, or lovely sculpture, or great antiquity, but very few for anything like grand simplicity of architecture. Of course, the classical styles prevail everywhere ; there is but one Gothic church in Rome, and that a very ugly one, built on the site of an ancient temple of Minerva. But the strangest thing about them is the way that antiquity starts out in their furnishings. The greater part of the columns are often antique, particularly the granite, of which the conquerors of the world imported a vast quantity from Egypt, and the finer marbles, porphyries, the *rosso*, and *giallo*, and *nero antico*, some varieties of which are, I suppose, not to be had now from the quarry at all. Porphyry is to be got in plenty, but the art of working it is lost,—no great loss to my taste,—for so glaring and variegated a material does not suit sculpture, and in architecture only goes to make ugliness magnificent, or grandeur hideous. I think it is in S. Maria in Trastevere that the Corinthian columns of Egyptian granite have figures of Isis and Serapis carved upon the volutes, and if you look a little closely into the altars and bas-reliefs, you are often enough startled with little bits of Paganism in the most sacred places of the Christian's temple, but nothing is disused here that can be turned into ornament ; and, for myself, I think a little Paganism quite as good as some of the Christian decorations, and infinitely more pleasing ; quite as good, for instance, as the majority of the saints and martyrs in St. Peter's, and far better than the martyrdom and diablerie of the Gesu.

But I must begin to draw this rambling epistle to a close. If you want to know anything systematic about Rome (which you are not likely to get from my letters) you should look at my father's *Rome in the Nineteenth Century*. In his last letter, my father tells me he has no news, and then ends with a notice that the Glasgow fish is about to drink up Loch Katrine ! Please to give me some particulars. I am glad to hear of your jollity. I hope I shall have another letter from Macrae soon, as I begin to be a little

impatient ; all the more so that we are under the influence of the Scirocco.

W. T. G.

P.S.—What a thing a storm is here when it begins ! The rain began as I wrote the last word of the letter, and the streets are already full of rivers. Bright lightning too ; and thunder tearing the sky to pieces."

He came back from Rome early in the summer of 1846.

HOSPITAL AND PRACTICE

On his return from Rome, he resolved to settle in Edinburgh, and at once took steps in the direction of active medical work. The first letter written to his brother after settling down in Edinburgh gives an humorous view of his position :—

" 24th August, 1846, 18 Hill Street, Edinburgh.

Dear Jim,

I beg pardon a thousand times for having neglected you so long, but I believe you have been tolerably well supplied with letters since you left us ; and certainly you have done your own part very well. I am delighted to hear of your having taken so well to your new duties ; and have no doubt you will some day or other light upon some interesting legend of our most illustrious family, which will amply repay you for all your researches amid musty Latin and mustier English. The fact is that ever since I discovered, through the medium of one of my dispensary patients (a man skilled in heraldic lore), that Colonel Gardiner, of Prestonpans memory, was descended in a direct line from Charlemagne, I have had no doubt whatever of our family history being mixed up with all that is great and noble in the history of Europe. So you need not be much surprised, if you do light upon some such document as you refer to in your letter to my father. Did you never feel those lofty aspirings within you, which are the surest indications that you are sprung from no serf's blood ? The ' aspiring blood

of *Gairdner* will not sink into the ground' without asserting its innate claims to nobility, I assure you.

John is determined to allow me no rest while he is here. One day there is an excursion to Larchgrove, another day to Burntisland. In short, he pulls me about just as he pleases, and will not allow me even a day among my medical cronies to discover what is going on in town. A little more of this kind of thing would, I don't doubt, rid me of the whole of my large and extensive practice, which, however, I am happy to say, has not yet become lessened materially in consequence of my truant habits."

He was appointed Resident Clerk to Dr. Duncan on 7th September, 1846, and, at the beginning of the ensuing winter term, he became House Physician to Dr. Andrew, then Junior Physician to the Royal Infirmary ; he continued in residence for eighteen months, serving under Dr. Andrew and Dr. Douglas in rotation. In the summer of 1848, he was House Surgeon with Mr. Miller. Amongst his fellow-residents were Daniel Rutherford Haldane, James Donaldson Gillespie, and James Warburton Begbie—all of whom were afterwards his colleagues on the staff of the Royal Infirmary. From the first, the dominating traits of his character were seen. Even when deeply engrossed with his duties as a house physician, he threw himself into clinical observation and original investigation with unremitting energy. A glimpse of his life as Resident Clerk is given in the following letter to his brother James :—

" Royal Infirmary, Edinburgh, Sept. 26th, 1846.

My dear Jim,

I take my pen as soon as the diminished burden of a severe cold (which I have had a full week) will allow me, to discharge a debt in the literary way, which I have lain under to you for some time. I had a letter from you before John left, upon a great variety of subjects ; none of which,

so far as I am aware, can be recurred to now with much advantage. You seem to be really in all respects very comfortable, and I am glad it is so, and that you are now beginning to find yourself at home in the buzz of the city, and amid the stir of business. If you accomplish the great feat of becoming a practical man, and reconciling your conscience to the ordinary pursuits of men by keeping it untainted in the midst of them, it can scarcely but follow that with the 'lair' to back you, you must become a most successful and estimable recorder.

I am tolerably comfortable here as to living; and have, as you may suppose, plenty to do. Three days in the week I am obliged to keep the limits of the Hospital, in order to be in the way in case of an accident occurring in the wards, or of some untoward event out of doors, causing somebody to come to us with something less than the average number of fingers or toes. The public are always wonderfully attentive to the interests of medical students, keeping the hospitals respectably supplied with 'beautiful cases,' and the railway and engineering work at present is so fertile in this kind of crop, that I think it becomes every medical man, and especially every surgeon, to encourage them to the utmost of his power.

It is dinner time, so for the present no more.

Ever yours,

W. T. G.

P.S.—Macrae has gone this evening to the Italian Opera of Edinburgh along with—whom do you think?—Dr. John Gairdner!!!"

Gairdner's father, in a letter written to his son James, tells him humourously that William had now settled down to practical work—he was acting as House Surgeon to the Royal Infirmary.

"My dear James,

The arrival of your letter last night showed your mother that the contents of her enclosed letter had been

altogether superseded by one which you had got from your sister, and, therefore, she kept it till to-night, in the hope that she might have something to communicate about Tom Bob.

And, accordingly, I have to say that a letter has just dropped in from Aunt Agnes, by which we find that he is greatly better. Having been compelled to give up all thought of seeing his sisters[1] before their departure, he had made up his mind to remain at Kilmarnock for the rest of the present week. He will probably come in about Saturday.

We look for your uncle and aunt from Craigend before that time. As to Uncle Hamilton, I have lost sight of him. We suppose that he is in some corner of our planet, and will turn up in due time.

Our weather is now decidedly Octoberish, but we have not yet begun our winter fires, though the early supervention of night shows that we must do so ere long.

The girls must have had a glorious passage.

Yours affectionately,

J. G.

Willie is fairly entered to the *varmin*, like Dandie Dinmont's 'Pepper' and 'Mustard.' A case of attempted suicide by a drink of acid was brought to him on Saturday night. On Sunday morning at an early hour the swelling of the wound in the throat threatened suffocation. The only remedy was the surgical operation of opening the windpipe lower down. This he undertook successfully, there being no time to wait for the regular surgeon. The man was relieved instantly from his breathlessness. But the case has ended fatally from other causes.

Edinburgh, Monday,
5th October, 1846."

The next letter shows that, in the midst of hard medical work, Gairdner was not unmindful of the muses : —

[1] They both went at this time by sea to Rochester on a visit to their uncle, William Tennant.

"Royal Infirmary, Oct. 13th, 1847.

Dear Jim,

I have nothing but this stuff called foreign letter paper to begin upon ; but as a great man is not to be stopped by small matters, here goes.

I received your reminder of the 7th, for which many thanks, and particularly for your next year's anticipations, in which I cordially concur. In the meantime, here I am hard at it again. To the best of my recollection, I have been at Arthur's Seat once, Roslin once, Dean Bridge once, Links once, Granton and Caroline Park once, and the theatre once, since you left ; when I have mentioned these escapades, I think I have enumerated the whole of my memorable doings since that time. No! by the Holy Poker! what was I about to forget?—my dear, exquisite Jenny Lind, whom I was fortunate enough to hear, by the kindness of Mr. Hardie, who sent my father a ticket ; otherwise, after being squeezed to a mummy in the endeavour to get tickets for Macrae and myself, I should have been obliged to give up all thoughts of hearing her. I am now most sorry that Macrae did not get a ticket ; for, if ever there was delicious singing on this earth, it is hers. But I need not bore you, who are at once a Londoner and no musician, with the merits of Jenny ; suffice it to say that, hearing her only a few days after Grisi, I do think her singing quite unique. Mr. Bordier would possibly tell you that, when I went to the train to see him off to London, we found Jenny was to go by the same train, and accordingly, after a little while, down she came from Tait's Hotel in a chariot, accompanied by a large crowd of people.

I am reading Sir W. Scott's life with great interest, and have got to the end of the 5th volume, reading every word, in spite of its great length. He was a noble old fellow, with a true Scottish heartiness about him that was worth anything.

We are to have Fanny Kemble here shortly. She is to begin with Julia in the *Hunchback*. I agree with you in thinking this by no means Miss Faucit's best character,

nor can I conceive how it should be anyone's best, for I think, despite some fine passages, it is a piece of downright folly. It does not act one-tenth part as well as the *Lady of Lyons*, and does not read one bit better. The actress who does it must of necessity rant, and Miss Faucit's acting, usually so natural, is in this part a gross rant, which does not affect me in the slightest degree more than a good melodramatic stage queen.

I must stop. If this letter makes you one half as sleepy as I am at this blessed moment of time, you will find it absolutely necessary to make a point of not reading my epistles during office hours.

Adieu, therefore, before the drowsy god gains the upper hand entirely, and believe me,

Ever yours affectionately,

W. T. G.

P.S.—If 71 Newman Street ever lies within your beat, you might call on my Roman friend, Brodie; introduce yourself, and see his studio and London doings. He called here lately, being down on a visit."

Another letter, written near the end of his term as House Physician, deals with the approaching election of Hughes Bennett to the Chair of Physiology, and refers to one of the two great riots of last century in the University.

"Edinburgh Infirmary, Feby. 22nd, 1848.

My dear Jim,

I am in your debt for a letter, and also for a very interesting number of the *Illustrated London News*, which I have had by me for a fortnight. It is certainly a most curious and unexpected discovery, and must have startled the merchant princes of your far-famed city thus to find a trace of Roman luxury in the very midst of their daily walks. I dare say that for solidity of structure it makes most of their villas look blue. The views seem very well drawn, at least they look very like the fashion of the bricks I used to see in the glorious days when I lived in

paradise among the visions of antiquity. I cannot say, however, that I ever was an antiquary in the true sense of the word: at least, I never could bring myself to understand thoroughly the different kinds of masonry and brickwork, although I have a very general notion what *opus reticulatum* is. This is all the more sure a proof of my bad taste, that I was constantly in the company of architects, who, however, seeing my natural bias, generally managed to avoid boring me with their technicalities more than I liked.

Did you see the panoramic view of Paris published in the *Illustrated News*? It was exceedingly good, being taken from the top of Nôtre-dame, and exceedingly well executed. I have bought it as a momento.

I am glad to hear from my father that you have taken, even though in a small way, to literary labour. I always think that you government men must have a large superfluity of time on your hands, which, as you are forbidden to read or write politics, could not be better employed than in the unoffending field of literature. I see that Bohn is looking out and advertising for editors of classics. I have at times seriously thought that, if you could get introduced to him, you might be the very man to do him some Greek. Have you ever thought of such a thing?

I have been doing my share also lately of proof-correcting and other work, as Dr. Bennett, who is editor of a medical journal here, being candidate for the Chair of Physiology, has thrown a great deal of his work upon me. You are probably aware that Dr. Allen Thomson has got old Jeffrey's Anatomical Chair in Glasgow, which makes a vacancy here. If you happen to have any interest with the Edinburgh Town Council, I bespeak it in favour of Dr. Bennett, who is, I think, decidedly the right man. Dr. Thomson has lately been concerned in a matter which will probably send him away from this by no means so much a friend of the students as when he first came. You have probably heard that the hot blood has been getting up here again between the students and the police. A riot arose which was in its

origin purely accidental, but which by the bad feeling and misconduct, I believe, of both parties, grew into rather a serious matter. The professors let the row blow over, however, without saying anything about it. About a dozen students were apprehended and brought before the Sheriff, and bound over to keep the peace in sureties of £20 each; a demonstration took place the next day, in which 400 students marched in procession through the town, but which ended peacefully. Since this, however, four of the supposed ringleaders in the riot have been rusticated by the Senatus; a shabby enough proceeding, I think, considering that no public warning was given, and that the authorities had already pronounced judgment upon the riot as a civil offence; and at all events a most stringent and severe punishment.

Be this as it may, Allen Thomson is reported to have been, with the Principal, the chief hand in rusticating the students, and I suspect he will be very unpopular during the remainder of his stay here.

I remain, my dear Jim,

Yours ever,

W. T. G."

During this period he laid the foundation of the admirable series of papers on the Pathology of the Kidney, which appeared in the *Monthly Journal of Medical Science*, within two years of his return to his native city. These papers contain much that is absolutely original, particularly in regard to waxy or amyloid degeneration.

Gairdner became a member of the Medico-Chirurgical Society of Edinburgh in 1848. Much of his work during the following years was brought before this Society. He was elected a Fellow of the Royal College of Physicians of Edinburgh, 7th May, 1850.

On 4th September, 1848, he was appointed Pathologist to the Royal Infirmary, and began to lecture upon Pathology.

This letter, written soon after a holiday in Ireland, and the appointment as Pathologist to the Infirmary, shows his first leaning towards the study of Public Health :—

" 18 Hill Street, Oct. 27th, 1848.

My dear James,

I have not heard from you for a long time ; and when I did last hear you did not, I think, mention if you had been dabbling at all in sanitary matters. I send you, however, a proof of a document which appears this month in our *Journal*, being an abstract of the Public Health Bill, which is, you will observe, exclusively for England and Wales. I shall also send you, if I can, a copy of a review on Sanitary Reform, which we publish this time. I have been very busy with this last number, and have only got it off my hands this afternoon. I had to write two long leading articles on the Cholera, besides a great number of other things. I am quite exhausted from want of rest.

I suppose you heard about my Dublin trip, which I enjoyed greatly. They are jolly boys, those Dubliners, and have a store of *bonhomie* and hospitality, whatever be their faults. I was equally well treated by Spring and Old Ireland, not to speak of Orangemen.

We have a letter from John this morning. He has been hearing Jenny Lind twice, and is enormously delighted.

I am sorry to hear that in the Record Office they deal so shabbily with the clerks, as your letter to-night indicates. My mother is gone to Craigend for a few days, so my father opened it.

I wish you would tell me when you write, if you have been thinking at all upon the sanitary business.

Yours ever,

W. T. G."

On the termination of his first course of lectures, in April, 1849, his pupils presented him with a copy of Bright's *Hospital Reports*, along with the following letter, signed by all the members of the class, several of whom were already

graduates, and amongst whom may be mentioned James Warburton Begbie, Henry Duncan Littlejohn, Thomas Keith, John Smith, William Millington, James Struthers, Arthur Scott Donkin, George Edward Allshorn, Alexander Fleming, and Alexander Borthwick :—

" Edinburgh, 12th April, 1849.

To Dr. W. T. Gairdner.

Dear Sir,

At this, the conclusion of your course of Lectures on Pathology, we are desirous of expressing the high value we have always attached to them, and the great regard we have been led to entertain for yourself as our instructor.

In reference to the first of these points ; though the Post-Mortem Theatre of the Infirmary has been always freely open, and the closest attendance invited from all students, still there remained this desideratum, which we believe your course of lectures has supplied, namely, the full explanation of the morbid appearances detected, their exact importance, and the relation borne by them to the symptoms of the patient. By the remarks after each dissection, which you have daily been in the habit of making, and more especially, by the fuller and more comprehensive survey which weekly you have given us of the post-mortem examinations, our knowledge of Pathology has been advanced, and many, we doubt not, have been led to take a warm interest in a subject, their knowledge of which, without such aid, would have been but barren.

We have been greatly pleased with the simplicity with which much important knowledge of this subject may be attained, and has been communicated by you to us ; and while you have on no occasion omitted to direct our attention to the value of microscopic research in Pathological Histology, you have given us what is practically of still greater importance, the possession of a facility of recognizing the principal lesions by the use of the unaided senses.

Lastly, we feel it a pleasing duty imposed upon us, of

expressing to you the high respect and great esteem we entertain for you as our instructor. The position we have held towards you is one in which no other class can again be placed : to us you have delivered your first course of public lectures. That during many sessions you may continue as faithfully and effectually to instruct other classes, and that every desirable blessing may attend you, is the sincere wish of, dear Sir, your obedient and faithful servants."

During the ensuing five years, he published researches on the Pathological Anatomy of Cholera ; on the Pathological Anatomy of Bronchitis and Diseases of the Lung connected with Bronchial Obstruction ; on Aneurysm of the Aorta ; on Pericarditis ; on the Registration of Causes of Death in Public Institutions and in Private Practice ; and on Homeopathic Hospital Statistics. Along with Dr. Warburton Begbie, he also brought out the *First Report of the Medico-Statistical Association.*

Gairdner's researches on Cholera brought him the appreciative recognition of many distinguished men, amongst whom were Sir Dominic Corrigan and Professor William Stokes.

" Dublin, 4 Merrion Square, July 10, 1849.

My dear Doctor,

Many thanks for your paper on the Pathological Anatomy of Cholera. With your observations I fully agree, particularly with regard to the flow of bile.

In many cases here, in 1832, bile in large quantities was found in the blood, generally in the two cava. I believe it has not occurred in any of the cases now noted by you.

I am myself so very much occupied in the ordinary routine of practice at present that I cannot find leisure to look after it in this epidemic.

Sincerely yours,

D. J. CORRIGAN.

P.S.—Excuse hurry."

"Dublin, Merrion Square, July 26, 1849.

My dear Sir,

Permit me to offer you my best thanks for your kindness in sending me your researches on Cholera. I may say that they appear to me to be of the greatest value. Altho' we have not yet discovered the nature of Cholera, yet it is a great thing to get rid of false notions about the disease, and if your examinations do no other good than to make men cautious as to the drug calomel in this affection, they will prove most valuable.

I do not know what your opinions may be as to the question of contagion of Cholera. In Dublin, at all events, we have had so many extraordinary cases of apparent communications of the diseases that it is impossible to resist the feeling that it is contagious. But why should not all epidemic diseases be contagious also?

Most truly yours,

W. Stokes.

P.S.—I have nothing to tell you on the subject of Cholera that is at all new. One of the first cases here presented, as I am told, a loud bellows murmur in the aorta and heart. Within half an hour of the setting in of collapse, I saw the parts, and there was a very large and long coagulum running through the aorta, and capable of producing the signs of imperfect closure. If the case had any interest, it is as showing the rapid coagulation of blood."

The current politics of the time, and especially the No-popery cry of Lord John Russell's epoch, are reflected in this letter and that which follows it.

"Edinburgh, 18 Hill Street, Dec. 9th, 1850.

My dear James,

I need not say that your letter gave me great pleasure. I am very glad indeed to find that you are like to have an engrossing occupation for your evenings. I care little what kind of occupation it may be, so it suits your humour, and forces you into practical contact with

the world—in your literary capacity, at least, if not personally. Have you read *Copperfield*? It is a fine picture of the building up of a literary man; and *Pendennis* contains some fine touches of the same kind.

Depend upon it, "a power of description" is to be acquired. It is not one of the poetical attributes which are born, not made. On the contrary, many a good poet is spoiled by the want of the power or the honesty to describe accurately; and several bad poets, quâ poets, have been set up in business solely by this one attribute. Witness Crabbe. With novelists it is the same. But your pictures must be drawn from the life. Set up a notebook immediately of character, not with the view of taking portraits for publication directly, but of jotting down little fragments of character, which may afterwards be combined into new forms. Let me know what you are cogitating in the way of a Doctor's tale.

But it is highly probable you may not succeed to your mind as a novelist. I would not mind this. I had much rather see you at the grave and earnest business of politics or social reform. But the novel-writing will show you your faults, and do more than anything else perhaps to knock the pedantry out of you. And it does not, unless overdone, spoil your hand for higher work.

Keep up your connexion with the newspapers, however, if you take my advice. Go a-head! I am sorry I did not see the Protestants' letters; the *Chronicle* not being usually one of my papers. Bordier reads it. Did you hear any remarks from him? I suspect he and Uncle W. are among the madcaps on the present occasion; at least, I had a very hot and absurd letter from the latter, which I suppose, however, means, as usual, much less than it says.

This Protestant fervour of *John Bull* is certainly beginning in some quarters to look like mischief to our principles of toleration. But I can scarcely think that, when practical legislation takes the place of public meetings, good sense will not come to our aid. In the meantime the Jesuitical scoundrels of cardinals and bishops richly deserve what

they have got, and I am not sorry to see the middle-age tendencies both of Papists and so-called Protestants brought face to face with the reasonable and unreasonable indignation of Englishmen of 1850. But it will be a lame and impotent conclusion if, after this grand burst of moral force, the frenzy should drive our ministers to fall back upon an act of Parliament. I hope it will not be so.

I like, though, a suggestion which I see quoted in Saturday's *Spectator* from the resolutions of an Orange lodge in Ireland! Can any good thing come out of Nazareth? Yes, truly. They propose to petition Parliament to issue a commission of inquiry as to whether there is anything in the education or practice of Roman Catholics (especially, I presume, the priests) inconsistent with the civil authority of the realm. This, I think, is really a good notion. The Catholics have themselves, by their aggressive act, laid themselves open to such an inquiry, which they could not shrink from without condemning themselves. It would be of service all over Europe. Let in the daylight upon the rogues, I say, but touch them not, save at tangible points.

Pray send us copies of any papers you write in, when the subject is at all important. We shall be very happy to pay for the papers in all such cases.

Ever yours,

W. T. G."

"Edinburgh, 18 Hill Street, March 13th, 1851.

My dear James,

I understand Lord John very well, but, like you, I approve not. It is to me perfectly intelligible how a high English churchman must necessarily take such a view of the Pope's blustering nonsense. The difference between tweedledum and tweedle-dee will not be sufficiently marked to the vulgar unless the former is laid under ban, as regards titles. A bishop is a bishop to an Englishman as 'a book's a book' to Lord Byron, although there's nothing in it. I cannot doubt that this is the real source of the outcry, and that Lord John is quite as sincere in his liberality to the Catholics

and Jews in respect of all these matters as he is bigoted in respect of titles.

But with many others the case is different. Indeed, the revival of what we would fain have thought the obsolete forms of senseless religious intolerance among the laity of England shows me more than anything has done of late, how very far they are behind Scotland in real religious liberality. This cannot be on account of the greater individual power of their pardons in England, nor yet on account of any want of liberal leaders. It is one more added to the many circumstances that daily make me value more that Presbyterianism which our ancestors won for us at so great an expense of our truest Scottish blood, and which, with all the fanaticism and even hypocrisy that it has to answer for, has planted a genuine religious spirit deeply in the heart of this country. We are too deeply and truly Protestant to make a noise about it.

But although this lame and impotent conclusion to the agitation looks absurd at present, I think it will teach an important lesson to the continental states. Our foreign visitors of this year will learn what a strong barrier there is in this country against religious fanaticism of every kind. The Pope counted upon the Catholic reaction in the Church of England, but he no sooner attempted to act practically on that feeling than he extinguished it, and raised a counter-reaction, which would in any other European country have thrown the people almost back upon revolution. Here it produced only a ministerial crisis, and ends speedily in smoke —its chief effect being the postponement of financial arrangements, and the temporary obstacle to a ministerial coalition.

Is the Doctor's tale to share the fate of the Papal bill ?

Ever yours,

W. T. G."

He was appointed Assistant Physician, as well as Pathologist, to the Royal Infirmary early in 1851, and had the honour of election to the Medical Society of Paris soon after. From the following, he was evidently very busy :—

"Edinburgh, 18 Hill Street, April 20, 1851.

My dear James,

We have just got to the close of our session. I have been much distracted for a month past with a variety of matters; and yet on looking back, I can scarcely realise what I would call a full amount of work. I fear I must have been a little lazy after my popular lecturing, which, in spite of your taunts, did a great deal more towards exhausting my energies than towards raising me in my own esteem. I never finished anything that I can remember with so much feeling of relief from depression and anxiety.

I have, however, written an article for the *Medical Journal* this month. The popular lectures set me on a course of old medical authors, and I have been dallying from time to time with Hippocrates and Aretaeus, as well as some others. On Sundays, I am reading a book which would interest you, Mander's *History of the Church*, lately published in Bohn's series. It is very good.

Mammy and Dad are at Craigend. I think Tom must be improved on the whole, but I have not heard their account. I am going this week, probably on Wednesday first, to Glasgow, and thence to Craigend, from which I return in the beginning of May to open the summer classes.

Ever yours,

W. T. G."

The illness of Gairdner's youngest brother, Thomas Robert, to which reference has already been made, forms the subject of the following letter:—

"18 Hill Street, Edinburgh, Dec. 7th, 1851.

My dear James,

You must be prepared by my father's letter and Uncle W.'s explanations almost for the very worst in regard to poor Tom. He is still alive, but only alive; and to all appearance the fatal event cannot be now very long delayed. His condition has been one of considerable suffering till yesterday; and I feel it almost a comfort to be able to

write you that he is so far dead to sensation and the outer world, as to be now apparently very little sensible to impressions, either of pain or pleasure. For some days, we have noticed a tendency to wandering of the mind at times, and along with considerable acuteness of mere physical perceptions, a gradually increasing torpor of the intellect; this, I have no doubt, is the result of the feverishness and failing strength attendant on the very rapid progress lately of the disease in the lung. On Saturday, while I was out, they had a great alarm, and for a while thought that his death was immediately impending; he brought up several mouthfuls of blood, and was nearly suffocated with what remained. Since this he has had very little, if any, suffering.

8th, Morning. I went to bed early last night, leaving my father up with Tom till 4 o'clock this morning. I find on the table a letter to you from him, which tells the same tale as I had written. I have only to add that he is sensibly weaker this morning 9 o'clock, but otherwise in the same condition of stupor and vegetation. I enclose this in my father's note, in all the feelings of which I concur.

W. T. G."

During the year 1852, Gairdner, although still Pathologist to the Royal Infirmary, became a candidate for the Chair of Medicine in the University of Glasgow, and, on that occasion, he submitted to the Home Office testimonials from Robert Christison, James Begbie, John Scott, James Miller, and James Syme of Edinburgh; Thomas Watson, Henry Bence Jones, Richard Bright, Walter Hayle Walshe, R. Bowman Todd, George Budd, Charles West, E. A. Parkes, James Paget, and P. M. Latham of London; Robert James Graves and William Stokes of Dublin; and M. Valleix of Paris. He was at this time only 28 years of age, and it therefore was no surprise to his friends that Dr. John Macfarlane, who was many years his senior, was presented to the Chair. In the year following he became Physician

to the Royal Infirmary (the date of his appointment being 10th October, 1853), and he at once began a course of systematic lectures on Practice of Medicine. His teaching in this department was attended by the same success which had followed his endeavours in Pathology.

The letter which follows shows him in his preparations for the new step :—

" Edinburgh, 18 Hill Street, April 17th, 1853.

Dear Jim,

Many thanks for your trouble on my behalf—the books will be valuable accessories to my medical library, and will be much wanted shortly, when I am concocting my Practice of Physic Lectures—but, as I have access to them in the College of Physicians Library, there is no immediate hurry as to sending them. Take an opportunity if you can find one ; if not, keep them till you come yourself. Hippocrates is a great favourite of mine, as I daresay you know. Even you would find some grand things in him. Look at the exordium of the tract περὶ τέχνης—or at the one on Regimen in Acute Diseases, or at the First Aphorism, or at the book on the conduct of the medical man—and at the famous medical oath, which was kept up in many a university till a late period, and might be revived with good effect now, if oaths are to be revived at all. In the consultations of Hoffman you will not be tempted to make many studies.

I yesterday delivered a lecture on Blood-letting in Acute Diseases to certain Fellows of the College of Physicians, who were appointed to examine me as to my capabilities as a lecturer. This is a step necessary for my recognition by the Boards. So I am now a constituted member of the Medical School, and therefore, of course, a much greater man than before. I only wish all this brought in a little more of the needful, as I find the road to reputation and station much easier than that to competence and independence ; but I have been made to feel that my position as Pathologist rather throws me out of the latter path, and I

hope this move will place me more in a position to gather in, after a time, the harvest which I have been sowing for some years past, but which is still very far from ripe.

I have been, and am, very busy with winding up the session, and preparing a paper for the Med. Chir. Society. I have also a long review in the April number of the *Brit. and Foreign Med. Chir. Review*, and have written an equally long article for the next number.

Health very good on the whole this winter, which is more than I ventured to expect last year. I am most thankful to God when I think of this, as it would have been a most serious blow to myself and to my father had I been laid aside for any length of time.

Ever yours truly,

W. T. G."

He was at once surrounded by a numerous and enthusiastic band of workers, a large number of whom won distinction in after years.

But it is characteristic of him that, just on resigning the position of Pathologist for that of Physician, he signalised his retirement from the former office by publishing his *Suggestions in Regard to the Performance of Post-Mortem Examinations*, and his paper on *Some Points in the Pathology of the Liver*.

Within the next two or three years, he built up a great reputation as a practical physician, as well as a successful teacher, and produced a large number of important observations, which will be referred to in their proper place. During these years, his greatest friends were James Warburton Begbie and William Rutherford Sanders. Gairdner was the eldest of the three, and was regarded, so far as can be learnt from those still with us who were familiar with them in those days, almost in the light of Athos of *The Three Musketeers*. May it be added that, in Henry Duncan

Littlejohn, there was a most excellent counterpart of the brilliant and fiery D'Artagnan ?

Questions of Medical Reform were at this time much in the air when this letter to his brother was written. It has an interesting as well as amusing contrast of the professions.

" Edinburgh, May 28th, 1853.

My dear Jim,

We are in some hope of getting Medical Reform introduced into the House of Lords this session. But what goes under that name is a very small affair. The real medical reform has not yet been agitated, and when it *is* set in motion, I advise you and everybody blessed with a sensitive stomach to keep your nose at a respectful distance from the mess. We are indeed free from some vices—we have not the political trafficking and villainy-mongering of the lawyers to answer for, nor the tradition-worshipping bigotry of the clergy ; our profession is not, like these, rotten at the core, and human nature still finds a voice in it ; but in our institutions and incorporations honesty and independence have about as low a market value as need be, and the public, who alone are really interested in keeping us right, have been taught by the mal-practices of centuries to consider us as a race apart, with whose internal organization it has nothing to do. This indifference of the public is the ruin of the medical profession. It leads to not less of mismanagement and scandal in our public institutions than of charlatanism and dishonour in our private relations. We are still nearly as much as in the early ages of humanity the ' mystery men ' or priests of Aesculapius. We strive after a false character, and wear a perpetual mask. The public, knowing this, ceases to hold itself fit to pronounce on our merits, and its verdict is given on grounds at which the knowing ones laugh, and the wise and true men may well weep. And for a worthy finale to this comedy of errors, the men of the gold-headed cane turn round to the public, whose natural good sense they have muzzled and misled, and call out for new powers to interpose their sophistries and

pedantries between physician and patient. The only idea of medical reform that these men have, is to protect the profession against the competition of the quacks, as if quackery was not 'bone of their bone and flesh of their flesh.'

No—we are not quite so bad—and we are improving. But the above will do for a sketch, à la Diogenes, of medical politics.

Ever yours,

W. T. G."

Although, as we have seen, a Whig to the very core, Gairdner could argue, perhaps mockingly, in favour of Mary, Queen of Scots, and something which his brother had written draws down upon him this criticism :—

" 18 Hill Street, Edinburgh, Oct. 20th, 1853.

Dear Jim,

You are an infidel dog—a degraded, miserable outcast from chivalry—to think of comparing, after such a fashion, your ugly Crookback with the beautiful Queen of Scots. I don't know how to describe you, except by saying you are almost as bad as John Knox, and ought to be punished by being barred entrance to every truly polite circle. Such fellows to step on a drawing-room carpet I never saw. Get thee to the Record Office—go !

Cobden has tumbled into a very quagmire of blunders apparently on the state of Turkey. The *Morning Chronicle's* exposure of his ignorance of the primary conditions of the Tangimat, and the *Scotsman's* discomfiture of his appeal to *Maculloch's Dictionary*, by showing he had taken the edition of 15 years ago, since when the trade with Russia had greatly diminished and that with Turkey enormously increased, are both excellent in their way, and have cured me of an uneasy suspicion I had that Cobden might turn out on this point to be better informed than his neighbours. He is a foolish fellow, to say no worse, to hazard his great reputation in statements so strong and so inaccurate.

I have entered on duty as Physician, and am in full force preparing for the session.

Ever yours,

W. T. G."

This shows Gairdner fairly started as a Lecturer on the Principles and Practice of Medicine. It is, further, of immense interest, inasmuch as it indicates how John Gairdner felt himself bound to sever his connexion with the Unitarian body, about which more will be stated by-and-by. It is also of great importance, as it refers to the snub which Lord Palmerston administered to the Edinburgh clergy over the prevention of infectious disease.

" 18 Hill Street, Nov. 5th, 1853.

My dear J.,

I have been unable to call at Bell and Bradfute's till to-day, having been very busy with my course of lectures, which began on Thursday. I find the price is 7s. 6d., but that it is out of print. There will probably be a new edition.

I have entered 3 pupils yesterday, and hope to get on to the half-dozen in time. I have likewise got fairly on duty in the wards, and am altogether pretty comfortably busy. I am glad to hear you are getting still a Sunday holiday at Mill-hill now and then. Uncle W. says it is quite retired like Craigend, and like the neighbourhood too in ignorance and priestcraft.

Mr. Woods, the minister of St. Mark's Chapel, has published a farewell sermon, which is meant to be very severe upon those who have left the congregation, and some of the most pointed and polite expressions so evidently meet my father's case that there can be no doubt as to whom he is aiming at among others. Dad says it is a piece of clerical impertinence, from which I suppose he is a little nettled. But it's scarcely worth while.

Have you noticed Lord Palmerston's bout with the ministers of Edinburgh ? and the speeches of Dr. Cook and

Dr. Barclay in the Presbytery? they are capital. We had a very beautiful tho' orthodox sermon from Barclay on the Fast Day (not the cholera fast).

We have just had the great Scottish Rights Meeting here. Lord Eglinton's speech was that of a gentleman, and a clever man, and gave them some respectability. The Provost also spoke well, but the leader of the movement, Henry Inglis, the editor of the *Caledonian Mercury*, was so furiously eloquent that they fairly hissed him down.

W. T. G."

A year later his success as a Teacher of Medicine is seen to be assured. The letter in which this is announced contains the news of the death of Edward Forbes, and embraces some interesting remarks on public and private services :—

" 52 Northumberland Street, Edinburgh,
November 24, 1854.

My dear Jim,

Many thanks for your kind congratulations and philosophical remarks on the advantages of a Government office over the medical profession. I am not sure, however, that you have quite got to the bottom of the subject. I believe the truth is that the medical profession is supposed by the public to have such fascinations in itself as to repay its cultivators without regard to the filthy lucre, which some of them at least are so lamentably in want of; while you poor devils have nothing but your pay to look for in the way of fun or satisfaction. I don't kick at these arrangements in theory, tho' in practice they are sometimes troublesome to those who, like you and me, require above all things the unrighteous Mammon. It would be the height of absurdity to say that, if this magnificent nation wants a quantity of trifling and fiddle-faddle work done, it should not be paid well for. I think that just as we pay extra for dangerous and unwholesome occupations, so we ought to pay extra for trifles. The more useless the work, the higher should be the pay. Only I fear that on the present

system, which admits of unlimited dawdling in our public offices, the idlers would take all the cream, and leave the work to better men. In our profession, though work is often long of reaping its reward, yet it can be said on the other hand that there is no place for the man who won't or can't work. Perhaps, if the rewards were easier got, it would only increase the number of do-nothings—those brilliant and wonderful creatures who seem to be made expressly for bearing off the prizes in every department—queen bees of humanity in everything but that they are utterly barren.

No, I am fully convinced that mankind has just and true instincts in this matter, though the race is not always to the swift or the battle to the strong. I acknowledge, however, that I am sometimes moved to anger, not on my own account, but on that of many poor fellows in medicine, whom I see trusted by the public and toiling out deserving lives among the poor and needy, with no thought for science or art, or for anything except the means of living ; scarcely able indeed to eke out a decent income by incessant drudgery. These men, it is true, often disgrace our profession, as in other cases they ennoble it ; but, if they are sometimes found wanting, is it not reasonable to plead that a really good man cannot always be had for £60 a year, which is about the highest that the unions in England pay their slaves of the (red) lamp for managing the health of the whole poor of a wide district. On the other hand, these gentlemen plead that they have no difficulty of procuring respectable and ' regular ' practitioners at that sum, and not only is this true, but there is commonly a rush at such situations.

Then, again, what do you say to Lord Raglan's treatment of the medical men in the East ? Not only has he allowed them to be starved of their medical stores and apparatus, but when they have been called upon to perform services of the highest order, under circumstances the most discouraging, he has not a word for them among all his praises of other officers. Yes, there is a word ; it is to spurn the whole medical staff, because one of the number

had made a mistake in the delivery of an order! What an outcry there would have been, if the non-medical officers had been spoken at in such a fashion. I firmly believe that a large portion of the mortality of this campaign has sprung and will yet spring from the inattention and systematic neglect of the medical portion of the army.

We have now our first snow. Since yesterday afternoon it has been almost constantly either hail, snow, or rain, and the streets are now covered with slush. Marion and my father are running the gauntlet of the colds which such weather often brings. For myself it agrees with me better than the summer heats, and I am thankful to say my health and strength are better than they have been for a considerable time; though I don't by any means feel as if I were fit for a winter campaign in the Crimea.

You will have heard ere this that we have lost poor Edward Forbes, who had begun to endear himself to every one here by his kind and liberal heart, and his modesty and worth, not only as a naturalist but as a man. I went to his funeral yesterday, and found quite a large assemblage of professors, students, and citizens. The English service was read, and, while some portions of it are very good, I must say I thought the profound and intricate speculations of Paul about the resurrection of the body were very much out of place. But it is a great many degrees better than the marriage service, which is abominable.

The class still goes on increasing, though the lists close next week. My number is now 44.

Ever yours,

W. T. GAIRDNER."

Gairdner's continuous interest in philosophical and theological subjects, in spite of the engrossing nature of his occupations, is well seen in what follows:—

" Edinburgh, March 18, 1855.

Dear Jim,

It is a good many weeks since I began a letter to you, which fell aside, and was forgotten till it had become

stale. Possibly the recollection of the high merit of that exordium had comforted my laziness, and laid flattering unction to my soul. But, whether it did or no, I had best not trouble you with such a miserable excuse; for you will not thank me for beginning letters to you, if such is to be their fate.

I have two notes from you lying by me; one of which challenges me to deny (and I do hereby deny) the authorship of a letter in the *Scotsman* upon Ragged Schools. The letter was a good letter notwithstanding. I believe it was by my enthusiastic friend, Mr. Jenner, who, though not hitherto much addicted to literary pursuits, except as an omnivorous reader, has been coming out lately in that 'Protestant's' letter, as well as in an article in the *Annals of Natural History*. The latter I have not seen yet; but it is pleasant to see a man of his energy of character overcoming the disadvantages of imperfect early education, and unrefined early associations in this way. His genuine benevolence and worth have, I am happy to say, secured him the friendship and esteem of all who know him and can appreciate these qualities; and I am sure that few rich men of my acquaintance make half so good a use of their means, though many of them, perhaps, mean as well, and exercise more self-denial to much less purpose.

I don't know whether anybody has whispered to you that I have a letter for the *Scotsman*, however, actually in type, and on this very subject of Ragged Schools. It has been some time standing over, being very long; but will, I hope, appear in Wednesday's paper. I will send you a copy, either of the paper or of the article.

I read your small letter on the Streets, but I wish you would fly at something higher than this. I was sorry that some one had put aside the first *Chronicle* you sent before I knew that you had a letter in it.

Have you seen a publication of Parker's called *Oxford Essays by members of the University*? I have just been spending an evening over it, and have been much interested in some of the papers. There is one which I should like to

hear your opinion of, from that champion of dogmatic scepticism, and faithless belief, and intolerant indifference, and sneering earnestness, Mr. Froude—on the best means of teaching English History—as full of paradox and conceit as the *Nemesis of Faith*, but possibly like it, containing some hints of truth for those who can see them. He seems to be employed on a History relating to the time of Henry VIII., and to have ended in being disposed to make a hero of that uxorious old boy, if not indeed to fall down and worship him. The coolness with which the fellow sets down all modern histories as deficient in veracity, while he exalts Tacitus and Thucydides, and Livy ! ! as the acme of truth, is something beyond even Oxford impudence and conceit. But I wish you would tell me what you think of the statutes at large as a university school-book, and also what you think of the old story of fat Henricus and Anne Boleyn as rendered by Mr. Froude.

We had a Mr. Congreve here a few weeks ago, proving to us that the Empire was the best period of Roman, and, indeed, I think he almost said of human history. The individual emperors had, it is true, their little faults ; but these did not really do much harm, and the Roman people enjoyed very solid happiness, as well as an unusual amount of material and moral well-being under the worst of them. Augustus was a noble character, and thoroughly embodied the genius of the Empire. Tiberius was also a great, though scarcely so perfect, a character. Nero had undoubtedly faults as a man, but was a great and good prince. Domitian had the misfortune to be goaded on into excesses which sully the harmonious colouring of the picture of his reign. This was the style in which, according to my recollection, this Oxfordian be-lectured us upon Roman History. I am not a stickler for historical any more than other orthodoxy, nor have I ever pinned my faith to the whole of Suetonius' 'chronique scandaleuse,' but really, if this is the style in which the past is to be read nowadays—if old Harry is to be made a saint, and Tiberius scarcely to be allowed to be a sinner, we may as well shut up History, and resolve

the past of humanity into a vision of the night. But I wait to know if you can extend the rainbow cloak, which you have thrown about Crookback, over the great and glorious memories of Harry and Heliogabalus, to whom we may add, when we are at it, Catherine de Medici, Louis XI., and George IV. For a considerable proportion of these worthies I am confident that a good deal might be said, as well as for him of Russia, just called to his account.

I suspect we are drifting towards Lord Derby; at all events Pam. seems to be getting into a pretty mess. But I am quite behind in my politics, having scarcely had time to read newspapers for some weeks, except in an overly way; and not seeing much light through the darkness. Looking on at a distance here, and knowing but little of the real character or rather the no character of many of our statesmen, one really does not know what or whom to trust.

When I say I have not had time to read newspapers, you mustn't suppose I have been killed with overwork, like the poor fellows in the trenches. On the contrary, I have been illustrating the proverb that a man has never so little time on his hands as when he has had little to do. I have been of course lecturing as usual, and doing my little practice, I trust, not unfaithfully—for the rest, Agnes and Maggie from Auchans, who have been here, have agreeably absorbed my leisure hours, and, whatever remained, I have been devoting at their instigation to—*The Newcomes*—which I had only dipped into before, but which I have now nearly read through with unexampled eagerness. If you have not read it, you have a great treat in store—it is undoubtedly the first novel of the day—and in some respects, I think, the first of my day.

Ever yours,

W. T. G."

The great and good Alison, Professor of Medicine in the University of Edinburgh, died in 1855, and thereupon at once ensued keen discussion as to the fittest physician to fill the vacant Chair. In the medical world it appeared

almost certain that the choice of the electors (then composed of the entire Town Council of Edinburgh) would lie between Alexander Wood and William Gairdner. But, when the appointment came to be made, the City Fathers, largely, as has always been understood, through the influence of Simpson, elected Thomas Laycock, a graduate of Göttingen, then practising as a physician in York. Laycock was undoubtedly a man of the highest ability, although his genius lay more in the regions of speculation than observation. He was universally regarded as far in advance of his own times, and contributed in no small degree to the furtherance of our knowledge of the nervous system. Upon two subjects, in particular, he made important additions, *i.e.* the physiognomy of disease, and the study of diatheses ; while he may be considered as one of the founders of modern psychological medicine.

Gairdner's application was supported by testimonials from Latham, Thomson, Peacock, Hewett, Gull, Bristowe, Budd, Fuller, Davies, Brinton, Clark, Kirkes and Jenner of London ; by Banks, Corrigan, Marsh and Smith of Dublin ; by Macfarlane, Easton and Bell of Glasgow ; by Fletcher, Fleming, Johnstone, Evans and Heslop of Birmingham ; Ormerod of Bristol ; Day of St. Andrews ; Barthez of Paris ; Rilliet of Geneva ; Lebert of Zurich ; and Sigmund of Vienna—as well as a large number of his Edinburgh colleagues, and of his former pupils in every part of the civilized world. It is very pleasant to be able to record that Gairdner proved a loyal colleague to the new professor, and that in after-life he gave constant expression to the high estimate which he formed of the character and abilities of Laycock.

That Gairdner, in his pre-occupation with other subjects particularly interesting him at the time, was apt to overlook

matters even of much pressing importance to himself, is clearly shown by the following communications to his brother, from himself and his sister Marion. It will be observed that, after his defeat by Laycock, Wood ceased to lecture on the Principles and Practice of Medicine, and Gairdner was left as the only extra-mural lecturer on the subject in Edinburgh.

"Edinburgh, Oct. 21st, 1855.

Dear Jim,

Did it ever strike you that there is a remarkable peculiarity in the versification of *Henry VIII.* as compared with many, if not most, of Shakspeare's plays. Take, for instance, Mark Antony's oration, or any of the soliloquies in *Hamlet*, or any portion of blank verse from *Romeo and Juliet*, *Tempest*, *As You Like It*; indeed, from the majority of his plays, you will find that, as a general rule, the ordinary measure of English blank verse is pretty constantly preserved, the line ending with a *long accented* syllable, *e.g.*

'O mighty Caesar! Dost thou lie so low
Are all thy conquests, glories, triumphs, spoils,
Shrunk to this little measure? Fare thee well.'

Compare this with the march of most of the verses in *Henry VIII.*; you will find a very large infusion of lines ending in a trochee (— ⏑); so large, indeed, that many passages are entirely made up of such lines, *e.g.*

'So farewell to the little good you bear me
Farewell, a long farewell to all my greatness!
This is the state of man; to-day he puts forth
The tender leaves of hope; to-morrow blossoms,
And bears his blushing honours thick upon him.'

Through the whole of this speech of Wolsey's, and the succeeding dialogue with Cromwell, such lines predominate; so, too, in Queen Catherine's speeches, and in a lesser degree, through the whole play.

I am half inclined to think that some use might be made of this point in testing the chronological order of the plays, for though none of them have so much of this peculiarity,

I think, as *Henry VIII.*, there are some in which it is much more frequent than in others. In reading the play through, it gives a very marked intonation to the verse. *Othello* comes nearer to *Henry VIII.* in this point than any other I at present remember, but I have not time just now to look to it in detail.

I have always had two or three crotchets about the chronology of Shakspeare's plays, which, as I am not at all familiar with the host of commentators, may be possibly very old notions. *Romeo and Juliet* is, I think, commonly set down as a late play. I don't believe it. It is impossible that Shakspeare in the maturity of his genius should have been guilty of such extravagancies, both of diction and of conception, as are to be found in it. The florid imagery, the fulsome rant, the inveterate word-catching in season and out of season, are characteristic of the time of Euphuism, when Sidney's *Arcadia* was the glass of fashion among scribblers. On the same grounds, tho' less strongly, I would refer the *Tempest*, perhaps the *Merchant of Venice*, and certainly *As You Like It*, *Winter's Tale*, the *Comedy of Errors*, to the period of immaturity, when the imagination was apt to overbear the judgment. In even the most rollicking of those, which I would term his later plays, such as the *Henry IV.* and the *Merry Wives of Windsor*, the portraiture of character is far more subtle, and the natural exuberance of the author's own genius is directed more constantly with a dramatic purpose, than in the others.

Have you seen an article in the *Edinburgh Review* upon the Newspaper Press, which has called forth the wrath of the *Times*, by speaking a little too much home to the seared conscience of the omnivorous and omnipotent giant? It is undoubtedly by some one well informed, and is well worth reading.

There is another, recommending a new translation of the Bible, to be undertaken by a royal commission of the heads of the Anglican Church!! A pretty job they would make of it!

W. T. G."

"Oct. 23, 1855.

Dear Jim,

The enclosed was written two days ago—but lost almost as soon as written, like many other things too good to live. It has, however, been plucked like a brand from the burning, and here it is. When I was deploring its fate—Macrae says—you are much more anxious about my class than about *Henry VIII.*, and that you would have valued a postscript, telling of my arrangements for the winter more highly than all the rest of the epistle. Much as I doubt this, I have no hesitation in telling you that I have completed arrangements, by which Dr. Alexander Wood retires from the teaching of Practice of Physic, and leaves me the room at Surgeons' Hall. This is an advantage in some respects, tho' I shall count it rather dearly purchased if I am obliged to take his museum at £100, as he offers. There is hope that the museum may be sold elsewhere, and that I may inherit at the value of the fixtures.

Ever yours,

W. T. G."

"Tuesday morning.

Dear Jim,

I add a few lines to W.'s letter, that I may enclose your valuable piece of leather. It was found unexpectedly the other morning under the drawing-room fender. Willie told us last night that he had been writing you a long dissertation upon Shakespeare's *Henry VIII.*, but I don't know if he has told you of anything having a more immediate interest. For instance, regarding himself, he has made an agreement with Dr. Wood, who will give him the class-room in the Extra-Academical School, so he will now have no one to compete with but the new Professor. We expect the Craigend folks to-day, but they will not likely remain more than a few days. Aunt M. is better ; she has got rid of the earache, and last night Uncle T. writes that a toe, which was sore a week ago, is now well again.

There was a long letter yesterday from Australian Bob, part of it to my father, and part to my mother, Macrae and

me jointly. I am glad to say it is a very good letter, telling about a run he had to Melbourne. Those who know the boy as we do may still discover a few of his eccentricities, but they are not so glaring as to be remarked by strangers.

He had visited on the mainland some of his fellow-passengers on the voyage out, from whom he had received kind welcomes. The letter is at present going to Auchans.

If you think Willie's letter of a very old date, the reason is that it went astray after he had written it, and was only found this morning.

Ever yours affectionately,

MARION."

The ensuing period was most fruitful in clinical work. Some important and original papers upon Aortic Aneurysm and Cardiac Dilatation appeared at this time, containing a number of facts absolutely unrecognised previously. His clinical retrospects of cases treated in the wards, comprising observations upon many diseases, appeared almost annually; while he also published his remarks on Dr. Hughes Bennett's paper on "Blood-letting," as well as three lectures on Medicine and Medical Education. These made their appearance in 1858. Pericarditis, Fever, and Pleurisy occupied much of his attention during 1859 and 1860, while *The Retrospect of Cases under Treatment* also made its appearance. *Infantile Mortality*, and *The Use of Alcohol in Hospital Practice*, absorbed a good deal of his time in 1860 and 1861; these subjects were followed by *A Short Account of Cardiac Murmurs*, which was part of a lecture on Modern Cardiac Pathology and Diagnosis, delivered before the Royal College of Physicians of Edinburgh. This paper merits particular attention, seeing that in it Gairdner suggested, for the first time, the use of diagrams in order to make the rhythm of cardiac murmurs clear in relation to the different phases of cardiac activity; in other words,

he succeeded in making the auscultatory phenomena connected with the heart appeal to the eye. In the teaching of practical medicine, this improvement has hitherto been, and always will be, regarded as an absolutely new departure. His methods have been adopted and modified by almost every clinical teacher since the date of his paper ; but it will remain, for all time, as a monument to his originality. His scheme of instruction attracted attention, not merely in Europe, but in America ; and the following letter, from one of the most distinguished medical men in New England, shows how lively was the interest aroused amongst American physicians :—

" Boston, April 29, '62.

Dear Sir,

This note will be delivered to you by my young friend, Dr. Cutter—of this State—who intends to spend a few months in Europe. May I ask you to allow him to visit with you the wards under your charge ?

I read with interest all the papers presented by yourself to the Medical Journals. Very recently I saw your introductory remarks to your Clinical course. In these you refer to certain diagrams to be used by students and practitioners. May I ask you to give Dr. C. one of them for me—in order that I may have it reprinted for the use of our students ?

Our mutual friend, Dr. Jackson, still remains a constant blessing ; as it were a perpetual benediction to us his former pupils, and to the community in which he resides. Though nearly 85 years old, he has still the wonderfully clear mind and really noble soul he has always had.

I remember with pleasure and profit my very brief visit to your beautiful city. May I hope that you have not forgotten one who signs himself,

Very respectfully yours,

HENRY T. BOWDITCH.

Dr. W. T. Gairdner."

The year 1862 was the turning-point in his career. It was memorable in two respects. During its course, he published two admirable works—one, which was of the greatest value as regards sanitary science, called *Public Health in Relation to Air and Water*; the other, entitled *Clinical Medicine; Observations recorded at the Bedside, with Commentaries.* In this he collected together a considerable number of the scattered observations which had been published during the years he had been connected with the Royal Infirmary of Edinburgh. This is not the place to make any remarks upon the value of the volume, but an attempt will be made to do this at a later stage.

It must be remarked, however, in this connexion that, on the appearance of his book on Clinical Medicine, he received hearty congratulations from many eminent men upon the excellence of the work. Amongst these letters, which he carefully preserved, were communications from Lionel Beale, London; John Beddoe, Clifton; H. Bond, Cambridge; Andrew Buchanan, Glasgow; George Buchanan, Glasgow; Andrew Clark, London; John B. Cowan, Glasgow; David Craigie, Edinburgh; James Duncan, Edinburgh; J. Matthews Duncan, Edinburgh; J. G. Fleming, Glasgow; H. D. Littlejohn, Edinburgh; Douglas Maclagan, Edinburgh; Charles Murchison, London; Patrick Newbigging, Edinburgh; James Paget, London; Alexander Peddie, Edinburgh; J. B. Radcliffe, London; Harry Rainy, Glasgow; Hyde Salter, London; Francis Sibson, London; James Spence, Edinburgh; Thomas Hawkes Tanner, London; Allen Thomson, Glasgow; H. Thursfield, London; Thomas Walker, Peterborough; Patrick Heron Watson, Edinburgh; and Thomas Watson, London.

It seems certain that, at this period, he was burning the candle at both ends; and a letter belonging to this time,

which he has preserved, throws a strong light upon his preoccupation. As we shall see, by and by, Gairdner was throughout the whole of his life one of the most religious of men; and during the later years of his early life in Edinburgh, having with his father and the family left the Unitarian body, he was a member of Old Greyfriars Church. The saintly Robert Lee was at that time Minister of the Parish, and the letter which is now appended from him is a beautiful mixture of the serious and the humorous.

"Dr. W. T. Gairdner.

Edinb., 28 April, 1862.

My dear Sir,

I am the last man in the world to turn inquisitor—though, of course, I have remarked that you have been seldom in church for some time past.

I shall not admonish you upon the theological view of the matter, but I shall presume to intrude into your own province so far as to warn you that such an amount of pressure as makes public worship impossible for a considerable time, must be most pernicious, and even dangerous, to the body, and as we expect our medical, no less than our theological teachers to exemplify their own doctrines—I do exhort you earnestly to *repent* and *reform*, and show *your faith by your works*.

For my part, I generally feel public worship rather a relief than an additional pressure upon the brain—it tending, if rightly conducted, rather to excite the emotions and sentiments than to exercise the intellect. And yesterday, I think, we were all somewhat refreshed and strengthened for the new struggles that doubtless are before us all.

The practical improvement of this whole discourse is this—Make conscience of taking things a little more easily. Don't run yourself out of breath. 'What is a man profited, etc.'

Believe me, my dear Sir,

Always yours very sincerely,

ROBERT LEE."

While the year 1862 was marked by remarkable evidences of literary activity, Gairdner's life was entirely altered in its course by his appointment to the Chair of Medicine in Glasgow, vacant through the death of Professor Macfarlane. It has been stated that he was a candidate for the Chair when Dr. Macfarlane was appointed, and when the venerable professor passed away, it was perfectly clear that, if he would now accept it, he was the one man in Scotland marked out for the post. It is quite true that three Glasgow physicians were anxious to succeed Professor Macfarlane, namely, Dr. Andrew Anderson, Dr. Joseph Bell, and Dr. John Cowan—the last mentioned being afterwards Professor of Materia Medica and Therapeutics in the University of Glasgow—but, when it became known that Gairdner was willing to accept the chair, he was almost universally expected to be appointed. This general expectation was fulfilled. The following letter was received by him in the month of September :—

"Home Office, 6 September, 1862.

Sir,

I am directed by Sir George Grey to inform you that he has much pleasure in offering you the Chair of the Practice of Physic in the University of Glasgow, vacant through the resignation of Dr. Macfarlane.

I have the honour to be,

Sir,

Your obedient servant,

HENRY B. LOCH.

Dr. W. T. Gairdner."

Her Majesty's Commission to Gairdner, as Professor of Medicine in Glasgow University, was duly signed, completed and delivered on 30th September, 1862, by Messrs. Hunter, Blair, & Cowan, W.S. The appointment was

received by the medical profession, both in Europe and America, with acclamation, and this was admirably expressed in the following letter from Sir William Jenner :—

"Coburg, Oct. 16th, 1862.

My dear Gairdner,

I learned just now from an old paper of your appointment. I am extremely glad of it, and congratulate you sincerely. When I see Sir George Grey, I think I ought to congratulate him. It speaks well for him under all the circumstances of the case to have disregarded every other consideration than that of the propriety of putting the best man into the post.

He was evidently desirous to do right, and satisfied, I thought, that you were the best man.

I am glad to say I shall be in London on this day week.

Yours always sincerely,

WM. JENNER.

We leave this for Brussels to-morrow."

Gairdner at once set about the necessary steps in connexion with his removal to Glasgow, and, amongst these, came the necessity of severing his ties with the institutions in Edinburgh, which he had served so well and so long—the principal among them being the post of Physician to the Royal Infirmary. In connexion with this, the following communication deserves a place :—

"Royal Infirmary, Edinburgh, 15th September, 1862.

Dr. W. T. Gairdner,
45 Northumberland Street.

Dear Sir,

Your letter to the Managers of the Royal Infirmary of date 12th inst., resigning your office of Physician to that Institution, in consequence of your appointment to an important office in Glasgow, was laid before them at their

meeting to-day, and they directed me to send you Copy of Minute hereto annexed.

I am,

Dear Sir,

Yours faithfully,

PETER BELL."

Excerpt from Minutes of the Managers of the Royal Infirmary, 15th September, 1862 :—

" It was moved by the President of the Royal College of Physicians, and seconded by the President of the Royal College of Surgeons, that the Managers, while they are happy to hear that Dr. Gairdner's services have met with so high a reward, beg to express their regret at losing his valuable aid as Physician in the Hospital, and to thank him for the assiduity and conscientiousness with which he has performed his duties during so long a period.

Extracted from the Minutes.

PETER BELL."

As it will undoubtedly be of interest to the families of Gairdner's House Physicians in Edinburgh, the complete list, from November 1854, to April 1862, may be appended :—

Dr. A. M. Inglis, Dr. W. Gilfillan, Dr. Spottholt, Dr. Yellowlees, Mr. de Faebeck, Dr. A. O. Cowan, Dr. A. Pow, Dr. G. Shearer, Mr. P. A. Simpson, Dr. Joseph Bell, Dr. W. M'C. Watson.

Before leaving his native city for that of his adoption, he was entertained by his professional brethren, and a few other friends, at a public banquet. The invitation which he received is now given :—

" Royal College of Physicians, Edinburgh,
Wednesday, 8th October.

My dear Sir,

It has been resolved by a number of the Fellows of both Colleges, to invite you to a Public Dinner at Slaney's

Hotel, St. Andrew Square, on Tuesday, the 21st October, previous to your leaving this city for Glasgow.

We have already received a number of signatures from Members of the two Colleges. But it is further proposed to allow Members of the Profession, not belonging to these bodies, to be present on this occasion.

I hope that the day and the hour proposed will suit your convenience, and I shall feel obliged by your giving me an early answer.

Believe me to be,
Dear Sir,
Yours very sincerely,
DAVID CRAIGIE.

Hour of Dinner, six o'clock p.m.,
on Tuesday, 21st October.

To Dr. William T. Gairdner,
Northumberland Street."

And those who assisted at the entertainment were the following :—

Professor Syme, Dr. J. G. M. Burt, Dr. Strethill Wright, Dr. D. R. Haldane, Dr. Andrew Wood, Dr. J. Begbie, Professor J. Millar, Dr. Matthews Duncan, Dr. P. D. Handyside, Dr. John Brown, Dr. A. Peddie, Dr. J. Y. Simpson, Dr. A. R. Simpson, Dr. Dunsmure, Dr. J. D. Gillespie, Dr. R. B. Malcolm, Dr. W. R. Sanders, Dr. H. D. Littlejohn, Dr. Black, Dr. Patrick Newbigging, Dr. R. Omond, Mr. B. Bell, F.R.C.S., Dr. Andrew Pow, Dr. George Keith, Dr. Grainger Stewart, Sir James Cox, Dr. Arthur Mitchell, Dr. Roberts, Dr. Strachan, Dollar ; Dr. John Smith, Dr. P. H. Watson, Dr. Somerville, Dr. T. H. Pattison, Dr. W. Robertson, Dr. J. Hunter, Dr. J. Carmichael, Dr. Thomas Keith, Dr. J. Simson, Mr. James Spence, F.R.C.S., Dr. William Cumming, Dr. Alexander Wood, Dr. Warburton Begbie, Dr. Halliday Douglas, Dr. James Duncan, Dr. W. A. P. Browne, Dr.

Stevenson Macadam, Dr. W. Sellar, Dr. John Struthers, Mr. R. Nasmyth, F.R.C.S., Dr. W. H. Lowe, Dr. George Keith, Dr. John Gairdner, Dr. Scoresby Jackson, Mr. Thomas Hunter, H.E.I.C.S., Mr. Alexander Smith Kinnear, Advocate, Mr. Robert Lee, Advocate, Mr. William Smith, Dr. James Struthers, Dr. J. Smith, Saughton; Dr. David Maclagan, Dr. T. A. G. Balfour, Dr. J. B. Junor, Peebles; Dr. Bell, St. Andrews; Dr. John Moir.

Amongst letters expressing regret at being unable to take part in the banquet, the following, from Dr. John Hutton Balfour, so long Professor of Botany in Edinburgh, may be allowed a place, even at the risk of rubbing up our friends in Glasgow :—

"18 Grove Place, Brompton, London, S.W.,
23 Oct., 1862.

My dear Gairdner,

It is only to-day that I saw a notice of the dinner given to you by professional friends in Edinburgh. I am very sorry that absence from Edinburgh has prevented me from being present to do honour to one of my earliest pupils, who has now risen to a proud eminence in the Profession. While I express my regret at being prevented from joining in the public testimony of regard to you, allow me to send my warm congratulations on your success, and to wish you all happiness and prosperity in your new appointment. There is now an excellent staff of Edinburgh men in the old University of Glasgow, and I have no doubt that they will increase its reputation.

I am,

Yours sincerely,

J. H. Balfour."

Gairdner shortly after removed to Glasgow, in order to commence his duties, and settled at 21 Blythswood Square, where he resided until 1868, when he bought 225 St. Vincent Street.

LIFE IN GLASGOW

The academic world of Glasgow, into which he now stepped, comprised a number of most distinguished men. The venerable Thomas Barclay was Principal; the Theological Faculty consisted of John Caird, Duncan Weir, and Thomas Jackson. In the Faculty of Law were George Skene and Anderson Kirkwood. The Faculty of Arts included William Ramsay (Latin), Edmund Lushington (Greek), Robert Buchanan (Logic and Rhetoric), William Fleming (Moral Philosophy), William Thomson—afterwards Lord Kelvin—(Physics), Hugh Blackburn (Mathematics), Robert Grant (Astronomy), William John Macquorn Rankine (Engineering), and John Nichol (English Literature). To the Faculty of Medicine, with which he was immediately connected, belonged Allen Thomson (Anatomy), Darwin Rogers (Natural History), Joseph Lister—afterwards Lord Lister—(Surgery), John Pagan (Obstetrics), George Walker Arnott (Botany), John Easton (Materia Medica), Andrew Buchanan (Physiology), and Harry Rainy (Forensic Medicine). Even at the time of his appointment to the Glasgow Chair, several of these men were famous. John Caird was not only recognised as an eminent theologian, but he had already attained the greatest distinction as the most eloquent preacher of his age. William Ramsay, Edmund Lushington, and John Nichol were models of exact scholarship; while William Thomson had reached a pitch of scientific renown which, although but the prelude to his future triumphs, had already thrown world-wide lustre upon the University. Macquorn Rankine, in the realm of Engineering, held a position universally recognised, and he added to his other distinctions the faculty of being a real poet. In their own way, Hugh Blackburn and Robert Grant were also men of much

scientific eminence and high social standing. Some of the Chairs in the Faculty of Medicine were likewise filled by distinguished men. Allen Thomson formed, along with Goodsir and Sharpey, a morphological triumvirate of rare eminence, and as his colleagues in Edinburgh and London were well stricken in years, the Glasgow professor had come to be regarded as the leader of the British anatomists. Of Joseph Lister, nothing need be said. He was already, at the time of Gairdner's advent in Glasgow, occupying himself with those researches upon the treatment of wounds, which have revolutionised modern surgery. His subsequent career is the glory of the medical profession in our land. George Walker Arnott was one of the most accomplished botanists of his age—the close friend, as well as the constant collaborator, of Sir William Jackson Hooker. John Easton, the Professor of Materia Medica, was a man of powerful personality, and from his particular type of eloquence was generally known as "emphysematous John." His name will be kept green in the memory of man by reason of Easton's syrup—in the same way as that of Gregory in Edinburgh, through his historic powder. Andrew Buchanan, the venerable occupant of the Chair of Physiology, had in youth rendered signal service to the development of his own science, and remained an excellent teacher of it. Harry Rainy, the representative of an old highland family in the county of Ross, threw an air of aristocratic distinction upon the Chair of Forensic Medicine. Of some of those men Gairdner gave interesting sketches in the "Book of the Jubilee." His contribution is reprinted in the present volume.

As his colleagues on the staff of the Glasgow Royal Infirmary he had, as Physicians, Dr. Ritchie, Dr. Fraser, and Dr. Orr; while the Surgeons were Dr. Martin, Dr.

Buchanan, Mr. Lister and Dr. Lyon. The Dispensary Physicians were Dr. Cowan, Dr. Steven and Dr. Dewar; and the Dispensary Surgeons, Dr. Macleod and Dr. Leishman. Gairdner used often to express regret that he had never had as his colleague the eminent, if quaint, Dr. Brown of Lanfyne, of whom many amusing anecdotes used to be told. The late Dr. W. M. Buchanan, H.E.I.C.S., a great friend, both of Gairdner and myself, knew him intimately, and told me many stories of the worthy physician. One of the most amusing concerned a patient who was in his wards with dysentery; after discussing the case with the medical students present, Dr. Brown wound up by prescribing writing paper, boiled in milk. With inimitable composure, the Clinical Clerk in charge of the case promptly asked—"Shall I order Post or Foolscap, Sir?" The reply has not been recorded.

The new professor at once entered upon a life of great activity. He not merely threw his energies into his systematic lectures on Medicine, which were always models of knowledge and of scholarship; but he brought the clinical teaching in the Royal Infirmary to a pitch which it had never previously reached. It is not too much to say that, with his advent in Glasgow, there was an absolute revolution in the teaching of Medicine. In addition to these duties, he continued to make and to publish original observations; while he, at the same time, engaged in the busy life of a consulting physician.

At the same time he published, as may be seen from the Bibliography, many important papers on medical subjects. Amongst the most valuable of these may be mentioned "The Action of Expectorant Remedies," "The Employment of Alcohol in Typhus Fever," "The Clinical History of Empyema and Pneumothorax," "Supposed Embolism in a

SIR WILLIAM TENNANT GAIRDNER, K.C.B.

From a Photograph taken during his early life in Glasgow, by Dr. Adamson, of St. Andrews, kindly lent by Dr. R. O. Adamson.

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remarkable Succession of Symptoms," "The Functions of Articulate Speech," "Aphasia of Speech of Cerebral Origin," "Two Lectures on Cardiac Diagnosis," "Fatal Acute Pleurisy," "Cases of Pneumothorax," "Clinical Lecture on Diagnosis," "Nasal Diphtheria," "Chorea treated by Chloral," and "Disease of the right side of the heart"—in addition to many Reports on the Sanitary Department of Glasgow; as well as the Introductory Address at the opening of the winter term of the University in 1866; and "Remarks on Certain Moral Aspects of Money-getting," published in 1868. All these calls upon his energies would seem sufficient to tax the strength of a more than usually powerful mind and body; yet they were added to in a way that must now receive our attention.

The history of the measures taken by the city of Glasgow to improve the sanitary conditions, and to promote the health of the humbler citizens, is of the greatest interest; and no part of the professional career of Gairdner was more fraught with blessing to his adopted city than that during which he filled the post of its first Medical Officer of Health. The enormous growth of the city of Glasgow during the first half of the nineteenth century, from a small town of less than 80,000 inhabitants to the second in the British Isles, had not been accompanied by efficient arrangements for supervising the health of the inhabitants. The sanitary movement in Glasgow was begun and carried forward to success by Mr. John Ure, to whose public spirit and wide outlook Glasgow owes more than has ever been realised. He was descended from an old family of Guild Brothers of the ancient city. His parents resided, and he himself for a time lived, in what was known as "The Briggate" of Glasgow, when that now humble district was the residential abode of well-to-do citizens engaged in trade. His father

was one of the old school of Incorporated Bakers, who combined flour manufacture with their baking business, and Mr. Ure was the only son. At an early age he joined his father's business, and on his father's decease (which occurred when Mr. Ure was quite young) he succeeded to its full charge. For a time he carried on both branches of the business ; but soon confined himself to flour-milling, which gave him more leisure for the prosecution of the philanthropic work on which his heart was set. When he was quite a youth, this bent of his mind began to show itself. It was his custom in his spare hours to see for himself how the poor were housed, and why their children died in such distressing numbers, as compared with those of the higher classes ; it became one of his ambitions to do something to improve the housing and the health of the working classes in Glasgow. From long investigations he obtained a wide knowledge of the conditions of the people ; and the facts which he thus gained, while strengthening his resolution, showed him the magnitude of the work on which he was determined to embark. How to set to work seemed for a long time a problem beyond his power to solve. Undoubtedly the Town Council was the most natural, as well as the most hopeful, channel for the energies of a reformer, and to it he sought election. When he entered the Town Council in 1854 he was its youngest member. The epidemic of cholera which occurred in that year awakened the country and the Government to the need of legislation on the subject of Public Health, and furnished Mr. Ure with the opportunity he sought to impress on his colleagues his views regarding the sanitary condition of the old parts of the city. The Registration Act for Scotland came into operation during the year 1855, and that year saw the passing of the Act which authorised the introduction of the water supply from Loch

Katrine—one of the most important sanitary events in the history of any city—which was completed in 1859. In 1856 " The Nuisances Removal (Scotland) Act " was passed, and under the powers it conferred, a special Committee of the Town Council was formed in 1857. At first Mr. Ure was an ordinary member of the Committee; but, in the year following its formation, Lord Provost Clouston, who heartily supported Mr. Ure in his aims, nominated him for the Chair, to which he was unanimously chosen. No sooner had Mr. Ure reached this post of vantage than he set to work in earnest. He submitted a scheme for the improvement of the sanitary condition of the city, in a resolution proposing that a special department should be created for the inspection of nuisances; that a staff of officers should be attached to the department for the discharge of its duties; and that the existing committees dealing with the Public Health should be united—the joint committee so formed to have the Sanitary Department under its direction. It was intended, by means of this department, to regulate the cleansing of the city; to prevent the occupation of unhealthy habitations and the overcrowding of small dwellings; to have close surveillance of lodging houses; to procure the abatement of nuisances arising from works and from smoke; and, generally, to take oversight of the sanitary condition of the city. It was also intended that, while the Sanitary Department should be distinct from all others, it should receive assistance from each, and especially from that of the Police. The sanitary force of the city had previously been entirely under police superintendence, and the need for improvement which the resolution implied, and the change of management which it foreshadowed, reflected, to some extent, upon the existing official arrangements. With this management some members of the Town

Council were perfectly satisfied; not a few were sceptical of any good arising from the proposed change; others hesitated on account of the financial aspect of the alteration in contemplation. In the interesting Memorial Volume of the writings of Dr. J. B. Russell, edited by Dr. A. K. Chalmers, entitled "Public Health Administration in Glasgow," will be found an excellent summary of the course of events:—

"In 1857 a 'Committee on Nuisances' was appointed, and for the first time Public Health was differentiated as a special function of municipal government. This was done under the Nuisance Removal (Scotland) Act. In 1859 the Chairman (Mr. John Ure) submitted a scheme for the improvement of the sanitary condition of the city by creating a special department under a medical officer with an adequate staff of inspectors for the discovery of nuisances and the oversight of disease, the whole to be under the Committee. The Town Council resolved to send Mr. Ure and another Councillor (Mr. Moir) with the Chief Constable and Master of Works (Messrs. Smart and Carrick) as a deputation through the chief towns of the United Kingdom to get information as to local organization and powers. They visited London, Leicester, Birmingham, Manchester, Liverpool, Edinburgh, Dundee, Aberdeen, Dublin, and Belfast. They reported in October."

It is to be confessed that Mr. Ure was somewhat disappointed to find that his official colleagues were not in agreement with him. They were not impressed with what they had seen in other places, and did not consider that it called for change in their own system; the Draft Report which, on his invitation, they prepared, fell far short of his ideal. Before entering upon the revision of the Report, it fortunately occurred to him to ask the deputation to visit the more densely populated parts of their own city; and the inspection of the slums which followed disclosed a state

of matters that had certainly not been realised. The result of the inspection brought about absolute unanimity in the Committee, and Mr. Ure was allowed a free hand in preparing a Report. It was not confined to what the deputation saw and heard ; but its outstanding features were the remedial measures which it recommended. Every one of these measures was, in after years, given effect to—with results of the most beneficial character.

The Report made a great impression upon the members of the Town Council, but no active measures could be taken until their powers should be extended by Act of Parliament. To this aim Mr. Ure gave his best energies ; but not until 1862 did he attain his object. In that year, however, the Police Act for the city was obtained, in which most of the recommendations of his Report were embodied. Immediately after the passing of the Act, a special Sanitary Committee was appointed with Mr. Ure as its head. He set to work in earnest. His first aim was to secure the highest medical skill that could be found. The words in which, years before, he embodied his ideal of a Medical Officer of Health seem prophetic of the coming of the first Medical Officer of Health which Glasgow possessed. Shortly before the passing of the Act, Mr. Ure's attention had been drawn to Addresses upon Public Health, which had been delivered by Gairdner. Although he was a distinguished member of the Edinburgh medical profession, Gairdner had not yet made his name a household word. Mr. Ure asked Mr., now Sir Nathaniel, Dunlop to accompany him to a meeting in Glasgow, which Gairdner had consented to address, in order that they might seize the opportunity of considering, in the first place, his suitability for the post of Medical Officer of Health ; and in the second place, whether he was likely to accept the office if it were offered to him. The zymotic

scourges, producing the appalling death-rate, gave Sir William the text for one of his most stirring addresses, and his extraordinary descriptive and persuasive powers aroused the feeling of the audience, impressing everyone who heard him with the need of doing something to check disease. Mr. Ure and Mr. Dunlop, who listened to the address with a special purpose, came to the conclusion that the man for the work had appeared as if specially sent, and it only remained to ascertain whether he could be induced to undertake the duties. Obviously, the task could only be undertaken with an adequate medical staff, of which the Chief Officer should be the head. It was proposed that five medical men should be appointed to serve under the Principal Medical Officer; and five men of ability and energy were found willing to undertake these duties—one of these was the late Professor James Dunlop, brother of Sir Nathaniel, to whom the city is so deeply indebted. Gairdner was then approached, and, as the scheme commended itself to him, he accepted office in 1863. In addition to the five District Surgeons of Police, who acted as Assistants to the Medical Officer of Health, a special non-medical inspector was appointed two months after the inauguration of the new system; he was the sole representative of the sanitary staff. How great were the difficulties by which they were confronted may be read in the volume already referred to :—

"The Medical Officer and his pigmy staff found themselves immersed in a rapidly rising flood of typhus. Since 1847 there had been no epidemic which, judged by the standard of those days, would be designated 'great.' Yet the disease was never for a day idle. In 1859 it touched the lowest point of prevalence; it carried off only 381 people. But its tribute was advanced steadily year by year until, in 1864, it rose to 1138, and in 1865 to 1177. As usual, nothing had been done to prepare for the evil day, still less

to avert it. The fever-flood ebbed and flowed, its movements registered by the capacity of the Infirmary fever-house, and remarked upon with gratification when the permanent wards were sufficient; with anxiety when they were full. Dr. Gairdner was in the position of a Commander-in-Chief newly appointed; an active enemy swarming over the land, holding every strategetical point, well-found and well-equipped, while he possessed nothing but his commission. He had to recruit and drill and equip his army, to subsidize mercenaries, to bear a brave front, and make the most of his meagre resources. In January, 1864, three 'non-medical officers, selected from the Police Force for special sanitary duty,' were added to the staff, and, shortly after, two shops (59-61 College Street) were fitted up as a 'Sanitary Office' at a rent of £25. In September, the first Municipal Disinfecting and Washing-house was established (66 High Street). The ground rent was £5, and the cost of erection and fittings, £244! A staff for fumigating and lime-washing infected houses was organized and placed, with the washing-house, under the Inspector of Cleansing. As the year advanced into winter, the usual difficulty of hospital accommodation arose. Conferences were held between the Managers of the Royal Infirmary, the Parochial Boards, and the Police Board with their Medical Officer. The Board resolved to provide temporary accommodation under the 1862 Act. Many offers were made for adaptable existing buildings, and at last a willing proprietor of a disused mill in Anderston was discovered. But the moment the proposal became public it excited the neighbourhood into an opposition which was frantic in its threats and impossible to withstand. There was nothing for it but to build, and a site was purchased in the neighbourhood of St. Rollox where, amid deep snow, a beginning was made, and a pavilion hospital of wood on brick foundations was erected, furnished, and opened with 136 beds on 25th April, 1865—*the first Municipal Fever Hospital.*"

Gairdner grappled with the position of affairs in a manner which, at this date, arouses our enthusiastic admiration.

Once more reference may be made to the Memorial Volume :—

" Notwithstanding the exigencies of Dr. Gairdner's position, within a few months of his appointment he found time to make ' a personal visitation of most of the epidemic localities within the boundaries of the Glasgow Police Act ' and to describe in detail, without rhetoric but with unreserved fidelity to fact, what he observed, which seemed to him to explain the prevalence of epidemic fever in Glasgow. The main body of these observations was published in his third Quarterly Report for 1863, before he had been a year in office."

Gairdner's work, though supported by all the influence that Mr. Ure and his Sanitary Committee could exert, was, for a time, much hampered. It became clear at length that one of two courses must be adopted—the Sanitary Department either strengthened or terminated. Mr. Ure was not the man to retreat. Knowing his worries, Sir Nathaniel Dunlop (then Lord Provost of the city) obtained from Mr. Ure permission to write a letter to the Editor of the *Glasgow Herald*, and a powerful communication, bearing his initials, appeared in that paper on 12th August, 1879. It elicited a leading article of great weight, which, with the arguments of Mr. Ure, and the letter of Sir Nathaniel Dunlop, changed the whole current of thought among the opponents of the Health Committee ; men who had previously shown no sympathy with the Medical Officer of Health and his herculean labours, suddenly discovered their mistake, and became his strong supporters. The Sanitary Committee of the Town Council became its most popular department, and members considered it a privilege to co-operate with its Chairman in his noble work. Sir Nathaniel Dunlop, to whom, as well as to Sir William Bilsland, my warm thanks are due for much information

THE SENATE LEAVING THE OLD COLLEGE IN 1870

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which is contained in the preceding paragraphs, has kindly summarised the results of this alteration of attitude in the following remarks :—

"The relief which this change brought to the over-strained leaders in the sanitary movement can hardly be realised. Dr. Gairdner had now for eight years stood bravely to his post as head of the medical staff. He had come to it, had seen its possibilities and felt its harassing care, and he had conquered ; conquered not only the disease and death against which he had so bravely fought, but the discouragement that had pursued him, which to his sensitive nature was not the least of his difficulties. Now that the way was cleared and the various departments set free to pursue their allotted tasks with augmented forces, it became evident that the medical section, the guiding and inspiring head of the movement, would have increased responsibilities imposed on it which would claim the whole time of its professional chief. This Dr. Gairdner began to realise. He had accomplished the dream of his life. He had done his part in showing how disease and death could be modified and controlled, and prepared the way for a successor who could devote his whole time to the work.

Dr. Gairdner's own high and special gifts were now no longer needed in it. There were for these, besides, other and not less important calls. His professional work in the University and the demands upon him as one of the leading medical consultants of his day gave a new and enlarged field for the exercise of his rare talents. The retirement from a field of duty to which he was attached was rendered less trying by the joy of feeling that he had contributed so largely to the beneficent results that followed on the great movement originated by Mr. Ure. The reduction of the death-rate by its means to about a half of what it had been, representing an annual saving of many thousands of lives, was indeed no common joy.

In the following year, along with his chief assistant, Dr. Dunlop, who had been invited to give his entire services to

the work, he retired. The organisation was changed by the appointment of an Exclusive Officer in the person of the late Dr. James B. Russell, B.A., M.D., who, following in the steps of his predecessor, made of the Department the great success to which in later years, under the guidance of Dr. Chalmers, it has attained."

Gairdner married, in 1870, Miss Helen Bridget Wright, daughter of John Wright, Norwich, whose residence at that time was at the pretty little village of Thorpe, about three miles from the city. She was a cousin of Mrs. Sanders, wife of his life-long friend, Professor William Rutherford Sanders, at whose house he had enjoyed the opportunity of meeting her.

He brought his bride home to his residence, 225 St. Vincent Street, where they and their children lived until 1877, when the family moved to the house belonging to him officially as Professor of Medicine, No. 9 The College. The union was not only supremely happy in its immediate relations, but was also ideal in the family ties by which husband and wife were even more closely united. This is not the place to dwell upon the subject of their family life, which will be fully detailed in a subsequent part of this sketch.

During the same year there also came about the memorable removal of the University from the old home in the High Street to the new site on Gilmorehill. Many of us felt a pang of regret at leaving a historic scene, hallowed by ancient associations, although everyone had come to recognise that a transference was unavoidable. The interesting picture representing the Senate on the old stairway is reproduced in connexion with the event.

Early in 1871 the birth of Gairdner's first-born took place—a son, who bears the name of John Francis Robert—and

the event filled the hearts, not only of himself and his wife, but of the surviving grand-parents, with delight.

After resigning the position of Medical Officer of Health in 1871, he was able to devote himself heart and soul to the duties connected with his Chair, his wards, and his practice. During the three decades, beginning with his demission of office as Medical Officer of Health, he was continuously occupied with his public and private engagements; but he devoted a considerable portion of his time to observation and research. In addition to Reports upon examples of the most varied forms of disease, he published an admirable Address on "The Progress of Pathological Science—John Baptist Morgagni and his Successors" in 1874; two lectures entitled "I. Lectures, Books, and Practical Teaching; II. Clinical Instruction; Being Introductory Addresses delivered in the University and in the Western Infirmary, 1877"; and in this same year appeared his classical article upon "Angina Pectoris and Allied States, including certain kinds of Sudden Death." This formed part of the Fourth Volume of Russell Reynolds' "System of Medicine." In the following year there also appeared the article in Finlayson's Manual on "The Physiognomy of Disease."

In addition to this mass of work, Gairdner continued to be one of the most stimulating members of the Glasgow Societies—The "Medico-Chirurgical" and the "Pathological and Clinical." The latter owed its foundation in great part to his energy and influence; he was the first President in 1873. In both he remained one of the kindest yet keenest of critics, as well as the most suggestive and most invigorating of debaters.

When the British Medical Association met at Edinburgh in 1875, Gairdner was Vice-President of the Medical Section, and, as substitute for the President, gave a pregnant address

on the Brunonian Theory. It is worthy of note that this was the meeting at which his friend Warburton Begbie gave the remarkable address on Medicine, in which he compared the times of Hippocrates and the Nineteenth Century.

During the year 1876 the death of Professor Laycock took place, and a lively discussion at once ensued as to who ought to be his successor. Three prominent Edinburgh candidates at once entered the field—George William Balfour, Daniel Rutherford Haldane, and Thomas Grainger Stewart. A very large number of men, not only residing in Edinburgh, but forming, as it were, the ramifications of the University throughout the length and breadth of the land, were warmly in favour of an invitation to Gairdner, to offer himself as a candidate for the vacant Chair. This, as a matter of fact, was carried out; but Gairdner, while warmly appreciating the request which had been addressed to him, replied that it was impossible for him to give the matter his favourable consideration until William Rutherford Sanders had been, in the first place, asked to be a candidate. This was done; but Sanders took so long to make up his mind, that the opportunity of successful action on the part of Gairdner's friends was lost; and, although he did allow it to be known that he would be willing to accept the Chair, after Sanders had declined to take any steps, his decision came after the Curators had practically made up their minds on the subject, and Grainger Stewart was elected.

As Laycock had been Physician-in-Ordinary for Scotland to Her Majesty Queen Victoria, this honourable office was also vacant; and, to the gratification of Gairdner's numerous friends, it was offered to and accepted by him.

It is characteristic of the man that he took the opportunity, at the commencement of the winter term, to make

some congratulatory remarks upon the new Professor, which brought forth a grateful acknowledgment from him :—

" 19 Charlotte Square, Edinburgh, Nov. 4th, 1876.

My dear Dr. Gairdner,

I feel that I must write you a line of thanks for your kind and generous reference to me in your introductory lecture. I value very highly the good opinion you express as well as your expression of it. I hope you will long enjoy the high honours which the Queen has bestowed on you.

Believe me,

Yours with much regard,

T. GRAINGER STEWART."

Gairdner must, about the same time, have written to his father in connexion with the episode of the election, but no trace of the letter has been found. The reply which it elicited, however, is now annexed :

" 45 Northumberland Street, Edinburgh, 10 Nov., 1876.

My dear William,

Our dear old father is much gratified by your kind letter ; I see that he is—besides, he bids me say so ; at the same time he distinctly asserts that Dr. Foulis must have mistaken, although in a most natural way, his thoughts or feelings. He says that the disappointment caused by the election once over, he has never since dwelt upon it, either as a matter affecting your interests, or as a subject for indulging even for a moment in vain regrets—that is what he says, and we all know how truly. He rests with pleasure and gratification on the thought that the whole course of your action—every step of it—has been right ; and recognises that every step only the more brought out the high estimation in which you are held, and the true worth of your character. He rests in full on the thought that you hold a high—a very high—position, and believes you have every prospect of a long and happy life. Whenever

he may have looked back on the election, it was in the way of regarding it as evidence that the state of things is unsound —unsafe for the profession and the public. In this sense he has often commented on it in my presence, observing that a system of patronage that has led to some recent appointments cannot and should not be allowed to stand. I heard him talk to Dr. Keith in that sense myself; but I have not yet met Dr. Foulis. However, our dear old gentleman says emphatically that Dr. Foulis has quite mistaken (however naturally) what was in his thoughts. He has told me just now what he remembers to have said to him, and he added, 'perhaps I was wrong, but I do not know that I was, in saying that I would scarce see out the present month; Dr. Foulis has taken that as if I were dwelling on the disappointment, when I only meant it as a sober reflection on my state of feeling.' He was telling me since I came, how there was nothing of gloom about his religion or thoughts of the future; how on the contrary there was much to make him happy; yet he added, 'you may pass it on to William when you write that without any gloom, there is, and must be, gravity,' and the thoughts that are only natural at a time like this may well have been mistaken as I, who best know, feel they were.

I have discharged my message; and I cannot dwell, as I might wish, on the many calm, and yet bright, and hopeful expressions which have passed from him to me even in the short time I have now since my return been with him. But it is scarce conceivable that any one could leave this world in a more grateful spirit as to the past, or more hopeful one concerning the future.

We were sorry to have come through Glasgow without a look in at St. Vincent Street, but on the whole felt it best to come on at once. I need not say we are truly glad to be here.

Ever affectly. yrs.,

JOHN S. GAIRDNER."

He delivered the Morison Lectures of the Royal College of Physicians of Edinburgh in 1879, dealing especially with

modern views as regards the nature and treatment of insanity.

During the eighties, Gairdner took part in the Glasgow Health Lectures; he was President of the Medico-Psychological Association in 1882, and Honorary President of the Glasgow University Medico-Chirurgical Society in 1885. In connexion with both, he delivered addresses—that given before the University Society being part of the Morison Lectures of the Edinburgh College of Physicians, which he had given in 1879, as already mentioned. He received the Honorary Degree of LL.D. from his old Alma Mater in 1883, and in 1887 the University of Dublin conferred upon him the Honorary Degree of M.D. In the latter year he was also elected to the Honorary Fellowship of the Royal College of Physicians in Ireland. He was President of the British Medical Association at its meeting in Glasgow in 1888, when he delivered the famous address, "The Physician as Naturalist."

During this period, his most important medical work was the delivery and publication of the "Lectures to Practitioners, Tabes Mesenterica," published in 1887. He published in 1884 an "Address to the Glasgow Graduates," delivered in 1883; and an "Address to the Sanitary Institute," also delivered in 1883. An "Address on Medical Education at Dundee," was delivered and published in 1885; "A Plea for Thoroughness" formed the Introductory Address at the Yorkshire College, Leeds, given and published in 1889. A number of his addresses were collected and published in 1889, under the title of "The Physician as Naturalist."

In the final decade of last century, Gairdner published some very important papers: "The Prevention of Epidemic Cholera"; "The Treatment of Typhoid Fever," in conjunction with Sir James Barr; the classical "Case of Obstruction

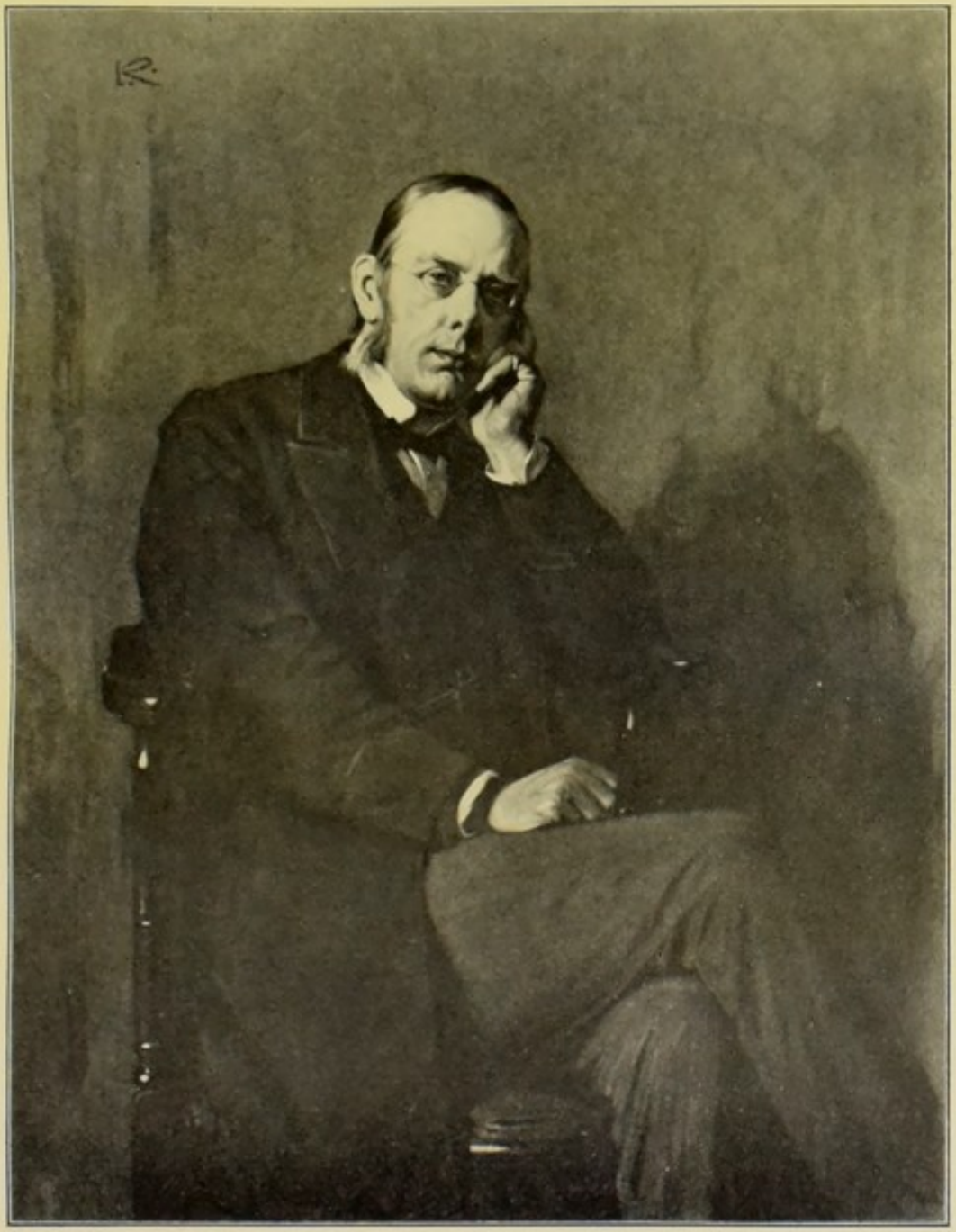
of the right Auriculo-ventricular Orifice" ; "Case of a Small Aneurysm of the First Part of the Arch of the Aorta opening into the Pulmonary Artery" ; and the important article "Aneurysm of the Aorta," forming part of the Sixth Volume of Clifford Allbutt's "System of Medicine." In addition, he gave the "Introductory Address of the Edinburgh Royal Medical Society," in 1894 ; an address "On the Advantages of Pathological Research," in 1896 ; the "Graduation Address in the University of Glasgow," delivered and published 1897 ; and "The Two Disciplines in Education" —an address delivered at the opening of the winter term at the University College of Birmingham. "A Tribute to the Memory of Oliver Wendell Holmes" ; and "Two Addresses on Church and Church Services" were respectively published in 1894 and 1899.

During the autumn of 1891, he visited the United States, and, contrary to his usual custom, he kept a diary, intended for the instruction and amusement of his children. As all the facts, with many extracts from his narrative, are detailed in the sequel, it is not necessary to allude further now to his interesting experiences.

He was, in 1892, elected a Fellow of the Royal Society of London ; this distinction was the more highly esteemed by him, since he knew nothing of the suggestion that he should be selected until the gratifying announcement was received by him.

During this period, at the unanimous request of the Fellows of the Royal College of Physicians of Edinburgh, he consented to become President, and in December, 1893, he was formally inducted to that office, which he held during the two following years, with universal acceptance on all sides.

He had throughout his academic career always striven to bring others forward, and to remain in the background



SIR WILLIAM TENNANT GAIRDNER, K.C.B.

From the original picture by Sir George Reid, P.R.S.A.

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himself, content to stand by so long as duty was well done by someone. When a vacancy occurred in the representation of the University of Glasgow on the General Council of Medical Education and Registration in 1886, he proposed Professor Leishman who was duly elected to the office. But on Leishman's death in 1893, there was no one so fitted to succeed him as Gairdner, and, yielding to the unanimous wish of his colleagues, he consented to be appointed. At the same time he also became a Member of the University Court.

Amongst Gairdner's old pupils, a movement arose during the early nineties, to present his portrait to the University of Glasgow, and to offer a copy of it for the acceptance of his family. The proposal had a very cordial, as well as widespread, response. As it was determined from the first that no one should be allowed to subscribe unless he had been a member of the Professor's class, either in Edinburgh or Glasgow, many of us who would have liked to take part in the presentation, as admirers of Gairdner, were prevented from doing so, as we had not been officially connected with any of the classes. The Committee of the subscribers secured the services of Sir George Reid, President of the Royal Scottish Academy, and the portrait of Gairdner is one of the most successful which this distinguished artist ever painted. On Saturday, 28th January, 1893, in the Bute Hall of the University, the portrait was presented to the Very Rev. Principal Caird, as representing the University of Glasgow, and a replica was at the same time given to Mrs. Gairdner. Principal Caird presided and there was a large attendance of ladies and gentlemen.

Dr. Coats, in making the presentation, said :—" I rise in the name of between 500 and 600 gentlemen, whom I think I may call my fellow-students, inasmuch as they have all been pupils of Dr. Gairdner, to present to Principal Caird,

as the head of the University, the portrait of one of its most distinguished Professors. For some years back, I have frequently heard the opinion expressed that a portrait should be painted of Dr. Gairdner, so that his appearance should be commemorated and preserved. The present movement actually took shape from a letter which I received from a much respected practitioner in Glasgow, Dr. Quintin M'Lennan, who wrote saying that the feeling was so strong that he thought the matter ought to be proceeded with, and he was good enough to say that he considered I was the person to take the lead in the matter. It was decided to limit the gift to Gairdner's old pupils. Dr. Gairdner began his career as a Pathologist in Edinburgh, and I think I am expressing his own opinion when I say that in his work as a Pathologist, he laid the basis of his attainments as a scientific physician. In those days, Pathology was not regarded as of sufficient importance to occupy the entire attention and be the life-work of a man, and partly for that reason he forsook Pathology in that sense and took to practical Medicine. But from intimate personal knowledge I can assure those present that whilst Dr. Gairdner became a practical Physician, he did not cease to be a scientific Pathologist. For ten years he continued in Edinburgh, lecturing on medicine and doing the work of a physician in the Royal Infirmary, and in 1862, he became Professor of Medicine in the University of Glasgow. During all the intervening thirty years he has passed through his class men who have settled all over the country and whose representative I am now in presenting this portrait to the University. It is long since I was a student in the technical sense under Gairdner; but, looking back through this long vista of years, I will try to embody the impressions which I formed of his teaching. I think it is acknowledged that Gairdner's teaching was more educational than didactic; that his endeavour was more to embody principles and to give philosophical insight into the phenomena of disease than simply to teach facts. To the average student, the utility of this is not always apparent. He is, perhaps, too

intent upon examinations to relish that kind of thing ; but, looking back with the foundation of a wider experience, I think Dr. Gairdner's students all through felt that the insight which he gave them into the science, the principles and the facts of medicine, the enthusiasm which he instilled into their minds in his class-room, stamped him as perhaps the man whose impress they bore throughout their lives. They had been able to profit by the practical carrying out of his methods in his teaching in the wards of the infirmaries. And here, again, the average student perhaps felt that this minute and careful observation, this painstaking recording of facts, was an arduous, and, I have no doubt, many of them thought, a bootless task. But, looking back upon those years, they now felt that those scientific methods which were exemplified in Dr. Gairdner's clinical work, had been to them a lifelong lesson. Dr. Gairdner's personal influence upon his students has been very great. The transparent truthfulness and sincerity of his character, the grand simplicity of his mind, endeared him to their affections, and there are few of his old students who do not think of him with feelings of the warmest regard. The function of the University Professor is not exhausted by his work in the class-room. To my mind, the University Professor should be a centre of work, should be a stimulator of those who desire to do scientific work. In that respect Dr. Gairdner has thoroughly fulfilled the functions of a University Professor. With most men, as years accumulate and youth fades away, the ideas are liable to get fixed, and the warm enthusiasm of youth to pass off ; but I think I may also say that, though we fondly speak of old Gairdner, he is in openness of mind, in readiness to appreciate new ideas, and to acknowledge the work of others, as young as any of us, and younger than most. All of us who have been trying to do work in Glasgow feel the stimulus of his encouragement. We all feel the truth, the directness, and kindness of his criticism ; and I personally cannot refrain from expressing my own gratitude to him in this respect. Before sitting down, I have only further to say that the

portrait which is about to be unveiled is from the brush of Sir George Reid, President of the Royal Scottish Academy, and I believe, without anticipating criticism, I may say it is well worthy of that master, in fact I think the opinion of those who can judge is that, if not the best, it is one of the best of his productions. I have also to say that those who have subscribed to the fund for procuring the picture will receive each of them a mezzo-tint engraving by Mr. James Faed, whom I am happy to see present. In handing over this picture to the University, I desire to say that it is an expression of affection, esteem, and regard for our old teacher, and that we think it will be a worthy commemoration of his great work in the University and in medical science." At the end of Dr. Coats' remarks the portrait was unveiled amid loud applause.

Principal Caird accepted the picture in these terms :— " I need scarcely say, on behalf of the University authorities, that I accept with much gratification the gift which, in name of the subscribers to the portrait, Dr. Coats has offered to us. It is the portrait of one of our best and most eminent teachers, a man, not only of ability of the highest order, but of singular elevation and purity of character, who has rendered incalculable service to the University during a long course of years, and whom we all delight to honour. I shall only betray my own ignorance if I attempt to say anything of the grounds on which Dr. Gairdner's scientific and professional reputation is based, of his profound and comprehensive knowledge of the various departments of medical science, of his marvellous skill in diagnosis and success in the treatment of disease. But Dr. Gairdner has long passed beyond the point at which a man's reputation is confined to the circle of professional experts. It is the proud boast of this University that it numbers amongst its teachers men of whom he is one, who are of more than European fame, and whose names are familiar as household words to multitudes of the general public as by universal admission foremost in their own department of the realm of science. In listening some years ago to that

admirable Address which he delivered in this Hall, as President of the British Medical Association, I remember that he was unconsciously drawing his own portrait in depicting that wide range of knowledge, and those varied and even non-professional attainments which went to the making of a truly accomplished physician. I cannot recall his words, but the general drift of his discourse was to this effect, that the scientific physician must know something of many branches of knowledge not strictly embraced within the technical limits of his own vocation. He must, he told them, be a scholar and a linguist, for not only did the history of the terminology of his art imply some acquaintance with the classical influence, but he must have access to the treasures of scientific research and observation which were contained in the medical literature, systematic and journalistic, of France and Germany. He must be a physicist, for whilst the living body was the seat of phenomena which could not be explained by mechanical and chemical forces, yet the latter, as everyone knows, both directly and in their action and reaction on external environment, played a part in the economy of the living frame as indubitable and important as in the inorganic world. And to name one other qualification, the scientific physician could not be said to be fully equipped for the study of man's physical nature without some knowledge of the science which dealt with that of the higher spiritual nature of which the body was but the organ and instrument, and yet whose activities were so closely conditioned by the healthy or morbid action of the latter. Lastly, if the scientific physician was not to be merely a student, but a teacher of his science, he must possess that gift of sympathetic insight into the aptitudes and difficulties of younger minds, and that power of clear, concise, and fluent exposition, which were indispensable in order to great success. It is not necessary for me to point out the particular application I would make of these remarks, or to show in what rare combination the qualifications I have enumerated are united in the man we have this day met to honour. But there are other and higher than

intellectual qualities which are not always found in combination with intellectual eminence. I am constrained by Dr. Gairdner's presence from stating all I should like to say of his personal character and worth, of his conscientiousness, his candour, his high sense of duty, and of that kindliness, considerateness and sweetness of nature which have drawn to him the respect and esteem of his colleagues and the affection of those whom he has admitted to the circle of his private friendship. And I will only venture to add that there are cases, and his is one of them, in which the absence of display and self-assertion, the genuine simplicity and modesty of a man's nature, half conceal his intellectual eminence from those who are in daily contact with him. The honours that would have turned many a weaker man's head have left him as simple-minded and unpretending to this hour as at the beginning of his career. Long may he yet live to devote his talents to the service of society and to enjoy the honours he wears so meekly. The presentation of a man's portrait is sometimes associated with the melancholy feeling that the subject of it is regarded by the world as drawing near to the close of his career. I think I know from personal experience that portraits are sometimes presented to men who have done little to deserve them, and in this respect his case and mine are not parallels, yet in one other respect Dr. Gairdner may take this comfort from my experience that a similar honour was paid to me some ten years ago and I am alive to-day to tell the tale."

Dr. Yellowlees said :—" The presentation will lose a great deal of its significance if special prominence is not given to the personal element in it. Dr. Gairdner's own pupils desire to honour not only his professional attainments and erudition, which are rare indeed, but specially to recognise his personal qualities and endowments, which are rarer still. We felt, therefore, that the presentation would be incomplete if we were only to place his likeness in the Halls of the University, and not on the walls of his own home. Therefore we desired the gifted artist, whose very successful work we have just seen, to produce another likeness of more

suitable size, which might be offered to Mrs. Gairdner and their family, as a further expression of our regard and affection for our honoured teacher. I deem it a great honour and also a great pleasure that the duty of presenting the portrait has been assigned to me. I owe it to the fact that I am one of Dr. Gairdner's oldest pupils, and therefore one of his oldest friends, for all Dr. Gairdner's pupils found him their friend. I was prizeman in Dr. Gairdner's class thirty-seven years ago. A year later, I was his house physician in the Edinburgh Royal Infirmary, and our friendship has been unbroken ever since. I remember the enthusiasm of the young pathologist; I remember his appointment as hospital physician. I remember also the equal interest with which all his old friends learned that he was about to assume a bride nearer and dearer than the medical science, to which he has been wedded so long; and all of you know how bright and how blessed that union has been. It was not only because of the influences of home upon him that they wished to present this 'Home portrait,' but also because of the intense degree in which his personal influence entered into his teaching. He taught us in the true philosophical spirit of modern medicine, which was his own spirit, how by minute and accurate observation they could get at the real facts underlying the symptoms; he showed us how by careful balancing of possibilities, errors of diagnosis could be avoided, how by useful deduction, the real ailment is to be detected, and how, founding upon all that, the rational treatment is to be adopted. He taught us, further, by his daily example, how a patient is not a mere specimen of disease, but an afflicted fellow-man; and the patience and kindliness with which suffering must be helped. All this Dr. Gairdner has been for forty years to thousands of students, and to all these students he has been an immeasurable power for good. It is this personal feeling which is so prominent in the presentation we desire to make this day. We honour the Professor and we love the Man. I ask Dr. Gairdner, in name of his old pupils, to receive this 'home portrait' for Mrs. Gairdner and their

family. We offer it with the very warmest feeling, with a true and deep sympathy for the shadow that at present darkens your home, and an earnest prayer that every blessing from on High may ever rest on your home, that you may be long spared to be its light, and believing that when we have all passed away, your descendants may look upon that portrait and learn what manner of man it was whom his students delighted to honour." The portrait was then unveiled amid loud applause.

Dr. Gairdner, in acknowledging the presentation, expressed the hope that he would not be expected to show that he was quite easy. "I feel the extraordinary difficulty," he said, "of speaking adequately—or, indeed, of speaking in any sense so as to convey what I mean—in answer to remarks that are so highly charged with eulogy. My reply must, to some extent, be in an autobiographical and therefore somewhat egotistic form, but I may disarm the egotism in some degree by a preliminary confession, which I make in all frankness and simplicity, for when I look all over my past career the thing that at once strikes my own mind is not what I have done, but what I have failed to do. It always appears to me that, with the grand and glorious opportunity that was presented to me as a teacher in two of the largest schools in the kingdom, I ought to have done a great deal more, both for the art and science of medicine and for the communication of it to others. At the same time, I may claim this, that with whatever success or the contrary I have fulfilled the function, it has been a very straight line of life to me. It has been from the first, and it has been maintained to the close, always the purpose of my life, quite definitely set forth to my own mind and adhered to with perfect and absolute unswerving constancy, from first to last. When a young man, or rather a boy, I thought that medicine was not the line of life I would like to follow. What was to be the line I am not quite sure that I knew myself, but I had a feeling that it would not be medicine. I do not know even now what has ruled it thus, but now that I come to look back upon the choice I

think it may fairly be called a providential ruling. I hardly think that in any other line of life I could have succeeded so well. One of the circumstances undoubtedly which was a largely-determining cause of my taking to medicine was the fact that it was the profession of my father, a man well remembered even now in Edinburgh as one of the most devoted, single-minded, and absolutely just and righteous of all her many men of professional distinction. Many recollections of my student days crop up. It is interesting to myself, at all events, to remember that some of those who sat on the benches with me as a student have risen to positions of distinction, and not a few of them have also filled positions as teachers. On returning from Italy, in 1846, I at once began hospital work, and I am not sure if I am not the only man in Scotland who can say at this moment that from the year 1846, when I entered the Edinburgh Royal Infirmary in the capacity of House Surgeon, and served for two years, until the present day—that is to say, forty-seven years—there has never been a break of quite three months in my service of hospital duty. In 1848, when Dr. Bennett obtained the Chair of Physiology in Edinburgh, I was at a very early age appointed pathologist to the Edinburgh Royal Infirmary, and it was that, as Dr. Coats said, which laid the foundation of whatever medical knowledge I have that is available for teaching. The very first, as this portrait is the last, testimonial which I received to my work was three volumes of ' Bright's Hospital Reports,' which I greatly valued, not only in themselves—everyone knows the value of that book—but because they bore on the fly-leaf the inscription, in the handwriting of Dr. Warburton Begbie—' Presented to Dr. William T. Gairdner, by the gentlemen attending his first course of lectures on pathological anatomy, session 1848-49.' Among the subordinates with whom I had to do when I was pathologist in Edinburgh were several men of distinction, some of them no longer living. I had the great satisfaction and pleasure of co-operating with Dr. Kirk, who was known as the friend of Livingstone, and long Consul of Zanzibar,

where he did an immense deal to preserve right relations between this country and that portion of Africa under British influence. Dr. Howden, Montrose, and Sir Alexander Christison were among those associated with me at that time. In 1853 I began to lecture on Medicine. For the encouragement of young Professors, I mention that my first class only numbered 12 students. Next year, the number had risen to 52, while, in the third year there were 124, which at that time was regarded as the largest extra-academical class of Medicine in Edinburgh. At that period of my career I was brought into very close contact with a dear friend, whose handwriting I saw in the volumes which I have mentioned, Dr. Warburton Begbie. It is one of the delights of my life to think that Begbie grew up, as it were, beside me, that he was my pupil in these pathological days, being two years younger ; that he was my competitor but close associate in hospital work, that he was my rival in teaching, and that, during the 13 or 14 years that followed our relations were never for a single moment strained, but always those of perfect friendship. In 1862 I was promoted to the Chair in Glasgow, and it is not necessary that I should say any more about that, because my career in the University is before you. I would like to say that I am a typical example in some respects of the difference between a Scottish Professor and a London Teacher of Medicine. A Scottish Professor is a Professor to the end of the chapter, while the London man is only a Teacher until he has flowed into such a tremendous lot of practice that he does not care to teach any more. There are a good many people who, meeting me in the club about lunch time, little know that the hardest part of my work has been done before 12 o'clock. They see me reading the newspapers, and think what a very easy life I have. They do not think it has been a case of strong tension of mind from 9 o'clock till 12, in the presence of patients in the hospital and of students in the class ; they do not know of the work that has to be done to keep the instruction up to the level that is required, for I am one of those who do not believe in repeat-

ing lectures. I never did believe in putting lectures on paper, so that they can be read session after session. All the lectures delivered to my class are to me a matter of great thought, carefully and freshly pondered over the night before. These few reflections, perhaps, may be enough of a biographical kind. I hope I am not giving too solemn a cast to the present proceedings if I recall to mind that, large as the number may be of those who have been joined with us in this complimentary gift, it might have been—would have been—much larger had the many old and dear friends whom I can look back to as the pupils of my earlier years been with us now to cheer and to help us. But this leads me to indulge another thought, which lies very deep with me, and is entirely in accord with other and more domestic feelings, too sacred to be dwelt upon. Those dear departed ones—almost contemporaries, but still who did not disdain to become in a sense pupils of mine in those early years—are they not indeed with us, with us in spirit, with us in intention, with us in heartfelt sympathy with what we are doing now? Warburton Begbie, Sanders, Murchison—I will name only these three where my memory could easily recall a score at least; and I name them because they were all men of the highest order, and bound to me through life by ties of the closest friendship. Can you suppose that, while my own heart yearns towards them on this occasion, they are altogether unconscious or indifferent as to what is happening to me? I prefer to believe that these men, and many others like them, are really with us this day. I confess in all good faith that ghosts, the spirits of those I have loved and honoured, are very real to me at a time like the present; not, however, the ghosts in white clothes and stage property dresses who terrify children and weak-minded people. I do not, however, envy the man to whom the spiritual world is so shut that he must needs have a materialised ghost to make it palpable. I remember on one occasion, not so many years ago, that an address I gave somewhere or other called forth a response in a private letter from one who wished to convert me to a philosophy—

which, to use his own words, had ' abolished ghosticism '—that was, had completely done away with the whole spiritual universe and God as the beginning and the end of it. I was not competent to argue with this gentleman, for he was far more deeply read in philosophy than I was ; but I made my main proposition by saying that the one thing I could not escape from—that I felt absolutely sure of—was that I at least have a real ghost inside ; and that, as I did not make the universe, it was reasonable to suppose that there were other ghosts about, not to speak of the Holy Ghost, the spirit of the living God, ' in whom we also live and move and have our being.' I did not succeed in convincing this philosopher, nor he me. On the contrary, it seemed to me that he only got rid of ' ghosticism ' by resolving all of what we call realities into phantasms of his own personality. I mention this only to show that in my most settled and calm thinking I regard spiritual things as the greatest of all realities. And this leads me back again to the business of to-day. In your very great loving-kindness, and in your too generous estimate of what I have done in this University, you wish to aid in handing down to posterity the memory, not, indeed, of my spiritual work, but of the frail and fleeting tabernacle in which that work was accomplished here. I am not worthy of so much devotion, but I thank you all the same. It will be an inspiring thought for me, in what remains to me of life, that after I am gone my ghost will still inhabit these halls—not striking terror like the stage ghost in ' Hamlet,' but rather, I would fain hope, shedding some genial influences for good upon future generations of students. And to my dear ones, present and to come, to my family in the stricter sense of the word, it will be an heirloom, and I hope a blessing, for generations yet unborn, to look upon the features which you see here so faithfully depicted by my friend Sir George Reid."

Gairdner was deeply affected by the warmth and kindness of his reception on this occasion, and he was more especially touched by the loving references to his family

relationships. This is abundantly testified in a letter to his old friend, Dr. Yellowlees :—

"9 The College, Glasgow, Feb. 17, 93.

My dear Yellowlees,

Owing to my imperfect hearing of your speech on the 28th ult. it is only now that, in reading a proof sent me by Dr. Coats of the revised proceedings, I can really say that I have become fully possessed of its contents, and of the very generous terms in which you have spoken of my domesticities. I do not indeed know that if I had heard every word, I should have replied in other terms than I did, because these sanctuaries of the spirit cannot be brought much into the light of day; but I think I owe it to others than myself to say to you now that I am *sure* my Wife and children will feel as grateful to you as I do for your graceful and heartfelt tribute to all that has made the evening of my life so much more satisfying even than the ardours and aspirations of its earlier time.

I remember once sitting at a dinner-table with a great philosopher and literary personage now in H.M. Govt.,[1] and hearing him discourse in a kind of contemptuous and disparaging way of men who departed from more noble ambitions in order to satisfy the claims of fatherhood and bury themselves in the family; and although saying but little I thought a good deal as to whether it was worth while to be a great philosopher on these terms! You and I will probably agree in holding the contrary; at least till we have emerged from the chances and changes of this mortal life, when it is just possible that our vision may have become cleared so as to make us more fit for the society of such as Mr. ——?

Yours most truly,

W. T. GAIRDNER."

[1] Mr. Gladstone's fourth administration. It is easy to guess the personality of the "great philosopher."

In a letter to his friend Dr. Dobie in Chester, he makes a humorous reference to the proceedings :—

"9, The College, Glasgow, Feb. 8th, 96.

My dear Dobie,

I am very glad to read in the Chester paper that they have had your portrait done, and also, that it is not to be regarded as the practical close of a long career of usefulness and honour.

When my old students organised a like presentation to me, or rather, to the University with replica to Mrs. G. the Principal (Caird) remarked in accepting the presentation that *he* had managed to survive his portrait-painting by ten years at least, and he hoped that I would do likewise. I am trying my very best to meet that indication, but if I fail it will probably be on the score of eyesight more than of general health. You are several years younger, I think, and yet you have your bairns further out in life than I have. One son only is as yet started in our profession—another is begun. It is not very likely, I fear, that I shall live to see them all in flourishing positions, as I believe some of yours are now.

Let me congratulate you warmly on the large amount of well-earned goodwill shewn forth in this presentation.

I am,

Yours very truly,

W. T. GAIRDNER."

Gairdner took part in the International Medical Congress held at Rome in April, 1894. Most unfortunately, his visit to the historic city, which in youth he had learnt to know so thoroughly and love so warmly, was marred by disaster. During his residence in Rome he was affected by detachment of the retina of the right eye, which brought about considerable disability.

As is well known, he twice declined the honour of knighthood ; but, in 1898, he accepted the distinction of K.C.B.

On this occasion, the esteem in which he was held was abundantly manifested by the shower of congratulations which came from friends all over the world, who thus gave their testimony to the pleasure widely felt on the distinction. The following letter to Dr. Dobie of Chester gives some of his views :—

"9 The College, Glasgow, March 28, 98.

My dear Dobie,

I should not have troubled you with another letter had you not been under a misapprehension in addressing me as 'Bart.' No, no, thanks; no hereditary honours for me! I had, indeed, quite enough of a mental struggle in accepting the K.C.B. and would have been, personally, very well pleased to have gone without all such honours to the end of the chapter.

But the personal distinction implied in K.C.B., however unworthily in my case, is greater even than that of a baronetcy, while it is not open to the objection of being an intolerable burden to a man with a large family.

Wherefore, although not coveting, or even desiring such honours, it appeared to me impossible to decline, and the satisfaction given thereby to so many friends and pupils is not without weight in reconciling me to the burden.

That, I think, is about the whole story.

Yours ever,

W. T. GAIRDNER."

He was the guest of the Glasgow University Club of London on 30th June, 1899, on which occasion Lord Kelvin occupied the chair, and in the prospect of the gathering, he wrote to Dr. Vincent Bowditch in the following terms :—

"225 St. Vincent Street, Glasgow, May 15, 99.

Dear Dr. Bowditch,

I need hardly say to you that anything which, like your letter, recalls your dear father's strong personality, and the sincere regard he had for me, as I for him, is as

acceptable to me as it can be to you. It recalls a time when I was still young and enterprising, when I had my book to write and my professorship to win; but when he was (I suppose) already mature and of high rank in his own country and state, and the kindly interest he took in my hospital work was therefore very marked and grateful to my self-esteem.

Then, although I never knew (never even saw) your mother, I suppose also that he had the additional advantage over me of having found an help-meet; a status which I did not attain for more than ten years afterwards.

It was very pathetic to find him so far down in physical and mental condition that an interview in 1891 was not expedient, but I retain the indelible sense of all that you told me about him, and of his great desire to see me again.

Now, it seems as if I too must be folding my robe about me with a view to a not distant end, as Lord Kelvin and I are the only survivors of the group of professors when I became one in 1862; and at the Glasgow University Club in London I am to be entertained as a guest on the 30th of June, with Lord K. (I hope) in the chair to keep me humble!!

I do wish I could have been in N.Y. on the occasion you refer to.

With kindest regards.

Believe me,

Yours very truly,

W. T. GAIRDNER."

With this, Gairdner's life and work in Glasgow came, it may be said, to an end. At the close of the winter term, in the spring of 1900, he announced his intention of placing his resignation in the hands of the University Court. The news was contained in a Valedictory Address, couched in his usual graceful phraseology, the terms of which are now incorporated here:—

"I have one other statement to make before closing, which may be of some importance beyond the bounds of

this class-room, and therefore I propose, contrary to the usual custom, to allow it to appear in the public journals. I have determined, after much consideration, to apply in the course of the present summer to the University Court to relieve me of the office I hold as Professor of Medicine in this University. It will be proposed that my actual demission should not take place until after the summer graduation, and at such time as may allow my successor to be installed for the winter session in October next. I hope thus to carry on my hospital work and clinical teaching this summer, and thereafter engage in the final examinations, and otherwise fulfil as well as I can my duties towards most of those who are now going forward for a degree in the present year. With the motives which have brought about this determination, it is not perhaps necessary to trouble you ; but it is due to my own feelings to say that in retiring from official life, I am not conscious of any less keen enjoyment of my work as a teacher of medicine than at any time before. My class-room and my hospital remain now, as they have all along been, the greatest of my occupations, and the sources of the most profound satisfaction in endeavouring to serve thereby my day and generation. But what I am coming to feel more and more, though very gradually, pressed upon me, is the difficulty, with failing eyesight, of keeping up with the enormous amount of reading required for a systematic course on practice of medicine at a time when medical science, in its immediate relation to the healing of disease, is making progress ' by leaps and bounds.' I have done my best ; but the new science of bacteriology alone, in its ever-increasing range of applications, both in diagnosis and in treatment, is getting beyond me, whether as regards reading about it, or the use of the microscope to verify what I read. I have therefore come to the conclusion that it is expedient, however painful to myself as the loss of a thoroughly wholesome and stimulating discipline of mind and of body, that in the interest of the University, and in that of your immediate successors, a new and younger professor, with at least a younger pair of eyes,

and perhaps a younger brain as well, is desirable; and I cannot doubt that the University Court will give effect to this opinion when called upon to do so. Meanwhile, it is a great satisfaction to me that I have been able to arrive at this conclusion at a time when my relations to you, gentlemen, are, as they have all along been, satisfactory. What I owe to the affection and enthusiasm of students of medicine during a much longer career than falls to most teachers can never be expressed in words. It can only take effect practically in the very cordial benediction which I now desire to extend to you in the name of many past generations of pupils, many of whom have grown to be aged practitioners of medicine, colleagues, rivals, or assistants in hospital or academic work. Of these last I can only say in general terms that it seems to me now a marvel how excellent, how truly helpful, how assiduous and devoted they have been from first to last, and never more so than now, when the burden of years has possibly made me more dependent on their ever-gracious courtesy and kindness. Few men, I think, can ever have had such a succession of helpers in class and ward duty as now comes to a close for me in the persons of our good friends Carslaw, Ness, and Watson. I can only hope for you, in your turn, that each of you may, during a long lifetime, come to feel that he has become a debtor to so many others in the only form of indebtedness which has high apostolic authority—'Owe no man anything, but to love one another.' The example of loyalty, of devotion, and of goodwill which have come to me during a very long professoriate is perhaps the best legacy I can leave to my successor, and I hereby leave it to him with all my heart."

Needless to add that, after well-nigh forty years' faithful service to the University, his resignation aroused loving memories amongst all his old pupils, when the news of his retirement came to be known. Of the long line of his predecessors, none had done so much to shed lustre upon the Western University, and to extend its reputation as a centre of medical education. His old followers felt that they would

ever hold him in the deepest reverence, as a man of the broadest sympathies, deepest culture, and highest honour; while they recalled his power of inspiring his pupils with the loftiest aims in medical practice, and the warmest feelings of personal affection. As the *Glasgow Medical Journal* remarked, when referring to the impending resignation :—

"The welfare and success of his students have ever been nearest to his heart, and wherever throughout the world good work has been done by graduates of Glasgow, Professor Gairdner was ever amongst the first to recognise it, and to prove, by personal communication, that the old master was watching the worker with feelings of the deepest pride and satisfaction. It is to this paternal interest in the students of the Glasgow School that a great part of the affection in which he is held, and of the beneficial influence his high character has exercised, is due. It will be long, indeed, before the familiar and endearing name of 'Old Gairdner' is forgotten in our midst, and we will follow him into his well-earned retirement with the sincerest prayers that he may long be spared to enjoy the pleasures of a green old age, and to see the results of his labour in the continued prosperity of the Medical School for which he has done so much."

On 19th June, Gairdner was entertained at a farewell banquet, held in the Windsor Hotel, Glasgow, at which Principal Story occupied the Chair, and was supported by many friends of the retiring Professor.

The Chairman, with his usual felicitous eloquence, proposed the health of the guest, and Gairdner in reply made the following impressive speech :—

"I do not intend to endeavour to settle the relative importance of the learned professions, but I cannot help saying that, within the past few days, I have received a copy of a very excellent book upon Materia Medica, known

as Dr. Whitla's 'Dictionary of Treatment.' It is a Chinese copy, in a second edition. I put it to Sheriff Berry to say if he ever knew of a book upon law translated into Chinese, and reaching a second edition. On the whole, I think it is probable that such a phenomenon does not exist. Therefore I think, as regards its catholicity and breadth of requirement all over the world, the medical profession may fairly compete with the profession of law. I do not venture to say that I am altogether in favour of the medical profession. I believe that China is one of those oriental countries where there are no lawyers. That may be for their good, or for their ill. The patriarchal system of that country tends to the discouragement of the family solicitor. The head of the house does it all himself. But perhaps a little more law in China would be a good thing just now. As regards the toast, which has been so kindly proposed, it would be exceedingly indecorous for me to say anything as regards its merits. I may say that its subject represents itself to me in a somewhat different light to what it does to the Principal. It so happens that my 38 years' connection with the University of Glasgow is exactly almost to a month the middle mile-stone of my life. I had lived for 38 years before I was a Professor, and I have lived for 38 years afterwards. If I put to myself the question whether the former or the latter 38 years was the better, or more fraught with agreeable associations and with real permanent feelings of happiness, I confess I would be puzzled to give an answer. I well remember the period of my active youth, and the aspirations and feelings of life before me, the joy with which one entered into even complicated things with the full feeling that I could not but somehow or other come through. I well remember it, and I pay tribute to it. It is a glorious thing to think that we have this dayspring of life that has been accorded to us by Providence. But on the whole, if I were to put it to myself deliberately, and think it over, I rather think I should prefer the later period. At all events, I feel that it

has been to me a period of singular happiness, tranquillity, peacefulness, and enjoyment, and even now that I have got to the time which, if prolonged, the Psalmist says, will be days of labour and sorrow, I really do not feel that the toil, or the infirmities, or the pain of life is so pressing on me in any sense as to make my life less enjoyable than it was before. Such is my experience. I do not congratulate myself on it, or take credit for it. I only remember two things I had to ask for; the one was my Professorship, and the other was my wife. All the rest have come to me, in spite of myself, as it were. I most heartily believe that many people make themselves unhappy, and destroy to a great extent the value of their lives by going about perpetually asking for all sorts of things, and perpetually indulging in grudges because they do not get them. I have been much struck with a remark made by Lord Kelvin on the occasion of his jubilee. He said that at an advanced period in a long life, the chief impression left upon his mind was one of failure. Many people did not understand it. But what he wanted to say was that, if he compared his accomplished work with the ideal he had set before him, his work had been a failure. There is a principle at the bottom of that. Every man who has come to my time of life, and who looks back upon his career, will pass exactly the same judgment upon it as Lord Kelvin did on his own. Most of you perhaps are in the habit of comparing yourselves with your neighbours. That is a very false position to place yourselves in, and can never lead to a satisfactory appreciation of your own objects in life. The true measure is Lord Kelvin's measure. Measure yourselves, not against your neighbour, but against the work that God has given you to do. Of myself, I may say that I began life a maimed person, my sight and hearing being both defective. I cannot but wonder at the marvellous kindness—luck you call it, but I would rather call it beneficence—of a kind Providence, which has somehow or other brought it about that a man who was thus maimed should have been planted in a position where these

things did not prevent him from performing his duties with a certain amount of credit. I feel the very kindly welcome accorded me. You are giving me a solace in my retirement that is not a little needed. It is a great tearing up of the roots, and a great tearing away from old associations of nearly forty years. I do need something like comfort, now I am dissociating myself from the active duties in which I have been engaged."

During the evening some touching verses, by Dr. John F. Fergus, were recited; and, as they are so extremely appropriate, they may well find a place here.

"TO SIR WILLIAM TENNANT GAIRDNER, K.C.B., F.R.S.

19TH JUNE, 1900.

A rumour starts and quickly spreads, as fans the wind some smould'ring fires,
We hear it whispered, spoken loud, and then proclaimed 'Old G. retires';
And hearing scarce can realise the loss, the blank to one and all,
When his loved presence leaves the stage whence he has held us all in thrall.

In memory's garden, 'mid the maze of bygone years, we fain would sit
And list to echoes of the past and watch Time's ceaseless shadows flit,
And mark the magic hands of Change darting her shuttle to and fro,
And, never tiring, weave new threads into the web of long ago.

And, sitting watching, we would grieve to see the pattern ever change,
And tho' so varied, yet, in sooth, how very small the utmost range.
'Coming and going,' 'Life and Death,' 'meeting and parting,' these the threads

That, subtly mixed and crossed and crossed, make up the web
each mortal dreads.
'Et tu Magister!' Change for you has waved her all-com-
pelling hand,
And, following her imperious call, here at the parting paths you
stand
Beloved by all: wherever placed by Fate's immutable decree,
We'll hold you still in memory dear, we never can forget 'Old G.'

Yet though 'Old G.,' Sir William now, an F.R.S. and K.C.B.,
But still the noble, simple soul that he was ever wont to be;
Sans peur, reproche, sans everything that mars the perfect,
courteous knight,
We loved him well in days gone past; we love and honour him
to-night.

'In days gone past!' the words call up a vista of the bygone years
When Youth was ours, when Life was warm, when beat alternate
hopes and fears,
When, fledglings in the world of books, we sate in Alma Mater's
nest,
And fed on Learning's crumbs the while she gently warmed us
in her breast.

And of the crumbs that then we picked none were so plentiful
or sweet
As when (to drop the metaphor) we sat disciples at your feet,
And heard you talk of Sydenham, or Locke, or Galen, or
Trousseau,
Or Graves, or Cullen, Latham too, Sir Thomas Watson, or Charcot,

Or Dupuytren, or Murchison, or Virgil, Dante, or Laennec,
Or Skoda, Traube, or Monro (and others we thought 'extra sec'),
Or Mead, or Hunter, or Defoe, Darwin, Duchenne, or Flint, or
Bell,
Or Walshe, or Stokes, or Corrigan, and others more than I
can tell.

A very mine of richest gems you opened freely to our gaze,
So vast, so bright, we could but pause in wondering and rapt
amaze

At range so great, at taste so rare, at erudition so profound,
And reverently admiring, hope to follow on th' enchanted ground.

And in the wards, methinks I hear the heart-sounds echoing in your voice,
'Lubb Tupp, Lubb Tupp, Luff Tuff, Luff Tuff,' the cardiac murmur's rhythmic noise,
You clothed with meaning ever fresh, and on our minds would oft impress
That Presystolics (wrongly termed) should properly be called A.S.

Though vast your learning, great your skill, and great the image of your name,
'Twas not by these alone you built within our hearts undying fame,
The spotless life, the simple faith, the guileless soul, the kindly heart,
These more than monuments endure, frail triumphs of the sculptor's art.

And thinking on these bygone years the future looms up blank and drear,
Our Alma Mater's not the same with your loved face no longer here,
And though Pandora's opened box was empty save for shining Hope,
We search again and memory find, and finding give it pleasant scope.

And dwell upon the days gone past when you were teacher, priest, and friend,
And hope that every blessing may on your declining days attend ;
That o'er the splendour of your life the sun may calmly, brightly set ;
Farewell, loved master, in our hearts we'll cherish thy remembrance yet.

J. F. F."

THE EVENING OF LIFE

His resignation of the Chair was really brought about by a severe attack of influenza which Gairdner suffered from in 1899; but he had apparently recovered entirely from all the effects of the illness long before leaving the West, and, as was remarked by everyone, he was in his most interesting vein at the great banquet held in his honour on the 19th June, 1900. The family left Glasgow not long afterwards, and he entered with zest into all the preparations for their settlement at 32 George Square in Edinburgh. During the time when the plans were being carried out, they resided at Levenhall, near Musselburgh, to be within easy reach of Edinburgh. His arrangements were unfortunately impaired by illness. Accompanied by Lady Gairdner and Miss Gairdner, he started one day for Edinburgh, and in the train it was observed that he had become unconscious. On reaching Edinburgh, Lady Gairdner felt that medical aid must be obtained, and he was at once placed under the care of Dr. Wyllie and myself. From the attack he recovered in the course of a few hours, and the following day was able to return to Levenhall. A similar attack, but less serious in degree, occurred in the month of October, while Gairdner was in London, where he had gone to receive the Moxon medal of the Royal College of Physicians; but, during the ensuing winter, he remained wonderfully free from all discomfort and occupied himself with various matters of interest. On the evening of Sunday, 4th November, 1900, he delivered a lay sermon in the venerable Abbey at Paisley, and the following day he wrote to his friend, Dr. Fraser in Paisley, a letter full of his quiet humour :—

"32, George Square, Edinburgh, Nov. 5, 1900.

My dear Fraser,

Nothing would have pleased me better than for you to have been present last night, because I know that you have reflected, more than most, on the matters to which my address referred; and it is so difficult to get criticism which is at once thoughtful, and unhampered by prejudice, or even reasonably open-minded and sincere, that I should have counted on you not a little for this.

Dr. Gentles did not say whether he was *officially* satisfied, or not, with my point of view. I had warned him when he pressed me into the service, that I might say one or two things not to his mind; but as he joined with the reporters in asking leave to have the MSS. I presume he at least thinks in a general way that it is worth reporting in part, and will do no harm when so reprinted. My own notion was to get it type-written to the extent of half a dozen copies, and to circulate these to some representative men. I am not caring at all to be approved by the 'orthodox'; but I would like to combine reverence with freedom, as far as may be.

Yours ever,

W. T. GAIRDNER.

You will always be welcome here, if you come to Edinburgh."

He spent a considerable amount of time at the Royal College of Physicians in Queen Street, as well as at the University Club in Princes Street, and busied himself with various literary, scientific, and medical matters; even returning to an old love—music—as may be seen from a letter to Dr. Macphail:—

"32 George Square, Edinburgh, Jan. 2nd, 01.

My dear Macphail,

That very beautiful and restful picture is to hand, and comes quite up to your description and intention in presenting it. Lewis Sutherland and his wife are here with us, and we all recognise the quiet power of the scene over a

troubled spirit, and the warm and kindly feeling for us all, displayed in your letter, and your seasonable gift. I am sorry to say we are not quite at ease about Frank at present, though we hope to hear again, and perhaps better news, shortly. He has been wonderfully preserved both from dangers and from illness, up to now, but in going down to Cape Town along with his friend Kay, with the idea of coming home together, Frank had a sharp attack of malarial fever, which caused him to go in as a patient to the hospital at Wynberg, whence Kay wrote that he was at the moment free from fever, but that we could not depend on his sailing by the first steamer. I hope he may, as the sea voyage would probably do him good. In the meantime, we are trying not to be too anxious, and to wait patiently for more news.

I have been occupying myself at this time with an old friend in the way of music—do you happen to know it? Perhaps the *grandest* of all the German student-songs, which they sing at the very close of the year—as the name implies —'Des Jahres letzte Stunde.' I have harmonised it from old recollections—as I had only the air—and Dr. Hastie has translated the whole song into English, close to the original rhythm which is not an easy job, and done perhaps as well as it is possible to do. It may possibly appear if the omens are favourable.

Yours ever,

W. T. GAIRDNER."

He remained a member of the General Medical Council, and went to London on several occasions in connection with its affairs. During July, 1901, he took part in the Annual Meeting of the British Medical Association, which was held that year at Cheltenham. He resided with Dr. and Mrs. Kirkland, by the former of whom this letter has been kindly sent :—

" Nouvelle, Cheltenham, Feb. 7th, 1911.

Dear Dr. Gibson,

Sir William Gairdner visited us during the Meeting of the British Medical Association here in July, 1901. We

had also as our guest Dr. Reynolds of Manchester, who opened the discussion in the section of Medicine on the previous epidemic of alcoholic neuritis in Manchester and Salford. He contended, and I think proved, it was due to arsenic in the beer. During his address Sir William quietly mused as if half asleep, but I remember as if it were yesterday his rising at the invitation of the President of the Section to join in the discussion. He animadverted on the fact, that in Scotland, where dram drinking was certainly not a lost art, one never, or hardly ever, saw a case of alcoholic neuritis. Clear, humorous, and incisive, his speech was the plum of the discussion and every one present was lost in admiration. Many came up to me afterwards and said what a real treat it had been to hear him.

The weather during the meeting was superb, and he loved to get out on the balcony of my house in the mornings smoking an after-breakfast cigar and chatting usually with Dr., now Sir William, Osler of Oxford and with other friends who came in. He, Sir William Osler, my wife and self used to drive in the afternoons to all the social functions which he thoroughly enjoyed, surrounded as he always was by a bevy of old friends and admirers. For Dr. Ferguson, our President of that year and my surgical colleague at the Cheltenham Hospital, Sir William Gairdner had the greatest admiration. He said he was one of the best, if not the best President we had ever had in his time and 'I have been present at many meetings.' Ferguson reciprocated this feeling, for after the visit he was never weary of saying—'Kirkland, what a charming, courteous, lovable man your old teacher is.' They corresponded for a long time after the meeting. In September of the same year Dr. Ferguson went for a holiday to America to see the various hospitals and their work. Sir William regretted he could not go, and envied Dr. Ferguson, for he remembered what a happy time he had himself spent in the States. In the evenings he used to relate to me many of his American reminiscences. For Professor Pepper of Philadelphia he had the highest regard, and he told me he thought he was the hardest working

medical man he had ever met. I believe he died of cardiac disease like Sir William and my friend Dr. Ferguson. The latter, as you may remember, died suddenly when operating on a patient in our hospital on November 27th, 1906.

Sir William looked forward to visiting us again in the following summer (1902) for he had a great desire to see the Wye Valley. But of course this never came off.

Of you yourself he often spoke and I regretted exceedingly I had never had the pleasure of meeting you.

On his return to Edinburgh I had many letters from him and one of my most cherished possessions is his work 'The Physician as Naturalist' given to me and inscribed thus—'To my kind host Dr. Kirkland in commemoration of a happy week from his old teacher W. T. Gairdner.'

I fear I have inflicted on you a very long and disconnected letter.

With kind regards,

Yours sincerely,

ROBERT KIRKLAND."

That his powers of criticism, as well as observation were keenly alive is proved by a letter to Dr. Vincent Bowditch about this time, in which he points out with much amusement that Mrs. Kate Douglas Wiggin has made a curious botanical blunder in her genial book, "Penelope in Scotland."

"University Club, Edinburgh, 14th July 1901.

Dear Dr. B.,

I am very glad to receive your letter of 3rd inst. and also to thank you for your inquiries into the O.W.H. poem on Lowell. Mr. Turton was kind enough to send me a quite recent one vol. collection of O.W.H.'s poems in which I found one somewhat resembling what I read on the walls of your club, but yet differing, if I am not mistaken, both in order and substance, so that I should still like to see again the real article as I then read it. Mr. Turton brought a letter to me from Blake; but I only saw him for a day or two, at Rothesay, where we were then residing in the early spring, and where he was charmed with the scenery

and the glory of the whin and broom just then bursting into flower. This was in the end of April ; and apropos of this I was talking the other day to Miss Cleveland and her uncle the Monsignor (who were staying at the Roxburghe Hotel) about a botanical slip which amused me very much in that delightful and genial book—' Penelope in Scotland ' by Mrs. Wiggin, which I daresay you know. Miss C. does not know Mrs. W., and I am far from wishing to make mischief about my little discovery, with so thoroughly friendly and well disposed a lady—but to a Scotsman it is, to say the least, very funny to find Miss Penelope walking out with her lover at a time of year which I take (from the context generally) to be about Midsummer ; and to find the romantic young man stopping to gather the ' white-blossomed slae ' —which flowers in early April, before the green leaves come, admiring at the same time the glory of the whin and broom (late April or May) and having the distant hills coloured purple with the heather (which appears in July and August !). I rather fancy that she has mistaken the hawthorn for the sloe ; as the former (in Scotland) is often more a June than a May flower, and is odorous, which the sloe is not, particularly. At all events, I think Penelope must have borrowed her ' white-blossomed sloe ' from Shields' (English) ballad ; and I doubt very much if she ever saw it in nature —certainly not at midsummer.

I am very glad to hear of your memoir of your dear father as so near completion ; and I hope it will introduce me effectually both to him and his English wife—the latter I never saw—and also to your happy family circle.

I gave your message to Mrs. Lattey who is always glad to hear of you and sends her kind love. She is wonderfully well, for 87 years of age.

Yours always,

W. T. GAIRDNER."

In the summer of 1901, he and his son Eric went to Norway. The visit was in every respect most successful, and Gairdner showed his unfailing love of investigation by

paying a visit to the Leper Hospital at Bergen, and by entering into a lively exchange of opinions with Dr. Hansen.

During the year 1901, his charming paper "Memories of College Life" appeared in "The Book of the Jubilee"; while he was engaged in the preparation of the volume—"The Three Things that Abide," which made its appearance two years afterwards.

A letter written in the early part of 1901, to the Rev. Dr. Carslaw, shows how keen was his interest in the doings of his friends:—

"32 George Square, Edinburgh, March 16th, 01.

Dear Dr. Carslaw,

I am happy to think of you as home from such a wealth of Indian experiences; but I think it is in the last degree improbable that I shall follow in your footsteps. Defects both of eyesight and hearing make me rather unfit to be a traveller, though my personal impulses that way have always been, and are still, very strong. When I was younger, I could always break away, alone, to Italy or elsewhere, and always enjoyed it, and I hope profited by it, to the full. Once I went to America, and with almost equal enjoyment. But now, when from my freedom, it would be comparatively easy to find the time, I feel as if I could not go *alone*, anywhere; and it is hardly worth while for a half blind man to have himself 'personally conducted' through new scenes and sights.

On the whole, I think God has been very good to me, in landing me here; and if it should be His will that I go nowhere else till I die, I certainly shall not complain.

If you are so bent on reading up your India, you should get to know my ex-colleague Dr. Hastie, who has a great deal to say about it, and is extremely well read in all things relating to manners and customs in Bengal.

Yours very truly,

W. T. GAIRDNER."

To Dr. Macphail he writes, early in January, 1902, thanking him for his kindly congratulations on the birth of his second grandchild, and telling of his frustrated intention to have the Hogmanay Hymn on New-Year's Eve :—

"32 George Square, Edinburgh, Jan. 12, 02.

My dear Macphail,

I need not say that your good wishes and congratulations are always welcome ; and I assure you that the great blessing we have, and feel that we have, in this precious new life, is far and away in excess of any uneasiness I have had lately as regards my own health, though the warnings conveyed to me on various occasions have not been such as can be disregarded.

I was very glad, also, to know that you set store by the Hogmanay hymn—'Des Jahres letzte Stunde,' which we did *not* sing here, although it would have been well in keeping with certain facts—on that particular evening my pulse-rate went down to 36, and I rather apprehended having a renewed attack like the one on Xmas morning ; but instead of that, things seem to have been coming right again ever since ; and I am now pretty much in my 'frail ordinar' again. But whether well or relatively ill, I can truly say that the always dominant note of my spirit is one of profound thankfulness for so many benefits enjoyed over a long life, and I can only wish for you, or for any one, that when the end of your days may be approaching, it may be so with you.

The music given to the Hogmanay hymn is *not* my setting, but I believe the original harmonising which was formed up somewhere since last year. I had been trying my hand upon it, and I fancy mine was not at all bad, but I believe Eric or someone had taken it over to Dundee, and it could not be found.

Yours very truly,

W. T. GAIRDNER."

A serious attack occurred on 6th July, 1902, in St. Cuthbert's Church, which repeated, almost entirely, the features

of the first seizure. Similar attacks occurred several times in February, 1903, both in Edinburgh and London, to which city Gairdner had gone in connection with the General Medical Council. He was really very ill whilst staying with our mutual friend, Dr. Guthrie Rankin, and, after his return to Edinburgh, on 26th February, he had a good many seizures.

After settling in George Square, Gairdner was under the care of Dr. Macdonald Church, his near neighbour, but he was frequently seen by me in consultation with Dr. Church ; and Dr. Wyllie was also occasionally consulted. The affection from which he suffered was the Adams-Stokes syndrome, as it is commonly called, consisting in brady-cardia, due to complete heart block, attended by attacks of unconsciousness, and sometimes slight convulsions. Gairdner took a lively interest in the development of his own symptoms, and discussed them and their possible causation with many distinguished physicians, as well as some eminent physiologists. He manifested the alertness of his mind by thoroughly grasping the various considera-tions placed before him, arising out of the application of our modern knowledge to the clinical features of his own case. With the assistance of Dr. W. T. Ritchie, numerous tracings were obtained at different periods from the heart, the arteries, and the veins ; while the arterial pressure was very carefully studied. He followed with the greatest attention the results of these observations by graphic methods. The tracings which were obtained were the subject of his critical analysis, and the discrepancy between the movements of the auricles and the ventricles—then greatly occupying the attention of scientific medicine—excited his clinical acumen in the highest degree. The immense difference existing between the systolic and diastolic pressure in his case—a matter which was then, for

the first time, studied—led him to reflect upon the subject of arterial pressure, and he never wearied of discussing with me the various factors concerned in its maintenance. It was significant that he determined to leave a record of his own impressions and observations. He began to write an account of the course of his illness in 1902, and finished it, partly by dictation, before the end of the next year. As this description is not only most characteristic of its author, but is full of interesting information, it is introduced in this place :—

" Having found reason to believe that certain attacks of illness to which I became subject in the year 1900 are, if not undescribed, at least not fully described hitherto, in their relation to other forms of organic or functional disease, I have thought it expedient to leave this record of them, and of my own case generally, for the instruction of whom it may concern, it being understood that my desire is that my medical sons, and all who have been so kind as to give me advice or assistance, shall have full discretion as to any proceeding that may throw further light on my symptoms, and also as to the form of any publication of the record that may appear to be most suitable.

At the time of the first of these attacks (August, 1900) I was in my 76th year, having been born on 8th November, 1824. As regards my previous health generally, I think I am justified in describing it as fairly good on the whole, especially in the latter half of life, extending to 30 to 40 years out of the 76. In early manhood I was far from strong, and, without having any apparently very dangerous illness, except typhus fever in my 21st year, I had several infirmities which bore the impress to my own mind of a rather delicate constitution—one of these being habitual constipation, culminating in very numerous and some excessively severe attacks of intestinal colic, which very uncomfortable, if not dangerous, proclivity pursued me at uncertain intervals during the whole course of my medical studies, and for many

years afterwards, becoming, however, less frequent and milder in degree after I had passed my 40th year. As a younger brother had died of phthisis I was not without an impression that this, also, might be one of my predispositions, and indeed my appearance to others, as well as to myself, must have been, on the whole, the reverse of robust. It is, perhaps, not without significance in relation to the present narrative that throughout youth and early manhood the rate of the pulse was almost uniformly high, often over 100, even without any cause of abnormal excitement. I can make no precise statement as to temperature, the clinical thermometer not being then in use ; but my impression is that febricula, perhaps not at all considerable, was not infrequently present, and with it a sense of being easily chilled, and also easily flushed and overheated, from early childhood onwards.[1] Still, nothing occurred that can be distinctly cited now as amounting to evidence of tubercular disease. I never had any kind of articular disease (except, perhaps, occasionally 'growing pains' in adolescence and these not severe). There was nothing at any time to raise a question as to disease of the heart, and nothing, except the delirium of typhus fever, to suggest cerebral disorder of any serious kind, during the whole course of my life.[2] Still, I am inclined now, looking back over the whole past, to say that up to the latter part of my third decade the sense of physical infirmity, and of a possibly unsound constitution portending a more or less short career, remained with me ;[3]

[1] I suffered a great deal, even acutely, as a child from numbness of the fingers in cold weather ; and as this was not allowed for by my nurses, it was the source of much real misery to me as a peculiarity that I was disposed to be somewhat ashamed of, and yet could by no means get over, besides the sheer physical suffering it involved. At school, I never could throw snowballs on this account, and up to a much later date was always what in Scotland is called 'cauld-rife.'

[2] A possible exception to this statement will be noted later on, in connection with the history of the attacks.

[3] This rather persistent impression of delicate health did not hinder me from working hard at my profession, but it certainly did prevent me from applying for life insurance till about my 40th year ; and probably, also, with other considerations, had its effect in delaying marriage.

and it was not till after removing to Glasgow in 1862 that I began to think it might turn out otherwise. In 1870 I married, and have no hesitation in saying that the years passed since this event have been much more full, not only of enjoyment of life, but of positive physical health, than those of my youth and early manhood. The colics in particular have ceased to trouble me, though the constipation has remained.

Two remarks may be made at this stage as completing, or qualifying somewhat, the preceding account of my general health from youth onwards. (1) It would not be difficult, I think, to make out, by implication, a gouty habit in my later life ; but nothing has ever occurred even approaching to an acute attack of podagra or chiragra. (2) A rheumatic tendency might possibly be inferred as derived from my mother and my ancestry on the maternal side ; but endocarditis (and organic disease of the heart of any kind) I do not know to have existed for three generations back, either on the father's or the mother's side, any more than regular gout. My brother John, indeed, when in his seventh decade, had *for the first time* an attack of regular gout, which indirectly proved fatal, for he had been for many years a glycosuric, and the gouty attack led up to an unusual ending in gangrene of the sole of the foot, under which, after several months, he succumbed, latterly with a form, I think, of diabetic coma. He, like my mother, passed for being ' rheumatic ' at various periods in his life, but never had, so far as I know, an attack of acute rheumatism or any kind of cardiac complication. This last remark applies, indeed, to the whole family history on both sides, as far as I can trace it.

The first departure from the strictly normal condition that had subsisted for years, in respect of the incidents about to be recorded, was on the 20th August, 1900, at which time the family were in temporary occupation of a house at Levenhall, while the present house in George Square was being got ready for their reception. Lady Gairdner was superintending arrangements, and in consequence of this there was frequent railway transit between Levenhall and

Edinburgh. On the morning in question I had left after breakfast with my wife and our youngest daughter, without any misgiving as to the integrity of my health, and nothing occurred until the collection of tickets in approaching the Waverley Station, when it was noticed that I did not respond for some little time to the collector's demand for the tickets, and no doubt was for some moments unconscious. Before the station was reached, however, I was again partially conscious, but on leaving the carriage, relapsed, and had to be conveyed into one of the rooms attached to the station, where, however, on again regaining consciousness, I gave directions to be taken along on a truck and put into a cab, and driven to Charlotte Square. I cannot positively affirm the state either of my consciousness or of my heart's action during this drive, when I was accompanied by the two ladies, but the result of it was that we were received by Dr. Wyllie, who placed me on a sofa in his drawing-room, and gave me certain stimulants, and other necessary remedies. During the 3 or 4 hours which succeeded, there must have been many attacks of unconsciousness, the nature of which was not quite obvious to myself or others at the time, but which were no doubt attended by rather extreme modifications of the pulse, and by apparently impending collapse. Dr. Gibson, as well as Dr. Wyllie, saw me during this period, which at last appeared to come to a crisis in an attack of vomiting, after which I was removed upstairs to a bedroom, and remained for 24 hours under observation before it was considered expedient to allow me to depart. It may be mentioned incidentally that this was the only occasion during the whole course of the illness on which an attack of insensibility ended in vomiting. Of course, the minutest possible inquiries were made as to convulsion, paralysis, etc., and also as to any apparent exciting cause for these attacks, but I think I am correct in affirming that their precise nature remained unexplained; my own impression at the time leading to the view of something epileptiform, though without the usual concomitants, other than unconsciousness, of a fit of major epilepsy. I am not

sure that the total number of unconscious attacks can be stated with accuracy, but they were numerous, and in all of them the heart was gravely affected, but the nature and extent of the affection was probably not very accurately noted at the time.

On recovering from this series of attacks, I resumed the ordinary course of my life, and had nothing definite to complain of for at least 8 weeks. On the 18th of October, however—St. Luke's Day—I was due to be in London to receive a clinical medal—the Moxon Medal that had been awarded to me by the Royal College of Physicians—and although the question was raised as to the propriety of my going to London, I felt so well as to leave on the 17th without misgiving, going to my club in St. James Street, where, fortunately for me, my friend, Dr. Gordon Sanders, was for the time also domiciled. On the invitation of Sir Dyce Duckworth, and after a simple and early dinner, we went down, all together, to St. Paul's to a service, in great part musical, on behalf of St. Luke's Guild, which was very largely attended, and even up to the commencement of the service I had still no misgivings, although feeling considerably fatigued by the very dilatory process of marshalling the robed guests who were in the nave in procession. As there was no insensibility on this occasion, and as I am not even now quite confident as to how far it resembled the preceding or succeeding attacks, I will only say that during the sermon I was only restrained from leaving the church by the prominence of my position, sitting immediately in front of the pulpit, and that as soon as the service was over Dr. Sanders and I made for my club in a hansom, only too glad to be relieved from a sense of impending disaster, which might have led to a scene. The succeeding night was one of great discomfort, attended by both sickness and diarrhoea, of which the only possible explanation that could be suggested as regards the dinner was that I had eaten of mushrooms. In this attack, however, there was no insensibility, nor am I aware that the rate or character of the heart's action was at all profoundly affected. At all events, on the 18th I

felt so much recovered that I not only attended at the College and received my medal, but attended the annual dinner of the College thereafter, and made a speech as a guest without any sensation of discomfort, returning home the next day apparently in my usual health.

Between the events in the autumn of 1900 and those now about to be related there intervened a period of what I should call fairly good health, in which I remember no incident at all worthy of notice in this record, unless it be that the knowledge of what had occurred may have made me more careful, both in taking ordinary exercise and in committing myself to engagements away from home. I attended the meeting of the Medical Council in the summer of 1901, staying in London at my club without any serious interruption ; it was then proposed to me to make a journey to Norway with my son Eric, but this proposal was made as much on his account as on mine, he being supposed to be in need of a holiday in the interval of his medical studies. I had been in Norway more than 20 years before, but I very gladly accepted the proposal, and we sailed in the beginning of August by the s.s. *Midnight Sun* from Newcastle, the trip occupying a fortnight. During the whole time I was perfectly well, and enjoyed everything very much, but was conscious of an indisposition for much physical exertion, and I did not, therefore, attempt any of the longer excursions involving either walking or climbing, but confined myself to such as could be done by driving, with a little walking uphill. It never occurred to me, however, that I was breathless in any serious degree, even in walking up the zig-zag road from Gudvangan, which I did to the extent of several hundred feet, though with a certain sense of fatigue, but not at all of serious oppression. I feel quite confident in stating that, apart from a sense of a diminution of vigour, and, perhaps, a degree of natural apprehension of the consequences of over-exertion, I never felt a single hour's discomfort during the whole of the Norway trip, and did not lose a single night's sleep.

About this time I had some slight inconvenience amount-

ing to pain, and even a certain amount of halting, with tenderness in the calf of the right limb, which in connection with the previous incidents led to Dr. Church and Dr. Gibson being consulted as to the possibility of some kind of thrombosis. Nothing of the kind was positively detected at any time, but the inquest into this led to the discovery of some very slight tendency to oedema, so slight, however, that I myself was not quite convinced of its actual existence, although both my sons and Dr. Church held it to be occasionally present, and I, therefore, agreed to pursuing a more or less diuretic course of treatment, with observations on the quantity of the urine, which, however, did not lead up to any very definite result. Digitalis was commenced about this time, and although the quantity of the urine was quite satisfactory, and sometimes, indeed, though not commonly, rather in excess, the digitalis was continued in various combinations, and, especially after consultation with Dr. Balfour, it was, as a rule, part of the treatment, even although not producing any marked physiological effect.

About a fortnight after this, in November, 1901, an attack occurred in the very early morning, being the first time that the seizures had taken place while in bed and asleep. It was noticed by my wife in consequence of some disturbance of the breathing, and my son Eric was immediately sent for; he found the pulse below 40 in the minute, but pretty full and regular. It is supposed that the unconsciousness on this occasion may have lasted an hour, and it must have led to some apprehension, for two hypodermic injections of ether were administered without any sign of sensibility. There was a certain amount of cyanosis and coldness of the extremities, which were met by a local application of hot water, and hot water administered by sips internally, which were swallowed unconsciously. On the termination of the unconsciousness, the pulse returned to the normal rate.

On Christmas morning, 1901, about 7 a.m., an attack occurred somewhat like the last, but more brief, the unconsciousness lasting only 10 minutes; during this attack the

pulse was found to be 36 per minute, regular, and not at any time difficult to catch ; similar remedies were applied. The only reason for mentioning this attack separately was that it was followed by a period of some weeks characterised sometimes by infrequency, and sometimes by irregularity, of the pulse, though without any further unconsciousness. During the Christmas week almost every possible kind and degree of irregularity was observed, sometimes alternating full and small pulsation, sometimes 3 or 4 comparatively rapid and small beats followed by a fuller one. As these irregularities were not accurately and individually noted at the time, and as no tracings were then got, it can only be said in general terms that diminished rate did not infer irregularity, but rather the opposite. On New Year's Eve, Dr. Church called and found the rate a little over 30 in the minute, but the rhythm so regular that he was asked to compare it with the cardiac rhythm in order to make sure that the low rate felt at the wrist did not signify double the rate at the heart, with alternating smaller beats which did not reach the wrist ; Dr. Church, however, satisfied himself that there was a true bradycardia, the rate at the heart exactly corresponding with that at the wrist. The subjective sensations connected with these variations were not very easy to define. Knowing, as I did, all the facts, it was not possible that I should be altogether unmoved by them, and I think it fair to say that this week was to me one of a certain amount of physical discomfort. On the other hand, there were none at all, or next to none at all, of what could be called grave cardiac symptoms, no trace of anginoid symptoms, or of pain in the cardiac region, or of dyspnoea properly so called ; the nights were, as a rule, undisturbed, and, if there was any relative insomnia, it was only from a certain amount of apprehension, founded upon the pulse, that one of the attacks already mentioned might possibly be impending. In carefully studying, as I did, the degree to which I had any consciousness of these irregularities of cardiac rhythm, I found that in certain positions when in bed I could count the cardiac pulsations

without feeling for them either at the heart or wrist, but in other positions, and for much longer periods, I was practically unconscious of the cardiac pulsation, and, on the whole, I find it difficult to determine whether the subjective state above referred to can be called morbid or not; it certainly did not *per se* amount to a serious inconvenience, and did not cause me much anxiety, or interfere practically with a single night's good rest.

On the first Sunday of July, 1902, I went as usual to St. Cuthbert's Church, being at the time, according to my own reckoning, and subject to the previous statements, altogether without misgivings, and as nearly as may be in perfect health. I had, however, to a certain extent, the habit of sparing myself, and not infrequently avoided standing up during the singing for at least a part of the service. On this occasion all went well until near the end of the sermon, and then for a few seconds (no one else observing the fact) I became aware that I had passed into an unconsciousness which was not sleep, but was attended with something of the disagreeable sensation of former attacks. There was a baptism of a number of children to follow, and being very much interested in this part of the service, I stood up, though quite realising that this was perhaps imprudent under the circumstances, and noticing also that the atmosphere of the church was somewhat oppressive. Perhaps as a consequence of this, another attack occurred, which attracted attention, and I had to be carried out of the church into the vestry, where I soon became sensible again, and found that Dr. Carruthers was attending, and administering some whisky, as I have no doubt he had recognised symptoms of cardiac failure. I was soon, however, in a condition to be taken home in a cab, and was then placed in bed, and received all the attentions that were considered necessary. For several hours following this attack of unconsciousness, I think it must be inferred, from all that came to my knowledge, that my life was in considerable danger; the pulse, which had been very small since the attack, being numbered as low as 16, and never

above 30 per minute, while the face was bedewed with cold perspiration, and the subjective sensations were certainly those of dissolution, at various times, but still nothing like cardiac anguish, or considerably disordered respiration. In fact, I can say with complete conviction that, although my state, medically considered, must have been dangerous, and though I was fully conscious of the danger, I had at no time any degree of physical distress or pain corresponding with this, and was perfectly free from anxiety, moral or physical, except such as is inseparable from the idea, several times expressed in words, of possibly, or probably, parting from this life, and all its interests and cares, within a space measured by hours or minutes, as the case might be. When two of my children came to tell me they were going to church in the evening, I took leave of them with the inward feeling that I might not see them on their return ;[1] but notwithstanding this, I had no sense of depression, or of overpowering emotion, and certainly no such fear or sense of instant death as to correspond with what I have myself written about as *angina sine dolore*. The clammy cold sweat, however, continued, and the sensations, though certainly not agreeable, were not painful, and not in any definite way to be referred to the heart, except by reasoning from the state of the pulse, which continued up to about 7 p.m. (or over 6 hours from the attack in church) to number not much over 30 per minute. At this time another attack of unconsciousness occurred, and my son, who was with me during the whole time, reports that this was, in his view, the most alarming of all, in respect that there was considerable cyanosis, and that the respiration appeared to be suspended, the eyes at the same time staring, and not responding to the reflex of the eyeball. There never was, as my son tells me, anything of the definite character of the Cheyne-Stokes respiration. The attack just described was,

[1] This remark by Gairdner vividly recalls the account of the symptoms of Mr. Hyde, father of the first Lord Clarendon (*vide* "Life of Edward, Earl of Clarendon," written by himself, Oxford, 1759, p. 9). The passage is quoted by me, "The Nervous Affections of the Heart," Edinburgh and London, 1904, p. 3.

however, very brief, lasting not more than a minute; the pulse continued without change at a little over 30 till after consciousness was restored, and then came a kind of crisis, the whole of the unfavourable symptoms passing rapidly away, the pulse returning to 70 a minute, and perfectly regular, with complete warmth and comfort. I have dwelt on these details because this attack, lasting in all about 7 hours, was by far the most serious I ever experienced, with the possible exception of the first one (or rather series of attacks) in the autumn of 1900; and although undoubtedly cardiac as regards the bradycardia, it left on my mind (as the previous ones did) the impression that the cerebral element predominated.

After considerable additional experience of the attacks previously adverted to during the last few weeks, it is now, I think, possible to classify the whole series occurring since the autumn of 1900 as follows:

1. In most, if not all, of the earlier attacks, namely, those that took place quite unexpectedly, and in the midst of what seemed to be good health, it now appears to me that there must have been quite suddenly a brief, sometimes momentary, loss of consciousness, more or less complete, associated with a practically simultaneous fall in the rate of the pulse of from 20 to 40 beats in the minute—the latter being always considerably more persistent than the unconsciousness, and extending over from half an hour to several hours, but ending in a return to the normal rate. It is difficult to be quite sure of the details as to the bradycardia, because of its having supervened so suddenly as not to have been quite closely watched at its beginning, but in typical instances when either I myself, or some near relative, was able to make the observation, the pulse immediately after the period of unconsciousness was found to be small, and of a rate varying from 20 to 40, but, as far as observed, always quite regular at this rate, and reverting towards the normal sometimes with the occasional omission of a beat, or slight irregularity during the rise to the normal rate.

2. The above description applies probably to all the attacks occurring unexpectedly in the midst of normal health. At a later period towards the close of 1901, when considerable irregularity of the pulse was noticed extending over several weeks, and of the most varied possible character, bradycardia still accompanied some of the attacks which were not, however, so characteristic or so long continued in their effects as in the first series; of this period, however, no very detailed facts can be given, and, on the whole, the frequency and character of the attacks of unconsciousness bore no proportion to the irregularity of the pulse, but rather, if anything, seemed to be less prominent when the latter was at its height.

3. In the later periods of the case, and especially within the last 3 weeks, bradycardia has been present, and is still present, with a continuity not known at any previous period, the rates varying from 20 to 40, and perhaps averaging rather below than above 30, but never quite so low as after the nearly fatal attack which commenced in St. Cuthbert's Church. During this last period the attacks of unconsciousness, though mostly very brief, and measured by seconds, have been much more frequent, and in a certain sense disturbing, than they ever were before; they have mostly, though by no means exclusively, occurred at night, and often for many nights consecutively (not infrequently during sleep), and attended by sensations almost indescribable, but possibly more allied to vertigo than anything else—very molesting at the time, and possibly tending to exhaustion, but not accompanied by any cardiac sensations whatever, and even as regards the pulse, of which I was always unduly conscious, not conveying in the least degree the feeling of syncope, or of angina pectoris, but rather that of an internal throb throughout the chief vascular areas which I often watched carefully, comparing it in my mind with the dull throb which I experienced in my berth in an Atlantic liner in 1891, and which conveyed no sense whatever of panic, or even of great inconvenience, beyond the fact of its being obviously abnormal. It is notable that while during the

last few days the frequent attacks of unconsciousness have entirely given way to a sense of comparative comfort, and perhaps an average of from 8 to 10 beats rise in the frequency of the pulse, which have never, however, exceeded 40, these vascular throbs have been much less apparent, and have indeed almost disappeared from the consciousness. I must again most emphatically state that, according to my later experience, the frequently recurring cerebral attacks appear to myself to have no cardiac symptomatic expression, and in most cases were not accompanied by any manifest depreciation of the rate of the pulse, but seemed, on the other hand, to be always accompanied and followed by cerebral perturbations, of which I find it quite impossible to give any description that would convey to others a clear idea of what they were to myself. I make this remark after having carefully watched an almost indefinite number of these attacks, in some of which the unconsciousness was so slight and incomplete that I was able to realise the subjective phenomena from beginning to end. A good many of these very minor attacks were distinctly attended by what I should incline to call an aura, mostly as if a cold breath of air were playing upon the face, sometimes, however, the sensation being rather of warmth than of cold, and never spreading over the body generally. I have put the question repeatedly as to whether any obvious motor spasm had accompanied these attacks. Anyone who has been with me for some time says there was some rigidity of arm and wrist, and also a degree of risus sardonicus ; these possibly belonged to the beginning of the attack, and to the period of complete unconsciousness. There was at no time any cry, but the occurrence of the seizure was often noticed through perturbations of the breathing. About 10 days ago, when my own sensations were certainly those of closely approaching death, there was still a complete absence of anything like cardiac oppression or inconvenience of any kind, the sensation being simply that of vitality ebbing away without suffering of any kind, and without anything approaching what I take to be the sensations of syncope (though I have

rarely experienced them), and can only further state that the pulse, even at the worst, though with marked reduction of rate to 20 in the minute, or thereby, was never at any time markedly irregular or imperceptible to the finger. I should like also to mention that though I have had a considerable amount of flatulence and other digestive disturbances during the varied course of these complaints, it appears to me now to be quite certain, after careful watching, that in no instance was either gastric or intestinal flatulence, or any other digestive disturbance, a contributor in any marked degree to the occurrence of the attacks."

As evidence of the interest which he showed in his own case, this letter to Dr. Murrell may well have a place :—

"32, George Square, Edinburgh, 30th July, 1903.

Dear Dr. Murrell,

I have been writing to my good friend Sir William Whitla to ask if there is not among the crowd of new or old Cardiac remedies known to him one which could be trusted simply to advance the rate of the pulsation without in any other way exerting any toxic or otherwise deleterious influence when used continuously. I have been for some time a sufferer from Bradycardia (the present rate being 25, and the normal rate for me about 70). My actual sufferings are not great considering all things, and I have practically no severe dyspnoea, cardiac pain, palpitation, or insomnia, but the rate does not rise, and sheer bodily weakness is, I think, rather progressive ; so that one often wonders if there is anything that would move one out of the *rut*, and most of the cardiac tonics and stimulants known to me either tend to lower the rate or have some other more or less objectionable quality. This state has lasted, off and on, for three years, and it has at times been accompanied by momentary cerebral attacks, which two or three months ago recurred very frequently, both night and day, but have of late been entirely absent. The pulse, what there is of it, is wonderfully regular, and not at all small or feeble. Alcoholic stimulants have, of course, been administered, but

to my mind they have simply no influence at all on the rate, and therefore I keep them for emergencies, as far as I am allowed ; taking, however, a small quantity with my dinner, and sometimes a little more, perhaps up to half an ounce of brandy at a time, when I am subject to any extra exertion. Practically, I feel as if, beyond warming up the stomach a little, they do me no further good, and I am always inclined to minimise the amount prescribed. The weakness, however, is such that, notwithstanding the freedom from acute suffering, one cannot look forward to any further retardation without some apprehension.

I have, in fact, at various times, though not quite recently, experienced really dangerous crises, in some of which the pulse has been counted as low as 15, but still remaining wonderfully regular on the whole, even at the lowest rates, concurring as these have done with the cerebral attacks ; so that I was at one time—perhaps am still—rather inclined to view the whole phenomena from the side of some influence transmitted along the *vagus* ; but still, entirely without paralysis, or any permanent sequela in the way of detriment to the nervous system ; and I think I can view my own case quite as a professional outsider, both as regards diagnosis and treatment. Various of my brethren in London have seen me incidentally, *e.g.* Broadbent, Hughlings Jackson, Pye-Smith, Stephen Mackenzie, Morison, and above all Guthrie Rankin, who is a close friend and old pupil, who had me in his house in February last. My advisers here are probably unknown to you except Wyllie, Gibson, and George Balfour, the latter of whom, however, has been for some months so unwell himself that we have not met. If you can give me a *lift*, I shall be sure to place it before one or other of these, as a suggestion from you, and meanwhile believe me,

Yours very faithfully,

W. T. GAIRDNER.

P.S.—I ought, perhaps, to apologise for so long a letter, but I think my case is rather an interesting clinical study and certainly a rare one."

His former colleagues in the University of Glasgow desired, in 1903, to confer upon him the honorary degree of LL.D. Unfortunately he had just passed through one of the serious phases of his illness, already fully referred to, and it was impossible for him to contemplate going to Glasgow for the purpose of taking the degree. In giving expression to his grateful appreciation of the kindly feelings of his old associates, he, in the following letter, manifests deep regret at not being able again to appear amongst his old friends :—

"32, George Square, Edinburgh, April 15th, 1903.

My dear Principal,

Although I am in many respects improving and am wonderfully free from pain or bodily distress of any kind, I am still for the most part confined to bed, and have had sufficient warning within the last two days that it will be impossible for me to think of making a journey to Glasgow for a good while to come. Much as I regret, and shall ever regret, not being able to renew personal acquaintance with old friends at the Graduation, it thus becomes my duty to give you official intimation, that my earnest wishes in this respect cannot be fulfilled, and I can only hope that the numerous friends who have been looking forward to seeing a new academic honour conferred upon me, may be assured that I none the less appreciate their kindness because of my not being able to meet them in person.

With all kind regards and good wishes, both for you, and the University,

I am,

Yours most faithfully,

W. T. GAIRDNER.

The Very Revd. Principal Story, D.D."

The last real seizure of the Adams-Stokes type which occurred took place on 6th March, 1903. After this date,

a condition of absolute blockage of the heart continued, with great weakness but little suffering ; he was very largely confined to the sofa, and spent most of his time in reading. He was, however, throughout, deeply interested in everything, and took great pleasure in the visits of his friends. As usual, he was an omnivorous reader ; but one fact always attracted my attention—on the Sunday, he usually spent a good many hours over Alford's Greek Testament. As it was my habit, as frequently as possible, to call on the Sunday, when he was less likely to have other visitors, this was constantly noticed. He read the newest books which appeared, as well as many of the old friends, and even fell back, as many another has done, upon Gibbon's " Decline and Fall." A letter which has been sent to me by Sir Clifford Allbutt still shows much of the old trenchant, yet kindly, criticism. It bears no date, but it arrived either on the last day of April, or the 1st of May, 1904 :—

" 32, George Square, Edinburgh.

My dear Allbutt,

I have been greatly interested in the disclosure on Friday last—perhaps too candid, and certainly not very chivalrous—of Herbert Spencer's relations at one time of his life to George Eliot. Assuming the facts to be as given in the Autobiography—we have, of course, no information of her side of the question—it is possible in her case, as in his, that a want of physical attractiveness outweighed sentiment and intellectual or moral sympathy : the thing for us to consider is what might have happened had these two rare spirits been connected by the physical band of marriage, and for the nonce I will presume a happy marriage founded on affection. I will leave you to judge if it is possible as regards George Eliot, because you knew her much better than I—but as regards H.S. it seems to me that the ' Synthetic philosophy ' would not have become anything like what it is, if the fact of *love* as a motive power,

and as an inspirer beyond reason and logic had been ever so closely, yet personally realised by H.S.; for, if I am not mistaken, the synthetic philosophy is simply an enormous mass of facts and generalisations,—of great importance, it is true, but wanting in that element of emotion, the deliberate exclusion of which leads life to the enormous abyss of the 'Unknowable.' Had the Author's own personal experiences been transformed in the way above suggested, I cannot help thinking that a good many things would have appeared to him differently in the immense compass of the synthetic philosophy,—but, to the end H.S. remained an example, perhaps the most conspicuous example, of a perfectly truthful, absolute scientist, who, with no really unamiable traits in his character, remained so far as we know, unloving and unloved, because devoid of the emotional nature on which love is founded.

I fear I have not expressed myself very clearly; but then you must remember that I have had innumerable though brief cerebral attacks, which perhaps make it more wonderful that I should write on this subject at all—than that I should write on it unintelligibly.

I am preparing to read Lord Acton's letters to Mary Gladstone, and perhaps I may have something to say to you about them if this letter is not too much.

Very faithfully yours,

W. T. G."

It was during 1904 that his last medical paper was published, "On the Methods of Percussion," which has a place in this volume.

To Miss Nora and Miss Lucy Mitchell he addressed this letter, in which he dwells, evidently with great affection, upon his first visit to Italy :—

" 32, George Square, Edinburgh, Feby. 22nd, 1905.

Dear Nora and Lucy,

It has not been easy for some time past to get hold of an address by which to write to you, and to thank you

for all your beautiful picture cards, as you are so much on the move, but as I suppose the address in Rome will serve for some little time, and therefore I am anxious to give you a few of the fruits of my experience, as I cannot hope to be with you there otherwise than by letter.

My first visit to Rome was in 1845-46—the last year of the Pontificate of Gregory 16th, and I need not say that very much is changed since that time—not always for the better. I hope, however, you will still find your way to some of the old land marks, and among them, none are sweeter in memory than the Basilica of Santa Maria Maggiore, which I never could pass without entering, and always as it were with bowed head and spirit attuned to worship. Of all the great Basilicas it is, I think, the one most harmoniously beautiful, and if you go there once you will often desire to go again.

A quite new interest attaches to the Mosaics on the roof, but you will hardly be able to work them out for yourselves —unless you can get hold of the large and expensive and illustrated work which has lately been published on the subject. I will only say, therefore, that the mosaics on the roof have hitherto been attributed to the 5th or 6th Century, but that there now seems reason to suppose that, so far as not restored by more recent hands, they reach back to the 2nd Century, and therefore coincide with a period of very early Xian imagery of which we have no other record except perhaps in the Catacombs—where the austerity of early Xian practice had cut off or obscured all picturesque, not to say ornate, imagery. I am not able to form an opinion of this matter from recollection, and you will not be able to do very much, as the details of the mosaics are not easy to make out at the height at which they are placed—but perhaps this little summary will give you a new interest in them. *Then*—if you would follow in my footsteps—you will not fail to find out the church of Santa Cecilia in the Trastevere—not only is it very interesting as containing authentic remains of the house of a noble Roman Matron—but the exquisite sculptured figure of the martyred Saint

by Maderna struck me more and more every time I saw it, as being by far the most pathetic and simple of all the Xian sculptures I saw, so that I could hardly look upon it without tears coming into the eyes. Then you will not forget the extremely interesting old church of S. Clemente which lies right in your way going from the Forum to the Coliseum. It is in point of fact 3 churches of different ages built one over the other, the oldest of them reaching well back towards Clemens Romanus, the first really authentic and almost Apostolic Bishop of Rome in the early part, I think, of the 2nd Century. One day or other you will read some of his epistles; they are about the earliest indications of the Xian mind outside the New Testament. I should like you also to understand that the Coliseum as I knew it in 1845 was a perfect flower garden of wild botany, being over grown with plants springing out of every hole and cranny between the stones,—which have now been cleared away, possibly rightly, but certainly not picturesquely, for the preservation of the main building.

If you should visit, which I think you will be likely to do, the graves of Shelley and Keats near the Pyramid of Cestius, do not forget if possible to look into the splendid description by Shelley in the 'Adonais'—beginning if I remember right 'Go thou to Rome.' It is a perfect memento of the scene and its associations. In the Protestant Cemetery immediately adjoining you will probably be able, by the help of the Custode, to discover the tomb of my dear sister-in-law, Nanny Gairdner (Mrs. John Gairdner) née Edwards, and I should like to know from you if it is still pretty well kept, as it was when I last enquired after it.

It occurred to me that if you have a few days to spare you might as well devote them to a little round of the hills, beginning with Tivoli and the Anio, and going on by Subiaco and the Benedictine country, and round so as to emerge again at Palestrina (the ancient Pianecte), from which you may return home by Frascati, or if time permits go on to Albano, and the exquisite lakelet which is connected with a very old legend to be found in Fraser's

'Golden Bough,' whenever you have time to hunt it up. The above is one of the most delightful tours to be found about Rome—but of course there is plenty in the City itself to occupy you, not to speak of the Appian Way, which you are not likely to overlook. I may mention that the two most delightful general views that I remember are the one just within the walls and in front of the Lateran, near the Santa Scala, and looking out over the Aqueduct. This one is specially delightful in the afternoon or early evening. The other is the general view of the City from the Fontana Paolina in the Trastevere, which has the great advantage of giving at one view Ancient and Modern Rome in a way that no other does, and it is quite worth the climb to get up to it. These are the experiences of one who has been through the whole thing on foot, and who knows quite well what is most likely to be overlooked in a Guide Book, but Rome even in these modern days cannot help being supremely interesting to all cultivated people. Of course one need say nothing about the Vatican Museum, St. Peter, etc. etc. etc. I wish my dear brother John was there to be your cicerone, but he died in 89—his Wife in 83.

Your affectionate friend,

W. T. G."

In the spring of 1907, Sir William and Lady Gairdner resolved to leave Edinburgh and live somewhere in the country, near the city; about midsummer, the family entered upon their house, Bracondale, Colinton. Shortly afterwards, the end came with great suddenness, but was very characteristic in its peaceful serenity. On 28th June, the annual dinner of the Royal Infirmary Residents' Club was held in Edinburgh, and amongst those present was Dr. Yellowlees of Glasgow. Early in the afternoon, he had come from Glasgow in advance of the hour of dinner in order to call upon Gairdner, and spent some time with him at Colinton. They had a long conversation, and

Gairdner sent a friendly greeting by him to the Club; he afterwards accompanied him to the door, and then lay down upon his couch to read. One of the household looked in upon him, and at his request brought him another book. When she looked in within half an hour afterwards, he was found to have passed away.

In accordance with his own express desire, and with the free sanction of Lady Gairdner and the family, a post-mortem examination was conducted the day following his death, and the whole clinical and pathological description of the case, including the autobiographical sketch of the illness, was published by Dr. W. T. Ritchie and myself.[1] It is not surprising that the subject attracted very great attention. The distinguished position which the patient occupied in academic circles; the eminent place which he filled in the scientific world; the affectionate sympathy with which his illness was followed by numerous friends scattered over the entire globe; the keen interest with which he analysed his own symptoms—all these considerations rendered it an instance of unusual interest.

The funeral took place on Tuesday, 2nd July, 1907. The service was conducted in the Bute Hall of the University, Glasgow, and thereafter the Lord Provost, Magistrates and Town Council, the University Court and Senate and the Faculty of Physicians and Surgeons went in procession to the Western Necropolis. Representatives from the University of Edinburgh, and from the Royal College of Physicians of Edinburgh were also present. Here the mortal remains of the deceased Professor were laid beside those of his children.

[1] *Edinburgh Medical Journal*, 1909, New Series, vol. ii. p. 315 and p. 507.

MARRIAGE AND FAMILY

We have already seen that in 1870 Gairdner married Miss Helen Bridget Wright of Norwich. His wedded life was in all respects ideally happy, and to his friends he never ceased to contrast the joys of real home life with the emptiness of what is ironically termed single blessedness.

Early in the following year, his first-born saw the light, and the cup of bliss of husband and wife was filled to the brim. Although written many years later, the following letter to his friend Dr. Yellowlees, on a similar occasion, reveals his state of mind so clearly that it claims a place in this connection :—

" Glasgow University, Feb. 21, 87.

My dear Yellowlees,

The delights and the anxious cares of *fatherhood* must, I think, come with peculiar force and sweetness upon one who has had such a brief and troubled experience as you had in a now remote past, and who has so patiently waited during long years for renewed experience and, let us hope and pray, for you more abiding blessings.

Mrs. Gairdner and I can never enough express our cordial sympathy and congratulations for Mrs. Yellowlees and yourself, but, as possibly the oldest friend you have in Glasgow, I must claim a special privilege in writing these few lines, which will convey at least the honest expressions of one who has ever found in his own children, whether in joy or in sorrow, the best gifts of a loving Father in Heaven to his rational creatures here below.

Believe me, with every good wish,

Yours most truly,

W. T. GAIRDNER."

Other children followed Frank in rapid succession, and it was not long before the family more than filled the nurseries

at 225 St. Vincent Street. Gairdner had let his house at the College to one of his colleagues, but a break in the tenure took place in 1877, when a move was made to the west. The following paragraphs give a complete list of Sir William and Lady Gairdner's children and grandchildren:—

JOHN FRANCIS ROBERT was born 16th February, 1871; he was educated at St. Ninians, Uppingham, and Glasgow University, where he graduated as M.B. and C.M. in 1894. After residence in the Western Infirmary he spent some time under Mr. Mayo Robson at Leeds. He volunteered as a Civil Surgeon for South Africa in 1899, and returned in 1901. He again went out to South Africa in the latter year, and is now in Pretoria. He married in January, 1909, Isabel O. Mitchell, youngest daughter of the late Mr. J. O. Mitchell, a well-known Glasgow citizen. In April of 1910 their first son, John Oswald, was born. Their second son, Francis Temple, was born, but died in infancy last year.

LOÏS ANNA MARY was born March, 1872, and died March, 1884.

WILLIAM HENRY TEMPLE was born in 1873, and educated at St. Ninians, Rossall, and Trinity College, Oxford, where he graduated in 1896. While at Oxford he rowed for his College. Being interested in missionary work, he went to Cairo in 1899 after ordination by the Bishop of London. He married in 1902 Margaret Dundas Mitchell, second youngest daughter of Mr. J. O. Mitchell, and on 9th November, 1903, one day after his 79th birthday, Gairdner was presented with his first grandson, who was called by his full name, William Tennant Gairdner. Subsequently three other children have been born—John Oswald Hugh, born May, 1905; Eleanor Mary, born August, 1908; and Douglas Montagu Temple, born in November, 1910. The last mentioned is named after Temple's late colleague in

Egypt, who died very tragically at the commencement of a very promising career. In 1908 Temple published a book entitled "D. M. Thornton: A Study in Missionary Ideals and Methods." This work was followed by "The Reproach of Islam" (1909), a text-book produced under the auspices of the Mission Study Council, and by the official Report for popular use of the World's Missionary Conference, held in Edinburgh, entitled 'Edinburgh 1910.' Among the many pursuits which he follows with great energy, his chief delight is in music.

HUGH MONTGOMERY, born February, 1875, and educated at Ardvreck, Loretto, and Glasgow University, where he had just begun to study Engineering, when he died in January, 1893.

HELEN CHRISTIAN ("DAISY"), educated at Highfield School, London; she married in 1899 Lewis Robertson Sutherland, now Professor of Pathology in the University of St. Andrews (University College, Dundee). By this marriage there have been four children, viz. Helen Bridget Gairdner (the eldest grandchild, called after her grandmother), born December, 1900; Margaret Amelia, born January, 1902; William Temple Gairdner, born March, 1906; and Nigel Robertson, born May, 1910.

ERIC DALRYMPLE was born 1878, and educated at St. Ninians, Rossall, and Glasgow University. While at College he volunteered during the South African War as a Student Dresser of the Scottish National Red Cross Hospital, and was stationed at Kroonstadt. He obtained his medical degrees at Glasgow University in 1902, and subsequently entered into practice in Ayr in 1905. He takes a keen interest in all local matters, and was a member of the School Board, of which he was Chairman for several years. Like Temple he is a keen musician.

AILSA BRIDGET ("AILIE") was educated at Highfield School, and in Brussels.

DOROTHEA MARION EMILY ("DOREY") was educated at Highfield School.

ANTHONY PHILIP was born 1885, and educated at St. Ninians and Tonbridge. He was apprenticed to Messrs. Chiene and Tait, C.A., Edinburgh, and became a member of the Society in 1909. Like Temple and Eric he is devoted to music.

When Frank was an infant, Gairdner and his wife passed through a terrible experience. They had just then returned from a holiday, and it occurred to him one evening that he and his son would be the better of a dose of a digestive powder, of which, as he thought, he had taken a small quantity in a bottle with him to the country. The bottle lay in his bag untouched during his holiday, and obtaining it, he administered a dose to himself and another to his son. Soon after going to bed, he observed that he was under the influence of a narcotic, and had only just time to rouse his household and summon several medical men living near him (his old friend, Dr. John Burns, being one of them), who promptly used the stomach pump upon father and child, and saved both from what would otherwise have been inevitable death. It was afterwards found that the bottle contained enough of morphine to last a household two or three generations.

At Lecture the following day, Gairdner, under considerable emotion, described the incident, winding up with the remark—"I nearly lost a beloved child, and you, gentlemen, your Professor of Medicine"—upon which he put his head down between his hands on the desk in front of him; when he looked up, he found the undergraduates in the act of leaving the room, with beautiful and generous

tact. The lecture for that day was, therefore, not given. The sympathetic attitude of the students to their Professor was genuine, and as touching in its way as the impression Gairdner wished to make upon his class of the absolute necessity of never giving medicine without carefully reading the label.

These facts were fully narrated in two communications to the *British Medical Journal*,[1] soon after Gairdner's death, by Sir Thomas Oliver of Newcastle-on-Tyne and Dr. Donald Stewart of Aylesbury.

That Gairdner's father had a keen enjoyment in his domestic happiness is abundantly manifested by a letter from him to his son James, in which he depicts the Professor romping with his children. The letter, it will be noticed, was written two years before the migration to Gilmorehill. The earlier part of the letter must be referred to elsewhere, in dealing with the attitude of the family to theology and religion.

"Edinburgh, 27th Feby., 1875.

My dear James,

When we all suffer by colds in this renewal of Winter, it is fortunate that, in the business of the house, your sisters have able assistants. To-day I found, on going rather late to the breakfast-table, that one of them had not yet arrived, and that the other, Marion, was seated with Frank on her left, and Loïs on her right, both very busy, especially Loïs, in giving her all the assistance imaginable. First, she ran off with a teaspoon, then, when that had been recovered, she was off with a cup. But, pray, what has become of the cover of this *Church Herald*? 'Oh!' says Marion, 'Loïs had torn it off before I saw it.' 'But where is it, for I want to know who it was that so kindly sent it?' 'Oh! she burned it!' Well, it must be admitted that this is exceedingly convenient for me and

[1] *Brit. Med. Jour.* vol. ii. 1907, pp. 58 (Oliver), 176 (Stewart).

for your sisters in our present colded condition—and so we get on beautifully.

Well, to make the best of it, I take up this *Church Herald*, and discover, what indeed I suspected, that my orthodox son was the person to whom I owed the donation. To the four great principles set forth, I have no objection to offer. I would only add a fifth, or it may be only a note of explanation of the meaning of a word. The word 'church,' might mean the Archbishop of Canterbury or the Convocation; my views of it, especially in its bearing on No. 2, is that it means the *people* of the Church. The faith of the Church is then the faith of the people. This is obvious, for faith is a personal thing, and the misfortune is that the people don't assert their rights to regulate the faith of the church, but leave it all to the officials, as if they thought that they themselves had nothing to do with it. As the constituency in the *State* is the people, the same as in the *Church*, it would seem to follow that the *State* is supreme in *Church* matters; in truth the perfection of Church and State would be their identification—which will not happen till the second coming of the great founder of the Church. In the meantime, State and Church both suffer. Both are infested by those who intermeddle with things about which they care nothing, or which they wish to destroy—the State by anarchists; the Church by those who care for it only as it serves their secular interests, or, worse still, who are strangers to, or even opposed to, the principles of all Religion. Fancy a Huxley or a Tyndall intermeddling with the Church's religious *belief*! He has not yet persuaded himself that there is any evidence for the existence of a God, the very first principle of all Religion.

And yet, I rejoice when such men honestly speak as they think. It makes us to think also, instead of allowing others to think for us, which many other people are always ready to do. Those who speak that which they do not think are a *mischievous* class having in them no redeeming quality, as the open, honest unbelievers undoubtedly have. If we could banish them, and anarchy along with them, both from

Church and State, it would be well: but I fear that we must allow both to 'grow together till the harvest,' and do the best we can to neutralize their present pernicious doings.

All goes on right with Nellie,[1] thank God—William was here about the middle of the week, but only for a quarter of an hour, during which he was delightfully occupied with a game at romps with his three children, and then off to a medical society banquet to which he had been invited. He slept at an inn, and went home early the following morning. He is going to be a great paterfamilias, and his present house will not be big enough ere long, for its numerous inhabitants. He tells me that Glasgow houses are getting to be very high-priced. This will probably increase the value of his present house, but also of his future one if he is forced again to buy. The misfortune is that Glasgow, while one of the dearest, is also one of the most unhealthy towns in this country, the eager pursuit of mammon having had the effect of crowding the buildings in with contempt of those laws of health which are sure to vindicate by severe penalties their claims to our obedience. But he cannot help living there and taking his chance; for though he has a good house at the College—he might as well, for business purposes, have one on the summit of Ben Ledi.

Yours affectionately,

J. G."

It is more than pleasant to put on record that John Gairdner's affection for, and pride in, his son were in full measure returned. In a still later communication to Dr. Yellowlees this fact is very prominently exhibited. Little wonder that such a loving son made a devoted husband and father.

" 225, St. Vincent Street, Glasgow, Nov. 8, 86.

My dear Yellowlees,

The death of your old Father moves me to write a few lines of sympathy, though I did not know him. I

[1] Mrs. W. T. Gairdner, now Lady Gairdner.

passed through the same trial about 10 years since (almost to a month) and I know well what the parting is from a life-long friend, the support of one's childhood, and the companion and ever venerated friend of one's mature years. I remember one thing which it pleases me very specially to recall because it has some little bearing upon your case. I am very thankful that the dear old man lived to see my children about his knees, for I don't think I ever gave so much pleasure to him by any single thing in my life as when I married—*he had given me up*. Yet such was his delicacy of feeling that during long years when he might have done so, he never even thought of pressing or urging me on the subject.

All the more did he rejoice when it came about. I cannot but think that *your* Father also must have had great joy in your second marriage (for which so many of us long waited in vain), though he did not quite live to see the consequences. My Father was 87 when he died, and it was the most absolutely pure case of the decline of nature that I ever knew. Not until he could not (physically) hold up a book or a newspaper did he cease reading in bed; Cicero 'De Senectute' and the Greek Testament were his constant companions, besides which he went over his Tacitus, Carlyle's 'Friedrich' and Burton's 'History of Scotland,' within a year or two before his death.

With much kind regard, believe me,

Yours truly,

W. T. GAIRDNER."

It was Gairdner's habit for many years to rent a house somewhere within easy reach of Glasgow, where he could enjoy the company of his wife and the children, and yet be able to spend a few days every week in Glasgow. In this way he spent every summer from the date of his wedding until 1884 at Ardrossan. Crieff was their country residence in 1885, and Barassie in 1886, but 1887 saw them once more at Ardrossan. Craigmore, Bridge of Allan, and Montrose

received them in the three years following, and in 1891 and 1892 the family were at St. Andrews. In the former of these two years Gairdner was in America and only saw the family settled in Fife before he started. The summer of 1893 was spent at Kilconquhar, and that of 1894 at Moffat. St. Andrews again was the scene of the annual holiday in 1895 and 1896; Comrie, Carradale, and Tobermory were chosen for 1897, 1898, and 1899. In the year 1900 the move to Edinburgh took the place of the usual family visit to the country. During the years which succeeded 1890, Gairdner spent more time with his family and his visits to Glasgow became rarer and shorter.

Throughout the whole of the period mentioned, he was in the habit of taking part in the Meetings of the British Medical Association and of the International Medical Congress, while the calls upon him for Addresses became more and more insistent until his retirement.

In making reference to the beautiful relations existing between Gairdner and his wife and children it is not my wish to raise the veil which hides the holy places of family life from the view of even the most kindly friends. He has himself said, in another letter to his friend Dr. Yellowlees,[1] "these sanctuaries of the soul cannot be brought much into the light of day," and we must respect the sacred relations of private life. But without invading the privacy of his family, it may be said that there never was a more tender or loving husband and father. No one who was privileged to enter the inner circle could fail to be impressed by the perfect confidence existing between parents and children, as well as between the children themselves. From their earliest years Gairdner tried to teach his children to think for themselves; far from any attempt at repression,

[1] See p. 117.

he ever sought to elicit from them the manifestation of their feelings and beliefs. Answering a letter from Temple, then aged sixteen, in which he referred to a sermon, Gairdner enters fully and frankly into the subject, and expresses his belief that character is all in all.

"Glasgow University, December 1st, 89.

My dear Temple,

The Bishop's thought of the two streams, one going to the Rhine and one to the Danube, is a fine and strong one, springing from his own experience no doubt, but you may like to know that it has been admirably treated by Oliver Wendell Holmes in a little poem under that very title, only with an American setting in the Rocky Mountains, whence one stream descends to the Pacific and one to the 'frozen sea'—'from the same cradle's side, from the same mother's knee.' It is a most affecting poem and you will do well to read it, and perhaps commit it to memory. The thought itself only grows upon one as one gets older, for in all human life one is so largely governed by what seem to be small things—for good or evil—that the best and safest conclusion is that nothing is really small that bears upon character; for character is built up, like the work of the coral polyps, silently and in the depths, and all out of small things from first to last. You may make a reputation at a stroke—you may wake up, as it were, and find yourself famous, one day, whether by battle, or accident, or invention, or by a strain of music, or by an equally ear-catching poem, but you can't make a character in that way. And therefore nothing is small, or at least everything is important, though small, that goes to make, and confirm, the habits through which a man becomes what he *is*, rather than does what he *does*. I am sure that the more you think of this, the more you will be impressed by it. It is just the same view of human nature that is contained in the Sermon on the Mount. The Jew, like all formalists, had grown to believe that acceptance with God was altogether, or almost all, an outward thing—a matter of rites, ceremonies, or at

most of the keeping of commandments, mostly of the nature of 'thou shalt *not*.' And no doubt the keeping of commandments is a part of what he called 'righteousness.' But Christ pointed out—not perhaps for the first time, but with entirely new force and authority—that this kind of righteousness was in reality a very small and poor affair, and that 'except your righteousness *exceed* the righteousness of the Scribes and Pharisees, ye shall in no case enter into the Kingdom of God.' It was not by simply obeying a law—'thou shalt *not*'—or by doing ever so many outward acts of ceremony that one could get right in God's sight, but by getting to be like God—perfect as your Father in Heaven is perfect. In short, the purity, the kindness, the loving trustfulness which make up character must come from within; they cannot be moulded by rules and regulations, or even by precepts; they must be a growth, and not a forcible twisting of the nature in accordance with a model or even an ideal. Nay, even Christ himself must grow within the heart, and not be merely taken as a formal example. 'Not every one that saith unto me, Lord, Lord; but he that doeth the will of my Father.' I agree with almost all you write of what the Bishop said, but perhaps, to my mind, he put it too much in the light of a battle. A battle or combat no doubt it is—but there is no reason why the Christian life should not come to be, to a considerable extent, a second nature and therefore easy and as it were spontaneous; though there will never be wanting occasions for striving against temptation. To some, indeed, nay to all of us at times, the struggle must needs be hard. I know and admire and could name to you if need be, dear friends with whom it is so, from day to day. But it is also within our power, I trust, to make our homes and families at least—something like a little Kingdom of Heaven below, and so to get hold even here of some of the blessedness which comes of being 'pure in spirit, pure in heart, merciful,' (as in Matt. v.). Try then, after this blessedness; not by any great or stupendous doings at any one moment, but in little things, done and thought, and said from day to day.

Persecution, opposition, offences of many kinds, may and will come ; but every man can to a certain extent so train himself as to get out of life a large amount of what is best, and in doing so to prepare himself, and also those around him and under his influence, for the blessedness promised and which *can* only come to those who are so prepared for it.

So ends my little sermon for the present.

Your affectionate Father,

W. T. GAIRDNER."

His lively interest in everything that pertained to the progress of his children is exhibited in the following letter. It will be observed that he espouses the same view (and for the same reasons) as that expressed by Sir Clifford Allbutt, the distinguished Regius Professor of Physic at Cambridge, as to compulsory Greek.

" 9 The College, Glasgow, May 31st, 92.

My dear Temple,

I must write you a few lines if only of congratulation—not only on your getting the essay prize, but on your noble resolution to read your Homer, which is far, far, in advance of what your father was ever able to do or to think of doing. I agree that an English school which has begot that grand ambition in an effective way shews that Classical discipline is not thrown away ; and although my sympathies went, on the whole, with the party in Cambridge *versus* compulsory Greek—with Butter and Warre against Jebb and others—it was not from undervaluing the little Greek I have, but because I believe that compulsory Greek crammed up for the B.A. is a premium on degradation of the Classical ideals. I do most sincerely wish I could read my Homer and my Sophocles—but that's past praying for in the sixties, and I almost fear I shall never live to have leisure to read even my Dante—which, so far as the language goes, I might hope to do well enough. As for the English prize, I hope you will bring your essay home, and I will engage to read it, during the holidays if I even don't finish Henry

Johnstone's novel, which he sent me, and I promised to read.

Give my very best love to Eric. I am glad you report him as so well planted at Rossall. You see I must needs cotton to my young folks, for all my old ones are dying off. I went to Berwick on Saturday to bury poor Dr. Philip Maclagan, an elder brother of the Archbishop, and one of my oldest friends. The same week brought the death of Dr. (or Mr.) Vander-Byl, not a close friend, but one of the medicoes of my early years. I went into a room at the College of Physicians in Edinburgh the other day with a dozen doctors in it, of whom I only knew two. Thirty years ago, I should have known every man of them. 'So runs the world away' as the great and world-wide Shakspere says. The only thing that lives and lasts, is inspired thought like his.

Yours affectionately,

W. T. GAIRDNER."

A couple of months later he again writes to express his pleasure in Temple's progress.

"Nottingham, July 28th, 92.

Dear Temple,

I thought it well to write a few lines to Mr. Tancock from this, to thank him for all his attentions to you, and to express the pleasure we all feel in your having been so successful, and so well thought of, as to win the Headmaster's prize. It was unfortunate for me that I could not catch the words he used, and in fact I was for a time doubtful if he was referring to you at all in what he said; but I have gone on the principle that I am sure what he did say was meant for true kindness and encouragement.

There is nothing, I think, that can so nearly touch a father's or mother's heart as any good words or deeds bestowed upon their children; and certainly nothing that will so brighten old age for us (or for me who am nearest to it) as the feeling that my boys and girls are all on the right lines of good faith and good conduct, as well as devoted to their parents and their home.

Home is not home, without that; and with that, any place is, or may become a home. So I must say to you from my whole heart, that you have it in your power—like all your brothers and sisters—to brighten the home of your father's old age, and I thank you, and thank God for you, in that you have done so much in that way already.

Yours affectionately,

W. T. GAIRDNER."

A letter to Sir Thomas Oliver, soon after the death of Hugh, gives expression to Gairdner's tender affection for his children.

"9 The College, Glasgow, Jan. 31, 93.

My dear Oliver,

I believe our dear Hugh was very probably 'the baby' when you visited us at Ardrossan, and even when in his cradle he had that distinctive note of refinement and graciousness of manner which made us call him from the first—and before he knew anything about it—'gentleman Hugh.' This impression about him only deepened with his years, and never was he more gentle and more winning than in a little tour he took with me last autumn in Devon and Somerset, he and I alone, when he seemed to be drinking in delight and knowledge at every step, and the fountains of his grateful heart were all laid open to me many a time. I have nothing but sweet and blessed memories of him, and we are thankful to have had one committed to us for a while who was so worthy to be transferred to the heavenly Kingdom. All his brothers and sisters have but one feeling about him, and he lives in our memory only as a dear bond of still closer union among ourselves.

Yours very truly,

W. T. G."

The sad anniversaries of the deaths of their beloved children, Loïs and Hugh, were never forgotten by their sorrowing parents. The letter to Frank, which follows,

gives the most touching expression to the deep tenderness of Gairdner's feelings.

"9 The College, Glasgow, March 29, 97.

Dear Frank,

It is fitting that I should give you a reminder—though very likely you have not forgotten it—that Wednesday next is a sad day in our family calendar—at least it would be sad, if we had not by this time outlived a good deal of the sadness, and retained only the sweet and enduring, and always lovely impression of our darling Loïs, who was in some respects so like, and in others so unlike dear Hugh—both of them, however, cherished and most sweet memories of children that never caused us any regrets except when they begun to suffer; and of whom Loïs, at least, remains as a lesson to us, to shew us the 'way of the Cross,' even now that she is at peace.

You will remember, I dare say, a conversation I had with you in walking home from the grave the Sunday afternoon after the funeral, when we were both well-nigh broken-hearted, and you upon the very verge of your serious illness; but when I expressed to you, what has never since left me, the feeling that the living influence of these departed ones is bound, under God's providence to close up the ranks of those that remain, and to bind them all together more and more in a close and loving family union. No doubt you will be with us in spirit on Wednesday, when we shall probably visit the grave; and no doubt also Temple will join in, if you send him on this letter, as I cannot afford the time to write it over again.

Yours affectionately,

W. T. GAIRDNER."

A letter to Temple, written a few days later, breathes the same spirit.

"9 The College, Glasgow, April 8, 97.

Dear Temple,

I am writing this in my bed—down with a cold again as in autumn, but I hope not so bad. In the mean-

time I have to use a fountain pen, and my eyes cannot follow its strokes very well.

That sad crisis which recalled you from school only to see the last of the wasted form of our sweet and bright darling Loïs—is only a source of pain to me now when I fix my thoughts on the pain of those seven weary weeks *to her*—to us also, of course, it was a most trying time ; to me especially at the beginning when I foresaw it all and had to keep my fears to myself, and to your Mother afterwards when the ruin of our hopes was complete. I almost think, from this point of view, that the sudden shock of a death like your Maxwell's is to be envied. I shall never forget the heart-breaking effect on me of the last faint glimpse of a jocular remark by our poor little dear when she was about half through her illness, and I knew well that it was just like a flash in the pan. But to see that darling's head down on the pillow, never to be raised again, on account of the pain it gave, and to think of the uncontrolled joy of her life up to the moment she came home from school with that fatal crick in the neck—is a very, very painful thought even now, and a great lesson. How she was guided into peace no one can tell, but there it was—seven weeks had transformed her, and when she actually slipped away from us, the overwhelming feeling was in the midst of all the sorrow of it—thank God ! for peace at last. I can't write about it now, without my eyes getting dim, but it is all a thank God ! from the bottom of my heart.

Affectionately yours,

W. T. GAIRDNER."

A year later again, on the return of the sorrowful date, Gairdner once more writes to Temple.

"9 The College, Glasgow, 31. 3. 98.

Dear Temple,

You will not forget that this is the sad anniversary —sad *to us*—of our darling Loïs being taken away from us, and as we trust, born into a new life. How sure I feel now, that it was all for good—for hers and ours alike ; and yet

what a terrible wrench it was, only alloyed at the very moment by the thought of rest and peace after those awful six or seven weeks of pain, and anxiety, and suspense. Daisy and I have just been over at the Cemetery with wreaths for the grave, and even at this distance of time, my heart cries out for the dear one, and is touched with a solemn pity at the thought of her sufferings. She would have been 26 if she had lived till now ; and, if one could even imagine her having gone through all that, and recovered, no doubt she would have been a noble woman. Looking back beyond that crisis, one thinks of her only as a joyous and happy child—so happy and so radiant with joy, that the only anxiety I had about her was—how could she ever bear sorrow ? How soon that question was to be solved ! Daisy, my dear eldest girl, has been proved in another way, and I now know well that she will not fail in the dark hour. Indeed they are all—and you are all—thank God, in training, I hope for noble and patient lives. Our Father has in his love and wisdom, seen fit to take away two, who were certainly as near my heart as any of you ; but in the case of dear Hugh, I had, all along, a misgiving as to how he could stand the wear and tear of life, and God has solved that problem in his own way. But with dear Loïs, I had absolutely no misgivings of that kind, till those terrible weeks when the poor child laid down her pained head on the pillow, never to raise it again.

All this is part of our discipline, and we must try to thank God for it, and believe in him always as working out the best. 'God moves in a mysterious way'—but even here we can attain glimpses of what it all means.

Yours always affectionately,

W. T. GAIRDNER."

The sad memories connected with the loss of Hugh draw from Gairdner another touching letter in answer to one from Temple. It is indeed so full of pathos that probably no one who is a parent can read it with dry eyes.

" 9 the College, Glasgow 22. 1. 99.

My dear Temple,

You may well say that the death of that much loved, and most gentle of brothers and of sons, was a turning-point in your life, as indeed it was bound to be, for you were too young, probably, to take to heart the death of our dear Loïs as Frank did, and Hugh was, more than anyone else, your chum and playmate. But even with the oldest and most experienced of us, these partings make and ought to make a dent that will never be effaced as long as memory lasts for each of us. It is one of the inward testimonies we have to the unseen, as being really more enduring, as well as more important, than the visible things ; ' for the things which are seen, are temporary ; but the things which are not seen, are eternal.' And my experience is also yours, I am glad to say ; that after the pain and the shock, and the deep tragedy of it, the outcome is always a peace, and even a joy, unspeakable ; that such happy, innocent and beautiful lives as our darlings, Loïs and Hugh, should have been given to us even for a few years, to gladden our hearts while here, and then to form a centre of loving communion in the family that is now absolutely indestructible, since it has already moulded your life, and in a thousand ways has been a silent witness of memory for all of us, young and old, that we are a united family, and that we have laid up our treasures in part, at least, ' where neither moth nor rust doth corrupt, and where thieves do not break through and steal.'

What I hear from you now about the dear boy's very last hours is (I fear) new to me, at least I don't remember to have got it in that vivid way before, and I thank you accordingly. It shews what way his thoughts were running, as long as he had his senses at all—I would not attach too much importance to the very words which might be a mere echo, almost without intelligence, of what had been sung to him. But we can have no reason whatever to doubt that Hugh, in that gracious and really humble and most loving heart of his, had a place for the ' lover of my soul ' of the

hymn such as his final accents of praise justly betokened. Even without any explicit declaration, I should have said with confidence that he died in Christ. Just remember what a sweet, gentle, confiding, true-hearted life it was; what constant willingness to oblige and to obey; what innate courtesy, what modesty, what devotion to his mother, and to you all, what genuine joy he had in spreading out his little gifts in the family circle, and how little he cared for getting the credit of them. For years, I was never able to hear the Intermezzo of the 'Cavalleria' without a tugging at my heart in thinking that the dear boy would never again play that over to us. It certainly never entered our dear Hugh's mind that he was a genius of any kind; and it is even hard to say whether he consciously had laid to heart the parable of the talents; but equally certain I think that he acted in the spirit of it, and all that he had, or felt he had, from God, he gave as freely up for us, and although my mind was filled with misgivings about his future—as I doubted his physical strength and stamina—I never doubted that Hugh would become increasingly dear to all of us, and that in one way or another we should come to be proud of him. And now, is he not still there for us, and are we not proud of him; and, obscure and unnoted as his life was, is he not moulding and influencing all of us, though withdrawn from our sight, by the virtue of the 'things that are not seen'?

I agree also, or at least do not dissent from, what you say about the 'true ambition' as expressed in the words of St. Paul. But I think it is possible, at least, to have that 'true ambition' without such a conscious submission of all one's personal instincts to the will of God as you point to as dominating your own case. Ambition is said somewhere (was it by Pope?) to be 'the last infirmity of noble minds.' It is often, I think, the first infirmity of very ignoble minds; but that does not make it, in its essence, wrong or even superfluous as an endowment of our poor humanity. True it is, that the noblest minds are the least prone to ignoble ambitions. One thinks (at least I do) with far greater

sense of the nobility of it—of poor Schubert composing that immense symphony which he gave away to the Berlin Musik-Verein, and never heard properly performed—than of Napoleon plunging all Europe into war and blood, to serve his own ambitions. But I am not sure that Schubert acted consciously on St. Paul's principle, or on any principle which he could have stated to himself. He simply did what God put it into his heart to do, and what he could not help doing.

Your most loving Father,

W. T. G."

This letter to Dr. Fraser of Paisley, reveals his delight in the birth of his eldest grandchild :—

" 32, George Square, Edinburgh, Dec. 22, 00.

My dear Fraser,

We had a few days ago, something like a Christmas card from Mrs. Coats, and I might very well have written to her by return, but was at the time more than occupied with other matters, of which anon. And now, I find I have not her address quite handy, and as I have the pen in hand, I shall pass on through you, instead of at first hand, the pleasing news (which she may have overlooked otherwise) that my dear eldest daughter, Mrs. Sutherland, who has been staying with us, had a bonnie and healthy little daughter born on the 17th inst. and that all is going on as well as possible both with mother and child. You will easily understand how agreeable it was for us to be raised to the rank of grandparents by having this first event in our own house, instead of at Dundee. It was all the more so, because although much better again now, I had not been well for some weeks, and it would have been very vexing for Lady Gairdner to be divided in her allegiance between two persons and places 80 miles apart.

But all is well now, thank God ! and we are all happy and peaceful once more.

With all good wishes for you and yours this Christmas,

I am,

Yours most truly,

W. T. GAIRDNER."

Once again Gairdner, shortly after leaving Glasgow for his native city, writes to Temple, then in Cairo, upon the subject of Hugh's death. He writes about the approaching baptism of his first grandchild—Helen Bridget Gairdner Sutherland—daughter of Professor and Mrs. Sutherland, and gives some news of several members of the family. The final reference is to Miss Margaret Dundas Mitchell, to whom, in the year succeeding the date of the letter, Temple was married.

" 32 George Square, Edinburgh, Jan. 16th, 01.

Dear Temple,

This is the anniversary of our dear Hugh being taken from us, and although we are saying but little, I am not unmindful of that dear one, lost to us in his beautiful and most attractive prime, just opening into manhood, as to which my only misgiving was, whether the somewhat frail tabernacle would stand the strain of a competitive age, and a rush for places in the battle of life. God answered that, in His own way, and we can bow to His decree without any murmuring, I hope, and certainly without anything morbid in the retrospect at this time ; but you must well remember what a wrench it was. I have never since been able to hear that fine intermezzo in the Cavalleria Rusticana, without a catch in the throat for the dear boy, and his now silent violin. What a loving, trustful, obliging nature he had. If one could only fancy him and dear Loïs as they are now, it would be indeed a strong magnet to draw one across the bourne—to ' depart and be with Christ, which is far better ' ; but here our mortal faculties fail us, and perhaps (no doubt) it is well that it should be so, that we may be content to remain yet awhile for those we love here.

Parliament opens to-night, and we are looking with some curiosity—but not with much excitement—to see what comes of the ' liberal party ' under the Rosebery and C. B. banners. It seems well-nigh impossible that these can unite in any way, with the one and only common object of upsetting

the present Government. It would be quite too gross a violation of all sound principle, not to say of decency, to combine only for opposition; and yet what other principle can even be suggested, that would bring C. B., Asquith, Haldane, Sir Edward Grey, into the same lobby, even not reckoning the Irish members, who must, however, be reckoned with, *pro* or *con*.

I believe this is all that I can afford for you this week. We all sympathise with you in your parting for a time from dear Margie; but I hope it is not for long.

Yours affectionately,

W. T. GAIRDNER."

Temple had intended on his way to Scotland from Egypt in 1901 to linger for a time in Palestine and Syria, but altered his plans so as to visit Italy. A letter, now included, shows that even in his later years Gairdner looked back with the greatest delight to his Italian days.

" University Club, Edinburgh, 4th August, 01.

Dear Temple,

Returning home last night from Cheltenham, I found to my surprise, that there had been a complete change in your holiday plans; and I fear that all my letters, post-cards and newspapers which have gone to Beyrout, have been as good as thrown away. Among them are copies of *The Guardian* which has been having a very voluminous correspondence lately about the Athanasian creed; and although I cannot engage to replace the whole of the missing lot, I have retained a few extra copies of the one in which I was tempted to express aloud my wonderment at the position assumed by Canon M'Coll, who, though he has the 'pen of a ready writer' in a high degree, has not, I think, made any points at all in reply to those who share in my amazement; but possibly there may be yet some 'shot in his locker'—as he is always going at it, in Number after Number. I sent you very particularly the No. containing my letter, with the remark that your Uncle James

and I seem to be, more or less, on opposite sides, as to the Athanasian Creed, and I have also some private letters which I may send you, but will not, until I am quite sure of a more or less abiding address. In the meantime, when I have been figuring you as standing, or about to stand, on the top of Mount Hermon, it appears that you have taken instead to the city of gondolas, and may be at this moment inhabiting one of the *palazzi* of the old merchant princes, and visiting the home of the Doges, and that marvellous fabric of St. Mark, where Moor and Goth, Byzantine and Roman, have contributed in their several degrees to the architecture of a quite unique temple. I can envy you your first view of Venice even in these degenerate days when it is commonly entered by railway. When I went there first, in 1852, there was no such modern invention to disturb the quiet of the lagune. But even the 'strada ferrata' can't quite modernise Venice; and I fear at this season you will also find—to your disadvantage as a holiday-seeker—that the canals are guiltless of modern sanitation, so that from the purely physical and health-giving view of it, I wish you had rather been exploring Lebanon, or taking a quiet stroll by the Galilean lake. But the interest of Venice—moral, historical, and political—not to count its artistic treasures, is immense. Only I fear you will be, as I was, and am, unprepared to grasp it all intelligently from the reading of Sismondi and his many followers. By the way, I hope you will in any case make a run to Padua—it is only an hour by train, and the frescoes of Giotto in the Madonna dell' Arena would alone repay the journey many times over. But Padua is also one of the most distinguished of Italian University cities, and in this way demands attention. Verona, too, is very easily accessible from Venice, and although (if in a hurry) you need not bother too much about Romeo and Juliet, you will find there a Roman amphitheatre about as complete as any going. The Mother and I went over all this ground in our wedding jaunt, afterwards crossing by the Brenner pass into Bavaria, and exploring Munich, etc., on our way homewards. I could

envy you in sunny and picturesque Italy were it not that I fear I should be too limp to enjoy it, in the heat of August. You know, I suppose, Filicaia's beautiful sonnet, which I think Lord Byron translated :—

'Italia, Italia, o tu, cui feo la Sorte
Dono infelice de bellezza, onde hai
Funesta dote d'infiniti guai
Che in fronte scritti per gran doglia porte :
Deh fossi tu men bella, o almen più forte.'

A noble and patriotic strain all through and it touches the very key of Italian history. I have lately been reading (and mean to read more) about Savonarola, and am reserving for you a little book about him, but will not send it on till I know how, and where.

We are all well, Dorie luxuriating in England with the Maxwells, Daisy and the Sutherland interest hiving off soon to Fort William ; Eric and I bent on going to the Norwegian fiords next Saturday for a fortnight. I had a very warm week in Cheltenham, but very pleasant.

Yours affectionately,
W. T. GAIRDNER."

Gairdner and Eric went for their holiday to Norway, and a cheerful letter to Temple records some of the impressions arising from the visit.

"Steam Yacht, 'Midnight Sun,'
Merok, Geiranger, Norway, 18th August, 01.

Dear Temple,

I have not the least idea where you are at present, but I am minded to send you a letter of some kind from the fiords, and this is one of the grandest, if not the very grandest, of them, which we have just been slowly permeating in a fine steamer sailing from Newcastle, Eric and I, having been now on board rather over a week, and having visited the Hardanger and Sogne fiords, as well as Bergen, which lies between them, on our way hither via the Nor Fiord and Stor Fiord. In all of these, we have the same kind of scenery ; great sea-inlets or 'lochs,' as we would call them in Scotland,

only that the Sogne and Hardanger are each at least five or six times the length of Loch Long, and lined with steep, often precipitous hills running from 3,000 to 5,000 feet, and leading up to further heights, mostly snowfields and glaciers, to 8,000 ft.

I am writing under some difficulties and disadvantages, the lighting such that I can scarcely see to write straight, and the pen spluttering and blotting to a high degree. I am reading steadily through a much more important work on Savonarola than the little one I returned to you, Villari's book, originally I think in two volumes, now compressed into one. I find it very interesting indeed, as a study of the Renaissance, when the infamous Alexander VI. (Borgia) was the 'Vicar of Christ,' when all Italy was seething with corruption and conspiracy of selfish despots, and when men were cultivating the Classics, and forgetting God. Savonarola's was the voice of doom that proclaimed in Florence a message like that of John Baptist; but after his tragic death the impulse for good seemed to be swallowed up, and so far from a Pentecost and a devoted band of followers filled with the Holy Spirit, the very monks of S. Marco, who made him their prior and gave him the great impetus at first, turned against him at the last. A truly tragic story, which I shall now read to better effect.

I hope you will get home in safety, so I shall address this to Cairo. But when it will arrive there, I have not any idea. It will not leave this secluded place, I expect, for a day or two. Much love to you from

Your Father, W. T. G."

It has been mentioned that Frank, after coming home from South Africa in 1901, resolved on returning thither again in order to settle down. Gairdner writes to Temple on the day when Frank was to sail from Southampton.

"32, George Square, Edinburgh, Dec. 12, 01.

Dear Temple,

I have no doubt that our dear Frank has sailed from Southampton to-day. He left us on Tuesday night,

amid all our blessings and prayers, and with a quite special feeling on my part, that though I may not live to see the dear fellow again, I am sure he has taken the right and the noble part, in obedience to a deep sense of duty to all of us, and to himself. So strong is this feeling with me, that when I lie awake in the early morning thinking of him, and of all of you, the ever-present and overwhelming cry of my heart is not one of sorrow at the parting, but of joyful thankfulness to our heavenly Father for giving me a family all so thoroughly one in love for each other and for the dear Mother, who has no doubt her own part to bear in all these changes, as in those greater partings in the past, when our beloved Loïs and Hugh were taken from us. And even these sad losses, how greatly the gaps have been filled to us—especially in the case of our eldest daughter now—who has so nobly taken the place of our darling Loïs, and is now giving us the hope and promise of a new generation. I cannot express to you how much all this comes home to me, when I reflect that only a year before my marriage in 1870, I was set down by most of my friends among the useless old bachelors, and might have been so till now, a poor and lonely creature, half blind and half deaf, and with nothing to do but to rust in a useless isolation, till my time came. So I said to Frank, and I now say to you, that nothing will ever keep me from praising God for all his goodness to me in all this ; and my last word of consciousness, if I am conscious at the last, will be an earnest prayer to Him to keep you all, as hitherto, in the unity of the spirit, and in the bond of peace.

We have just heard that F. had an interview with the Director General, Dr. Taylor, in London. Taylor is a great friend of cousin David Gairdner, and was a pupil of mine somewhere about 1863 or 64. I have no doubt he will do what he can to help F., but it may not be much—especially as he does not seem greatly to favour a career permanently in the army. But I have not heard much about this yet. Perhaps there is more to come.

W. T. G."

The following letter to Temple dwells upon the birth of Mrs. Sutherland's second daughter, Margaret Amelia.

"32, George Square, Edinburgh, Jan. 5, 02.

My dear Temple (and Margie if she is still with you),

'Bless the Lord, O my Soul' for this house, I think, is a very household of blessing to-day, from the half-unconscious infant which is growing sweeter and (as D. quaintly says) 'more human' every day, and from the dear mother in her hour of glory and triumph, to the frail old grandpapa, who for the first time since Xmas, finds himself with a regular pulse again, and with the feeling that the gracious providence of our heavenly Father has once more 'redeemed his life from destruction.' Truly, it is a wonderful combination of mercies, whereby we may know how much we owe to Him, now and in the half-forgotten past of these many years bygone. Only, one is tempted to think, sometimes—where has been the 'chastening' hand, which according to the famous passage in Heb. 12 bespeaks the love of the true father—'if we endure not chastening.'

I think it was in the very last correspondence I had with your dear Margie, that in answer to some question she put about prayer, I replied that the true and only key to the mystery of prayer, was to consider it in the light of the Fatherhood, viz. as a perfectly natural and inevitable consequence of the child's response to the invitation to 'ask, and ye shall receive.' It is not a matter of logic at all, but of confidence; and just as the loving child must needs express its wants to the earthly parent, so we, if we have rightly apprehended and taken home to ourselves the spiritual relation we bear to our Father, must needs go to Him with every want and every desire in our heart, unless we are conscious that we are asking those things which he cannot and will not grant, even to our most earnest entreaties. It is thus really of no consequence at all, whether or not we get exactly what we have asked for; our 'ignorance in asking' will not prevent our Father from giving us those 'good things that we are not worthy to ask,' any more than

it will compel Him to give us what He and He alone knows to be not the real want of our nature, as seen through by Him.

And it is truly a blessed state of confidence in God when you can say to Him, but with a yet greater confidence than the author of the 139th Ps., 'Whither can I go from,'—with the *arrière-pensée,* that we cannot at all go from his spirit, or flee from his presence; but that, and just because of that, He is and will ever be, our loving Father still.

I have read with great appreciation an Address by our dear Dr. Macgregor to his congregation, written from a sick bed two days before Xmas, and I will send it to you whenever I can get another copy. As I tell him, it is written 'De profundis,' and therefore it commands a degree of regard not always paid to pulpit utterances, but which I have no doubt will be a power for good in the hearts of his congregation.

This reminds me that I have not told you of the latest phases of my case. Ever since the sudden attack on early Xmas morning, the pulse had continued irregular, more or less, and often more rather than less, with a tendency, on the whole, to be abnormally slow in its irregularity. On Tuesday last (31st Dec.) Dr. Church paid me a late visit, in consequence of some disquieting observations in the morning, and found me with a pulse beating only 36 in the minute, but not irregular as regards rhythm. I was almost prepared to have had a renewed attack that night, but it did not come, and ever since then, the pulse has been getting more steady, till now, as I said, it is, I believe, almost perfectly normal again.

With much love to Margie,

Your Father,

W. T. GAIRDNER."

The following is the only letter in which Gairdner speaks of the family name and personal characteristics of his race, as well as of the influence of the inheritance upon himself.

"32, George Square, Edinburgh, Feby. 13, 02.

Dear Temple,

In one of your late letters, you said something about the name of the Gairdner family. I think it was àpropos of Frank. Well, I should like to say to you, what I may possibly never be so autobiographical as to put down on paper otherwise—that I don't seem to remember, in my early days, having been greatly influenced by the desire of 'making a name' in the ordinary sense of the words. There was, indeed, one sense in which the name I inherited from my dear father always seemed to place me under an obligation of the strongest kind. He was, I think, about the most inflexibly just man, and also one of the least self-seeking men I have ever known in all my life. He was not the most successful among his brothers, in worldly matters, but none of them—not even my uncle Charles, who was the best man of business I ever met—excelled my father in the above respects. But of all the Gairdner brotherhood, in that generation, it might be said that whether of greater or less ability or distinction, they were emphatically righteous and pure in their conduct, public and private; and it is a great privilege to be able to add that from first to last, I never once heard of the smallest breach among them, of true brotherly affection and sympathy. That was the inheritance that I really had and valued for myself as a lesson in life; not at all the desire of getting to the front, or of 'making a name,' in the way of eminence or distinction. Indeed, if you want the secret key to any such career as I have actually had, you will find it in this. I very early discovered for myself that the handicapping in general practice that I could not escape, owing to defects of sight and hearing, was such that I must aim at teaching and consulting practice, or nothing; and hence it came quite naturally to me to spend long and apparently unprofitable hours and days in pathological investigation, when others around me were striking easily for popularity in practice. The Glasgow chair came just at the right time, and made me, for the first time, feel as if

I were in the way of something like what might be called success. But how easily it might have turned out otherwise! and then (I have often thought since) would it not have been a career of sheer disappointment, not to say failure, and, very likely, an old bachelorhood, and not one of you all to carry on the 'name'—or to comfort the present-day patriarch thereof! Think of that, my boy, and how much, therefore, I have reason to thank God for it all!

But the moral that I think comes of it all, is that in starting in life, it is best not to bother about name, or fame or anything else that is an accident; but to put your heart into your work, such as it is, and to leave the result in a higher disposal than ours. 'Seek ye first the kingdom of God, and his righteousness, and all these things shall be added unto you.'

All well here. I think of going up to London in the end of next week, taking Mama with me, for a special meeting of the G. M. C.

W. T. GAIRDNER."

This letter, one of the last written by Gairdner to Temple, should perhaps be placed in the section devoted to his opinions and practice in regard to theology and religion. Arising as it does, however, out of the anniversary of the death of Loïs, it may well have a place here.

" Edinburgh, March 30, 02.

Dear Temple,

When I have been in a meditative mood as regards our darling, Loïs, I have often entertained, but as yet without any clear insight, the question—what is she like now? and how shall we come to recognise and to know her when we too pass into the 'glory that is to be revealed'—in us, as well as in that dearly-beloved child? I find that it transcends not only my reflection, but my imagination; and perhaps it is well for us that it is so; for 'Eye hath not seen, nor ear heard.' But some things may be set down here, even in the way of confessedly crude specula-

tion, for whatever they may be worth; and surely no day can better move such thoughts and imaginings than this Easter Sunday; for this letter, I calculate, will be posted to you not only on the same day of the month, but also of the week, and very nearly at the same hour (I was obliged however to postpone, as will be seen afterwards), as I printed my last kiss on the brow of our darling in 1884.

Now what occurs to me as regards the merely physical side of the question, is this:—the poor, miserably wasted and tormented frame of our darling on that Sunday evening, after her seven weeks' agony—her cross (why should we not call it so, in all reverence?) could not well be more different from anything she is now, than it was from the radiant and ever-joyous child, who was always the light of the house, and who never caused us either pain or anxiety till she laid her poor stricken head upon the pillow, from which it was to rise no more. No outsider could possibly have recognised our Loïs, in that sadly dilapidated tabernacle then; nor should we have known her by our physical senses, to be indeed the same Loïs, if we had not had the spiritual contact with her, all along to the very last breath; all this is summed up in the mysterious and literally unfathomable conception of 'personal identity'—which, moreover, whatever it may be in fact, is not a physical, but purely a spiritual matter. Now, it is surely not possible, in the light of modern science—not to say of common sense—to assume anything else, than that from that wretched body—the 'body of her humiliation,' on the 30th March, she parted finally and for good on the early morning of the 31st. It was thoroughly worn out, and had got to be finally done with; and *was* finally done with I believe when we laid it in the grave at the top of the hill in the Western Cemetery. But when we come to the question of her glorified body (Phil. iii. 21) we can only say that it could not be even imagined to be anything more different from the 'body of (her) humiliation,' than the earthly tabernacle of the healthy, happy, child Loïs was, in our view, from the sad and mournful wreck that lay before us on the day before her death. If

we had to indulge our fancy in making her alive again, it would certainly be in the garb, and with the lovely face and figure, of her childhood.

But is it to be thought of for a moment, that the dear child should have remained a child during all these 18 years —only to please us? a 12 year old Loïs, the same as ever, only with the effects of her illness and mortal agony cleared away. That would be stagnation, not evolution, and almost impossible to conceive of in a universe of God's creating, and governed by His laws. We must, therefore, I think, conceive of our darling as having advanced in some direction, whether in that of womanhood, or of some other state which we can only define to ourselves by negatives, or by calling it 'angelic' or by some other name which only conceals our entire ignorance. But if we once admit that God has had the will, as well as the power to preserve to our dear one the mysterious sense of spiritual identity with our Loïs, then there is but little more difficulty in apprehending that the glorified Loïs may have been clothed with a new body (see 1 Cor. xv. 38) which no more interferes now with her own sense of personal identity than did the daily, monthly, yearly transitions of her earthly framework, and especially the great and awful transit from health and happiness, to unspeakable suffering and agony, in the short space of seven weeks. The body of her glory will differ from the body of her humiliation—the spiritual from the natural body (see as above) in being perfectly adapted (we may at once admit that we know not how) to the functions that God has assigned to it in His own Kingdom and presence. It will have new organs, new senses, new relationship with time and space, of which we can have no conception at all while we remain in the flesh, and have our sense of personal identity dominated by a nervous system in connection with which our bodily senses of eye, ear, skin etc. are the only 'gateways of knowledge,' as far as known to us. But when we, too, have passed out of the body of humiliation into the glorified state, it would be only in accordance with what we believe, and indeed seem

almost to know, of God as a spirit, that we should have new channels opened out to us not only of access to Him, but of recognition and access also to our own lost dear ones, who have been taken over before us, and made to ' put on incorruption.' We may call these new channels, provisionally, new senses, by an important analogy; but the truth is that we may know no more about them than a man born blind would know of visible outline and colour ; or a man born absolutely deaf of a symphony of Beethoven. And so it comes literally true again that ' eye hath not seen, nor ear heard, neither hath it entered into the heart of man to conceive the things that God hath prepared for those that love him.'

I am not at all confident in alluding to the much debated subject of our Lord's resurrection-body—(which is not the same as his glorified body) ; and therefore does not furnish analogies, or indeed much of guidance to us as regards what I have written above. But one thing, at least, may be said about it as on the face of the gospel narratives, which you and all young clergymen would do well to consider, the risen Christ was by no means always easily recognised, even by those who were in close association with the human Jesus. The two who went down to Emmaus with him on the very day of the resurrection walked with him, talked with him, and even discussed with him the whole phenomena of the teaching, life, death, and resurrection of Jesus, as if he were a perfect stranger—' only a stranger in Jerusalem ' —and it was said that ' their eyes were holden that they should not know him.' Nay, even although they had heard of the resurrection from the report of ' certain women' they were not thereby convinced, but only ' astonished,' and had resigned themselves to the belief that all was over, although before the crucifixion they ' trusted that it had been he which should have redeemed Israel,' *i.e.* from the power of the Romans, for as yet no higher view of ' redemption ' seems to have dawned upon any of them, and they were hopeless accordingly. And what at last opened their eyes was a spiritual, not a physical, fact, a something which

he alone could do, in the way of reaching their deeper consciousness, and so revealing his identity. 'He took bread and blessed it, and brake, and gave it to them.' As they put it later on, 'he was known to them in breaking of bread.' The same difficulty was experienced by the others to whom they had spoken of this interview (see Luke xxiv. 36-43).

Mary Magdalene seems to have mistaken the risen Christ for the gardener (John xx. 15) and if we put the usual and natural interpretation on Mark xvi. 12, it would appear that the 'form' of the risen Christ was different, to those who journeyed to Emmaus, from what it had appeared to those at the sepulchre. All this is perhaps rather puzzling; but it seems to shew that it was the spiritual power that emanated from Jesus, rather than the mere bodily presence, which carried conviction as to his identity even to those who knew him best. And, no doubt, the last awful glimpses of the bodily form, to those who had been present at Calvary, must have shewn as sad a change in the 'body of his humiliation' as in our darling Loïs on that bitter 'cross' in March, 1884.

31st March. The reason of the delay in posting this letter is a sad one. Our cousin Jane Hannah Gairdner died last night from pneumonia; and I have had to see her, and make arrangements, and to write to her only brother in New Zealand.

W. T. GAIRDNER."

UNIVERSITY AND HOSPITAL

As a systematic teacher, Gairdner had, in the second half of last century, no superior. Whether he had an equal has always appeared to my mind at least doubtful. In giving this expression of my opinion, it is far from my intention to minimise the achievements of other men. We have in our own age seen many a man most successful in imparting information, who has yet had absolutely no

influence in educating those whom he taught. My meaning is that Gairdner did not belong to the ranks of those who regard their pupils as requiring more or less digested facts, in the shape of alliterative mnemonics and verbal artifices. He believed in the honest cultivation of visual memory and rational mind. His lectures were not merely full of knowledge, but they were instinct with suggestion. The information which he imparted was not only philosophic in its tendency, but orbicular in its extent. It was illuminated by frequent flashes, at once of sagacious criticism and subtle humour. His hearers, therefore, never failed to receive from him fresh lights upon any subject which was under discussion.[1]

Gairdner was at least as much in his element at the bedside as in the lecture theatre, and his clinical clerks never failed to obtain from him the deepest inspiration. They received the profound lesson that a great man, intensely engaged in occupations of the most engrossing character,

[1] While these lines were preparing for the press, my friend, Dr. A. Freeland Fergus, has published the Presidential Address which he delivered to the Medico-Chirurgical Society of Glasgow, 6th October, 1911, "The Origin and Development of the Glasgow School of Medicine," 1911, and this contains (p. 22) some points which bear directly upon the subject. "Once," he says, "the question was propounded to me—'Wherein was Gairdner a great teacher?' Now the answer all depends upon the definition of a great teacher. If by a great teacher is meant a good coach, then, I think, Gairdner's most enthusiastic admirer would admit that he was not in that sense a great teacher. Grinding is not study, and a coach never gives a pupil the only basis upon which permanent knowledge can be built—a physiological view of the subject. If a good coach is the sole interpretation of a good teacher, then it must at once be admitted that Gairdner was no great teacher. If, however, it is instructive to youth to be brought into contact with one of the most physiological minds of the time, to observe his extraordinary conscientiousness, his high ideals, to note the pains he takes, and the time spent in examining the minutest details, then Gairdner was, in that sense, pre-eminently the best teacher of his day."

never failed to fulfil his hospital engagements, and that he could afford the same amount of time to a humble inmate of the Infirmary as to an important private patient; that the trust reposed in the Professor by the Board of Management was the most sacred duty of the day. They saw in him, moreover, unwearied patience in observation; unsurpassed acumen in seizing the scientific facts; and unrivalled sagacity in the analysis and synthesis of phenomena. They found him, besides, not only willing to impart his stores of knowledge, but anxious to discuss every problem with his clinical clerks. He never spoke to them *ex cathedra*, but, pursuing the Socratic method, he acted more as if he were the chairman of a debating society. He extended to each of his pupils the most respectful consideration for any views which he might express, and the warmest encouragement for all of his hearers to question and criticise. His manifest kindliness to the patients under his care, and his tender consideration for their feelings never failed to impress his followers. In this connexion it may be said that one of the few men whom he ever denounced to me was a well-known German professor, for whom, it may be added, my own feelings were those of profound repugnance. When Gairdner visited the great medical school in which he exercised his vocation, he heard him for an hour and a half, on two consecutive days, giving a full description of every symptom exhibited by an unfortunate patient; an entire diagnosis of the malignant affection from which he suffered; and, avoiding all question of treatment, the orator concluded—"Meine Herren, die Prognose ist ganz schlecht." It was generally believed that this eminent Teutonic professor, when pressed by a private patient by whom he had been consulted to tell him by what disease he had been attacked, answered in a burst

of confidence—"My dear friend, we shall see at the post-mortem examination."

As an examiner, Gairdner was unsurpassed for his fairness and forbearance. He never expected any out-of-the-way facts from candidates, and never put questions which involved freaks or cranks. His patience, even with an absolute blockhead, was unwearied, and he invariably did his best to set every candidate at his ease.

As an administrator, although he could not be considered as what is usually termed "a man of affairs," Gairdner faithfully performed every public task which was demanded of him; but, if he thought that others were better fitted for the discharge of such duties, he urged their appointment to public posts in preference to accepting them himself.

It was most characteristic of Gairdner that, during his connexion with the University of Glasgow, he invariably attempted to bring others forward, instead of taking the prominent position to which he was well entitled. He declined, for example, to act as Representative of the University in the General Medical Council, and induced his colleagues to appoint Professor Leishman to that position. In the same way, he withstood the wishes of the Senate to elect him a Member of the University Court—again insisting upon Dr. Leishman taking this place. On the lamented death, however, of Dr. Leishman in 1893, he found it necessary to succeed him, and, in the month of November of that year, he was, as has been already stated, accordingly elected a Member of the University Court and the Representative of the University on the General Medical Council.

His relations to his colleagues were marked by the warmest friendship and the greatest loyalty. This may be said equally of those who were associated with him both in

the discharge of his civic trusts, and in the performance of his academic duties. A letter to Dr. Fraser of Paisley shows this fact, with regard to his assistant and successor in the Public Health Department of Glasgow.

"32, George Square, Edinburgh, 24th June, 1905.

Dear Dr. Fraser,

I have been so much engaged for these many years back in writing appreciations of men younger than myself, that it cannot but occur to me that in the course of a year or two there will be but few remaining who could speak about my teaching in such terms as you used the other day; to my no small contentment in respect of the evidence it gave me that the seed had fallen on good ground. When the time comes for writing my obituary, I think my 'Manes' would be greatly placated if such a testimony as yours were to appear, apropos of the occasion, but this I must leave entirely in the hands of survivors. The last service of the above kind which it has fallen to me to perform was in a brief introduction to a volume which is to be published of Dr. J. B. Russell's collected papers, and which I think will appear to all interested in Sanitary matters to be one of the most stimulating we have ever had. You are probably not unfamiliar with some of his writings, but the whole when put together by his successor, Dr. Chalmers, will, I think, be a most remarkable collection.

Yours faithfully,

"W. T. GAIRDNER."

No member of the Senate ever gave a warmer welcome to a new colleague than did Gairdner, of which this letter may be taken as a type:—

"Glasgow University, Nov. 9, 86.

Dear Dr. Story,

I have very real and sincere pleasure in congratulating you on your appointment to the chair of Church History

here, not only on public grounds so well known and so clear that I need not insist upon them, but because it will make us near neighbours, as near, indeed, as can well be in this world, perhaps for the remaining term of our natural lives. I need not say that you and Mrs. Story will be received with the greatest cordiality in the College court, but can you suppose it possible that some small parties in No. 9 at the College are not satisfied as to your competence for the appointment, and troubled in their little minds with my report, in answer to an urgent question at breakfast, that I have not actually seen any children younger than a young lady of (say) 'sweet seventeen.' The opinion appears to be that to write ecclesiastical history successfully a much younger 'staff' is requisite, and that No. 8 College (being so close to No. 9) ought to have a neighbourly couple who will get up playmates to order for the denizens of the latter No. I only mention this little apparent disqualification in the hope that you and Mrs. Story may, like another distinguished personage in ecclesiastical history 'tak' a thocht and men'.'

I am,

Yours very truly,

W. T. GAIRDNER."

The following letter, also addressed to Dr. Story, is full of loyal friendship and personal encouragement :—

"Glasgow University, June 29, 98.

Dear Dr. Story,

I was meditating, in connection with the announcement we have had yesterday and to-day, of your appointment to the Principalship, and of the vacancy in the Church History Chair, whether it would be possible to submit to you, on the part of the Medical Faculty, a proposal for your presiding at the Medical graduation on July 21st. Dr. Stewart, however, informs me that it is impossible that you should be in a position legally to act as Principal and Vice-Chancellor at that date, as the actual vacancy in the Principalship does not occur till the end of July.

To my sorrow, therefore—unless Lord Kelvin can be got to come to the rescue—I fear I shall have to confer the degrees.

I need not say for myself, and for all our people—and I believe Lady Gairdner has already written to this effect—that we shall be most happy to have you to reign over us, and have no doubt that the great and dignified office which has been conferred upon you, will be filled in a manner commensurate with its importance, and with the wishes of us all in the Senate. The only thing that we have heard—or that anyone had heard, I believe—against your appointment, is that you have been supposed to be so much involved in church politics as not to be able to attain to the kind of aloofness that the Sovereign has in the politics of the nation, and that may be reasonably expected of the head of a great educational institution that is of no party, and ought to be all-comprehensive. I note, however, that very little has been heard of this objection—at least audibly—in quarters where it might have been expected; and even the North British Daily Mail (although wanting to have Lord Kelvin) has refrained from any kind of unregenerate or Cameronian language as regards so great a pillar of the Scottish church, and foe to disestablishment.

I, therefore, sincerely trust that the worst that can be said has been said; and that we shall be able to find in you a Principal of the same type as our dear Dr. Caird—sitting in sovereign state above the welterings of church and party politics, though of course holding, none the less, the opinions that every man who is worth his salt must individually hold as regards all public matters.

With kind regards to Mrs. Story, whom we join in congratulating not less than we do yourself,

I am,

Yours very truly,

W. T. GAIRDNER."

When death or resignation brought about the severance of an academic tie, it was deeply felt by Gairdner—a fact

which is abundantly testified by the following letter to Dr. Fraser of Paisley, upon the death of Professor Coats :—

" 225, St. Vincent Street, Glasgow, Jan. 24, 99.

My dear Fraser,

Just before going to lecture to-day, I had the news conveyed to me of Dr. Coats's death, which, though not unexpected in the end, took me by surprise as regards the moment, for only last evening I was at the University Club at a house dinner given in honour of Principal Story, where a message was read from Coats as President of the Club, couched in very hearty terms as regards the Principal, of whom he said that it was evident that he (the Principal) 'means business.' There was no hint given as to his own illness, and altogether it was a very brave and even remarkable message under the circumstances.

I need not say that it is the severance for me of a very old and dear association, and every one here will sympathise alike with the family of one who after fighting the battle of Pathology in Glasgow for years as an unrecognised, or only partially recognised teacher, and doing it so manfully and well, is now cut off just when his means and opportunities had come to full fruition, and when all the world, both in Glasgow and out of it, had come to recognise in him one of the ablest members of the professoriate.

I am, Yours very truly,

W. T. GAIRDNER.

P.S.—Of course this is meant for Mrs. Fraser as well as for you."

That he himself was keenly appreciative of the warm friendship with which he was regarded by the other members of the Senate is shown by a letter to the Principal, who had made some remarks at the close of the winter term :—

" 9 The College, Glasgow, April 18, 1900.

My dear Principal,

I was unable to appreciate—because I was unable to hear distinctly—most of your remarks yesterday, although

CARTOON OF SIR WILLIAM TENNANT GAIRDNER, K.C.B.

A Hospital Group, Western Infirmary, 1881, from a Drawing by the late Dr. John M. Smith, then House Physician.

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I did manage to catch up, in an indefinite sort of way, that there was a reference to myself.

Now that I have read it all in the *Herald*, I must simply say that I am deeply touched by the way in which you have spoken, thus giving voice to what I above all have tried to live, so as to deserve, if possible—the approval and personal regard of my colleagues. Under your predecessor, the opportunities that occurred from time to time of realising in some way or other the great goodwill and affection he always bore me, and which I more than reciprocated (if possible), left me in no doubt at any time of his support in any kind of difficulty or trial; and happily the occasions were not many for years past when I have had to give him any trouble in this way.

Your reign, so far as we of the Senate are concerned, has hitherto been one of peace and quietness for the most part, and now after August, I shall be officially 'where beyond these voices there is peace'; at all events, there will be one member of the Senate, who for good or for evil will 'cease from troubling.' It is a great satisfaction to me to carry into retirement the expression of so much goodwill and affection from the head of the Senate.

I am, Yours most truly,

W. T. GAIRDNER."

As regards his relations with his old pupils, it need only be said that he never ceased to retain a warm interest in their career. Let us consider, for example, another letter to Dr. Fraser of Paisley, in which this is very clearly shown:—

"9 The College, Glasgow, March 1st, 1895.

My dear Fraser,

I am glad to observe that you have got your new Infirmary fairly launched. Is the architect thereof any relation of that extremely fine fellow Dr. Abercrombie, now in London, and a most valued pupil and friend of mine? I noted that the heating and ventilation is to be on the principle of the Victoria Infirmary, but with the difference

that it is to be possible to exchange the mechanical for the more ordinary method, should circumstances so require. Dr. Mackintosh, who was Superintendent at the Victoria and now at the Western Infirmary, called here this morning, and I put the question to him, as it had not occurred to me that such an alternative was possible. He says he does not think it will work at all, and that one or the other plan, but not both, should have been adhered to.

I hope you are getting on well with Carslaw. They are a very clever lot. The man I have as assistant, John H. Carslaw, is up to anything, and your man was just the same when with me some years ago. John H. has had to do my whole lectures for the last fortnight, as I have been laid up with influenza, but now nearly well.

W. T. GAIRDNER."

Dr. Young, Stoneyroyd, Leicester, tells me that, after the account of the brilliant exploit of Major Babtie, R.A.M.C., for which he received the Victoria Cross in the South African War, appeared in the *British Medical Journal,* he wrote to Gairdner, recalling the fact that the major was one of his old pupils. Gairdner replied :—

"9 The College, Glasgow, May 9th, 1900.

Dear Dr. Young,

I have written to Lord Rosebery, suggesting that he might very appropriately bring out the fact of Major Babtie's academic position in his speech at the Glasgow University dinner in London, at which his Lordship is announced to preside. I am sorry that I cannot be there, as I am due to be in London in the succeeding week for the G. Med. Council, and I can't possibly desert my clinical class twice over. But I think it quite likely that Lord R. may cotton to my suggestion, and it will thus be reported in the newspapers in a proper oratorical setting, for the benefit of his actual Mother, as well as his Alma Mater. So pray look out.

I am, Yours v. truly,

W. T. GAIRDNER."

This postcard followed :—

" 19. 5. 00.

I had a very satisfactory letter from ex-Provost Babtie, who does not know precisely where the Major is at present, but is to communicate with me as soon as he can. He expresses great regret that Mrs. B. (Mother) is not alive to hear of her son's honours, but is most gratified with it all.

Lindsay Steven has undertaken a notice in the next No. of the G.M.J. You would observe also that Lord Rosebery made good use of my suggestion in his speech at the G.M. Club dinner.

W. T. GAIRDNER."

Dr. Young also wrote to Major Babtie, with whom he was very well acquainted when a student, to congratulate him on his bravery, and received the following :—

" Maritzburg, 6th June, 1900.

Dear Young,

Many thanks for your kind letter of congratulations. I have received many messages from old friends in all parts of the world, of which I am almost as proud as the decoration itself. Among others I was deeply gratified at a letter from Sir W. Gairdner in which he mentioned your name.

Yours sincerely,

W. BABTIE."

Dr. Young goes on to remark :—

" I was a year old when my Father was appointed to the care of the Hunterian Museum, Glasgow University (which post he held for 40 years), and had many opportunities as a boy, and afterwards as a medical student, of observing and admiring Sir William's quiet demeanour and skill, as well as his ' thoroughness ' as a teacher. He was affectionately termed ' Old G.' by his pupils, but no one ever thought of taking a liberty with him, so greatly was he held in reverence. I have in my possession a testimonial he gave me many years after I left Glasgow, in which he incidentally mentions

my Father as 'one of the ablest practical geologists and mineralogists in Scotland'—another example of his kind nature."

The two letters which are appended breathe the same spirit of warm interest in his pupils:—

"9 The College, Glasgow, Feby. 19, 97.

My dear Macphail,

I am really much touched by your kind consideration, not only in preparing for me, at the cost of so much trouble, the valuable illustrations of Smallpox, but for the more than complimentary terms in which you have conveyed your gift as in the letter that accompanies it. I am sure you will give me credit for entire sincerity when I say that in looking back on my career now from a fairly advanced standpoint of years, it always seems to me to be redeemed from 'vanity and vexation of spirit,' when I can look at it in the reflected light of young and ardent minds still working hard and to good purpose, having felt, like you, that they have derived some kind of impulse or inspiration for good, from my teaching. I could not otherwise feel sure, at all times, that my professorial and hospital work, which has been, on the whole, so profitable and pleasant in the doing, has been in a like degree acceptable to those who have had to 'sit under' me; but in the case of the whole *gens* Macphail, as well as in you, I have had, as the great apostle said 'wherein to glory' and to thank God accordingly.

Believe me always,

Yours most truly,

W. T. GAIRDNER.

Dr. Alexander Macphail."

"9 The College, Glasgow, March 9th, 00.

My dear Macphail,

Many thanks for your little 'Memorial' pamphlet on poor Hugh Calderwood. I was not aware that the affair was in such a state of progress, but I am glad to find it is so.

I don't think I ever saw him after he was a member of my clinical class, for when it came to the fatal illness, though he came to my ward, the end came about before I had seen him.

I quite understand how universally he was loved and respected. It is never too soon to die, when such feelings can be left behind.

I am,

Yours very truly,

W. T. GAIRDNER.

Dr. Alexander Macphail."

His grateful appreciation of the personal affection of his old pupils is beautifully shown by a letter to Dr. David Grant of Melbourne, which the latter has very kindly forwarded to me :—

"9 The College, Glasgow, June 2nd, 97.

Dear Dr. Grant,

I am afraid I did not quite clearly remember you as on that occasion long ago ; but I am most grateful to you now for giving me the opportunity of sending my message of sincere condolence as regards our friend Dr. Gardner, whose loss I feel very sensibly, as it was not many years since I renewed acquaintance with him here, and he was then apparently in unquestionable health and vigour, very pleased to see us all again, and only sorry that I could not see my way to go out to some colonial gathering at Melbourne, or Adelaide. He was an excellent man, a good surgeon I believe, but I knew him chiefly as my resident in the Royal Infirmary, and therefore on the medical side. I heard incidentally of his death at Naples, and would have written, but that I had no information as to whether he was accompanied by his wife or any of his family, and whether they had returned to Melbourne. Will you be so kind as to convey my sincere sorrow and hearty sympathy to them if you have an opportunity. It is very sad to find that I am surviving so many younger men who, one would think,

had a claim to outlive one who never at any time wished for a very long career, and only strove to make it such as would preserve his memory among young and more active men.

It is pleasant to read in your letter that some of these old pupils are near you, and that I am still remembered among them.

I am,

Yours most truly,

W. T. GAIRDNER.

Sanders was altogether, I think, the best of my friends in Edinburgh; besides the fact that our wives were related as cousins.

Dr. David Grant,
Melbourne."

MEDICINE AND SCIENCE

Gairdner was undoubtedly endowed with great originality of mind, and looked on every question from a fresh point of view. He was in this way enabled to throw new light upon many of the problems with which he concerned himself throughout his career. He was also possessed of much steadfastness of purpose, so that it was almost impossible to deflect him from any particular line of inquiry until he had satisfied himself that he had probed the matter to its depths. He had, further, a most useful tenacity of opinion, which would not allow him to swerve from what he considered to be true until, by force of circumstances, he was obliged to admit that other ideas had more reason than those for which he had been contending. The union of these different gifts, when applied to pathological research and clinical investigation, formed a combination of rare strength for the advancement of scientific medicine.

We have seen that, even as an undergraduate, when a member of the Hunterian Society, he was a hard worker and keen debater. In the records of the Medico-Chirurgical Society of Edinburgh, his name is to be found in the minutes of almost every meeting, contributing articles, demonstrating specimens, or criticising papers. He pursued the same course after his transference to Glasgow, and his presence was a most stimulating factor in the medical circles of the West. It must be added that the list of papers and addresses contained in the Bibliography published in this volume does not comprise more than a moderate proportion of his total contributions—much of his most valuable work was simply given in the form of impromptu remarks at the various societies. As a debater, he was remarkable at once for his searching analysis of every subject under discussion and his generous appreciation of the work of his contemporaries. His receptive mind is proved by this letter from Dr. Scott Alison, who had introduced a differential stethoscope—a form of instrument which is even now occasionally in use :—

" London, 80 Park Street,
Grosvenor Square, June 4, 1861.

My dear Sir,

I am much pleased to find you have taken the trouble to try the differential stethoscope and that in your hands it promises to be useful. I think in teaching a class it will convey in a very striking manner to the mind various acoustic signs. One of the prettiest results is obtained by taking the voice of a healthy side and a side enclosing a vomica. The lowest whisper in many cases is conveyed most distinctly and as if through an uninterrupted column of air, into the ear connected with the one cavity whilst silence reigns in the other.

I regard your testimony as of great value, for one meets

with so many impediments from various causes, from apathy, from general distrust of what is new, and from failures depending upon want of care and due diligence to obtain the promised results.

I met Dr. Watson[1] last night, and having your letter in my pocket I lent it to him for perusal. He had been asking me about the instrument. Your opinion will deservedly have great weight with him, and I therefore hope you will excuse me for giving him an opportunity of learning it.

I am my dear Sir,

Very faithfully yours,

S. Scott Alison."

The same attribute followed him throughout the whole of his career, and his mind was always open for every new advance.

The value of his original contributions to medical knowledge was indeed a very high one. In his earlier days, his pathological contributions were characterised by the application of normal anatomy to morbid processes; and his researches into the structural alterations of the kidney, the lung, the heart, and the arteries will always remain monuments of his pathological perspicuity. At a later period, when he had devoted himself more decidedly to clinical medicine, he showed how the comprehension of physiological processes was a prime factor in the explanation of symptoms. His contributions to the physical examination of the chest —more particularly in the elucidation of the murmurs produced by cardiac disease, and the effects resulting from aneurysm, will never cease to be quoted, as long as medicine is cultivated. The introduction of graphic symbols for acoustic phenomena, not merely as an assistance to visual memory, but as an aid to the understanding of medical problems, marked a new epoch and constituted a fresh

[1] Afterwards Sir Thomas Watson, Bart.

departure which has been followed by almost every teacher since his time.

Above all these characteristics, were his philosophic integrity and scientific accuracy. He could not be satisfied with " words and their wonderful play," but was impelled by his inquiring mind to penetrate at first hand into the secrets of nature.

The attitude of Gairdner to scientific questions has always seemed to me instinct with the free and soaring spirit of the glorious age of Attic supremacy. He approached every subject with a total absence of preconception and prejudice, anxious only to " prove all things." There was undoubtedly a deeply philosophic tendency in his mind, which led him not infrequently into the paths of the metaphysician. This is well seen in the Address reprinted in this volume—" The Two Disciplines in Education." Some of the views therein expressed led to most interesting letters from Professor Burnet of St. Andrews and Professor Butcher of Edinburgh :—

" St. Andrews, Sept. 27, 1899.

Dear Sir William Gairdner,

I think there can be no doubt at all that the interpretation of *νούσων φύσιες ἰητροί* which you have adopted is the correct one. It fits in admirably with the account of the original sense of *φύσις* which I gave, I think for the first time, in my ' Early Greek Philosophy.' I do not wish to dogmatise about Hippokrates, but I think I see certain points very clearly.

In the first place, there is no reason to doubt that the phrase is for all practical purposes Hippokratean. Even the later books of the *Ἐπιδημίαι* may safely be referred to the generation immediately succeeding the master's death.

The attitude of Hippokrates to *φύσις* was, I take it, this.

In the first place, it is impossible to be a good ἰητρός without a knowledge of the φύσις of the world as a whole, that is of φύσις in the sense I have explained in my book. Only we must avoid the mistaken idea of the early cosmologists that φύσις, the primary substance of the world, is one. But this is not enough for the ἰητρός. Besides the κοινὴ φύσις ἁπάντων he must know the ἰδίη φύσις ἁπάντων the 'constitution,' (σύστασις) of the individual patient. The following passage is quoted in the most recent work on the subject, Fredrich, 'Hippokratische Untersuchungen,' p. 4 sq., from Ἐπιδημίαι Ι· τὰ δὲ περὶ τὰ νοσήματα, ἐξ ὧν διαγινώσκομεν, μαθόντες ἐκ τῆς κοινῆς φύσιος ἁπάντων καὶ τῆς ἰδίης ἑκάστου κ.τ.λ.

This explains the φύσιες which are νούσων ἰητροί. The plural is used because the 'constitutions' of different patients are referred to, and the reference is primarily to the κρᾶσις of the phlegm, gall, etc., in the individual under treatment.

So far as I can understand, this comes exactly to what you say.

I think it is quite certain that the idea of φύσις as a quasi-intelligent force is later than any part of the Hippokratean corpus. It is true that Aristotle often used language which, if taken literally, implies this idea; but even in Aristotle the metaphor is more or less conscious. The idiom of the Greek language allows the use of such words as βούλομαι of any tendency whether conscious or unconscious, and this suggests the metaphor at once. But, when he is speaking scientifically, Aristotle means by φύσις that which 'has its source of motion and rest in itself,' *i.e.* that which begins to grow of itself without the aid of man, and stops growing of itself when it is full-grown (τέλειον), that is when it has reached its true 'form' (εἶδος). There is no mythology in this and *a fortiori* not in Hippokrates.

I send you with this a little paper of mine on a kindred subject.

Yours sincerely,

JOHN BURNET."

" 27 Palmerston Place, Edinburgh, Nov. 15, 99.

Dear Sir William,

That Hippocratic saying you quote is indeed a treasure, *νούσων φύσιες ἰητροί*. I was not acquainted with it.

As for the idea of *φύσις* or Nature; it is implied in all the early Ionic philosophers, but it is not easy to speak with certainty as to this first use of the word in that sense. My impression is that it occurs first in a fragment (No. 10) of Heraclitus (flor. circa B.C. 500) *φύσις κρύπτεσθαι φιλεῖ* 'Nature loves to hide herself.' The saying is quoted by several ancient writers (Themistius, Philo, Julian), but we don't know the context, so that a certain doubt must remain as to its exact force. But it is not easy to see what it could have meant if *φύσις* had any other signification.

Empedocles (flor. circa B.C. 460-470) has generally been cited as the first who used the word in this sense. The passage quoted does not, I think, bear out the statement. It is in the poem

36–39.

ἄλλο δέ τοι ἐρέω φύσις οὐδενός ἐστιν ἁπάντων
θνητῶν οὐδέ τις οὐλομένου θανάτοιο τελευτή,
ἀλλὰ μόνον μείξίς τε διάλλαξίς τε μιγέντων
ἐστί, φύσις δ' ἐπὶ τοῖς ὀνομάζεται ἀνθρώποισιν

'Yet another thing will I tell thee:—there is no coming into being of anything that is mortal, nor yet any end for it in baneful death; but only mingling and separation of what has been mingled, and " coming into being " is the name that men give to these things.'

By the way the title *περὶ φύσεως*, which Empedocles, like several other of the Ionic Philosophers in the 6th and 5th centuries B.C., gave to his work, does not appear to have meant *de rerum natura*, but rather 'concerning primary substance.' The search for permanent primary substance was their main preoccupation and this is the prevailing sense of *φύσις* in their writings. Other uses such as *ἕκαστον κατὰ φύσιν* 'each thing according to its nature' (Heracl.)

are not uncommon. The word is still with them pretty fluid in its application, and I think it is probably a mere accident that we have not the meaning of 'Nature' more unequivocally found in our extant fragments. The idea of 'Nature' as a creative and unifying force runs through their thought.

I have not had time to make further research into the point. But this is the gist of some notes I made a few years ago when I was looking up the question for myself.

With many thanks for your most kind letter,

I am, Yours sincerely,

S. H. BUTCHER."

The phrase "orbicularity of information" has already been applied to Gairdner in one of the preceding pages, and this letter to Sir Thomas Oliver of Newcastle, evidently revealing, while apparently disclaiming, considerable acquaintance with the trend of modern science, deserves a place :—

"Western Club, Glasgow, Aug. 27, 89.

My dear Oliver,

It will be very agreeable to me to meet Mr. Romanes, as I did some years ago, but not very intimately, at Dublin.

But I am afraid he will find me not quite *au fait* in star fishes, nor yet in the elaborate psychological analysis by which he is building up a Darwinism *in excelsis*.

However, I am—within the narrow limits of my somewhat desultory reading—open to conviction that we are all animals like the rest !

Yours ever,

W. T. G."

Unfortunately, in consequence of the death of his brother John, he was unable to meet Mr. Romanes.

His keen insight in every branch of medicine is shown by his having entered into correspondence with Professor Charcot of Paris on the subject of hysterical hemianaesthesia.

What he wrote to the distinguished French Professor is not known ; but Dr. Hawthorne has very kindly furnished me with a copy of Dr. Charcot's reply :—

" Paris, May 28th, 1893.

Dear Dr. Gairdner,

I did indeed at one time believe that hysterical hemianaesthesia can at times assume in all points the characters of an organic anaesthesia. Since then things are changed somewhat, thus I no longer think that concentric narrowing of the field of vision belongs to the organic lesions. It is the special character I think of hysterical hemianaesthesia. Moreover, organic hemianaesthesia never is so complete and so absolute as is hysterical hemianaesthesia, and in this latter the muscular sense and the sense of position are remarkably affected, whilst this is most rare in the former. I will add that when persistent organic anaesthesia is accompanied by motor hemiplegia the latter is very soon marked by rigidity of the affected members, exaggeration of the knee jerks and also of the tendon responses of the upper extremities, which last never takes place in hysterical hemiplegia. There may be rigidity—contracture—without doubt ; but it is really rare for the reflexes to be exaggerated. It would be difficult for me to give an opinion of the case, the résumé of which you were kind enough to send me. Hysterical or organic ? that is the question. Let me strongly recommend to you the research into the equal and concentric narrowing of the field of vision, which, in general, involves both eyes ; one, however more than the other. I think it is an hysterical characteristic.

There came under my observation in time past, a number of cases of organic hemiplegia with hemianaesthesia and narrowing of the visual field ; it is these which deceived me, the autopsy having shown in these cases the existence of a focal lesion of the internal capsule. But I have since been able to assure myself that in these cases there was room for supposing a combination, which occurs not infrequently,

of the organic together with the hysterical element ; that is organic hemiplegia with a superinduced hysterical character (hemiplegie organique avec revêtement hysterique—suivant le jargon du jour).

Always at your disposal,

Believe me, my dear colleague,

CHARCOT."

The mention of Charcot leads me to introduce a very interesting letter, in which, after referring to the Professor, he points out a curious slip in regard to the nationality of Wier :—

" The Manse, Kilconquhar, Fife, Aug. 28, 93.

My dear Allbutt,

I read, of course, both with pleasure and edification the remarks of ' a distinguished English Physician ' with respect to Charcot, and I agree with most of what he has said, though my personal acquaintance with Charcot was not great, and with Trousseau almost none at all—' tantum vidi,' I may say. But I have detected a curious slip—curious that is to say for you—in the assumption that John Wier was an Englishman. John or Johan Wier (Lat. Wierus) was Flemish, or at least a man of the Low Countries, a contemporary of Erasmus, and either a Protestant or associated with some of the early reformers, and (if my memory serves me right) with that strange compound of Caballistic learning and nationalism, Cornelius Agrippa (see Henry Morley's admirable life of the latter). Wier's book—' De Praestigiis Daemonum '—which Charcot edited, was a wonderful book for its day ; but not so wonderful (I think), or at least so thorough-going as the nearly contemporary one of Reginald Scot—a Kentishman, ' The Discoverie of Witchcraft,' which I got my old friend and fellow-graduate Richard Brinsley Nicholson to edit, as a most interesting bit of Elizabethan literature. I hope you have—no doubt you have—it in your Cambridge library. Both Wier and Scot were included in a common damnation by King James,

in the preface to his Demonology, for having dared to doubt about witches. You will find all this very interesting, if you have time to look at it.

Yours ever,

W. T. GAIRDNER.

P.S.—I am just going off to the west coast of Sutherland on a professional visit."

A further proof of his wide information has been very kindly furnished by Sir Dyce Duckworth, who has placed in my hands the following note upon Glisson :—

"Glisson—'Tractatus de natura substantiae energetica,' 1672.

Virchow's more extended reference to Glisson will be found in the Croonian Lecture, 1893, Lancet, March 16, 1893, p. 571.

(The book is in the Library of the Med. Chir. Soc., but we have no copy of it in Glasgow.)

I submitted the book to my colleague, Professor Adamson, who informs me that he is well aware of the philosophical status of the book, as being in form a strictly scholastic work—Glisson being considered to occupy an intermediate position between the great schoolmen of the middle ages, and Leibnitz.

There is certainly very little, if anything of Harveian method in the book, and still less of Harvey's spirit. In fact, it is simply amazing to me to find such a book written a full half century after Harvey began his investigations. Virchow says of G. that he borrowed some of his ideas from Paracelsus—surely quite another kind of fountain from Harvey, and a very muddy one too !

But I did not light upon anything Paracelsian in my rapid reading of the Tractatus, and I doubt if G. had in his mind anything so modern.

W. T. GAIRDNER."

Gairdner's undying pleasure in the problems of physical

examination is revealed by the following letter to his friend and former pupil, Sir James Barr of Liverpool :—

" 225 St. Vincent Street, Glasgow, Nov. 9th, 1887.

Dear Dr. Barr,

I am glad to hear that you are still wide-awake as to A.S. murmurs and I need not say that I agree thoroughly in much, if not all, of what you write. A paper at the B.M.A. in Glasgow would be very appropriate —provided that the vein is not thoroughly worked out before then by Bristowe and Sansom, who are both at it— the latter, I fancy, much in the same direction as you.

Bristowe's paper will be read immediately to the Med. Soc. of London. I was asked to go up and speak, but could not possibly do so.

Sansom is preparing a paper for the Med. Chi., and from what you and I know of him it is pretty sure to be good.

Surely we shall get the errors and confusions about it settled ere long, with so many independent workers. I have no misgivings at all as to the essential accuracy of what I have delivered on the subject, though I am aware it is far from complete.

I agree in the main with what you say as to the apparent 'loudness' of the murmur ; but it depends, I think, more than on anything else, on the effect of direct convection to the apex with the blood current entering the ventricle, and is therefore an additional argument against Dickinson's view.

And those who insist on the feebleness of the auricular contraction forget that it is not feeble at all when superadded (as it is) to a powerful *vis-a-tergo* derived from the blood pressure in the great veins.

Ever yours,

W. T. GAIRDNER.

I am glad to know from your note that you are well and I hope the same is to be said of Mrs. Barr and the chicks —also your brother John."

Another letter to Sir James Barr recalls the warm debates which took place in the seventies regarding the use of antipyretic measures, and shows Gairdner's constant desire to make use of every means holding out the prospect of alleviation of suffering :—

" Glasgow University, March 25, 90.

My dear Barr,

I am much interested in your Lancet articles, and anxious to consider them more carefully when I have this load of the end of the Winter Session off my shoulders ; at present it is hopeless to read anything with my whole heart.

I have always thought, and taught (ever since I got into controversy with Liebermeister in 1878, that if an antithermic method is to hold its ground permanently, yours is the proper way to do it, and not the ' battledore and shuttlecock ' plans of L. and others. I have not tried it, because I have had none but dropping cases of Enteric Fever at uncertain intervals, never any proper field ; and I could not get them to try experiments at Belvidere. But I suppose you know Stokes tried something of the kind in smallpox, and was pleased with the results. And there is also the precedent of the treatment of pemphigus at St. Louis, I forget at this moment by whom. I shall look forward to your next article with renewed interest.

Yours ever,

W. T. GAIRDNER."

A letter to Dr. Ainslie Hollis of Brighton shows how constant was his interest in the subjects of his early studies :—

" 9 The College, Glasgow, Dec. 30, 97.

Dear Sir,

In your very interesting paper on the lungs as dust-ejectors, I do not find that you have taken any count of a mechanism which I believed I had, many years ago, if not established, at least rendered very probable.

I have been in the habit of speaking (in my lectures and

otherwise) of the muscular walls of the finer (non-cartilaginous) bronchial tubes as 'scavenger muscles,' and in my early papers on bronchitis and pulmonary collapse (1850) in the *Monthly Journal of Medical Science* I adduced many considerations bearing on the subject, which I have no time now to particularise. At a later period, I wrote in the *Glasgow Medical Journal* a paper on the Action of Expectorant Remedies, which was rather passionately, but I think also ineffectively, combated by my then colleague, the late Prof. Easton, but without disturbing in the least my own oral teaching on the subject.

Prof. Whitla of Belfast is almost the only therapeutical writer who has applied himself seriously to this discussion, and you will find his remarks in his 'Dictionary of Treatment' 3rd edit. p. 95, with further and more elaborate references in Pharmacy, Mat. Med. and Therapeutics, 6th edit. p. 350.

I am, dear Sir,

Yours very truly,

W. T. GAIRDNER.

Dr. Ainslie Hollis."

Gairdner retained his vivid interest in scientific medicine to the very end, and during the penultimate year of his incumbency of the Chair in Glasgow, he wrote this letter to his friend Dr. Vincent Bowditch :—

" Glasgow, April 13, 99.

My dear Vincent Bowditch,

I am sitting in the reading-room of the Faculty of Physicians and Surgeons, turning over the recent vol. (14) of your Climatological Transactions, and have been much pleased with several of the papers, yours included. You have done well in remarking on the too great insistence of certain modern bacteriologists on the infectibility of phthisis, although it is a fact that we cannot get rid of. That expressive phrase—' I am a leper ' contains a whole volume of precaution in it, and when I find people erring here in that direction I shall remember and make note of it. Agreeing with you as I do, I have but little to say further as to your

contribution, but the one by your President Dr. Otis, is most interesting to me, as without going out of my way to write about it, I have given a good deal of attention to the side-lights on the characters of the three great men he describes. Will you tell him that it has been a great refreshment to my mind to read his paper? One little note of Laennec's character he does not advert to; he will find it in the Latin introduction to the 2nd edition of the 'Auscultation Médiate,' where he gently reproves Andral for carrying on researches in the Charité, without any communications to him, although he had been teaching the same facts to men from all parts of Europe in the same hospital, and could have cleared up his colleagues' difficulties at once! The quietness and modesty of this remark struck me as much as anything I have read about Laennec. I think I have referred to it somewhere, but cannot exactly remember where, at present.

You do well to refer to my friend J. B. Russell's admirable production on Tuberculosis. I have been trying to get him to publish a collection of his sanitary reports, now that he has been removed from us, to go on the Local Government Board at Edinburgh. Few men have imported a more rare genius and at the same time accuracy of statement, into the work of sanitation.

I am directing the attention of Dr. John Macintyre who is the first and most original of our local Röntgenists, to the papers by Dr. Williams, of which I hope also to make some personal use in my teaching, as I cannot myself practise the art.

There is an interesting contribution to the same subject in the last No. that has reached us here (March) of the *American Journal of the Medical Sciences* by Dr. Joseph A. Blake. It is most valuable to have the exact position of the air passages in the chest determined in this way, and marks a decided advance.

I am, dear Dr. Bowditch,

Yours most truly,

W. T. GAIRDNER."

Two letters to his friend Dr. Fraser at Paisley show the same open mind to everything making for progress :—

" 225 St. Vincent Street,
Glasgow, 28th December, 1899.

Dear Fraser,

It strikes me that I have never seen what came of the Paisley meeting—*re* tuberculosis. I think I glanced at the newspapers but being on my back at the time, I may have missed it. I am now quite well again, but we had some heart searchings last night on parting with Frank, who after returning from Leeds brimful of surgery—to take an office in the Sick Children's Hospital here—has got himself engaged as a civil surgeon for South Africa, and leaves I believe on Friday (to-morrow) by steamer from Southampton.

Yours very truly,
W. T. GAIRDNER."

" 225 St. Vincent Street, Glasgow, Jan. 22, 1900.

My dear Fraser,

Somebody has been kind enough to send me a cutting from the *Paisley and Renfrewshire Gazette* 30. 12. 99 and the inscription is not in your hand, so I verily believe that your usual modesty has led to your hiding your light under a bushel, so far as I was concerned, up to date. In fact, I rather think the said extract came in a note from my clergyman Dr. Strong, about something else ; and has remained with me for a week or two unread. But it is all very interesting, and very well put.

It is curious that in Germany the pathology which got its main impulse from Virchow in the ante-bacterial epoch, had in the hands of Niemeyer taken a shape that went right athwart the open air impulse, by attributing all tuberculous lesions to the caseation of inflammatory products, and insisting on the inflammatory initiative in the lungs being restrained by coddling. I don't say this was Virchow's

own view but it certainly was Niemeyer's, and his book always seemed to me a marvel of perverted ingenuity.

With kind regards, believe me,

Yours ever,

W. T. GAIRDNER."

Two years after his retirement, we find him addressing Dr. Middleton upon certain important questions connected with tuberculosis :—

" 32, George Square, Edinburgh, March 6th, 02.

My dear Middleton,

The pathological data I had as to tubercular disease long ago were never published in great detail, but at a discussion in one of the Glasgow Societies some years ago they were stated from MS. notes which I cannot lay hands on now ; but I think the main points were somewhat like what follows :—

1. In a series of bodies examined at a time when there was no epidemic or other cause of exceptional mortality—*i.e.* ordinary or average and mostly chronic cases in Edinburgh R.I., from $\frac{1}{2}$—$\frac{2}{3}$ of the whole series were considered to have in one shape or another tubercle—or the remains of tubercle in one or other organ—(all organs in chest and abdomen, at least, and often also the nervous system, having been carefully and critically examined).

2. The difference between these fractions ($\frac{1}{2}$—$\frac{2}{3}$) covers a series of doubtful cases, amounting to perhaps 10 % in which the remains were more or less obsolescent, but which I personally regarded as mostly of healed or healing tubercle.

Keeping this not quite defined minority out of the reckoning, I counted that at least $\frac{1}{3}$ of the cases was of progressive tubercle—mostly phthisis—and perhaps another retrograde or not very progressive tubercular disease, of such characteristics that the subjects might be said to have died with, but not necessarily of, tubercular lesions.

3. Healing processes were, therefore, found in a large, but still not quite definite proportion ; and absolutely healed tubercle in a not inconsiderable proportion, but one

still less exactly stateable ; because almost all of the doubtful cases, of course, were found in this class. You will not fail to remark, or to find for yourself if you look it up, that all this was faithfully brought out, though not exactly on numerical lines, in the ' Auscultation Médiate ' of Laennec and afterwards insisted on by Roget and Boudet (I think) and by Hughes Bennett ; so that I have never had any doubt as to the curability of phthisis in a proportion of cases ; and I always held, and also taught, that the cases that got well were not those that were coddled, but that presented a good stiff back, as it were, to the disease.

4. But still, it remained, and I am afraid still remains, true, that a considerable proportion is difficult of cure, disappointing from the liability to relapse carried over many years and often into even old age ; so that broad or too sanguine estimates of ' cures,' numerical or otherwise, require to be very carefully scrutinised and often largely discounted.

5. The older physicians—*e.g.* Morton in his ' Phthisiologia,' were fully aware of this ; and they did not doubt the curability of phthisis, though they drew the distinction between a Phthisis (simply) and a Phthisis confirmata or inveterata. It was the pathologists or pathological anatomists who—neglecting Laennec's careful induction and bringing to the front only the ruins as observed in dead bodies—gave the first impulse to the popular notion of incurability ; and then, of course, the stethoscope with its ' Pectoriloquy ' and other cavernous signs, was wrongly made responsible for ' signing the death warrant ' for so many unhappy patients.

6. All this, observe, is doctrine which has been brought together and taught no doubt by many, quite apart from any special mode of treatment, only, I have never had any doubt that the introduction of cod oil in my own day, with a more generally hygienic treatment, as opposed to the old and too often reducing methods, has brought about a vast improvement, which is now, probably being further developed by the ' open air ' treatment. But, as regards

the latter, we are still in the tentative (though I think very hopeful) stage of inquiry, and must necessarily be so for some years yet.

If you can make any use of the above, you are very welcome, though it is written *currente calamo*. Meanwhile, I am glad to note what you say about your Uncle, and also about Eric.

I am,
Yours very truly,
W. T. GAIRDNER."

Two years before the end, he again addresses Sir Thomas Oliver, this time upon the interesting case of the Rev. Dr. Chalmers, which was fully discussed in his classical article on Angina Pectoris :—

" 32, George Square, Edinburgh, Jan. 28, 1905.

Dear Dr. Oliver,

I have treated of the case of Dr. Chalmers with ample detail, and after full investigation of all the accessible facts in my article on ' Angina Pectoris and Sudden Death,' in ' Reynolds' System of Medicine ' to which you may be referred for much more on the same subject. The case of Chalmers is a most interesting one, but I do not think there is any evidence of his having had ' A.P.' either at the time of his death or previously.

Yours faithfully,
W. T. GAIRDNER.

P.S.—I am holding on pretty well as to my own health so far."

The width of information to which reference has already been made, is very clearly brought out in a letter from Gairdner to Dr. W. G. Dun of Glasgow. Dr. Dun has kindly given me the facts which led up to the writing of the letter :—

" 15 Royal Crescent, Glasgow, W., 2nd July, 1909.

Dear Dr. Gibson,

I notice you are asking for letters of the late Sir William Gairdner. I don't know whether the enclosed will

be of any interest or use. In October 1901 as President of the Glasgow Medico-Chirurgical Society I gave an address on 'Blood-letting in the Treatment of Disease.' This was published in the *Glasgow Medical Journal* and read by Gairdner there. Many years before I had been struck by a story which Christison tells in his 'Life,' and in my paper ascribed it to Laennec instead of to Broussais. This led to Gairdner writing the enclosed letter, which seems to me to throw a light upon his intimate knowledge of an important period of the history and practice of medicine. Probably no one but himself noticed the mistake, and it was like him to write the kindly letter he did, for he was always ready to take the most friendly notice of the work of any of his pupils.

With kind regards, I am,
Sincerely yours,
WM. G. DUN."

"32, George Square, Edinburgh, 25th March, 1902.

My dear Dun,

I feel pretty sure that there is some mistake about the story attributed by you, on Christison's authority, to Laennec—I don't mean that the mistake is yours, but that it is somewhere.

Laennec, for all I know of him—and I do know a good deal—was about the last man in the world to sport a fanciful diagnosis like that of 'gastrite' and the 'langue pointue,' and still more to clinch a fanciful and extravagant diagnosis with 50 leeches to the stomach. He was in all respects the polar opposite, or living antithesis of the more brilliant and versatile Broussais, whom such an anecdote fits like a glove. Broussais founded his whole reputation and the theory on which he prescribed so many leeches, on 'gastro-entérite,' which he found present everywhere, and treated accordingly.

Laennec, on the contrary, was a man who did not like to go an inch beyond his facts and observations, and even in those bloodletting days he was, not certainly an opponent of bloodletting, but on the whole, a very moderate user of it.

Sir Robert Christison tells us in his autobiography that he (R. C.) was so entirely engrossed in the chemical laboratory work which he went to Paris to follow out, that he did not even enter the Hôpital Necker, or pay any attention to Laennec, though at that time all the world was flocking to him in respect of the newly invented stethoscope, and I do not think he could possibly have personally witnessed what you have written as respects Laennec, but what I think is far more likely to be true of Broussais.

Yours most truly,

W. T. GAIRDNER."

Gairdner, even in his early years, was connected with periodical medical literature, and took an important part in editorial duties, as well as in original contributions.

The *Edinburgh Medical and Surgical Journal* was founded by the younger Andrew Duncan in 1805, and was published by Archibald Constable as a quarterly magazine. For a quarter of a century it was the only medical journal published in Edinburgh ; but towards the close of the year 1840 an influential committee announced the intention of publishing a monthly journal, and in the beginning of 1841 the *Edinburgh Monthly Journal of Medical Science* made its appearance, under the editorship of John Rose Cormack. In the year 1848 Cormack retired, and the volume beginning with July of that year bore the names, as joint editors, of George Edward Day, Alexander Fleming, and William Tennant Gairdner. The three editors remained in office until 1852, when, on their resignation, John Hughes Bennett occupied the editorial chair, and presided over the destinies of the journal until, in 1855, it was amalgamated with the *Edinburgh Medical and Surgical Journal*—the two forming the *Edinburgh Medical Journal*, of which the first editor was Henry Duncan Littlejohn.

The reviews and critiques of medical and scientific work appearing in the *Monthly Journal of Medical Science* were evidently written in an eminently fair and remarkably just manner, as is abundantly testified by the following letter from Dr. Graves of Dublin :—

" Dublin, 4, Merrion Square South, 26th Feby. 49.

My dear Sir,

Allow me to thank you very gratefully for the flattering notices of my work which have appeared in your Journal.

The very judicious and able manner in which the monthly Journal is edited, makes, in my judgment, any praise in that periodical doubly valuable.

If you had the curiosity to read an article of mine on Cholera in the last *Dublin Journal*, you will find that in my Postscript I cite from your pages some useful observations.

I have the honour to remain, my dear Sir,

Faithfully yours,

ROBT. J. GRAVES."

The constant encouragement held out by Gairdner to all the younger men in the medical profession who were interesting themselves in original observations is clearly demonstrated by a letter addressed to me by Dr. Shaw Mackenzie of London, who enclosed one from Gairdner to him :—

" 50 Green Street, Park Lane,
London, W., June 3rd, 1909.

Dear Sir,

Seeing your request for any literary remains of the late Sir W. T. Gairdner, I am enclosing an original and unpublished letter I received from him some years ago. I do not know if it will be of any use to you, but to me it was evidence of his kindly and generous nature and his unwillingness to wittingly throw any slight on any one.

Some years ago I was much interested in the subject of infectious disease, as a factor in the chronic abdominal conditions in women, and I read or published numerous papers on the subject. In the *Medical Press and Circular* of June 13th 1900, some of my work was kindly referred to in an Editorial Article. On June 27th 1900 in the same Journal a letter from Sir W. Gairdner was published on 'Verification of References' in which he suggested that references to work commented upon should be given, for easy reference, and with regard to myself, occurred the statement 'especially Dr. J. A. Shaw-Mackenzie, who is for me by no means a negligible, but a quite intangible quantity, as I never heard of him before, and I don't know where to find him.' I think, not unnaturally, I felt somewhat concerned, and wrote to Sir W. Gairdner with some reprints of my papers.

You will quite understand, I feel sure, I have no desire for publicity. His kindly letter and encouragement are all sufficient, and what could be more touching than his reference to his failing sight, and his evident longing for the 'quiet and calm of my native city—Edinburgh'?

I am,

Yours very truly,

J. A. SHAW-MACKENZIE.

G. A. Gibson, Esq., M.D."

" Dr. Shaw-Mackenzie.

Glasgow University, July 23, 1900.

Dear Sir,

It would be very ungrateful of me not to admit, as I do, that I have been hitherto at fault in not having made acquaintance with your numerous publications; but you will probably accord me some indulgence when I tell you that the failure of my eyesight has been a serious bar to my keeping up due acquaintance with contemporary

literature, and is even now the main cause of my resignation of my Chair, and of all official work.

Were it not for this, I have no doubt I should have been attracted by your views on Colles' law, and on the transmission of hereditary disease, as I have been in the habit of lecturing on these subjects, if not every winter, at least once in every two winter Sessions.

I have no claim, indeed, to any special experience in these points, but as questions of theory and bearing on general practice, I am deeply interested in them, and shall give to your views all the attention that I can, as soon as I am fairly settled down as a professor *defunctus officio*, in the quiet and calm of my native city—Edinburgh.

I am,

Yours very truly,

W. T. GAIRDNER."

Some attempt must be made to express an estimate of Gairdner as a practical physician. He invariably brought to the consideration of every patient the clinical insight and the therapeutical resource of the highest type of physician. Great as were his acquisitions, he was not one of those whose stores of acquired knowledge have been allowed to smother their gifts of native wisdom. As in every matter which occupied his attention, he was thorough,—the patient whose case was under consideration was subjected to the closest, yet gentlest, examination, and Gairdner was never satisfied until he had reached finality in his investigations. In his later years, his vast experience of disease, both from the pathological and the therapeutical point of view, gave his opinion an overwhelming weight.

As a consultant, he was supreme, in virtue of those endowments and experience. His unwearied patience and his unbounded skill were only equalled by his kindly interest in the patient and his courteous urbanity to the medical

attendant. He was willing to spend as much time as he had at his disposal in the discussion of every problem connected with the case, even when he had fallen into the hands of some interminable bore ; and, although it would scarcely be true to say that in such a connexion he " suffered fools gladly," it may be remarked that he endured them with resignation.

His interest in the patient did not cease with the termination of a consultation, but he, in the true spirit of the scientific physician, followed up further developments. It was indeed a most distinctive feature in his character as a medical man that he kept in touch with his patients, through their doctors, even years after they had been seen. Numerous letters which have been placed in my hands exhibit this feature in a remarkable manner ; amongst such a large amount of material to select from, it is rather difficult to choose ; but the letters which follow furnish a good example of my meaning :—

" 9 The College, Glasgow, May 20, 95.

Dear Dr. Fraser,

I thoroughly enjoyed my Sunday afternoon, and thank you very much for giving me the opportunity.

I am getting Dr. Love to make a thorough investigation of the electrical reactions in Mrs. A.'s case and will report the results. Meanwhile, my notes of it otherwise are pretty complete, only there will need to be also a special investigation of the field of vision and the colour vision when we can get time and opportunity.

I am quite satisfied that on the affected side the muscle and tendon responses generally are increased, and ankle clonus is very marked, while the plantar reflex is quite absent. On the not affected side, the plantar reflex is moderate, the patellar jerk also moderate, and there is no ankle clonus. I do not think there is much, if any, true

motor paralysis, apart from what may be a part of the anaesthesia.

Yours very truly,

W. T. GAIRDNER."

" The Bath Club, 34 Dover Street, W., May 31, 1895.

My dear Fraser,

I am not sure that I clearly apprehend M. Janet's doctrine about hysteria, though dimly I seem to catch up a little of it in your extracts.

If there is anything to be made of hypnotism in Mrs. A.'s case, the man for it would be Dr. MacLennan, who has done a good deal in that way otherwise, with professional antecedents and sympathies. I will send your letter over to him, and ask him to consider it against my return. Like you, I always feel in reference to hypnotism, that it is in the man not a proper medical attitude to be throwing your patient by an exercise of volition on both sides, into a state of disease.

The G.M.C. to-day will be occupied entirely with some of these most painful penal cases. I do not think we shall get away till the middle of next week.

Yours ever,

W. T. GAIRDNER."

This other letter not merely bears out the same statement, but it further shows that, as regards professional relations, Gairdner took a high stand :—

" 225 St. Vincent Street, Glasgow, Nov. 25, 1874.

My dear Fraser,

In respect of the Rev. Dr. J. C. L., I should be only too happy to think my visits gave any comfort, and so far from expecting fees, I should be glad to consider my visits as paid *en passant* at your request.

Pray arrange this as you please, and be assured the result will be amply satisfactory to me in any case. You have not told me anything for some time about the S.'s child. I had a present of game from Invergarry the other day, and

in replying touched briefly upon the subject. I should not wonder if Mrs. E. gives you a call in passing south. If so you will find in her a very fine mind, disciplined by much experience.

Yours,

W. T. GAIRDNER."

POLITICAL AND ECCLESIASTICAL

Educated in a Liberal atmosphere, Gairdner not only warmly sympathised with, but even took an active part in, all political reforms. The abolition of abuses in the army, and the disestablishment of the Church of Ireland had his strong support. In connection with the proposals for terminating the connection between Church and State in Ireland, some letters to his brother are of great interest :—

"Sunday Evening, 20th Sept. 1868.

My dear James,

When you talk of a Church deriving 'life' from the State, as a limb from its connection with the body, I do not doubt you have an idea in view which appears to you to justify the figure. But to my view, the figure is at the least as outrageous as the *Spectator's* one about all the same people putting up precisely the same prayers. Was it the Emperor Constantine that gave 'life' to the Christian church, or was it the Christian church that infused a new life into the Roman Empire? or suppose that St. Paul had gained the ear of a few very powerful persons in the Praetorium, and so secured an establishment for the 'saints' in Rome or Philippi in the time of Nero, would those churches have taken on a 'new life' as national churches, and thus ceased to be 'sects'?

I am afraid there is a great deal of what I must view as special pleading both in the *Spectator's* arguments and in your answers. But in nothing are they less satisfactory to me than in the references to the Copernican and Ptolemaic

astronomy. In this case, I admit that I see a certain degree of force and truth in the Archbishop's illustration; for, if a fund were left for the purpose of teaching a science, whether astronomy or theology, I should certainly hold that to restrict that fund to the teaching of falsehood was 'contra bonos mores,' and incompatible with sound administration. I have no doubt, therefore, about our right to alienate (so far as it is alienation) funds left by Catholics to the nation for purposes which the nation, as a whole, disapproves. But this is a purely legal view of the case, which almost no one disputes. The question before us now is not a question of science, or of truth and falsehood. It is not even a question of the legal right of the nation (meaning by nation what *you* mean by it) to do what it pleases with the endowments of what is called the Irish Church. No one, so far as I know, disputes that right, in a legal sense. What I hold, and what I assume is generally held by those who are for disestablishment, is that high motives of policy and still higher motives of religion, demand of us the sacrifice of any legal advantages assumed on behalf of Protestantism in Ireland.

But after all, no such comparison will meet the case. Religious controversies are not like scientific controversies. The question of true or false in religion must be left open because the mind and heart of man refuse to submit to dictation on matters involving conscience. The *Spectator* has in my opinion rightly stated the case of the Irish Church as a matter of commonsense, in representing it as one where 'the minority are enabled by an alliance with an external body of an opposite opinion,' to do violence to the conscience of the majority.

W. T. G."

"Glasgow, September 24, 1868.

Dear James,

I should never have suspected you of such an opinion as I ascribed to you; but look at your own letter to the *Globe*, and especially at the simile of the dead branch (or whatever it was—for I left the paper in Edinburgh)

and see if it is not there full blown. The *Spectator's* argument was that the spiritual life of the Irish church did not, or at least should not, be dependent on establishment, and your argument was that Dr. Trench possibly did entertain the view that a Church cut off from the State might be compared to a limb cut off from the trunk of the body (I remember it now) though having the same muscles, bones, blood, it was not the same limb. In fact, I believe that both your arguments and that of the *Spectator* were alike illogical, and that neither of you said that of which your calm judgment approved. ' Tantaene animis '—you philosophers and historians are just as bad as other folks when you get into a tantrum of disputation.

I agree with you that the State derives much of its life from the Church, or rather, I would say from the widespread feeling of religious obligation of which the Church, or churches, are the most distinct expression. But I certainly do hold, in a sense, the converse opinion to yours, and I would say, not exactly in the words you ascribe to me, but nearly so—' that religion is chiefly (not ' merely ') an individual and personal matter.' That, I feel pretty sure, is the view of the New Testament, although there also the principle of association is of course fully admitted, and even allegorized (in the Epistle to the Ephesians) into something that has in it the germ of, not a national, but a universal Church. For nothing is more plain to me than that your grand historical idol of a state controlled, and therefore national, church, is utterly foreign to early Christianity. It grew out of the loins of the Roman empire, not on the soil of Galilee. It was the direct product of Greek civilization, which made the citizen, body and soul, the creature of the State, as we see in Plato, and Christianity raised up against it the conception of an individual conscience and a free soul—responsible only to God, and constrained only by the love of Christ.

Now this great idea is to be worked out of a church, not national, but Catholic, not controlled by, but controlling, states ; not taking its stand upon dogma, but on the love

of Christ ; not adverse to freedom, but on the contrary owing its life to the freedom of the individual soul ; is the problem of the future, too long for the end of a letter, or for a letter at all.

W. T. G."

" September 30, 1868.

Dear James,

What we have owed as a nation, and still owe, to the breadth and tolerance of the Church of England is, to me, compensation, and more, for all its faults ; but yet it is impossible not to see that this very tolerance is largely weighted with the alloy of earth—it is not always or chiefly the tolerance of large charity and earnest faith, but the lordly indifference of power and aesthetic culture—a feeling (largely diffused among the lay members at least) like that of Tennyson's grand personification in the ' Palace of Art ' —' I sit as God, holding no form of creed, but contemplating all.'

I take this church as I find it, admiring what is good, passing over what is bad ; and were I in England, still more were I an Englishman born, I should probably adhere to it, and do battle for it, up to the point of a reasonable and strong, but not a blindfold or superstitious attachment. But don't ask me to submit to what you seem to hold as the principle of your national church—viz. that this very birth of time which we call ' English church '—*i.e.* the practical arrangement which was found convenient in the 16th century as a compromise between Catholics and Puritans, and was accepted as such by the English people, is to have a permanent and indefeasible birthright of dominion, whenever half a dozen Englishmen, or men representing English feeling, are met together in a parish. That was Queen Elizabeth's theory for Ireland ; and it has led to the strange fact that the country which Henry II. was commissioned to bring over to the Pope, has now become the only thoroughly ultra-montane Catholic country

in western Europe. That, too, was Laud's theory of a national church for Scotland; but we all know what happened to Scotland, and what happened to Laud, thereby.

Your argument as to the early Church is exactly mine—indeed I wrote it to you at length some months ago. The Christianity both of Jesus, and of his apostles Peter and Paul at least, not only practised as an expediency, but enjoined as a high spiritual duty—submission to the civil power. It assumed that the existing framework of society was there because in some sense or other God meant it to be there. 'The powers that be are (therefore) ordained of God.' But while it was freely and fully admitted that God had committed authority (ἐξουσία) to the magistrate, as the minister of the wrath, *i.e.* God's wrath upon the wicked; and while the civil magistrate was even bound to do that which the saints were emphatically ordered not to do—viz. to do vengeance on wrong doers, so to anticipate God's vengeance; it was on the other hand maintained absolutely and without the slightest qualification that the saints were to judge the world; *i.e.* that in matters involving conscience, they were to have κρίσις, freedom to think and freedom to act according to conscience. It was as plain as daylight that this idea must lead to persecution, and the early Christians therefore made up their minds to suffer persecution; but the idea of submitting the κρίσις or power of judgment (of right and wrong) to any authority except that of the one Master, was the thought of a later age, and it was that later and worse age that produced the phenomenon of an established church out of the decay and corruption of the Roman empire.

W. T. G."

He always had some distrust of Lord Beaconsfield, yet he has frequently told me how much he admired the policy of that statesman which preserved for us the command of the route to India. A letter bearing upon some points

connected with the administration of Lord Beaconsfield comes in here :—

"225, St. Vincent Street, Glasgow, Oct. 18, 1878.

Dear James,

Many thanks for sending me the *Standard* for my edification, but mere party writing of that kind makes little or no impression upon me, whether *pro* or *con* the Government. In fact I have regularly on my table the *Scotsman* (violently *con*) and the *Glasgow News* (violently *pro*) both well written, but whenever I find either of them ranting in the style of your friend in the *Standard*, I simply do as Dante says :—' non ragionar di lor, ma guarda, e passa.'

All that sort of rot is only so much bunkum, which these anonymous fellows are obliged to do to save their salaries ; not matter for you and me either to read or to quarrel over. What we want just now in reference to this deplorable Afghan business is to have the convictions of responsible and well informed men, like Lord Lawrence and Sir Bartle Frere—of these I will read any amount you like ; in case you don't read the *Times*, let me commend you to yesterday's, which contains Sir B. F.'s long letter of four years ago. That kind of writing really appeals to my convictions, and I set it thoughtfully against Lord L.'s equally strong views on the other side with the remark that here our able editors, and even political big wigs and Cabinet ministers, are the ' fools who rush in.'

Especially foolish does this seem to me nowadays in the light of Sir Bartle Frere's letter. A few months ago a certain distinguished statesman about whom you and I have had some differences, executed what was immediately lauded to the skies by the whole of your party hacks as the grandest masterstroke of war policy of the present generation.

At the cost of about £100 a man—at the cost, too, of a fatal violation of all the best traditions of our Parliamentary Government—this person was supposed to have discovered a new and inexhaustible source of power in European wars, especially with Russia, by bringing Sepoys to fight our battles in the Mediterranean. Lord B. might have said

with Coriolanus, 'Alone I did it,' and got honour and credit accordingly. But instead of 'fluttering your Volsces in Corioli,' it appears that, but for 'Peace with honour,' we should now have been in this very ugly position, of having weakened our Indian reserves at the very moment when Russia was able, by a simple diplomatic manœuvre, without moving a man or spending a rouble upon military preparations, to flutter our Volsces from the Himalaya to Cape Comorin, and to compel us to think of sending every European regiment to India that we can possibly spare, besides calling up all the reserves there, and sending them to the line of the Indies! There's statecraft for you! There's your fine sensational Premier, with his gasconading speeches, and his secret conventions, and his beautiful nest egg of Cyprus, as a place to bury a few of our troops in, and Asia Minor to absorb our treasure, and his pet Viceroy, Lord Lytton, to get up this lovely little scare among the blacks just when Consols are at 94, and iron is being made at a net loss of 2s. 6d. on every ton, and every industry in the country is paralysed as it has not been in my time. This is he who boasted (be it remembered), when most of us thought there was no need of such tall talk, that England did not need to count the cost of a campaign, or whether she could bear a second or a third! and who made the Queen an Empress, to inspire a wholesome dread at St. Petersburg, and paralyse Russian aggression in Khiva and Khokand! Your able editors may be set on to work up the war-fever as they please by making out Russia on every occasion a mere monster of iniquity and treachery. I prefer Sir Bartle Frere's account of her as that of a large minded christian man of world-wide experience:—that she is just doing what we did and are still doing, in India, Transvaal and elsewhere, following out a law of her being, and one which almost inevitably besets every powerful and more civilised state in contact with less organised or semi-barbarous communities. Had we not been under the influence of our insular pride and of a frenzy studiously got up by your Jingo editors, and city Jews, and others who

had interests at stake in upholding that rotten concern in Europe that we have been making as far as possible our own for the last year or two, we might have kept our breath to cool our porridge in Hindustan, or at all events been in a position to take a calm view of their Ali and his relations to us, in respect of our Indian responsibilities. But the moment we moved our fleet into the sea of Marmora and called up our reserves (including the Sepoys) and made extensive preparations for a war in Europe in which we should not have had an ally, it became obvious to Russia that a check in Asia and in the direction of the Indian frontier was a plain and indispensable move in the great game of chess into which we had gone headlong (*duce* Lord B.) and (as his Mansion House speech shewed) without either counting, or caring to count, the cost thereof. I must say that he has exactly fulfilled my idea of him from the first, and will, I believe, before he is done, prove the costliest minister we have had in England for some generations, to say nothing of his unsettling all the traditions of our representative government, and keeping the whole country (nay the whole world) in a state of perpetual distrust and alarm that paralyses industry, and has nearly ruined all the arts of peace.

But I must stop, and as I have written at such length, just to shew you that I am still alive and as much your brother as ever, I will send this viâ brother Jack who is coming to us next week, so that he may be prepared, if necessary, to have it out vivâ voce!

We are all well, on the whole, barring colds. I hear there is whooping cough at Mt. Vernon so with all the strain of this horrid bank business,[1] I am rather avoiding Charles.

With kind love to all and yourself,

Your brother,

W. T. G."

Twitted by his brother with lack of consistency as regards his views at the time of the Crimean War of 1854, and the

[1] Failure of The City of Glasgow Bank.

possible outbreak of hostilities between this country and Russia in 1878, he writes as follows :—

" 225, St. Vincent Street, Glasgow, Oct. 22, 1878.

My dear James,

I don't think I am bound any more than others to vindicate my active consistency as between 1854 and 1878 ; but I don't remember, as matter of fact, being at all in the extreme bellicose party in 1854 ; and certainly according to my recollection there was nothing like what I regard as the war-fanaticism of the Jingoes. Even among the most bellicose the objection taken to Lord Aberdeen's cabinet was, that with an amiable peace-loving man at the head of it, we were none the less ' drifting into war.' The *casus belli* may or may not have been a good one—Kinglake has opened the eyes of some who thought in 1854 that we were wholly in the right—but there was at least no doubt of the crossing of the Pruth and the massacre of Sinope being very gross and unprovoked outrages upon the law of nations—for which the destruction of Sebastopol was a just retribution. In fighting with and for Turkey in the Crimea, we were not visibly and in the eyes of the world—as we should have been had we fought in 1877-78—aiding her to defy our own pleadings and even requirements on behalf of her oppressed races—we were not consciously abetting the hideous facts of 1876—which once for all, and as I think, for ever made it happily impossible for us to think any more of defending the ' integrity and independence of the Ottoman Empire.'

These are only a few hints—I cannot of course, any more than you, re-open the whole question. But although I have no great faith in the ' popularis aura '—especially in foreign affairs—yet without defending it throughout either in 1854-6 or 1876-8—I am inclined to think that both in challenging Lord Aberdeen's policy of ' drifting '—and Lord B.'s of bullying and rushing into war, public opinion was sound at the core :—nay more, I am almost confident that this last war between Russia and Turkey with all its horrors, would have been avoided had Lord B. not been in London

and Sir H. Layard at Constantinople prompting Turkey to her doom against the public opinion of this country and of Europe. But as you say, it is not likely that we shall agree upon these affairs for some time yet.

We expect John and N.[1] to-morrow for a few days, after which they go down to Ayr. All well.

Yours affectionately,

W. T. G."

Like many other generous-minded men, he thought that Mr. Gladstone's capitulation to the Boers, after the disaster at Majuba Hill, was based upon magnanimity, and it was a painful revelation to him when the later facts transpired, showing that something quite other than generosity constrained him to make terms with the Transvaal. He was profoundly shocked by the attitude of Mr. Gladstone on the death of Gordon, and Mr. Gladstone's sudden conversion to Home Rule, when in want of such support as might keep him in office, not only startled him, but led him at once to sever his connection with the Separatist Party. Yet, in spite of this, as will be seen by a letter to Dr. Dobie of Chester, written immediately after Mr. Gladstone's death, there was not a note of bitterness :—

" 9 The College, Glasgow, May 19, 98.

My dear Dobie,

I specially refrained from writing when I knew you were burdened with anxieties, having no right to be more curious than other people in their millions. But now that it is all over, I cannot refrain from again touching the note of sympathy, which is none the less sincere that it comes from one who was for many years a ' Gladstonian '— but who was in a single day converted into a ' Unionist ' by that one speech of Mr. G.'s in 1886.

But this is not the time for political discussions, and I am very far astray indeed if there will not be but one note

[1] Mrs. John Gairdner.

of respectful and affectionate regard now for the greatest, and in many respects the noblest, of our public men within my recollection.

The feeling will be accentuated by the knowledge we have, that in all his past political differences, there was rarely any note of bitterness in Mr. Gladstone's own estimate, or in his conduct towards opponents.

Pray do not answer this. It is only the overflow of what needs to be uttered, once for all.

Yours very truly,

W. T. GAIRDNER."

During his later years, we had many conversations on political matters. He expressed himself heartily in favour of Mr. Chamberlain's idea of Old Age Pensions—a proposal carried into effect by Mr. Asquith's Government. Like many other thoughtful men, he would have liked, however, to make the receipt of an Old Age Pension dependent upon each man having performed what he deemed to be clearly his duty to the State by undergoing thorough training for national defence. He had no fear of the extension of the Franchise, provided that it also was only conferred upon those who carried out the primary duties of citizenship; and he had a singularly open mind as regards widening the basis of taxation, which he considered to be at present founded upon conditions which have passed away.

THEOLOGY AND RELIGION

The position of Gairdner in respect of theological opinion and religious conduct, as well as ecclesiastical policy and ritualistic practice, can only be grasped in the light of the relations held by his family in regard to such matters. Mr. James Gairdner has again come to my help in respect of these subjects. In continuation of some information which he kindly supplied on family matters—information which has

been incorporated in a previous section—he furnished me with the following remarks :—

" Perhaps I may say just one word on a subject where differences of opinion have led to serious contentions in other circles, even in our own day. As to religion, we were all brought up Unitarians. Our father and mother, and virtually all our relations, were of that way of thinking ; and our parents belonged to the one Unitarian congregation in Edinburgh, which during my boyhood built a chapel for itself on Castle Terrace—for a long time the only building there. The reign of Calvinism was strong in those days, and Unitarianism was certainly unpopular ; but I am certain that no one respected my father one whit the less for belonging to an unpopular religion. On the other hand, I could not help remarking that years and experience, as we all advanced in life, made a remarkable difference on this point in the attitude of one and all of us, and not least in that of my dear old father. His Unitarianism was that of Priestley, Belsham and Channing. But a more advanced School rose up, with which my brother William sympathised more than he did—a School whose views seemed destructive of the idea of a divine revelation altogether. A non-miraculous Christianity seemed to my father—as I confess it always did to me—to be absolutely unintelligible ; and ultimately my father seceded from the Unitarian congregation of St. Mark's and joined that of Old Grey Friars' Church, of which Dr. Robert Lee was minister. He felt really that there was more breadth and freedom in an Established Church than in a sect—and so, many years later, did I in London. I may add that in my own case my views have developed long since into complete Orthodoxy. I will not say that this was the case either with my father or my brother William before their deaths. Yet I almost think that there was some approach to it in each case, from opposite points of view. My brother John, I believe, was really orthodox many years before I was ; but he was not so, I think, till after he was married."

Mr. James Gairdner and his brother William frequently

wrote to each other with absolute freedom on matters of Church and State, Theology and Politics, Creed and Conduct. The subjoined letter,[1] written more than fifty years ago, shows how deeply interested both brothers were in subjects of this kind :—

" Rolls House, Chancery Lane,
London, W.C., 18 July, 1861.

My dear W.,

You seem to me like the worthy who was said to have ' renounced the errors of the Church of Rome for those of the Church of England.' Having long been familiar with Scots bigotry and come to like it no better than at first, you take kindly to the English article as a more respectable sort of prejudice. Be it so ; but let me disabuse you at all events of the notion that the English hate Popery as a foe to civil freedom. Uncle W. does, because he is a Scotsman born. The leopard cannot change his skin, however it please him to howl with the lions. That notion savours

[1] Mr. James Gairdner informs me that the subject of the correspondence was the result of a recent lawsuit. The circumstances were as follows :— Lord Romilly, the Master of the Rolls, had appointed the late Mr. W. B. Turnbull to edit a "Calendar" for the Record office. Turnbull was a Scotsman, and a convert to Rome ; the latter fact, with perhaps some other points about him, made him unpopular in his own country. *The Athenaeum* was ungenerous enough to stigmatise the appointment from a theological point of view, as the period with which Turnbull's "Calendar" had to deal was that of the Reformation. The "Protestant Alliance" followed this up with an insinuation that a zealous Papist was capable of putting historical documents which told against his religion into the fire. On this, Turnbull took an action for libel against the Secretary of the "Alliance," and lost it by the verdict of a very Protestant jury who acquitted the defendant ! The verdict was criticised at the time by the press ; the *Times* at least suggesting that it was most unfair. But opinion was divided on the subject ; and some who thought that the appointment was a bad one, or that there was some little weakness in Turnbull himself (as perhaps there was, in prosecuting for an insinuation that reflected more disgrace on the libeller than on him), were not disposed to quarrel much with the verdict. Indeed, it must be said that Turnbull had committed another mistake. He resigned his appointment before he brought his action, from undue sensitiveness, which really weakened his case.

far too much of your Scots consistency and idea of always acting on principle. No, no. The English don't hate popery, but papists; just on the old principle—or rather with the same sort of animus, as the old country squires hate a Frenchman, a 'furriner,' or a Scotsman. I don't know what may be your authority for saying that the Papists themselves brag that they would burn or suppress things; but it seems to me simply a thing that no honest man would do, and how you can approve the verdict without caring to go into the question of Turnbull's character I don't understand. To me it seems that it was entirely a question of personal character,—that the Protestant Alliance ought to have believed in the trustworthiness of a man appointed by the Master of the Rolls unless they could show good cause to the contrary, and if they could not do this, they had no business even to hint their suspicions in public. Say what you will, it is one law of libel for Catholic and another law of libel for Protestant—a monstrous thing for Newman to libel Achilli, even though he convinces everybody that he had said nothing but the truth, but a perfectly justifiable thing for Bird to libel Turnbull though he does not even attempt to support the charge. That is a British Constitution you may be proud of if you like, but depend upon it, it won't tend to increase our reputation in the eyes of foreigners.

I will get you the *Examiner* and send it. I can't tell you about the article on Ragged Schools till I see Morley [1]; but there is one article of mine in it on a subject you care less about—England in the Middle Ages.

Yours ever, JAMES GAIRDNER."

How independent Gairdner's theological views were, may be seen from a letter about ten years afterwards to his brother:—

"Edinburgh, Jan. 12, 1873.

My dear James,

It seems to me as though our differences had almost reached the vanishing point, or rather that point

[1] Henry Morley, Editor of *The Examiner*.

(if so it may be called) which used to be represented in our mathematical days by the formula $\frac{dx}{dy}$, wherein both the numerator and the denominator are infinitesimal, and the quotient anything from 0 to infinity. In other words, by advancing along different lines of thought, not inconsistent with each other, but often divergent, we have got to a point where the real tangible differences are small or even 0, but where the impress of the method still clings to the result; *servabit odorem testa diu*. I take your illustration; I can equally well make it serve my argument. I don't think you do, in fact, produce any interference with the law of gravitation when you sustain a body against the earth's attraction by your will through the muscular force; *i.e.* if by interference you mean anything even remotely analogous to the so-called miraculous or specially providential interferences with the established order. What you really do is to control the movements of a particular body or mass of matter in accordance partly with the law of gravitation and partly with the forces emanating from you as a living will, through laws just as well-established, and in one sense just as fixed and clearly statable, as the law of gravitation itself. The law of gravitation is not in the very least interfered with; it acts on the body, when sustained by you, just as much as when you withdraw your hand and let it fall to the ground. No doubt the spiritual acts on the physical in this as in every instance where a living will originates force; and no doubt the process of the action is in its ultimate recesses a mystery, a miracle if you will, as profound as any recorded supernatural interference with the laws of nature. But is not the law of gravitation itself just such another mystery, if you go back to its ultimate metaphysical expression? Argue on in this line, and you arrive at the paradox which I remember meeting with once in one of Canon Kingsley's gossiping sermons, and which struck me at the time as being as near as possible to the line of quasi-scientific nonsense, a deliberate talking-down to the intellects of his rural parishioners, in order to maintain

a quasi-orthodoxy of tone. 'Why should the dust of the ground not have been made into lice, why should the water of the Nile not have been turned into blood? the birth of one louse from the egg; the creation of one blood corpuscle from the various elements of which it is composed, the mystery of birth, of generation, of development are perpetual miracles going on under our eyes from day to day, and I don't see (quoth the Canon) anything more wonderful, or more difficult to believe in a literal sense, in all that is recorded about the plagues of Egypt, than in the case of those everyday matters.' I give you the sense, of course, not the words; but it is very near the sense. At this rate, why not believe equally in table-turning, in Mr. Home's 'spiritual ascents and descents,' in men who paint pictures worthy of Raphael or of Claude unconsciously and without previous knowledge of art while in a state of catalepsy? Why not believe in poor Dr. Gregory's snail-telegraph, where a series of snails duly mesmerised on this side of the Atlantic, indicated the letters of the alphabet to a series of snails in a state of rapport in Boston or New York? He uses the very same line of argument as you do, viz. that we do not know the whole of the possible connections of spirit and matter, and where we do not know, it is at least possible to suppose any thing!!!

I have admitted, and indeed, maintained with all the strength of my convictions, that wherever we do not know the whole purpose of God, as expressed in his laws, prayer is in a general sense not only a reasonable service but a duty; and we may (nay we must) pray for what, as it seems to us, we most need. But I do not admit that all prayer of this kind is equally reasonable, or even that prayers sincerely, and therefore legitimately offered in the wide field of the unknown, may not be wholly unreasonable, when they come to be read in the light of scientific truth, which is just as much an exposition of God's will as prayer is.

I think that most of the formal prayers (I do not say your prayers, or indeed any of those informal prayer-wishes you alluded to in a former letter) for rain and fine weather

are unreasonable, because they tacitly disregard or set aside the known fact that in such matters, God works by laws, and that they have really no more right to look for an interference with these laws, than the person I presupposed in my letter to Dr. Smith, who should pray that the order of time might be set aside, in order to prevent a bill from becoming due which he had not money to meet.

I trust that M. will be with you to-morrow (so I understand her plan) and I will try to enclose a line with this for her.

W. T. G."

That theological discussions were not only familiar matters to, but favourite subjects with Gairdner, can be seen in a letter to Temple, written when he was on a tour in the Netherlands, along with Frank. It deals with that problem, as eternal as insoluble—the origin of evil :—

" S.S. ' Anglia ' entering Amsterdam, 31. 3. 90.

My dear Temple,

I had really no time to answer your last letter, which reached me in the very middle of our hard work for closing the winter Session on Friday, and after getting that off my hands, I had to go unexpectedly to Edinburgh, and then had arranged with Frank to leave for Holland on Saturday evening. Nor can I now attempt to deal thoroughly with your difficult problem from Milton and St. Paul, which I accept however, as evidence that you are putting your mind into what you read both in the one and the other ; which is always a good thing in itself, and I applaud accordingly. But as regards that awfully difficult and really insoluble problem—the origin of evil—whether in Nature or in the heart of man—you may take it as one on which the profoundest thoughts of the greatest men in all ages have been exercised in vain, and will continue to be exercised in vain, to the end of the chapter. Milton himself describes it as one of the occupations of the more contemplative among the fallen angels, who ' sat on a hill retired '—pondering exactly the same vast questions as have found their way into your mind ; and that they—

with their superhuman intelligence, like you with your human—' found no end, in wandering mazes lost '—as assuredly you will also do, like ever so many before you.

I think, however, that if I had time I could at least manage to shew you that the passage you quote from Milton is on the right lines of interpretation, and I accept his general statement (as put into the language which God might be supposed to use towards us), indicating as it does (in my opinion justly) the clear inner consciousness that we all have, and cannot but have, as reasonable and moral beings, that it is, after all, by our own free choice (and therefore by our own fault, as it were) if we ' choose the evil and refuse the good '—in other words fall into sin and into the condemnation thereby alike of God and of our own hearts. ' Our own hearts condemn us ' (as St. John says) ; ' and God is greater than our hearts, and knoweth all things.' You can't get past that, anyhow ; that is the practical and clear issue on which we have to determine our life and our conduct. If God, in a certain absolute sense, foresaw this, and therefore, being all-powerful as well as all-knowing, may be said to have predetermined it, that is no concern of ours ; when we know what God knows, you may be sure we shall then see that He is, and was, right, and that we were wrong. But as regards the apparently inconsistent, and to you puzzling and even shocking (I suppose that was your feeling) way of putting it in the 9th Chap. of Romans, I would at present only remark—1. Although I am as thoroughly and as devotedly an admirer of St. Paul as any man can be, I decline to be bound exactly by his way of putting things, when it comes to a matter of logic and metaphysics, picked up as most of his reasonings were, at the feet of Gamaliel, and representing (in form at least) a kind of argumentation which we know otherwise as that of the schools of Alexandria—not one whit less addicted to quiddities and queer notions than were the scholastic philosophers of the middle ages, who were caricatured as discussing ' how many angels could dance upon the point of a needle ' ; and 2. (even more important) I think I could shew you by

taking the whole argument, and not only a bit of it, that St. Paul was not really thinking here of the relations of God to the individual man; but of his relation and his purpose towards Israel as a people on the one hand, and the whole of the Gentiles on the other. At all events, I think that, for you and me, Milton states the argument better than St. Paul in this case; and while I am thoroughly at one with St. Paul in the general drift of his ideas, I decline to be bound by snippets of his particular arguments, taken out of their connection as texts, and made to support this or that doctrine on the most inscrutable of all subjects—one on which St. Paul himself writes like a man puzzled and anxious, and putting it (as it were) all round and in every kind of way, to see if he can find out the right one. Beyond all question the Jews generally believed that God was the creator of evil as well as of good, in such a sense as almost excluded free will on the side of man. The book of Job, from beginning to end, is the attempt to resolve this riddle into a poetic form, and present it under the aspect of what we should now call a religious drama. St. Paul had to speak in a language, and to men, who were saturated with whole centuries of discussions revolving round this point, and what he had to do was to face as best he could, this problem—'Why did God allow his chosen people to reject Christ, while even the Gentiles (not chosen in any way according to the Jewish understanding) were equally allowed to accept the gospel on the teaching of Paul himself.' To this he answers in substance, that God chooses whom he will and in this instance has chosen the Gentiles, because he in his wisdom foresaw that they would come in, and that the Jews would not. Their coming in, voluntarily, is the evidence that God has chosen them to come in; and all the hide-bound prejudices of you Jews that you yourselves, and you only, are the chosen people, must in the end give way to that. But God does not mean (he goes on to say) that because he has now chosen the Gentiles to come in, he has also chosen you (Jews) to stay out. No! God means to save both Jew and Gentile through Jesus

Christ, and if you resist him, it is your own fault entirely. God's mercy is illimitable; even if he 'hath concluded[1] them all in unbelief, that he might have mercy upon all'[2] *i.e.* Jew and Gentile, in the end, alike.

That, I think, is St. Paul's real meaning, taken broadly, not only in the passage to which you refer, but in the whole scope of his argument through several chapters of the most intricate and difficult part of all his epistles; intricate and difficult, of necessity, because it deals with points that transcend human understanding, and on which we can only say at the last, what we may just as well say at the first—that God's will is certainly the best; and just because it is so, we must strive to make our will a part of His, so that we, acting along with Him, and conscious of our own freedom to choose, nevertheless choose to do what we feel and know to be His will—so that we can say from the heart, as Christ said, 'not my will, but thine be done.'

I don't know if I have made myself at all clear to you, and I should not have thought of entering on such questions at all, unless you had led the way; but I hope that these hints may by and by enable you, as I think they have enabled me, to read St. Paul with less sense of difference between his statement and that of Milton in the passage you quoted.

Your ever affectionate father,

W. T. GAIRDNER.

Finished at the Bible Hotel, Amsterdam on 2nd April, 1890."

Again, some of the profound problems connected with the origin of the Christian creed, particularly as regards the sonship of Christ, occupy another long letter to Temple—a letter which ends with a pleasant reminiscence of two of his old schoolmates:—

"Edinburgh, Nov. 3, 01.

Dear Temple,

Your mother's birthday found me still confined to my room, tho' not really ill in any way beyond a soreness

[1] *I.e.* shut them up. [2] Rom xi. 32.

in the muscles of my leg, which has required a prolongation of bed and room keeping beyond what I hoped would have been necessary otherwise ; but which the Doctors are using for all it is worth, in order to keep me within bounds ! I admit there is some reason in this ; but I am really getting better, and hope to be downstairs to-morrow, when Eric goes to Glasgow to take up his winter's work, being, I hope, quite recovered.

The little book I have sent you by Dr. Robertson, of Nottingham, touches upon a great many points, and these the most central and essential of all, as to our Lord. Before I fell in with this book, however, I had been pondering one of these points, and found that all I had been working out for myself was so strongly and clearly supported, that naturally the unpretentious little volume took a stronger hold on me from this cause. My course of thought was as follows :—Had Jesus brought us no more than the one great aspiration ' Our Father '—would it not have been a gain to humanity far exceeding that of all other teachers ? In this, as in all things else, He was ' the way, the truth, and the life '—he pointed the way to us for the first time ; he opened the truth, not by arguments for it, but by *living* it. Dr. Robertson has failed to make one remark which I think is important, and shews how completely the feeling of Sonship as regards the Father was bound up from the first with our Lord's inner consciousness. At 12 years old —long before he began to be a teacher—we find him in the most simple and natural way declaring to his parents that in the temple at Jerusalem he must needs be ' about my Father's business '—or ' in my Father's house '—and it is clear from the following clause that this expression of his was not understood by Joseph and Mary at the time (Luke ii. 50). In the 8th chap. of St. John, and again in the 10th Ch. vv. 30-38, we find that the affirmation of his Sonship, and of course conversely of the Fatherhood of God, is the ground of the chief revolt against him ; the Jewish mind conceiving it only as ' blasphemy ' that any one should claim to be—not, observe, *the* Son of God, as in A.V. and

R.V., but (as in the Gr.) *a* Son of God. And in Ch. xix. 7, the very same ground of antipathy comes out in their accusation to Pilate—' we have a law, and by our law he ought to die, because he made himself *a* (not the) Son of God.' But on our Lord's part, the consistency of this great, and heretofore, unknown attitude is maintained to the very last when on the cross he says—' Father, forgive them '—and again, ' Father, into thy hands. No one else, surely that ever lived in this world, could have taught us in like manner —When ye pray, say Our Father. And then, look at this prayer itself—the Pater Noster vulgarised, rather than sanctified by ages of routine, and myriads of unintelligent repetitions—was it, do you think, fully apprehended at the time it was given, or is it fully apprehended in all its grand simplicity, even now? The differences in St. Luke's and St. Matthew's gospel perhaps tend to shew that it did not at once become a fixed liturgical form; yet there are but few examples in the N.T. of prayers cast in this particular mould. So that when St. Paul (Rom. viii. 15) speaks about the ' spirit of adoption whereby we cry Abba, Father ' —it is still as though there were a certain conventional difficulty which had to be overcome before the unwonted and, to the Jewish mind, ' blasphemous ' idea could be entertained that ' as many as are led by the spirit of God, they are sons of God.' I have of course read very little theology of the abstract law, and don't even find that in what I do read of that kind, I get much spiritual nutriment—but I sent for two books on ' The Fatherhood of God ' —one by Dr. Crawford and the other by the much more famous Dr. Candlish—the latter, however, to my mind a very unsatisfactory book, the former much the more convincing of the two. But in neither of them do I find the matter presented exactly as I am doing it here in this brief letter to you, and therefore, I think it may be more or less useful to you to set down these thoughts just as they come. The chapter in Dr. Robertson's book (iv.) is by far the most satisfying that I have come across anywhere, on this most important matter.

The jealousy and bitter antagonism of the Jews—as noted by St. John—to the bare conception that a mortal man could under any circumstances claim to be a Son of of God, is not at all difficult to understand, in view of their history, and the fact that a 'divus imperator' was at that very time the representative of the Roman power which held them, as a nation, in subjection. Indeed, looking to the fearful times of persecution through which the Jewish nation had passed from the time of the Maccabees, and the noble way in which the national testimony was upheld to the one supreme Jehovah by the élite of the Jewish people, after their glory had passed away, and the race of the prophets had long ceased to exist, one cannot help expressing a suspicion (at least) that St. John, writing afar off in Ephesus, more than half a century after the facts recorded, may have given a colouring to his narrative that is less than just to the more ardent spirits among his countrymen. But in any case, we cannot fail to perceive what an immense arc of spiritual revolution must have been traversed before any Jew, bred in the traditions which gave force to the law, whereby a man 'ought to die, because he made himself a son of God,' could bring himself to utter the great words of 1 John iii. 1, 'Behold what manner of love the Father hath bestowed upon us, that we should be called sons of God.'

St. Paul, too, born and bred a Pharisee of the strictest sect, and no doubt fully instructed in all their traditions, and writing not much more than a quarter of a century after the crucifixion, must have felt a marvellous change in the whole spiritual atmosphere, when he could write without qualification or hesitation 'as many as are led by the spirit of God, these are sons of God. For ye (Jews and Gentiles alike) received not the spirit of bondage again with fear, but ye received the spirit of adoption, whereby we cry Abba, Father' (Rom. viii. 14, 15). And all this he owed (notwithstanding the allusion to heathen testimony, Acts xvii. 28) entirely to Jesus, 'declared to be the Son of God with power,' and calling us into his own loving fellowship with

the Father, when he instructed us—'When ye pray, say Our Father.'

I had hoped to send this away to you this afternoon; but I find I have lost the mail, owing to a visit from two of the oldest of my medical friends now extant, Sir Henry Littlejohn, and Dr. John Smith. Littlejohn was a High School boy in the same class with Wm. Maclagan (now the Archbishop of York); and John Smith was, like your father, a Hill Street Institution boy, but two years or so behind me, so that he ranged with your uncle John. You may imagine that we had a good many old stories and recollections to disinter from the pigeon holes of our cerebral gray matter!

Nothing new as regards M.D.M. God bless you and her.

Your loving Father,

W. T. GAIRDNER."

A letter to his brother James, of considerably later date, is devoted to theological matters—more particularly as regards the meaning of Presbyterianism:—

"225, St. Vincent Street, Glasgow, June 1, 89.

My dear James,

I have no fear for, and I have no feeling against, Presbyterianism, notwithstanding your doubts. As one of the many forms into which Christianity has been moulded during these almost 19 centuries, it will no doubt fulfil its appointed mission, and then give way to something else, which you and I can't well foresee, and therefore it is vain to discuss. With its 30 millions of adherents throughout the world, it is at least numerically as strong a form as any other, the Roman and perhaps the Greek church excepted. And when we consider that it has had its battle to fight against almost all the 'powers that be'—and is even now frowned upon, or contemptuously sniffed at, by all kinds of fashion hunting and lordly disposed people—our good Queen being a notable and shining exception (to the infinite disgust of your ritualists), it might almost be held that Presbyterianism is even now a stronger religious force than

most others, or perhaps any other. It seems a strong thing to say, but I have for some time held that—setting aside eventualities that lie hidden from us and are, no doubt, as much wiser than our little calculating as God is wiser than we are—Presbyterianism, or something which is to be evolved out of it—is probably the ' winning card ' as I sometimes profanely call it—among the ecclesiasticisms of the world, not one of which, as you well know, commands my sympathies except in a very moderate way. Roman Catholicism is thoroughly rotten, and is almost everywhere honeycombed with unbelief of the most ghastly kind, because it is unbelief that reaches down to the roots of the moral nature. I admit at once, and freely, the grandeur of its historic position and the magnitude of its good works, even in the present day. But the Church that within our own time has almost excommunicated Montalembert, and quite excommunicated Döllinger, in order to establish Pio IX and the Syllabus, and the Papal infallibility and the Immaculate Conception, and which even Leo XIII only hopes to reconcile with science and philosophy through renewed study of Thomas Aquinas —is already far on the road to extinction, and will some day go out in its own mephitis—all its good works and good people notwithstanding. In every R. C. nation of Western Europe it has come to this—that there is practically no *via media* between absolute submission and atheism more or less decently or indecently veiled ; and that sincere belief, and even sincere unbelief, are merged in a seething mass of sheer spiritual quackery, in which nobody trusts anybody else, and in which the very elements of society are likely to go to pieces, unless there be some strong awakening and new impulse of faith in its highest sense—by which I don't mean either papal infallibility or the works of Thomas Aquinas ! For the Anglican church, I have a very great respect, and can thankfully acknowledge the good it has done, even in my time, in broadening the basis of belief, and quickening the dying embers ; but after all is done, we find it hide-bound by traditions which will give it no rest till it is burst up from within, into a party that believes

without thinking, and one that thinks without believing—*vide* the prosecution of the Bishop of Lincoln as the last illustration. Almost all the churches, in fact, seem to be getting fatally entangled in their own machinery—and are failing to recognise the mere apparatus of clerics and creeds and formalities as being (after all) only machinery; while the rough commonsense of the world is coming more and more to perceive this, and even by such queer expedients as the Salvation Army is giving a new meaning to the text that—'the Spirit of God bloweth where it listeth.' In all this there seems to be a call to every individual man of us to give up the worship of our idols, be they inborn or afterwards assumed, to 'cease from Man whose breath is in his nostrils,' and to hold fast by the ever-living God. And I, for one, think that Presbyterianism will probably hold its own for a while yet, now that it is shaking off the ultra-Calvinism that grew up side by side with it, and in Scotland largely moulded it into a system in the face of regal tyranny and episcopal arrogance. It has facilities for expansion and comprehension that have been practically unknown; and even in the most iron-bound days it had (in theory) a free laity, and thus acknowledged that the church was made for the people and not for the clerics.

I have your note as to my Father's (mythological) gout, which only shews how difficult it is to write history—even for a recorder! But I will send it on, with this, to Macrae—perhaps they will help to amuse her!

W. T. G."

A later letter, also to his brother, is of very much interest, as dealing with Roman and Anglican beliefs:—

"17. 8. 02.

My dear James,

Enclosed you will find a letter for Sir Leslie Stephen, which you may send on, if you please, after perusal, as I think it will interest you.

I am taking the opportunities of a quiet Sunday morning to respond to your last letter, as I do not feel confidence in

myself sufficient to lead me to St. Thomas's church, where Temple is to preach, as their missionary, and where I should have liked to have been present, had it not been for a slight recurrence on Thursday morning of my symptoms of 6 weeks ago, so that I do not think it would do to risk, even remotely, another scene in a church when my son was in the pulpit. But for this sense of insecurity, however, I am really fairly well. My pulse (which as usual went down among the thirties at the time of my attack) is quite regular and normal now, and the only thing I can complain of is an indefinable something allied to giddiness at times, but never coming to anything definite since Thursday.

Now, as to your letter. I will take as a part of my argument the interesting correspondence which you will have read in the last *Spectator* or two (or three) where a R. C. priest charges the editor of the *Spectator* with he says, unfairly refusing the title of martyr to the R. C.'s who were condemned under Elizabeth, being priests, for exercising their office as such, or otherwise practising their religion in England. The *Spectator's* reply—logical enough in a way—is that these 'martyrs' were not properly such, because although they may, in fact, have suffered—and in some cases even innocently—on account of religion, their legal position was that of traitors, and, conspiring against the Crown, they were supposed to be so, even if there may have been a 'miscarriage of justice.' In other words, the Pope was himself responsible for having in their case, struck down the distinction between treason and religion, by excommunicating Elizabeth, and declaring that her death or defeat within her own realms—*pro bono ecclesiae*—was a religious and good act, and not a crime. I think the *Spectator* is logically right; and yet it is hard to deny the title of martyr to such of these men, at least, as had in their hearts the thought of their religion only, and were practically condemned on account of it, though constructively made guilty of treason. In like manner, though from the opposite side of the question, I cannot do as you often seem to do, withhold my sympathy from those who were burned

as heretics, but whom you regard chiefly as disturbers of order, at a time when heresy was universally considered to be a crime, not only against religion, but against the State as well. Why should the whole of the Lollards, for example, be treated as incendiaries and disturbers of the peace because some of them mixed up rebellion (as I suppose they did) with Lollardy? and why should the bringing of copies of Tyndale's N.T. into the Kingdom (even with the King's tacit approval) be stigmatised as heresy, and as a crime against the State, because the notoriously corrupt church of that time chose to call it so? I think you are quite right to hold the balance of history impartial as between parties and extremists, but I cannot understand your denying (or seeming to deny) credit to those who, in those distracted times, were urged by the inner spirit of truth-seeking (there was for the most part little else to urge them) into positions deemed heretical, and legally punishable by fire, if by abjuring once or ofttimes in the face of death, they shewed that they, too, were human, and therefore frail. Cranmer did so; and however much we may qualify our praise of Cranmer for this and other things in which he fell short of perfection, he was a martyr in the end, and surely the excuse which covers his faults may be extended to James Brewster and Wm. Sweeting, whom you won't allow to be martyrs, any more than any of the rest of the 39 (in p. 52 *et seq.*) who did not go to the stake, and whom you appear, in p. 55, not only to regard without sympathy, but to condemn as guilty of 'arrogance' and as 'wanton disturbers of men's minds.' It may well be that Foxe is credulous, often inaccurate, and even reckless in his special pleading; but in dealing with these cases, I cannot help feeling that he shews more of the true historic spirit than even my brother! Nor am I in the least convinced by your remarks upon Foxe in p. 54; because on the face of it, I think he is quite justified in discounting a charge of 'horrible and blasphemous lies against the majesty and truth of God,' as being (if repudiated by the accused party) simply the expression of the prejudice and fury of the accusers.

Moreover, the case of Joan Baker, the only one you give in detail as an illustration of Foxe's delinquencies, seems to me (so far as appears) absolutely deserving of Foxe's sympathies, and undeserving of your remarks against Joan in p. 57. Are we not, then, to be even allowed to sympathise with a woman who in 'persuading a friend of hers lying at the point of death' is led to strengthen her by consolation of which she was, no doubt, profoundly convinced, but which you, in writing history 3½ centuries later, pronounce upon summarily as 'ill-judged'—and therefore, presumably, only to be condemned? I admit at once that you have a perfect right, and even a duty, to explain, as you do in p. 55, how it was historically impossible to expect 19th century tolerance in those days. But why throw in your voice, as it were, with the oppressors against poor Joan Baker? Here, again I think that Foxe has made the best of it.

I have exactly the same feeling about your view of the later cruelties of Henry VIII.'s reign narrated at p. 129 *et seq.* Why Thomas Hitton, or Richard Thomas Bilney, should be slighted as 'so-called martyrs'—I must say I fail to discover. Tyndale may have been quite wrong in trying to make (*proprio motu*) Hitton into a saint, but why attempt, on the other hand, to decry him as a fraud, seeing that he shewed the utmost courage and constancy, and that all the offence that appears against him was the importing of Tyndale's N.T. and the exporting of letters to 'heretics,' abroad. You surely don't believe that such a man was justly arrested 'on suspicion of having stolen some linen cloths from a hedge'! or that Bilney's burning was justified by the fact of his having (like Cranmer) recanted more than once!

I don't think it needful to go further in this line, because you admit that you expected (and probably still expect) such criticism in more influential quarters, and I am not going to make out a new case against you in respect of the Marian martyrs. But can we be quite sure that even the credulous and inaccurate Foxe was quite without justification in attributing even to so good a man as Sir Thos. More

the torture of heretics (p. 131) when even on the next page of your book, we have the admission by himself that 'he had caused the keepers of the Marshalsea and other prisons to elicit information, by means which could do the prisoners no permanent hurt'—by torture in fact, with a probably verbal caution to men steeped in cruelty to use moderation. No one, surely, would wish to drive home a charge of this kind against Sir T. More. But when you argue from his own general declaration further down (p. 132) that there was no foundation at all for Foxe's remarks, you seem to me to lose sight of the fact that—as in the case of the flogging of the lunatic in the same page, he might easily (as you do) lay the flattering unction to his soul that it was not for heresy, pure and simple, but some other civil offence, that he allowed his own humanity and clemency of disposition to be thus overborne. Moreover, did not you yourself once suggest to me that in the atrocious and evidently false and malignant charges laid at Wolsey's door by the Lords, More had appended his signature as Chancellor only in a ministerial sense, otherwise you would find it difficult to understand how he could possibly have done it.

No time for more, as the post hour is close at hand.

Your ever affectionate brother (though having a mind of his own in some things),

W. T. GAIRDNER."

Note by Mr. James Gairdner.

"The greater part of this letter, as will be seen by the contents, is a brotherly remonstrance with regard to some passages in my then recently published volume—the fourth volume of Stephens and Hunt's 'History of the English Church.' Of course, I replied to my brother's criticisms at the time, as is sufficiently shown by the next letter (of 21 Sept.), and indeed there must have been some intervening letters not published. None of this correspondence, it is true, was meant for publication; but I do not object to its being made public, if the reader will only bear in mind that he sees here but one side of a controversy. Nor do I wish in this place to discuss all the criticisms; but there is one to which I must beg leave to reply, as the error on which it is founded constantly reappears, and seems almost invincible. I had the presumption to think, in the face of an unchallenged tradition, very differently about 'the

martyr,' Bilney, from what anybody else did; and when *The Spectator* pointed to this as an instance in which I had shown less sympathy for Protestant martyrs than for Roman Catholic ones (although really I had shown not a little sympathy with Bilney), I pointed out that the reviewer had read what I wrote about him very superficially, as I had shown—I thought sufficiently—that he was not a martyr at all, least of all a Protestant one! Going to original sources, the matter to me is perfectly plain; and what is curious, Foxe himself, the *fons et origo* of the whole erroneous tradition, says distinctly that Bilney believed in the Mass all his days like 'the most gross Catholics,' and never varied from himself in that matter. Yet Foxe sought to discredit Sir Thomas More (who was dead and could not answer him) for saying, on positive evidence (which still in part survives) that Bilney was penitent for his offences against the Church, and on evidence of his penitence was allowed (not without hesitation of the authorities) to receive the Mass once again just before he suffered."

Here, again, we have a discussion of the relations of Church and State, dealing with the opinions expressed in More's "Utopia":—

"September 21, 02.

Dear James,

I do not know that there is much more that is worth saying on the subject of your last letter otherwise than as I have already written to you, I think, since receiving it. I do not dispute your defence of Sir T. More, and in fact I was very unwilling even to seem to put a case as against his real amenity of character and disposition. But you and he together seem to regard heresy as simply sedition, and that the 'unsound opinions' were the cause of the heretics being 'seditious mischief-makers'—it is surely not a false argument to shew that in his 'Utopia,' More actually had the distinction in view, and thought that the 'unsound opinion' even of an atheist, ought not to be punished, apart from violent action and crime. When in the 'Utopia' he brings up the case of one of their own (Christian) party becoming a public nuisance, and being harshly dealt with accordingly, he hits the nail on the head so admirably that it is difficult to believe that he wrote in the 16th century, and in the brutal reign of Henry VIII. But

is it fair, after all, (not to say generous) to make heretics of that date responsible generally for the violence of language, or even of more than language, which we find among some of them ? Tyndale, for instance, no doubt used unjustifiable language towards More himself ; but did More not lay himself open to it ? and is it not plain from Tyndale's whole career, that he was a pure-hearted and almost wholly Christian-hearted enthusiast, whom to persecute to the death was a baseness that demands just indignation ? and if Knox and others were found to justify violence, are we bound to apply 19th century standards to him, any more than to the people who put him in the French galleys, and to such scoundrels as Beaton (and later on Archbp. Sharpe) who had simply made it impossible for God-fearing and earnest men to live quiet and peaceable lives, in all godliness and honesty. I think even now that the current rhyme ' Although the deed was foully done, the loon was weel awa ' expresses very nearly the ultimate verdict of history in the murder of Beaton ; and Sharpe was, if possible, a more unspeakable rogue than Beaton.

I shall be glad to be informed of any reviews of your work which may appear to you to be of importance. Meanwhile, I have said my say.

Ever yours,

W. T. GAIRDNER."

Two letters to Sir Clifford Allbutt are of exceeding interest, as they embody Gairdner's open mind and his appreciation of honest doubt :—

" 32, George Square, Edinburgh, July 10, 02.

My dear Allbutt,

They are sitting upon me rather severely (doctors and womankind I mean) in consequence of my having had a return on Sunday last of some symptoms—no doubt ugly for the time—for which I am to be controlled and hampered indefinitely in all I want to do ; but I must nevertheless write a few lines, even on rising late in the afternoon from

bed, to acknowledge your kind and amusing letter of yesterday. I am reading your reference in it to Lord Acton simultaneously with Grant Duff's notice of him in the *Spectator*. I must say that my poor mind is fairly 'flummoxed' by the very idea of such omnivorous reading, and such admirable omniscience, ending in not being conscious of having at any time felt the slightest doubt of any doctrine propounded by the Church—it is to me, as no doubt to you, simply incomprehensible, though no doubt (as G. D. says) in its way sincere.

I am sorry to read what you write of Leslie Stephen (the disease I mean); but please remember that a little reading goes a long way with me, and when you recommend a certain 'Defensio Pacis' by some one whose name I can't make out, please be a little more explicit as to where I am to look for it, and how or why I am to 'get' it.

Human life is not long at the best, and my sands may possibly be nearly run out.

Yours ever,

W. T. G."

"Edinburgh, July 24, 02.

Dear Allbutt,

I fancy that Acton's amazing declaration to Grant Duff may well have been a kind of *ad hominem* appeal, as who should say to a confessed agnostic—'Don't bother me with your sceptical suggestions, for on all matters of faith my mind is made up'—or rather is handed over to the Church. We know, as you say, that he did in fact go so far in the way of dissent from the Vatican Council as to hold with Döllinger and the Old Catholic party; but no one has told us, I think, whether (with Newman) he formally 'caved in' when the Papal infallibility was declared, or continued in unobtrusive and silent opposition—perhaps he did not care to know himself! We must take it, I think, that there is a kind of sincerity about even such an illogical position as this; and I suppose we may give Jowett, Pattison, and others the benefit of the doubt likewise, as logic is not the

be-all and the end-all in spiritual matters; but that a man of Acton's immense and varied reading and large discrimination should have really confessed that he had never doubted, is simply a state of mind incomprehensible to me, and rather humiliating, I think, to human nature. I am sure that the best men are (in the present day at least) those who have doubted much—perhaps indeed who never cease doubting—but who can overcome their doubts in favour of a larger faith carrying greater issues than those that can be reached by mere argumentation. Only to these, I think, can we attribute Faith in its highest sense, the sense first given to it in the N. T., where it stands for something much higher than all the creeds. This, no doubt, is the prose of Tennyson's famous two lines.

I was out of doors to-day for the first time this week—but was astonished and a little dismayed to find how much my strength seems to have ebbed away, without any suffering or acute symptoms. Perhaps it will return gradually if I persevere, but in the meantime, I doubt if I could walk a quarter of a mile, and yet they say I am looking well.

Yours always,

W. T. G.

P.S. By the way, I never heard before, either from R.C. or Protestant, of S. Carlo Borromeo being anything less than a good man."

The Rev. Colin Campbell, D.D., Minister of the Parish of Dundee, most kindly placed at my disposal the following letters, referring to some interesting points connected with possible demoniacal possession.

"9 The College, Glasgow, Nov. 4, 99.

Dear Dr. Campbell,

My attention was directed yesterday to a translation in one of our medical journals some years ago, which I had previously overlooked, from Prof. A. Harnack's 'Medicinisches aus der ältesten Kirchen-Geschichte,' Leipzig, 1892, of the few pages on St. Luke, the Christian Physician of

Antioch. It may not be known to you that there is a reference to your researches in a footnote, to the following effect :—' It must also have been noticed that the third Evangelist has endeavoured to draw a distinct line between the process of exorcising and the curing of " natural " diseases both as regards their description and the methods used in curing them ; see Campbell, " Critical Studies in St. Luke's Gospel," Edinburgh 1891 (known to me only through J. Weiss, *Theol. Lit. Ztg.* 1892 No. 3).'

It may, perhaps appear to you worth while even at this time of day to bring your work more completely under Prof. Harnack's eye. His remarks on St. Luke's personalities are very interesting, and he attributes to him high culture and ' an excellent style '—which is more than that Oxford or Cambridge Don would allow to St. Paul, when he remarked on the apostle as writing on some rather important matter ' in execrable Greek ! ' But in truth, the pen that gave us the parable of the Good Samaritan and still more the Prodigal Son must have been deftly held even from the most artistic and literary point of view.

I don't know if you are acquainted with the vol. of the Rev. Mr. Hobart, a clergyman in the north of Ireland, who has ransacked nearly all medical literature to establish—with quite too much and too minute documentary proof—the medical character of St. Luke. A late Harveian orator at the London College of Physicians (Dr. Vivian Poore) puts it rather pleasantly though perhaps not quite convincingly —that Luke in his day and generation may probably have known more of real medical science and art, than any man between his date and that of the immediate predecessors of Harvey. But if so, and if he did—as Harnack half-suggests—model the introduction to his gospel on the lines of Dioscorides ; still more, if he was cognisant of Greek medicine so far as to have read Hippocrates' treatise ' On the Sacred Disease ' (epilepsy) ; he must have been profoundly staggered at the Jewish conception of daemonic possession, and the apparent allowance of it by our Lord himself.

But what a time it has taken to clear the human mind of that awful superstition! I went to hear Father Ignatius at the Portland Rooms when in London about a year ago—and found him declaiming (apropos of the Gadarene miracle) that if medical men would only speak out, they could tell us that there were numbers of demoniacs, even in the present day!

Yours very truly,

W. T. GAIRDNER."

"Glasgow University, Nov. 8, 99.

Dear Dr. Campbell,

As regards St. Luke and demoniacal possession—you should certainly see—if you have not already done so—the treatise in the Hippocratic collection on the Sacred Disease (*περὶ ἱερῆς νοῦσου*) as edited by Francis Adams for the Sydenham Society. Adams and most of the good critical authorities consider it as a genuine Hippocratic work and if so, we have evidence that in the 5th century B.C. we have a man of priestly caste (the Asclepiadae) and presumably deriving all his traditions of disease from the records preserved in the temples, who was, nevertheless, so completely emancipated as to argue that the 'disease called sacred' (epilepsy) was neither more nor less sacred than other diseases, and that all of them depended on natural causes.[1]

The question is—did the cultured and Greek-speaking Luke know this document or did he not, and if he knew it, how could he miss the obvious moral?

I do not remember if Hobart deals with this point—probably not—but he is so overwhelmingly minute in tracing out all Luke's little phrases in their correspondence with ancient classical and medical literature that it is a marvel.

Yours very truly,

W. T. GAIRDNER."

[1] He does not, curiously enough, even allude to the demoniac theory, but only to 'the gods.'

One more letter to Dr. Campbell gives a very clear expression of Gairdner's belief that the New Testament may fall far short of the rigid requirements of scientific history :—

"32 George Square, Edinburgh, Oct. 23, 01.

Dear Dr. Campbell,

I am sorry to have appeared as a pressing suitor for reply, when you were in a state of disablement, unknown to me ; but it is eminently satisfactory to know that you have recovered from what might have been an awkward accident ; and also that Mrs. Campbell is so much better.

And this reminds me, that in my last to you, I was somewhat remiss in not having made any remark on the printed matter you were kind enough to send me, and which arrived, unfortunately, when I was confined to bed.

The λογία Ἰησοῦ I had heard of, and read about, at the time of their publication ; but on the whole, the impression left in my mind was that they are chiefly interesting as showing how, at a very early period, the manufacture (if we may call it so) of such 'sayings' began ; so that it is difficult not to suppose that even in the canonical gospels—especially that of St. John—there should not be a good deal that is rather to be regarded as a record biassed by tradition, and really a good way removed, both in fact and in spirit, from the *ipsissima verba* of the Master.

I suppose that Matthew's gospel comes nearer to the absolute λογία than anything we have elsewhere. But the man who worked up for us the parable of the prodigal son and the narrative of the woman of Samaria must have been deeply—almost miraculously—inbred with the spirit of our Lord ; though it is hard to see how (in the case of the Samaritan woman at least) the mere words could have ever reached a reporter's ear.

As there was, I think, no letter or explanation of it, I am not quite sure what it was that led you to send me that curious fragment (the λογία) ; but I will return it at once to you, unless you should authorise me to send a reading

of it to my son Temple who is stationed at Cairo, and might do well to have an eye open for Oxyrhynchus.

W. T. GAIRDNER."

The Rev. Dr. Carslaw has sent me the following letter, enclosing two from Gairdner, showing his perfect humility in accepting a lesson in charity :—

" 36 Sutherland Avenue, Glasgow, S., Nov 6, 1910.

Dear Dr. Gibson,

The only time I ever met Sir William Gairdner was in 1895 at the funeral of my eldest son, who had been his assistant, and with whom, as well as with his brother, who succeeded him in that office, he seemed to be on intimate terms. That day I realised how much I had missed in not meeting him sooner. His name, however, as ' Old G.' was a very familiar one in our manse at Helensburgh, and occasionally when my sons came home at the end of the week they brought me messages from their Chief. Thus on one occasion, in connection with his study of Gout in Egypt, he bade them ask me if I knew of any mention of it in the Bible and seemed pleased when I told him that King Asa is said to have been ' diseased in his feet.' Having heard of a severe Typhus attack on my way home from the East, he was particularly interested on learning that my delirium (like his own, which had been a stock illustration in his lectures) assumed a professional aspect ! He had been present at his own *post mortem*, whilst I attended my own funeral and was buried in unconsecrated ground at Rome, with some of my parishioners as melancholy spectators !

At this late hour of the day it is too bad to trouble you with these incidents, but your kindly reception to the letter sent you by Professor Sutherland and now returned has reminded me of the two addresses which I now enclose, and which probably you have already seen.

Sir William's resignation must have been a heavy loss to the University, on its spiritual as well as its medical

side, and I sometimes wish I had been bold enough to ask him to my Manse and even to my pulpit!

Yours sincerely,
W. H. CARSLAW."

"February 25, 89.

Dear Sir,

I was pleased in getting a lesson in Christian charity from your sermon (as reported) on Mr. Kidston of Ferniegair. I did not know him personally at all—except by sight; but I at one time had contracted such a distaste to his methods of political canvassing and his narrow views in religious matters, that I quite determined *not* to know him, and even to show him the door, if he ever came to press me for my vote. He never did, and so the matter rested. Now in the far distance it appears to me quite possible that I misjudged him, but his 'cock sureness' and his narrowness together were very irritating, and I used to say of him that he was probably intended by the Creator for a good Christian, but had been entirely ruined and spoiled by a diet of too exclusive 'Shorter Catechism' in early childhood!

I don't think I should have written this little screed but that it gives me the opportunity of inquiring for your son of whom I have not heard for some time—not, I think since he left the Western Infirmary. Where is he now?

Yours very truly,
W. T. GAIRDNER.

Rev. W. Henderson Carslaw, M.A."

"Heanish, Tobermory, Aug. 29, 99.

Dear Mr. Carslaw,

I make it a rule, on principle, not to trouble my subordinates or my pupils much with my opinions on Church and State; because while it is right that I should hold my opinions, and not wrong that they should know that I hold them—it is not a part of my duty to obtrude them as professor.

To you, however, I may submit the enclosed without any risk of misinterpretation as to my official duty.

I take the opportunity of congratulating you on the engagement of your daughter, which reached me as news in a round-about way through my son Eric, but which I hope is true, as we all like Miss Nina very much.

Yours most truly,

W. T. GAIRDNER."

Gairdner's Presidential Address at the Glasgow Meeting of the British Medical Association in 1888 contained some theological ideas, and referred with admiration to the character and conduct of Darwin. In connection with this, the following letter, from Dr. Charles Henry Allfrey, of St. Leonards on Sea, naturally finds a place :—

" Rowlands, St. Mary Cray, August 19th, 1888.

Dear Dr. Gairdner,

It is many years since we met, and I hardly think that you will even remember my name. I attended your clinique occasionally in 1861, on the introduction of our dear friend, Dr. Warburton Begbie. I was not present at the recent meeting of the British Medical Association ; but I have read your Address with much interest—more especially that part in which you speak of Darwin. I wish to tell you how well I think you have gauged his religious position. It was my privilege to attend him during his last illness, and I was by his bedside for several hours during his last night alive. As I have often said, I never saw intense suffering endured more patiently, and with more unselfish consideration for those around him. The impression produced upon me was such that I am still able to say that I never met a more Christ-like man. Darwin's conduct on his death-bed was in keeping with all that we have read of him, as so well summarised in your Address.

Hoping I am not troubling you too much with what I thought you would be interested in,

I remain,

Your very faithful,

C. H. ALLFREY."

In a letter to Dr. Barnes of Carlisle, there is a humorous reference to his many heresies, and it shows his care not to obtrude any personal opinions upon the late Bishop of Carlisle :—

" St. Andrews, Fife, Aug. 10, 96.

Dear Dr. Barnes,

In sending you through the publishers last week a copy of my ' Physician as Naturalist '—I was not without an *arrière pensée* that you might, perhaps, after due consideration of them yourself, take some quiet opportunity of introducing (if not of recommending) some of my numerous heresies therein ventilated to the notice of the Bishop, whose kindness as a host rather indisposes me than otherwise to obtrude upon him my personal opinions in such a way as to compel him out of mere courtesy to acknowledge them.

Your address at Carlisle took you over much safer, and not less interesting ground, than mine at Glasgow, which caused me to be mentioned, if not denounced, in several pulpits on the succeeding Sunday for my appreciation of Darwin ; and yet it was not a very dreadful heresy after all, as a distinguished preacher, since Moderator of the General Assembly, told me he had read it carefully, every line, and saw nothing wrong in it.

It would not be fair, perhaps, to ask a Bishop to read and remark upon such things ; but as I believe you are a *persona haud ingrata* at Rose Castle, it may well be left in your hands. At all events you will kindly accept the vol. from me in recognition of your kind favours as Pres. B.M.A.

I am,

Yours very truly,

W. T. GAIRDNER."

His calm confidence as regards the future is beautifully revealed in a letter—the last upon any theological or religious subject which he seems to have written :—

"32, George Square, Edinburgh, Sept. 17, 1906.

My dear James,

You may tell the Archbishop[1] if you like (for I have no personal cause for initiating a correspondence) that it would not have been surprising at any time within the last 3 years if the telegram as regards our cousin at Torquay had actually, as he supposed, referred to me. I am indeed, and have been repeatedly within the period referred to, more or less of an invalid, and at times, I believe, in very serious and immediate danger. I am, however, just at present, as you have correctly put it, not so much in immediate danger or even very greatly suffering, but only 'very weak,' so much so that I have to be taken about when at all in a Bath chair. But with all this, I have cause for great thankfulness to God that I am as well as I am, and have been able for the last 6 weeks to enjoy the society of our two charming grandchildren, who have only just returned with their Mother to Dundee, leaving the house comparatively desolate. You may tell his Grace also if you wish that I am under no anxiety either of mind or body, and can afford to await my release at such time as it please the Great Disposer of events to remove me from the earthly tabernacle in which He has shewn me throughout a long life so much of his love and goodness that I cannot think of it as otherwise than well that I should be ready to go to Him.

I had to-day a bath of sunshine in the garden, but I never left the Bath chair, and when I found myself suddenly in need of being taken home, it was discovered that my Wife had gone home to lunch and taken the key with her, intending to come back, and that we had only the Nurse to manage my vehicle so that the strong arms of Uncle James or L.S.[2] were for the moment much wanted; *per*

[1] William Dalrymple Maclagan, Archbishop of York, had received a telegram announcing the death of Gairdner's cousin William, at Torquay, which he supposed referred to Sir William Gairdner; and he had written a letter of condolence to Mr. James Gairdner.

[2] Lewis Sutherland.

contra I found myself able, not perhaps quite prudently, to manage the one stair without assistance, and therefore I hope perhaps that there is a slight increase of strength, although it is agreed that the experiment is not to be repeated.

I have just finished reading the review of your book in the *Church Quarterly*, and am glad to find that they have much to say in praise of it—although the points on which they remark as being 'spots in the sun' are not precisely, though they are partly, what I should have selected for criticism.

With much love to Annie and Susie,

I am your brother,

W. T. G."

SOCIETIES AND CLUBS

No one ever heard Gairdner echo the sentiment of Horace —*Odi profanum vulgus*. He was essentially gregarious, and eminently realised the idea expressed by Samuel Johnson, when he spoke of his friend as 'a very clubbable man.' He never became a member of the Royal Medical Society, but he was, as an undergraduate, a member of the Hunterian Society, in the proceedings of which, according to Sir Henry Littlejohn, he took a very active part. Shortly after returning to Edinburgh from Rome—as a matter of fact it was in 1848—he was elected a member of the Medico-Chirurgical Society, of which he became Secretary in 1851 and Vice-President in 1861. It may be added that he received the honour of being elected a corresponding member in 1884.

Comparatively early in life he was invited to join the Medico-Chirurgical Club of Edinburgh, a purely social body, which is composed of about a score of the most eminent physicians and surgeons, who dine together three times in

the course of the year. Gladly acceding to the request of its members, he was elected in 1858.

But he did not restrict his social interests to clubs with purely professional leanings. The fact was known to me that he belonged to that brilliant coterie of which the late Professor Blackie was the centre, and amongst his papers the following interesting invitation finds its place :—

"24 Hill Street, Edinburgh, May 9, 1869.

The Club of Superexcellent Fellows, under the Captainship of Prof. Blackie, dines at *Wardie Hotel, Granton* on Friday the 23rd inst. at 5 o'clock P.M.

JOHN S. BLACKIE.

Please say if you will be present.

The Brotherhood.

Rev. Dr. Lindsay Alexander.
Rev. Dr. Hanna.
Rev. W. C. Smith.
J. Hunter, Esq., Craigcrook.
J. Hunter, Esq., Junior, Advocate.
A. S. Kinnear, Esq., Advocate.
Dr. J. Brown.
Dr. W. T. Gairdner.
G. Harvey, Esq., R.S.A.
D. O. Hill, Esq., R.S.A.
J. Noël Paton, Esq., R.S.A.
Professor A. C. Fraser.
Professor Lorimer.
Professor Blackie."

Of the Blackie Brotherhood but two survive—Lord Kinnear, Senator of the College of Justice, and Emeritus Professor Campbell Fraser. Lord Kinnear has been good enough to send me some facts :—

"Melville House, Collessie, Fife, 29th July, 1909.

Dear Dr. Gibson,

I hope you will forgive my delaying so long to answer your letter. I have been trying to recall some

recollections of the Blackie Club which might be of use to you, but I am sorry to say without success. I can tell you little more than that it was started by Professor Blackie in the early sixties for the purpose of dining in good company without champagne. The first members were the Professor himself, Mr. Hunter of Craigcrook Auditor of the Court of Session—a great lover of books and full of literature and wisdom, Professor M'Dougall, Professor Fraser, Dr. John Brown, Sir Noël Paton, his brother Waller, D. O. Hill, Dr. Gairdner, Mr. Hunter's son Jack, then a Junior at the bar, and myself. There may have been one or two more, but these are all of the first set of Members whom I can recall at present. Later on Professor Masson and Alexander Nicolson were highly valued additions to the Club; and I rather think also Dr. Walter Smith. I can recall many delightful evenings in this society, but nothing that was said or done at any of them. It was all the more interesting to my friend Hunter and myself because all the Members belonged to an older generation, except Dr. Gairdner, and he was older than we were. I am sorry to say I cannot remember much of his share in the proceedings, except that he did take his share in much good talk, while I rather think his juniors were only good listeners.

He soon went to Glasgow however; and after that, I do not think he appeared often at the Blackie dinners. They went on with great success for many years till the Club died away, as these things will, and some time in the eighties, before Blackie's death, it became extinct. All that remains of it now is a song of Nicolson's which I think you will find in a little book of verses by him, which was published after his death by Dr. Walter Smith. It was a welcome to the Professor by the Blackie Brotherhood on his return from Egypt. This is all I can tell you; but you might possibly learn more from Mr. David Douglas the publisher, who was the last Chairman, though not one of the earliest members.

Apologising again for a tardiness of reply, which has

been really owing to a vain hope that I might in time recall something more worth saying.

Believe me,

Yours very truly,

KINNEAR."

The reference to Gairdner is eminently suggestive, for it is to be remembered that, in the give and take of society, he was not only a remarkably good talker, but he was an exceedingly good listener as well.

After his transference to Glasgow in 1862, he became a member of the Medico-Chirurgical Society of Glasgow, to which he largely contributed. He was one of the original members and the first President of the Pathological and Clinical Society, and he also took a keen interest in the divisional medical societies of Glasgow. It need hardly be added that he became attached to the Glasgow Western Medical Club—a body with purely social functions—with which he rarely failed to dine, not only in Glasgow during the winter, but, as is the wont of the members of the Club, in some country inn, amid beautiful surroundings, in summer.

With a view to bringing about a closer association between the medical profession in Edinburgh and Glasgow, he founded in 1890 the Edinburgh and Glasgow Medical Club, which meets once a year, alternately in either city, and which has done a great deal to foster friendly intercourse between the medical men of the two towns. The inception of the Club was entirely due to the initiative of Gairdner, who, with Sir Douglas Maclagan, Dr. William James Fleming, and myself, carefully considered the whole matter at an informal meeting, as the result of which it was resolved to issue circulars to a number of medical friends in the two cities, inviting them to associate themselves with the scheme.

The proposal met with the most cordial response, and it was arranged that a dinner should be held in the Hall of the Royal College of Physicians of Edinburgh on the evening of 7th November, 1890, Maclagan occupying the chair, and Gairdner acting as croupier. The second dinner was held in the Bath Hotel, Glasgow, on 11th December, 1891—George Macleod being chairman, and Douglas Maclagan croupier. It will be remarked as somewhat significant that Gairdner himself was not in the chair on this occasion; but it is just another example of his constant desire to keep as far as possible in the background, so long as others could be found to stand in the foreground. At this second dinner it was unanimously resolved to form a permanent club, and it was also resolved that all those who had been present at either of the two dinners, or who had been invited to take part in either of them, and had been prevented by circumstances from doing so, should be eligible for membership. There can be no doubt that the Club has, during the twenty-one years of its existence, amply fulfilled the intention of its founder, in promoting friendly relations between the followers of the medical profession in the two leading Scottish cities.

On his return to Edinburgh in 1890, he was invited to join the Aesculapian Club—a venerable body comprising eleven Fellows of the College of Physicians and a similar number from the College of Surgeons, to which it may be said many of the leading men of the medical profession in Edinburgh belong. He gladly accepted the invitation.

VISIT TO AMERICA

Gairdner visited America in 1891, and wrote a most interesting narrative of his experiences under the title of 'Impressions about America for my Children.' This was

written from time to time, both on the outward and homeward voyages, as well as during the sojourn in the United States. It has, therefore, the freshness of recent observation.

" I promised my dear children," the author begins, " that I would try to give them some account of my visit this year to a country that some of them know very little about, and that none of them, any more than their father, has ever seen. This book is meant to fulfil that promise, and although I shall write in it very little that is new, or that has not often been described before, that will not be of much consequence to them, if they wish to read what their father saw, and heard, and felt, and what they may, one day or another, be able to compare with their own experience."

He sailed from Liverpool on Wednesday, 5th August, in the Inman steamer " City of Paris " ; amongst his fellow-passengers were Dr. J. Batty Tuke and Professor Chiene of Edinburgh, Professor Cunningham of Dublin, and Professor Young of Manchester. The voyage was only remarkable for a terrible dream, and the consequent painful anxiety which it produced. My late friend, Professor Cunningham, told me of it on his return, and the following account is given by Sir William, under the heading of

" A Confession.

Now, I have a little confession to make. I dare say you don't think your father is superstitious ; and yet (though not believing it) I have been led to spend a pound in telegrams on account of a superstition.

One night during the voyage (Friday, 7th Aug. about 4.20 a.m.) and à propos of nothing at all, for I was not in the very least degree anxious that I know of, I dreamed a dream which instantly awoke me, and the impression vividly remained (though at first without any sense of great pain or horror, so clearly it was not a night-mare) that

I had actually seen a telegram with these words—'Miss Dorothea died at —' and then came in a black blot, so that nothing more was readable.

Happily, my dear little lass will be too young to understand this, or I should not care even now to write it down; but Frank and Temple will remember a queer story we got from Dr. Allman in Donegal, and there are many such queer stories going about—indeed I have heard dozens of them and so will you all. I thought it right that Dr. Allman should send his story to the Society for Psychical Research, which occupies itself with such matters, as I believe he did.

Now, here is the other side of the question. My dream had, I think, every single feature of those that have come true; and yet, thank God, there was just nothing in it at all. It struck me at once that no telegram in these words could have come from Mamma, or from any of the family—it must have been a servant or an officious friend, if any one. So it was probably not a case of what they call 'telepathy.' But of course it might have been a case of that more ancient and in some quarters prevailing tradition of 'second-sight'—or what the mesmerists call 'clairvoyance'; and therefore, though not believing, I was not at rest till it was settled. The telegram this afternoon—'All well'—relieved my mind."

The "City of Paris" reached New York on 12th August, when the fellow-travellers separated. Although it is not germane to the immediate subject of this narrative, yet the temptation is irresistible to mention an amusing circumstance which took place during the journey of these four fellow-travellers, after they had parted from Sir William and were on their way westwards. When they arrived at Winnipeg, Manitoba, they were somewhat struck by the numerous crowd which came to meet the train, and at the appearance of interested animation on every face at the depôt. Judge of their amusement when they found a

paragraph in one of the morning papers giving, for the information of all whom it might concern, the startling intelligence that a party of English scientific gentlemen, under the care of that eminent alienist, Dr. Batty Tuke, was expected to arrive that afternoon !

After spending a few very hot days in New York, Gairdner went by a magnificent steamer of the Falls River Line to Newport, R.I., and took up his abode at the Cliffs Hotel, a delightfully situated and perfectly appointed establishment. The well-known beauty and the palatial residences of the famous watering-place charmed him very much. His main object was to visit the widow and mother-in-law (Mrs. Bruen) of his late friend, Mr. Charles Perkins. Mr. and Mrs. Perkins were the parents of Mrs. William Hooper of Boston. Gairdner remarks that Mrs. Bruen, who was ninety-eight years of age at the time of his visit, was very lively, and extremely communicative—the only trouble being that her recollections of Scotland dated from a period twenty or thirty years before he was born. One of the old lady's youngest friends was the Rev. Dr. Horatius Bonar, the distinguished hymn-writer, whom Gairdner met amongst the Italian lakes on his honeymoon.

At Newport he also met Mrs. Blake of New York, who had been very kind to him on his arrival, and the famous Bishop Whipple of Minnesota, the great missionary to the Red Indians of the West. He remarks—" I found the good Bishop a very dear and fine old man, perhaps 75 years of age, and with the quiet dignity (not without humour and large general interest in all things human) that is befitting in a veteran." There were two other clergymen also present at the party where he met Bishop Whipple—one of them, Bishop Henry C. Potter of New York, " a very cultivated and courteous sample of the

episcopate," as he remarks. "I had to hold my own," he says, "in this very spiritual atmosphere from 8 to 10 o'clock. How I did so I won't say here; but I told them the story of the highland gillie's excuse for snuffing as against smoking, and many anecdotes of English and Scottish church people, which passed the time very pleasantly. Bishop Whipple had a great regard for the late Bishop Ewing (whom I knew) and was also very sympathetic as to many of our friends in Scotland whom he had either seen, or heard of. When I asked if he lived very far West he said—'No; just take the map and find the very centre of this great continent, and you will about drop down my chimney!'"

At Newport he saw a great deal of the distinguished Dr. William Pepper, head of the University of Pennsylvania; and also of Dr. Limholtz, the famous explorer.

On Thursday, 20th August, he went to Boston, where he spent some days, interrupted by a delightful visit, from Friday till Monday, to General Loring, Director of the Art Museum, in which he was closely associated with his old friend Mr. Charles C. Perkins. At General Loring's charming sea-side house he met Mr. James Elliot Cabot, author of the "Life of Emerson," and Mrs. Asa Gray, widow of the distinguished Professor of Botany in Harvard University. Not being quite certain to which branch of the distinguished New England family Mr. James Elliot Cabot belonged, it was necessary for me to apply to some one of the members of the house, by whom, on many occasions, much kindness has been shown me. Mr. Elliot Cabot was the father of Dr. Richard C. Cabot, Professor of Clinical Medicine in Harvard University, and uncle of Dr. Arthur D. Cabot, Emeritus Professor of Surgery and Member of the Governing Body of that University.

Gairdner remarks: "As compared with New York, I found Boston incomparably more attractive. It is surrounded by a beautiful and well-wooded country, especially in the Brookline direction, in which the view ends in the Blue Hills, about 12 miles away, and about a thousand feet high. The Harvard College buildings are in the very midst of this fine natural and wooded scenery, and the centre of the town is diversified with parks, and has fine broad streets, beautiful and tasteful churches and public buildings."

While at Boston, he met a number of the prominent medical men of the city, although a great many of them were out of town. He spent some time over the arrangements of the Medical School, which was, even at that time, extremely well appointed, although its condition at that period has been completely eclipsed by the wonderful developments which have since taken place.

One visit which he paid must be more particularly mentioned :—

"I must not omit to mention here that an afternoon (22nd) was most delightfully given to dear old Dr. Oliver W. Holmes, who lives at Beverly Farms as his summer quarters, along with his son, Judge Holmes and the wife of the latter. The old 'Autocrat' is full of life and apparently of the joy of living, even now, and drove me about the whole neighbourhood, shewing me all the fine trees, and full of talk about the country. His gentleness and his charm are, no doubt, reflected from what one knows of his literary work in a measure, but even without this he would be a wonderful and a most charming octogenarian. As I told him, he teaches us all the lesson 'how to grow old' in a young spirit, and in charity with all men."

Gairdner was evidently very much amused by some lines which he found in one of the newspapers, and which he has copied verbatim :—

"A True Bostonian.

A soul from earth to heaven went,
To whom the saints, as he drew near,
Said: 'Sir, what claims do you present
To us to be admitted here?'

'In Boston I was born and bred,
And in her schools was educated:
I afterwards at Harvard read,
And was with honours graduated.

'In Trinity [1] a pew I own,
Where Brooks [2] is held in such respect,
And the society is known
To be the cream of the select.

'In fair Nahant—a charming spot—
I own a villa, lawns, arcades,
And last, a handsome burial lot
In dead Mount Auburn's hallowed shades.'

St. Peter mused and shook his head;
Then, as a gentle sigh he drew,
'Go back to Boston, friend,' he said,
'Heaven is not good enough for you.'"

Mr. Edward Perkins, son of his old friend, invited him to pay him a visit at his summer quarters, near Windsor, Vermont; and, relieved to escape the heat of the city, he gladly accepted the invitation. He found Mr. Perkins had married the daughter of Senator Evarts, a distinguished statesman and minister, who lived in retirement in a house

[1] The most frequented and fashionable of the Episcopalian Protestant churches in Boston.

[2] The Rev. Phillips Brooks, D.D., just then appointed Bishop of Massachusetts, and by all my friends said to be a man of great power, breadth, and catholicity.

closely adjoining that of Mr. Perkins. On 27th August, the very day after his arrival, the President of the United States, General Harrison, in the course of a progress, arranged to speak in public at Windsor. At the house of the Senator, Gairdner was introduced to the President, and was afterwards one of the audience at the Oration. He remarks that the speaking was admirable, and the tone unexceptionable, without a trace of the spread-eagle style, often erroneously attributed to American speakers. He goes on to remark that he had observed at Bennington, Vermont, the same sobriety of tone, and the same sincerity of intention, in the speeches of Mr. Phelps and Mr. Russell, as well as of the President.

From Windsor, he proceeded to Bethlehem, N.H., in order to pay a visit to Mrs. Cleveland, sister of his old friend, Mr. Perkins, and her daughter. Independent of its associations with his old friend, he found a rare charm in the surroundings of Bethlehem ; and he mentions that he was struck by the resemblance of the scenery to that of many a highland home of his acquaintance.

After leaving Mrs. Cleveland's hospitable residence, he visited the Bishop of Albany, Dr. Doane, at North East Harbor, Maine, where he was most kindly entertained. He remarks that Mount Desert is a lovely island, made very pleasant in his memory by the interesting and friendly folks to whom he was presented. On leaving the Bishop, he went to Boston from Bar Harbor, by the " Olivette," and started immediately for Albany, to see the new Cathedral and other interesting buildings, proceeding next day to Elmira, where he stayed with Dr. Hamilton Wey and his father, in whose company he visited the great New York State Reformatory Prison.

From Elmira, he proceeded to Niagara, by Seneca Lake,

the charm of which he describes. As regards Niagara, he observes :—

"My Sermon about Niagara.

Sunday, 13th Sept., 1891.

'The very hairs of your head are all numbered,' Matt. x. 30.

It has been said by a very great authority (Lyell) that the volume of water that passes over the Falls of Niagara is probably not less than 90,000,000,000 cubic feet per hour. What is that in drops ? but it is needless to try to count, or to 'number' it ; you could not do it, if you spent all your life in counting, from infancy to old age. I have reckoned that there are nearly 3000 millions of seconds of time in a very long lifetime (say 90 years) and there are 16 millions of cubic feet of water passing over Niagara falls in each second of time. And each cubic foot must contain not less than many millions of drops. You see it is in vain for mortal man to try to reckon with the drops that go to make the great fall of Niagara. But Niagara has been going on, not for the lifetime of one very old man, but for countless ages before a man ever appeared on this earth, and perhaps before life of any kind appeared on it ; and it will go on, to all appearance—how long ? God only knows. But what I want you to think of now, is that God does know, not only that, but he knows also exactly what becomes, or has become, of every drop of water that has been flowing over Niagara falls during all these apparently endless ages. When Jesus therefore says that 'the very hairs of our head are all numbered' it is not an oriental exaggeration at all, but a very positive fact ; and if you can be assured that God does indeed 'number' the drops that go over Niagara falls, and are tossed up again partly in spray, or apparently lost in vapour, but in the main go down into Lake Ontario and thence by the great river St. Lawrence into the immeasurable ocean, you will have some guidance towards the grand, and to all of us most beautiful, idea, that the very hairs of our head are also within God's reckoning ;—very much indeed

within it, if the mere number of them is in any way to be compared with the number of the drops in Niagara, throughout all those countless ages.

For it is one of the absolutely surest things I can tell you—it is, in fact, a truth in science which every one admits—that no drop of water or of anything else, ever is lost, ever disappears entirely from the universe. Consider what that means. God, who rules and directs the whole universe, reckons with even a drop of water: knows exactly where it is to go and what it is to do; has got its proper place prepared for it, all through these ages, so that you may say that God has really known, and has arranged from all eternity about every drop of water that goes over Niagara falls, and that these drops of water, like the hairs of your head, are all numbered. Of course there are no errors in the books of the Universe; but that is because the reckoning is all so perfect, so absolutely exact, that not even a drop of water can get out of its proper place, or do anything else than just what God has arranged from all eternity that it has got to do at this particular moment. So that a drop of water might go down to the sea and be carried along by the great ocean currents towards the North Pole, and there get frozen up in an iceberg, not to be released again perhaps, for hundreds of years, or perhaps never at all; or it might, on the other hand, go up in vapour into the air, and then get poured down again in rain in one of the other continents, say in Asia, or Africa, under the hot Equator.

I have always thought that this is one of the most wonderful views of God's providence that can be presented to any mind, whether of old or young; that is, if you are only able to take it in at all. In one sense, indeed, no mind can ever take it in. Newton, and Galileo, and Lyell, and Darwin, and Faraday, if they were all living now, would all agree that it is impossible for any one even to conceive of how it is that this great order is accomplished—it is utterly and entirely beyond all of them to tell how the atoms of matter get all into just their proper places, and do exactly their proper work. We, who believe in God, believe that

He does it ; though we are as far as ever from knowing how He does it. But, if we can even dimly perceive the fact that God does indeed reckon the very drops of water in this earth of ours, in this tiny world of His—immense as it appears to us—in which we dwell ; what shall we say about all the other worlds, planets, and comets and suns, and solar systems, circling in the endless depth of space, of which we can only form any idea at all through the calculation of astronomy ? It was surely no careless saying, that of the Hebrew prophet—like Christ's saying above, it was no orientalism, but a very positive statement of a fact—' He counteth the number of the stars—He calleth them all by their names—not one faileth.'

And so we may come round again to our text, which, you will now see clearly, I hope, is not at all an exaggeration, but even an understatement of God's care for every one of us. He who counts the number of the stars, and who has reckoned all the drops of water in Niagara from the beginning of time—and all the waters of all seas, and not only on this earth of ours, but on all the planets in which water exists, can have no difficulty in numbering the hairs of our head, and we may therefore accept the fact that they ' are all numbered ' as one quite in accordance with what we know of God and his works."

To Dr. Middleton he also gave strong expression to his feelings of wonder at the grandeur of Niagara. The letter refers to some of the sentiments of the " middle west." Needless to say, statements of the same kind have been made to many of us who have visited the United States. Each of us has no doubt felt inclined to say, " New England and New York are good enough for me."

" The Cataract House, Niagara Falls, N.Y.,
September 12th, 1891.

My dear Middleton,

You will observe from the above that I am now at the very seat of one of those wonders which alike baffle

description, and make poor and contemptible all words intended to convey one's feelings about it. I have spent the whole of this day lingering about every aspect of Niagara, and as to-morrow is Sunday, I do not think it can be better spent than in resting here, and continuing to gaze and to wonder. To see Niagara all round is indeed a work of several days, and no amount of time which I could reasonably give would come near the requirement. I must, however, be in Toronto on Thursday; and it is at present about an even chance whether I go on by the St. Lawrence to Montreal, or go across country from this to Chicago, to endeavour to grasp the idea of American enterprise and go-aheadness at its maximum. Strange to say, the feeling expressed to me here is that in all I have seen hitherto, in New York, Boston, Vermont, Maine, I have only seen an effete and played out side of American life. All the same, it has been very grateful to me, and the kindness I have met with on all hands has been extraordinary and quite unexpected.

In any case, I am sure to be in Washington on the 22nd inst., and up to near the end of that week—Hotel Normandie being my address there. After that the address already given will be the best, as I am somewhat uncertain whether I shall accept an invitation to go back to Boston on October 1st for the opening of Harvard College, or return to New York and its neighbourhood to await my steamer.

In any case, you will hear of my return.

I am,

Yours very truly,

W. T. GAIRDNER."

On 14th September he took what he calls "a dip into Canada"—that is, he spent twenty-four hours in Toronto. Here he met Professor Goldwin Smith and Sir Daniel Wilson. From conversations with these eminent men, he was led to the conclusion that there was, on both sides of the frontier, a strong feeling in favour of some form of loose federation

of the Anglo-Saxon races. He found Toronto to be a handsome and progressive city, with a great future before it. It had been his intention to run down the St. Lawrence, in order to see Montreal; but time forbade his carrying out this plan, and on the 15th September he turned backwards, in order to visit Detroit. He was greatly delighted by the beauty of Detroit, and more particularly by the charm of its lovely Island Park. He stayed in Chicago, by which he was greatly impressed, from the 16th to the 18th of September. He observes that, about half a century before his visit, it had been a small Indian trading station; whereas, even in 1891, it covered 175 square miles, while its width was 15 miles. The majesty of the modern buildings, and the beauty of the public parks attracted his particular attention. He was also deeply interested in the stock-yards of Chicago, and particularly in those of Messrs. Armour and Company. Although admitting that a good many of the sights in this great establishment are by no means pleasant, he was, nevertheless, much impressed by the mechanical organisation and marvellous promptitude with which the whole business was carried out. The Exchange of Chicago aroused his deepest interest. He says:—

"In Chicago bulls and bears are in full concert every day at noon, the one endeavouring to cry down, the other to cry up prices upon the greatest possible scale. Hundreds of thousands of dollars are disposed of in a few seconds; it is an immense auction, or rather a dozen such auctions going on all at once—men shouting, gesticulating, looking as if they were frenzied or in a passion, but all the while as cool as cucumbers, with a single eye to the main chance, and bound to bring about, by hook or by crook, if possible, a state of opinion or of feeling in the crowd that will favour their own little transaction of the moment."

It was at Chicago also that he made his first acquaintance with the interviewer. "Going about very quietly from place to place, without previous announcement," he says, "I had hitherto escaped this; but in the books of the Auditorium Hotel I had inscribed my name (as every one does) and after it 'University of Glasgow'—and that was enough! I knew not a soul in the place; but in less than half an hour—at about 8 a.m., the ubiquitous reporter was about, and his card was put in my hand by the hotel manager before I had well breakfasted. In the course of the day *two* others put in an appearance, but finding me out, and being thus anticipated by the first man (who acted for an evening paper) they did not turn up again. The gentleman in question was very civil, and in every way quite agreeable to talk to. I said to him—'You are the inevitable interviewer, I suppose; but I am not a public character at all, and you won't get anything worth having out of me.' He then said that Dr. Cameron, M.P. from Glasgow, had been there the day before, and he would like to know how I viewed certain questions on which Dr. C. had spoken. 'Oh! but,' I said, 'Dr. Cameron is a public character, and he and I don't always agree.' 'About Home Rule, now?' 'Yes, we don't at all agree on that.' And here followed a little more fencing which, I must say, was on the whole very fairly put when I came to see it in the evening paper aforesaid. I then said that I had come from Detroit and was going on to Washington; expressed my wonder at the growth of American towns of which I considered Detroit perhaps a fair example among scores which had grown up from small beginnings during my own lifetime.

All this very commonplace talk was taken down in the memory (there was no notebook) of the reporter, and not only chronicled in print, but was made the subject of a leading article!! From which I gathered that it is much easier to become (for the nonce) a great man in Chicago than it is in Glasgow!"

He writes, in conclusion, that he was very glad to have

had the opportunity of visiting Chicago. A letter to Dr. Middleton, written from Chicago, also gives his impressions of that great city :—

" Auditorium Hotel, Breslin and Southgate Streets,
Chicago, Sept. 16, 1891.

My dear Middleton,

Just to remind you that I am still in existence, I send you this little memo from about the ugliest and most immense of hotels in U.S. ! it is, however, a very comfortable as well as splendid house, founded some years ago in connection with a great concert room and theatre, and as my bedroom on the 7th story is No. 763 you see I am only an atom in the vast, like say a single bucket in the falls of Niagara ! I came here only this morning, but have already driven (say) 15 miles or more through the hundreds of miles of streets and avenues and boulevards which compose Chicago, and I can report it as all very grand and some of it—the parks etc. very beautiful and well laid out. You all know something of the marvellous growth of this city since the great fire of 1871, by which 17,450 buildings, mostly of wood, were absolutely destroyed, and nearly 100,000 persons rendered homeless. Between this and another fire in 1874, nearly £40,000,000 worth of property was destroyed, and yet business went on in extemporised wooden offices, and in a year or so, stone and brick buildings replaced all that had been done away with ! It is now, therefore, the most thoroughly modern, and at the same time one of the finest cities in America, if not in the world. I hope to get a notion of it before I leave, but as yet I have only driven about some 15 miles, or more, in one quarter of the vast whole, and they say that a mere round of all the parks and boulevards would come to some 40 or 50 miles. I came hither from Toronto, taking a look at Detroit by the way, which is really a beautiful and delightful city on the splendid river that flows from Lake Huron, through Lake St. Clair into Lake Erie ; and you may imagine what these expressions mean when I tell you that this Lake St. Clair,

small as it looks on the map, is an inland sea five miles longer than Loch Lomond and quite as broad as it is long. Of course you can't see across it at all, the shores not being hilly, and it looks for all the world like an open sea with a wide horizon of water. In crossing Lake Ontario (as I did the day before yesterday) you have the same feeling of boundlessness, but otherwise there is not much in it to write about. The falls of Niagara I need not (indeed cannot) describe—they are simply overwhelming in their grandeur and beauty. I lived upon them and drank them in as it were, for some part of 3 days; but as so many people have said to me that I must see Chicago, I altered my plans so far as to give up Montreal and the St. Lawrence in favour of coming here, and I shall now go directly from this (two days hence) to Philadelphia to have a quiet Sunday there before going on to the Washington Congress. It is very warm here, though not nearly so insufferable as I found both New York and Boston soon after my arrival. I am more and more struck, as I go on, with the immense resources and the irresistible material prosperity of this great people, and I am bound to say that, although they are very much addicted to turning out their 'seamy side' to the public in newspapers and elsewhere, I have had abundant proof of their wonderfully generous and essentially sound feelings in private and domestic life. Everywhere the evidences meet me abundantly of a strong, healthy, well-developed race, full of animation and vigour, both mental and physical; they take a perfectly sane view of their own condition, and so far from being afraid of criticism in general, are more severe on themselves than any foreigner, such as I am, would like to be on them."

He left on the 19th for Philadelphia, going by Pittsburg and the Alleghanies, Harrisburg and the Susquehanna. On his arrival at the Quaker City he spent a quiet Sunday, "without observing anything specially quakerish about it." He had the advantage of an introduction, quite accidentally obtained, from two young ladies with whom he travelled

to Bar Harbor, at Mount Desert, to their relations, Mr. William Tatham and Mr. and Mrs. Biddle, who did everything in their power to show him their historic town. It was a happy chance, as his friends, Dr. William Pepper and Dr. Weir Mitchell, were not at home. His kind hosts showed him Fair Mount Park, which is certainly one of the most lovely in the world.

On 21st September he reached Washington. "Washington," he writes, "is not only the seat of Central Government for the whole of the United States, and the residence of the President, but also a very fine and a distinguished City, as such, among the number of those I visited. Without attempting anything like guide book details, I will set down here a few of my impressions, it being understood that both on account of the time occupied by the Congress of Physicians to which I was invited as a guest, and on account of the great and unseasonable, and to me most oppressive, heat lasting over the whole week, I was less able than usual to do much sightseeing. Every one complained of this heat, and even the natives declared that it was quite too much, and that they had rarely had, in the later autumn, anything so overwhelming and so opposed to the work of the Congress. Indeed there was a good deal, not perhaps of serious illness, but (as in my case) the feeling of just being on the verge of illness, the whole time. Added to which, the mosquitoes which attacked me first at Newport, and then had ceased their troubling in the White Mountains and Maine, began again in full force at Chicago, and kept at it pretty constantly in Washington. Notwithstanding all which I attended the meetings of the Congress very faithfully, and took my part in them, apparently to the satisfaction of my brother medicoes, if not of my own. Their hospitality was only too abounding; and I must record that both to Dr. Weir Mitchell, the President of the Congress, and to Dr. Pepper, the President of the College of Physicians, I was indebted for numberless acts of great kindness which can never be forgotten. Dr. John Billings,

also, the Surgeon General, an old acquaintance and friend of mine ever since he visited Glasgow (when I was still unmarried) was, on his own ground, one of the most notable and admirable of our hosts ; and from every great medical centre in America I found gathered together here friends whose reputation and hearty good-will made it a real privilege to form, for a time, a part of such a convention.

Washington stands alone among American cities, in more than one respect. The original seat of the Federal Government was Philadelphia, which was both a much older, and a much more important place. Although selected for this honourable distinction, Washington never was of any account as a seat of manufactures or of industries of any kind ; and I believe it is not so now. It has, however, perhaps for this very reason, and because of its own attractions, become to a considerable extent of late years a place of residence, permanently, not only for politicians and statesmen, but for persons who aim at cultivating the best society that America affords, in a climate which—in winter at least—is less rigorous than that of the more northern cities. Moreover, it is not only in possession of a fine situation, and attractive surroundings ; but a great deal of money has been lavished on it by the general consent of both Houses and of the country at large, which seems to think no cost too great to make the city of the Capitol, of the Senate and House of Representatives ' a thing of beauty and a joy for ever.' And this pride in adorning the seat of the central government is the more remarkable, because the expenditure of the money which is thus liberally furnished is not controlled in any way by a popularly elected body. Washington is practically governed by the President of the U.S. for the time being, who nominates directly a body of Commissioners (at present 3 in number, and responsible only to him) to whom all details are committed of local government, and all public works of every kind: and that the federal city may have a certain independence and plenty of elbow-room, a district is reserved around it of about 10 miles square, to be administered in like fashion.

The residents and occupiers in this district, and in Washington itself, have no vote at all, either for state elections, municipal elections, or in the Federal Government. And this, in a country of universal suffrage and extreme democracy !

The District of Columbia (as it is called, D.C.) is absolutely set apart and kept out of the general government altogether, very much as the Vatican is, as regards the government of Italy ; and like the Vatican, is under the practically absolute rule of one man—who, it is true as regards America, is only absolute for 4 years. And it seems to be on the whole very well administered, and flourishes under this system. The streets are broad and handsome ; the roads better than almost anywhere else in America ; the public buildings are certainly noble and very striking ; the gardens and open spaces well laid out, and full of picturesque vistas ; the libraries, museums, etc., are wonderfully well-furnished, and all the means of living and of getting about in public conveyances, at least as well regulated as in any city in the U.S. and much better in all respects than in New York. Washington, in fact, seems to be a kind of object-lesson in favour of a benevolent despotism, in the midst of a democratic nation.

Not the least of the really magnificent things in Washington is the administration committed to my friend John Billings, as director of the Army Medical Museum and Library. As this is not mentioned in most of the guide books, I will here put in a few lines upon it."

He goes on to give a full description of the Library of the Army Medical Museum in Washington, and he pays the highest tribute to Dr. Billings.

After spending a day in seeing the Capitol, as well as other prominent buildings in Washington, he went, on 26th September, to Baltimore. He passed a day in the inspection of the Johns Hopkins University and the Johns Hopkins Hospital, under the guidance of Dr. H. M. Hurd, to whom

he expressed his indebtedness for his attentive courtesy. Gairdner was much impressed by the appearance of profuse expenditure in the construction of the University and Hospital, which he regarded as depriving them of the right of being, or becoming, models for us in Europe. As medical institutions, they aroused his unqualified admiration, in their vast capabilities for medical instruction and investigation.

The following day was spent at Philadelphia, under the hospitable roof of Dr. William Pepper, and, as the heat was more oppressive than ever, he was only too glad to take refuge indoors and rest entirely. On the 28th September, he returned to New York, and renewed acquaintance with his good friends Mr. Leech and Mr. Blake. With the latter, he proceeded, on the 29th September, by the Steamer "Albany," up the River Hudson. They were accompanied by two of the three Misses Leech, and the sail was delightful. Gairdner felt, as all of us have done, who have been up the Hudson River, the charm of the beautiful and picturesque scenery of the banks—especially at the points well known as the "Gate of the Highlands" and the "Palisades." He gives a full description of New York harbour, with its imposing vistas and its enormous traffic, and then proceeds to say :—

"It is marvellous that, with all this commercial and maritime supremacy, America has, until quite lately, had no navy to speak of ; and, I believe, also but little carrying trade in ships of her own (comparatively speaking). In a late manifesto of President Harrison I read that he desires for the U.S. not only a considerable navy, but also an adequate mercantile marine service. Does this mean that the republican party are to abolish or revise the Navigation laws, which are so intimately bound up with their own policy of protection ?

Into all the causes of this I cannot enter here; but it is characteristic of the sense of inward power of this great people, that they have accustomed themselves in this respect rather to be ministered unto than to minister; confident alike that the general interest of the world at large was in favour of all their wants being supplied; and as they could, at a pinch, get all that was needful among themselves, they did not require to trouble themselves about any other nation. America wishes, of course, to live at peace with all the world; but she has lived hitherto as if it could be no one's interest to have a quarrel with her. And yet, what mischief might have been done, at any time, by a sudden surprise, and raid upon New York, before a force either by land or sea, could have been got together to resist! Having no colonies to defend, like Great Britain or Holland; not being encompassed by jealous and unfriendly nations, with great standing armies, like France, Germany, Italy, and every state on the continent of Europe, she has lived at ease within herself, and taken no count of apparently remote consequences, resolute only neither to make nor to meddle with quarrels that were none of hers. She appears, however, now to have come to the determination that her navy, at least, must be strengthened so as to protect her interests abroad and defend those at home.

Certainly this absence of combativeness—in a national sense—is characteristic of the American. The Civil War of 1861 shewed that, fit occasion being given, there was no want of courage in him, or even of the war spirit, with all its good and evil, its destructive and constructive issues written in blood. But has there ever been in the whole history of the world before this an instance of a nation excited to a fierce and terrible combat by internal commotion, taking up and laying down arms with such marvellous equanimity, and in so short a time? Just think what a harvest of hatred, perpetuated through whole generations and passed down from father to son as a kind of sacred legacy (like Hamilcar's to Hannibal, or on a smaller scale, like a Corsican or Sicilian vendetta) might

have sprung out of that mustering of the Southern against the Northern States, with Slavery as a cause of quarrel at the back of it! And then, look at the fact, as we now know it—armies disbanded, generals and colonels innumerable, even though flushed with victory, returning into private life without regrets and without vain boasting; a vast war debt incurred and in a few years completely cleared off; and almost nothing remaining to shew for the heart-rending and bloody struggle except the monuments of dead heroes in every town, and the honorary titles of those who survived the fight, and who had risen to military rank. I call that a sublime, and so far as I know, an unique fact in history; it gives to the citizen of the United States, apart from all questions as to the cause of the combat, the right to say that he belongs at once to a courageous, and a peace-loving nation. The partizans of the South in our country—among whom, by the way, was Mr. Gladstone—may now rest satisfied that the South (Slavery being out of the way, and the triumph of the North complete) is as loyal to the Union as if she had never risen in rebellion. On the other hand, the North has no cherished grudges against the South. All the differences and jealousies that there are or will be, apparently, will be between the old-world, Dutch-built New York, and the young, upstart, go-ahead, recklessly money-making and money-spending Chicago?

I will not venture to ask in what European nation, or whether in our own, any similar merging of political differences on so large a scale, in favour of the verdict, or apparent verdict, of the nation at large, could possibly take place. I only wish here to take note of this element of placableness (to coin a word for it) in American politics and American character. It is a considerable element to the good, in a system which we must allow—which the Americans themselves freely allow—to be extremely imperfect."

Leaving New York for Boston on 30th September, Gairdner proceeded by Newhaven, New London, and Providence, greatly delighted by the glimpses obtained of the coast and

of Long Island Sound. On the following day he visited Harvard, being entertained at luncheon by Mrs. Asa Gray, widow of the late eminent botanist, whom he had met at General Loring's, and who had pressed him to pay her a visit in October. Dr. Morill Wyman of Harvard University and Professor Jackson, a nephew of Mrs. Gray, were both present, and in the afternoon Professor Jackson conducted Gairdner over the University. In the evening he dined with Mr. Samuel Eliot, a cousin of the distinguished President of Harvard University, with whom Gairdner records that he had much delightful converse.

On the 2nd of October, he lunched with Dr. Vincent Bowditch at the Somerset Club, and found that Lowell had been entertained there not long before, on the occasion of his birthday, on which Oliver Wendell Holmes recited a really admirable tribute, beginning " Who is the critic ? " The original of this poem, which was published in the *Atlantic Monthly*, is framed and hangs in the Club, in Oliver Wendell Holmes' own handwriting, where it is an abiding source of delight to all the members. Gairdner remarks :— " It is a poem which will live, altogether apart from its being a dedication from an immortal to an immortal in the presence of a company worthy of hearing it, and loving both men on account of the qualities commemorated so well."

It is a matter of pathetic interest that Lowell died on the very day when Gairdner arrived at New York, and the evening papers of 12th August, as well as those of the next few days, were full of tributes to his memory—both in America and in England. Gairdner dined, on the evening of the same day, with Dr. Henry Bowditch, in his country house, in company with a large number of doctors, as his visit coincided with the meeting of one of the regular dining clubs :—

"The mention of these two Bowditches—the son and the nephew of the very old Dr. Henry J. Bowditch—leads me to say here that it was judged better for me not to see the latter, on account of his state of physical and mental weakness. My relations with Dr. Bowditch, Senior, were of a kind that had led me to look forward to the pleasure of meeting him. He was in Edinburgh in 1861, and he then communicated to me personally, in hospital, his views and practice with regard to what has since been called 'aspiration' in pleurisy with effusion. I adopted the practice and was, I believe, one of the first in our country, or indeed in Europe, to adopt it, as may be seen in my 'Clinical Medicine.' Ever since then, Dr. Bowditch and I have been occasional correspondents and fast friends. When I arrived at first, he was in summer quarters some miles away from Boston, and although he was very desirous of seeing me, after conference with friends I judged it better not to go, considering his advanced age and infirmities. I had a most affecting letter from him, acquiescing sorrowfully in this, and yet expressing a hope that 'somewhere or somehow'—we still might meet."

On the 3rd October, Gairdner dined with Mr. Charles Eliot Norton, Professor of the History and Philosophy of Art in Harvard University, and on the following day (Sunday) he availed himself of the opportunity of hearing a sermon in his own church by Dr. Phillips Brooks, who, as already mentioned, had recently been appointed Bishop of Massachusetts. He was delighted with the sermon, and recognised how abundantly the Bishop's popularity (in the good sense of the phrase) was merited. Gairdner's remarks on the good Bishop recall vividly to my recollection one of the stories related to me on one of my visits to Boston, in which his goodness of heart was prominent. Walking along Commonwealth Avenue one day, he observed a little mite of a boy, trying to reach the handle of the bell of one

of the beautiful houses in that charming street. With his usual kindness, he went up to the small urchin and said—"My little friend, you seem to be in trouble. Is there anything I can do for you?" The boy promptly responded: "Yes, please Sir, will you ring the bell?" The Bishop at once gave the handle a pull quite in keeping with his athletic proportions, and then, turning to the small boy, he patted him on the head and said—"Is there anything else I can do for you?" To which the little urchin replied—"Yes, Sir, you can run like Hell; I'm off." No information has ever reached me regarding the nature of the explanation which the worthy ecclesiastic must have felt bound to offer for his unintentional call.

Gairdner spent the 5th and 6th of October again in New York, winding up such business matters as required attention, in order to be ready to sail on Wednesday, 7th October, in the "City of Berlin." He makes some remarks on the Republican and Democratic parties; on exports and imports; on Protective Tariffs and Free Trade; in connexion with which he reveals quite frankly the feeling of difficulty which all of us of the Motherland experience in the United States when we attempt to understand the two chief parties in the political world.

"I chanced, when in Vermont," he says, "to be in company with Senator Evarts, formerly a distinguished Secretary of State, and at one time a lawyer in very active practice in New York, who may, presumably, be regarded as thoroughly acquainted with the whole matter. I put to him the question (speaking as a stranger, wanting enlightenment) 'Since the War, and the great issues there finally decided, what are the principles which can be said to divide the Democratic from the Republican Party?' His answer was epigrammatic, and was that of a thorough-going and confirmed Republican—'Sir, the Republican Party were first on the

ground, and they got all the principles; the Democrats only came in for the leavings, in so far as was necessary for fighting purposes!' This, I confess, left me about as wise as I was before. It seemed a little on the lines of Lord Randolph Churchill's exhortation to his own party, when he thought them too much restrained by principles, that 'the chief duty of an Opposition is to oppose.'"

After paying a high tribute to many of the great leaders of both the American parties, as high-minded and pure-souled statesmen, and giving expression to his warm sympathy with many of the difficulties which beset their paths, he draws some general conclusions for the information of his children :—

"With this close (for the present) of my visit to America, I am anxious to leave on the minds of my children some final, and I hope pleasant, impressions of the great world of men, women, and children among whom I have lived for almost exactly 8 weeks; a very short time, indeed, to take in even impressions of so vast a country and people; but still a longer time than I have been able to devote to any holiday, or to any people, since the oldest of my boys was born. Mr. Bryce tells us that on his first visit to America he made some notes, and blotted them all out again after his next visit, and so again, in repeated succeeding visits, each of them probably much longer than mine, he found himself obliged by circumstances to change his mind, and his notes, over and over again; till at last he has succeeded in producing an elaborate work which even my American friends themselves say is more complete, and exact, and representative of American character and institutions, than any of their own books on the subject. I have no expectation that these hasty notes for my children's use can ever have any other value than the personal one of letting them know how their father thought and felt about it; and therefore I am not to be disconcerted in setting down here mere first impressions, even if it should turn

out that these are (as no doubt they are) a great deal more open to correction than Mr. Bryce's.

One thing, however, may be fearlessly stated to begin with. It has been my good fortune to be (owing to circumstances which have been already alluded to) introduced at once, not only without difficulty, but with effusion and thorough sympathy, into what I will believe until I know better, to be the very best of American society. And by the very best, I don't mean the millionaires, or the very fashionable and select people, or the most exclusive and would-be aristocratic (or plutocratic) 'sets'; but the people who by their well-regulated lives, and warm domestic affections, and profound admiration for whatever is venerable as being holy, or noble, or beautiful in itself, apart from the mere fashion or nationality of it, may be said to be 'the salt of the earth,' not only in America, but in every country under the sun. I wish I could write down here the debt I owe to these people, and the love I bear them; the high regard I must necessarily entertain for the country which has bred them, and the institutions under which they have grown up. The Rev. Dr. Macgregor, Moderator of the General Assembly, said to me before I left—'Speak straight out, everywhere, as you are doing to me; there are endless numbers of good people in America, and they will understand you.' I now know of my own knowledge that this is true.

But what is more, these numbers of good people in America have almost all—I might have said all, so far as I know them—a most warm and real sympathy with Scotland. Many of them have come among us, and even our frigid climate has not chilled the enthusiasm which they heartily feel for the country of Walter Scott and Robert Burns, whose works I found to be known to almost every cultivated American as much as (perhaps more than) to very many in my native land. A curious illustration occurred to me of this. I had taken with me the edition recently published, of the Journal of Sir Walter Scott, which in the course of last winter was the theme of countless reviews and notices, and was justly considered by Scotsmen as having even exalted the character

of that great and good Scotsman in the eyes of all his countrymen, by fully portraying him for the first time as he was, from day to day, in the later and darker days of his nobly spent life. I intended to finish my own reading of this book and then to leave it as a parting gift to some of my friends. But here the difficulty came in. My friends, at least those with whom I was most closely associated, had all got, and not only got, but had carefully read, the book already. I need scarcely say that the feelings they expressed about it were exactly those I was accustomed to at home; but it was only by adding the book to the perhaps not overstocked library of a clergyman, who did some walking with me in the White Mountains, that I was able to leave it as a suitable memento of my visit.

But I do not claim for my friends this high position in American society merely because they know and love Scotland and Walter Scott. They are mostly New England people, and therefore represent only one side of a vast and many-sided aggregate; and that, the side nearest to ourselves. Still, they are Americans, loving their own country, and standing by its institutions; and they are, and would everywhere be acknowledged as, Americans of a high class. To be taken into the heart of that class, at once, and without any previous reserves or tentative formalities, and with an absolutely assured welcome, is surely distinction enough for any man. I will not allude further to the very pathetic story out of which all these happy meetings grew. An intimacy of 45 years ago renewed, when the dear hand that I should have so enjoyed holding again, is in the grave—renewed in the persons of his wife, and sister, and their children and grandchildren—this is an experience that does not easily fall to every one. I wish my own dear children to note this much about it; that even a little seed of true love—ἀγάπη—is never entirely cast away; it grows and multiplies even while we are not regarding it, and sooner or later, in this world or the next, in God's good time, it blossoms out into something that is perhaps more lovely and more exquisite, the longer it is hidden or neglected in appearance.

One thing that nobody, even the most superficial observer, can fail to notice, is precisely the abounding vitality—this almost unmeasured and immense force of life that is in the American people. For good or for evil it must lead them a long way yet, as it has led them a long way already. When the folly and, as it seems now, the wickedness of our rulers in the time of George III. led them to engage in that fratricidal war which ended in the American revolution, did either they, or we, for a moment suppose that in severing the bonds which up to that time had made them British, we were leaving them, in effect, the best part of a continent to expand in? Did William Penn ever entertain such an idea, when he bought from the Indians, amicably, his little tract of land wherein to found his Quaker settlement? or did the Puritan fathers who sailed in the 'Mayflower,' and thus originated New England, ever dream of a posterity, however remote, to whom should be confided an empire of 63,000,000 of inhabitants, spread over a territory greater than the whole of Europe, and with climates more varied, mountain chains more extensive, rivers larger, and all supplies for the 'relief of man's estate' more abundant and more capable of increase by cultivation, than the whole of Europe can furnish? All this the Americans have won for themselves in the course of three centuries; the most of it, by far the most of it, since the revolution of 1776. And in thus subduing the earth, to possess it, they have also made the fortunes of many other civilised nations besides their own. Englishmen, Scotsmen, Irishmen, Germans, Italians, French, Poles, Hungarians, Bohemians, have all crowded at various times into the United States, and in two or three generations have ended by becoming simply Americans, and have prospered in so doing. With certain exceptions, it may be said that at this hour all these various races are assembled under one banner in the U.S., live peaceably together, and have no desire to be other than they are. Is not that a marvellous fact, and does it not speak volumes not only for the power of life, but also for the capacity of good, in the American people? and is not the English

language, which they and we alike speak, a bond of union which, as spoken in our colonies and in their manifold and ever-expanding nation, may one day or other bring about a brotherhood of English-speaking men and women over nearly the whole earth ?

I find this English-speaking race, or combination of races, in America, honest, prosperous, law-abiding, and living at peace among themselves, and with all foreign nations. One of the greatest civil wars of modern times threatened to rend it asunder, 30 years ago ; in that war was involved an issue far greater than that of most European wars ; nothing less than the issue of negro slavery, an evil transmitted down for generations, and into which hundreds of thousands of slaves had been born and brought up, and died, so that it had grown into the habits, and had tinctured the most cherished beliefs, of the whole of one side in that great contest. And yet, after the war, the southern armies being destroyed, and the negroes all set free, the northern armies were almost immediately disbanded, and returned into the general population. The war debt has been paid off, voting has taken the place of fighting, the Confederate States have returned to their allegiance, and whatever individuals may feel, there is no whisper of general disaffection or of a new rebellion. Nay, it has been stated to me—but I admit without the means of verification—that in some of the Georgian plantations twice as much is produced under free labour, as ever was known under slavery. The individual proprietor may have, probably has, been subjected to great losses by the emancipation ; but the people at large, even in the Slave States, have gone on prospering and to prosper.

I have spoken above of the placableness of the American character, speaking of him not as an individual, but as a nation. This happy ending of the civil war is perhaps the most remarkable, indeed the most surprising instance of it. But I have mentioned this before, and I will now add that, take him all in all, the American citizen is devoid of any feeling of offence, or quarrelsomeness towards any nation in Europe. For this, there are at least two good

reasons. One is, that he really does not care much for what goes on in Europe. He willingly lets us go on our way, if we will only let him go his way. He has plenty of elbow-room at home, has no colonies to speak of, has only lately awakened to the idea that he must have a navy, and only keeps an utterly insignificant quota of soldiers, in case of any unlooked-for internal disorder. Why should he meddle in quarrels that concern him not? The other reason is that he is really too busy to be quarrelsome; and his business lies in managing, and peopling, and cultivating, his own vast territory. Whenever, therefore, any friction arises between Jonathan and any European power, Jonathan is pretty sure to have his way in the end, right or wrong. He is perfectly confident that nobody wants to get into a war with him, and equally so that he does not want to get into a war with any body. He therefore goes on arguing the point till both sides are tired out, and he then proposes arbitration, as in the Alabama case, or refers it to the courts, as in the case of the seals in the Behring Sea, and generally manages to get a considerable slice of what he wants, with which he is satisfied and at peace again.

The American is said to be occupied unduly with the things of this world, and to that extent is sometimes called materialistic, but he is not, I think, in the grain and texture of him, irreligious; on the contrary he takes to all manner of religions with a sense of equality, or indifference (in the old English sense of the word) which might be construed into and perhaps sometimes is, indifference also in the more modern sense, but is more charitably, and more truly, explained by his fundamental attitude towards all institutions. He is accustomed from his infancy to regard the rights of the individual as sacred against all political combinations, so long as he does not encroach on the liberty of others. He applies this same principle and measure to religious associations; every form of persecution, or even intolerance, is hateful to him. But wherever you go, east or west, you find abundance of churches, and every sign that in some way or another, God is worshipped,

no doubt as devoutly and as sincerely as elsewhere. Eighty-two years ago (1809) I find as good a Liberal as Sydney Smith concerned for the future of Christianity in America, because of the want of an established church :—' We have now too much reason to believe,' he writes, ' that the system of greater latitude, attempted naturally enough in the New World, will end fatally for the Christian religion, and for good practical morality ' (*Sermons*, vol. ii. p. 96, London, 1809). His fears have proved to be baseless.

I am not certain that the average American is quite as domestic in his habits as the Englishman or Scotsman ; all I can say is that in the families to which I had the pleasure of being admitted, I could perceive no essential difference. It is said that the custom of living in hotels and boarding houses interferes with the cultivation of domestic habits in the towns. I shall not, however, write more on a subject which is beyond my experience and means of observation. On one point, there is a general concurrence of testimony in Jonathan's favour. His respect for womanhood amounts to a kind of high chivalry. I am told that in no society, even of roughs, can any woman be insulted or abused in any way with impunity. Some one will be sure to take her part, and mete out summary justice, which in certain aggravated cases in the remoter districts, may take the form of lynch law, stringing up the offender to the nearest tree. But while this great and general feeling of tenderness and chivalry towards the sex certainly exists, I do not learn that the ' woman's rights ' movement, and especially the woman's franchise in state or in federal elections, is at all so much favoured as in our country. Those very ' advanced ' ladies, whom *The Saturday Review* very impolitely calls ' the screaming sisterhood '—and whom we are apt to associate in our minds with bloomer costumes, excite very little attention, and according to my information, still less general sympathy, either among their own, or the other, sex. I rather think the average American simply takes no notice of the claim of advanced womanhood to equality ; and that he has never either read, or cared to

read, the writings of Mr. John Stuart Mill on the subject. He will allow any number of erratic ladies to claim to be the superior sex, and act accordingly. He even will not abate his chivalry towards such self-asserting women, being confident that as long as policemen and soldiers form part of the machinery of government, his claim to be at least the stronger sex will not easily be disputed!

And now I think I must close these notes with a general expression, sincere as far as it goes, but inadequate as far as my own feelings are concerned, of the quite extraordinary and generous hospitality extended to me in America throughout my visit. Scarcely had I landed, and presented my first letter of introduction (from my cousin William Tennant) to his friends Mr. Leech and Mr. Blake, when I found at once every assistance that it was possible for a stranger to have. The heat being extreme, I was at this time only anxious to get away again as speedily as possible to Newport, but one more day in New York being unavoidable, a young member of their firm was detailed to look after me and shew me as much of New York as could be compressed in that one day. Everything, in fact, was arranged for me; and Mrs. Blake being at the time in Newport, she was written to, and was to call upon me at the hotel, in case I wanted any guidance there. At Newport, however, I had other friends, already alluded to, who occupied me entirely for the first two days (except a drive with Mrs. Blake in the afternoon of one day); and then Dr. Pepper took me up entirely for several days more. At Boston I was again taken in hand by some of my medical brethren; and had also to go down by invitation, for a Saturday and Sunday, to General Loring's delightful seaside house at Beverley. On returning I met Dr. Fitz, at the station, and was placed by him in the way of spending two days more in Boston, after which Mr. Edward C. Perkins at Windsor, Vermont, and Mrs. Cleveland in the White Mountains, successively took me in hand; and I passed from them to Bishop Doane, of Albany, and Mr. Ed. N. Perkins at N.E. Harbour, Mount Desert. All this enabled me to tide

over with some equanimity that terrible 'hot wave' which had beset me in New York and Boston, and really was next to intolerable there, for my Scottish constitution. At Albany, I encountered Mr. Edward Bowditch (a son of the veteran Dr. Bowditch, but himself a man in business) and dined with him and his partner; at Elmira, Dr. Hamilton Wey and his father claimed me, and the former was my guide for a whole day to the great state reformatory, of which he is physician. At Philadelphia (after visiting Niagara and Chicago) I had attention and kindness from Mr. W. P. Tatham and his household. In Washington, during the week of the Congress of Physicians, I was most munificently entertained, as were all the other guests from our country; the only difficulty in my case being to know whether I was the guest of the President of the Congress, Dr. Weir Mitchell, who wrote to me from Rome in the spring, or of Dr. Wm. Pepper, the President of the Medical Section, who invited me from Philadelphia shortly before. I was obliged to leave it a matter for arrangement between them, my quarters being with both of them at 'La Normandie' Hotel, where, like all the other invited guests I found at the end there was no bill to pay! At Baltimore, we were received in the Johns Hopkins Hospital by so many confrères that I cannot make invidious distinctions; on returning to Philadelphia my friend Dr. Pepper took me up once more for a day at his own house (where it was necessary to do some writing for the Transactions of the Congress). At New York again, I found my friends Mr. Leech and Mr. Blake, who arranged for me the visit to the Hudson, as mentioned above; and finally, on my closing day in New York, having sent my baggage on board from the hotel, I had to lunch at Mr. Leech's house in Brooklyn, where I had the pleasure of meeting his sisters again, and seeing Mr. Blake's two pretty little girls, whom I had not managed to see in Newport. I must not omit to refer again here to that dear old man of true genius, Dr. O. W. Holmes, my visit to whom was one of the real gems of my American experiences. He gave me as a keepsake a

little 'birthday book' founded on selections from his own works, culled and put together by a lady admirer. It was a most graceful leave-taking on the part of the veteran (whom it is only too likely I shall never see again); it enabled me a few days later to discover that his 82nd birthday occurred on August 29th, just in time (I hope) to allow of my anticipating it by a letter.

Of all these friends it may be said that their friendliness knows almost no bounds; and I should be the most ungrateful of men did I not remember it to the end of my life. But all this expresses but feebly the real help I derived from their friendship. By thus welcoming me into their home circles, I was made, for the time, a part of themselves, and thus was admitted freely into other society and other circles in which further experience was gained, and the brightness of my American visit much enhanced. This happened particularly at Newport, at Bethlehem, at Windsor, and at N.E. Harbour; all of which places were, at the time, brimming over with life, and society of a kind which shewed me America at its best.

And so, I leave them all behind with my hearty goodwill and blessing, and in the hope that either I or some of my sons, may be able to repay in our own land some of this great kindness which will ever endear America and Americans to

W. T. GAIRDNER."

This seems a fitting place to introduce some letters to Dr. Vincent Bowditch, which show Gairdner's warm affection for the United States and his grateful appreciation of the kindness he received :—

"9 The College, Glasgow, April 26, 92.

Dear Dr. Bowditch,

I send you some copies of the short notice of your dear father in the Edinburgh Medical Journal, which were thrown off for me afterwards by desire of the editor. They are of no use here, as not one survivor is known to me, except myself, of his friends of 1861. It occurs to me,

therefore, that you may possibly wish to have the disposal of these copies, and if you think fit to send any of them back to this country they will come better from your hands than from mine. The only thing that occurs to me is that by sending separate copies to public libraries they might be entered in the catalogues under 'Bowditch' and so be found upon search more easily than in the Journal. This I am almost sure would be the case in our University library, and in that of the Royal College of Physicians; probably also in your great Washington library and its magnificent catalogue.

It was a great satisfaction to me to pen the few lines which led up to the extract from the Boston memoir.

Yours very truly,

W. T. GAIRDNER."

"9 The College, Glasgow, Nov. 11, 94.

My dear Vincent Bowditch,

I reciprocate entirely the kindly feeling of your letter received two days ago, and send you herewith another little contribution to the tidal flow of sympathy across the Atlantic which I am always so glad to witness and to assist as far as I can.

It may interest you farther to know that I am sending this also to an old lady whom I saw not long ago, and who spoke of you and yours with the most lively regard for your having visited her in her beautiful house at Gare Loch Head—Mrs. Macdonald, of Belmore.

With very kind regards to your sister and all friends,

I am,

Yours very truly,

W. T. GAIRDNER."

"9 The College, Glasgow, Jan. 28th, 95.

My dear Vincent Bowditch,

I was somewhat taken aback, and not a little annoyed, at finding the other day that the magnificent *éloge* of Lowell, which you shewed me on the walls of your club, and which must have been the very last—not to say the finest,

as I believe—thing said of him while he lived, is not in the Riverside edition of the works of our dear Oliver W. Holmes, published in the same year (1891); the Address at your club having been printed, I think, in the April No. of the Atlantic Monthly in that year.

This is to my mind so serious an omission that it greatly diminishes my desire of having that edition, although I have just been giving it away in presents to others.

I wonder if there is any chance of a supplementary volume or edition which will contain that very noble poem.

What a striking fact, that it should have been spoken in April in presence of the living man, by one so much older, and that on August 12th (the day of my landing at N.Y.) the younger man should have been dead, the older surviving for more than 3 years!

With kind regards and all good wishes of the season,

Believe me,

Yours most truly,

W. T. GAIRDNER."

"9 The College, Glasgow, June 20, 98.

My dear Vincent B.,

Did I not see your name, or some of the Bowditches at least, set down as to be present at the B.M.A. meeting in Edinburgh next month? The receipt of a note from you without any allusion to it looks as if it were not so. But I need not say that I should look forward with great joy to meeting you again, and would certainly wish to have you in Glasgow, only that my family will have gone by that time to summer quarters almost 100 miles away, and in a rather remote and inaccessible district in Kintyre.

Yours most truly,

W. T. GAIRDNER."

"32 George Square, Edinburgh, April 4th, 1901.

My dear Vincent Bowditch,

The marriage of your cousin the eldest daughter of Dr. Henry (conveyed to Mrs. Lattey) would itself have

been a proper subject for a letter ; which, while heartily congratulating you all, I address to you specially in order to open (or perhaps re-open) another subject on which my memory does not serve me as to whether I may not have written or spoken to you before now.

When you were kind enough to take me (in 1891) to your club in Boston, you may remember that I was particularly struck with a poem (which had been framed and placed upon the wall) by our dear old Oliver W. Holmes, then in his 82nd year, in praise of Lowell, who had been entertained at the Club early in the year, and who died just the day before I arrived at N.Y., viz. 11th Aug. 1891. It was said to have been printed in the Atlantic Monthly—I rather think in April—and I fully counted on seeing it in the Riverside edition, then in course of publication, and completed a year or two later. But, to my regret and disappointment, it was not there, nor yet in a smaller edition of the poems alone, issued late in O.W.H.'s lifetime. I wrote to him, if I am not mistaken, remarking on this omission ; and, as the Atlantic Monthly is not very accessible to me, I then rested on my oars till a friend only the other day sent me what professes to be an absolutely complete small edition of the poems only, up to 1899, from the Riverside Press. In this, I find three poems connected with Lowell, but one of them is plainly a posthumous tribute, and another quite as plainly not the one I was in search of. The intermediate one is like the one I read on your walls, but yet, I think, it differs considerably. It is at p. 376 of the 1899 edition and is dated Feb. 22nd 1889, being Lowell's 70th birthday.

The differences are rather remarkable ; for while both of them deal with Lowell in the threefold character of poet, critic, and statesman, the lines I remember as on your walls are wholly appreciative and non-aggressive, while the 1889 declamation is full of antithesis, and extremely pointed, almost bitter, in its sarcasm as regards the unworthy kind. Can you explain this discrepancy, and is the poem still extant, as spoken at your club ? and am I right in suppos-

ing that it was so spoken only a few months before my visit ?

Yours very truly,

W. T. GAIRDNER."

" 32 George Square, Edinburgh, July 26th, 01.

Dear Dr. Bowditch,

I am really delighted to have the Atlantic Monthly, with the genuine edition of the splendid tribute to Lowell by dear old O.W.H., a wonderful bit of sunny warmth and also of contrasted moral anathema—the fire that burns as well as warms—in a man of 82 years I think, or at least over 80 years of age.

I am more than ever impressed with this poem, as I was in the first hasty reading of it, in your presence, on the walls of your club.

I am just going off to Cheltenham, for the B. Med. Association Meeting, but I am afraid that, with my infirmities of sight and hearing, I shall not be able to enjoy these annual outings much longer. Still, one always meets with old pupils and unexpected faces from all parts of the British Isles and sometimes of the British Empire.

Yours most truly,

W. T. GAIRDNER."

PERSONAL CHARACTER

Sympathy will doubtless be extended to my feeling of no ordinary difficulty in attempting a summary of the character of one for many years the object of my warm affection and deep respect. Some endeavours to estimate his position in the world of medicine and science, as well as his relation to the realm of Church and State, have already been made in the foregoing pages, and all now left is to express an opinion of him as a man. The character of Gairdner was marked by absolute nobility of mind and perfect integrity of purpose. He possessed unflinching courage in the face of every difficulty, and unswerving loyalty to those with whom he was associated. Magnanimity and generosity were the out-

standing points in a character noted for its sweetness of nature and goodness of temper. These latter characteristics were not the outcome of weakness; he has been known, on occasion, when goaded by the persistent attacks of shrewish opponents—attacks sometimes resembling the irritating attentions of a gadfly—to give rein to his opinions in language of scathing contempt. But, having done so, he was afterwards perfectly willing to bury the hatchet and freely forgive the assailants, if he could not quite forget the incidents.

There never was, in truth, anyone who more thoroughly embodied the conception of the Prophet by "doing justly, loving mercy, and walking humbly." He was, as it were, an incarnation of the "Threefold Reverence." There was absolutely nothing small or mean about Gairdner. Those of us who were privileged to know him most intimately never found that, like many another prominent man, he babbled or prattled of self and pelf; and not one of us admitted to his friendship ever left his presence without feeling a better man for our intercourse with him.

There is no need, in view of the previous parts of this biographical sketch, to say any more about his width of culture and breadth of sympathy. His devotion to truth and his patience in seeking it were an example for everyone with whom he was brought into contact. His scientific enthusiasm—at least in the prime of life, and even more in his later years—was tempered by a wise restraint. It was as if he had applied to himself the lines of the greatest of German poets, who was himself a man of science :—

"So ist's mit aller Bildung auch beschaffen :
Vergebens werden ungebundne Geister
Nach der Vollendung reiner Höhe streben.
Wer Grosses will, muss sich zusammen raffen :
In der Beschränkung zeigt sich erst der Meister,
Und das Gesetz nur kann uns Freiheit geben."[1]

[1] Goethe, "Gedichte"—*Natur und Kunst.*

He was, withal, a many-sided man and strayed in many fields beyond the domains of science and medicine. He loved poetry, and in his early years used to write verse. He adored music, and was himself a capable performer. He appreciated painting and admired sculpture; but, unlike his old friend, Dr. John Smith, he was contented to remain a spectator. Widely versed in the literature of many languages, intimately acquainted with the drama of modern Europe, deeply interested in politics, both ancient and modern, and quite an expert on matters ecclesiastical, even beyond the domains of Christianity, he thus presented many facets to society.

This apparently complex nature, however, was one of transparent simplicity and perfect sincerity. His was not the affected and artificial simplicity of Thoreau, but it was, on the contrary, the very essence of his being. One outstanding example of this may be found in the fact that he was quite the same, whether in the smaller world of home-life or the larger world of public affairs. He was in nowise like the shield mentioned in antiquity—with its silver face turned in the one direction and its copper back in the other. How abundantly happy he was in his home-life has been clearly shown in previous pages, and to his family, as well as to his friends, his memory is invested by a singular charm.

Humour he had in plenty, of the droll, dry type pertaining to the Lowland Scot. Although shrinking from notoriety, he could and did tell a capital story, and at such times his aspect was lighted up by an expression of thoughtful mirth. One of the stories which he sometimes told by special request dealt with the unavailing efforts of Jeffrey, and the more successful attempts of Cockburn, to obtain some information from a more than usually stupid witness from

a farm in the country. The tale dealt with a Proof, arising from an action to reduce the will of a certain Mr. Thomson, on the ground that he had been *non compos mentis*. Jeffrey, well known to have lost his Scottish tongue at Oxford, and to have picked up no English in exchange, could elicit no response from the dense farmer in front of him, who was a witness on behalf of the plaintiff, except the embarrassing reply :—" What's yer wull ? " Cockburn, coming to his rescue, remarked to the witness :—" I'm thinkin', Mr. Wilson, ye wad ken Mr. Tamson gey weel ? " " Aye, Sir, a kent him brawly," was the reply. " Ye had trokins wi' him, aiblins, at the Market ? " " Deed aye, Sir, gey an' aften." " Was he the kind o' man that micht hae coft a coo for ye ? " " God ! man ! I wadna hae lippened a cauff till him." Whereupon Henry Cockburn sat down contentedly.

One of the most amusing traits in the character of Gairdner was absence of mind, which followed him from his earliest days until the end of his life. This aspect simply served to emphasize an interesting personality, although sometimes, it must be confessed, it not merely led him, but many of us, into positions of considerable difficulty. It showed itself, perhaps, more particularly in certain vagaries during his attempts to reach country districts, whither he had been summoned on consultation, and it must be admitted that the feeling of uncertainty as to the probable time of his advent interfered to a considerable extent with the ordinary routine of many a general practitioner. Even after a telegram had been dispatched from Glasgow, announcing that the Professor had started, it was by no means certain, let us say, whether he would arrive at Kilmacolm or Kilmarnock. The famous story of how, when trying to get to Strathaven, he arrived at

Hamilton, in the Lesmahagow train, when he ought to have turned westwards at Blantyre Junction, has often been told by himself; he keenly appreciated the humour of the situation, when, two hours later, repeating the attempt to get to Strathaven, he, for the second time, arrived in Hamilton, and was greeted by the same porter who had put him right before with the exclamation :—" Guid God ! is this you again ? " Dr. Bruce Goff of Bothwell has told me that, on another occasion, Gairdner, on his way to see a patient along with him, had in some mysterious way been lost, and arrived, as it were casually, late in the afternoon, announcing to Dr. Goff that he was sorry he would not have very much time, as he had to be back in Glasgow at half-past seven to entertain a party of twenty doctors at dinner. Dr. Goff went into the booking office, in order to inquire about trains for Glasgow, and, just as he emerged, he saw, to his consternation, the Professor jumping into a train which had actually started on its way to the South—away from Glasgow altogether !

This absence of mind was never attended by any of those grotesque perversions of phraseology such as rendered two other academic celebrities so conspicuous. He never could by any chance have made the ridiculous blunder perpetrated by the Aberdeen Professor in describing the last moments of a serious case, and who in doing so said—" Gentlemen, my poor friend was just able to say a pint of porter and swallow the Lord's Prayer before he passed away " ; and great as were the trials to which he exposed guards, porters, and other railway attendants, he was never known, like the Oxford Don, to describe his luggage as " Two rags and a bug." On his travels, with all the innocent eccentricities and inevitable uncertainties referred to, he never reached the high-water mark of a certain Edin-

burgh Professor, who is generally believed to have sat for the best part of two hours in a cabman's shelter, and then to have plaintively complained that, although he had recognised the Edinburgh tramway car to be like the crawling things mentioned in one of the books of Moses, this was certainly the slowest that he had ever entered! It need hardly be added that Gairdner has never been accused, like William Clerk of Eldin, of having asked a night policeman to inform him where William Gairdner lived. It has further not been recorded that, on his wedding day, he emulated a former distinguished Professor of Oriental languages in Edinburgh by going to bed instead of changing his clothes, in view of the prospective ceremony.

Sometimes his wide acquaintance with literature and science was somewhat interfered with by slight eccentricities of an otherwise most retentive memory. On one occasion, after the appearance of an appreciative criticism by myself of a modern work, in the pages of the *Edinburgh Medical Journal*, in which some illustrations were culled from one of Stevenson's masterpieces, the following post-card reached me :—

" Dear G.,

I have sometimes had to admit a greater acquaintance with modern literature in you than myself. May I ask what you mean by ' Christian diet ' and who was, or is, ' Ben Gunn ' ?

W. T. G."

Occasionally—in later years especially—he might converse with someone for quite a long time without recognising whom he was addressing. This was most amusingly shown by an anecdote related to me by Dr. Philip, who succeeded me as Secretary to the Edinburgh College of Physicians when Gairdner was President. At a medical dinner party

in Edinburgh, Dr. Philip was seated next to the President, and had a long and interesting conversation with him. The following day he received a post-card from Gairdner, asking him, as Secretary, if he would kindly find out who had sat beside him at table the night before, adding that he did not think he could be a doctor, as they had sustained a very interesting conversation for the best part of an evening and never mentioned any medical subject!

As was only natural, stories illustrative of this endearing trait of his character found ready currency among his followers. The authenticity of these anecdotes could not always bear close investigation. To this category must be assigned a very amusing tale dealing with the marriage of Miss Gairdner to Professor Sutherland. The engagement had received the parental sanction and blessing, and on the following morning Dr. Sutherland met Gairdner in one of the corridors of the Western Infirmary in Glasgow. The Professor greeted him as they passed, and then, turning suddenly, said—"Oh, by the way, Sutherland, you will be glad to hear that my eldest daughter is engaged to be married." The recipient of the information was necessarily staggered for a moment, but, promptly righting himself, he remarked that he was delighted to hear the news, and mischievously inquired—"May I ask who is the happy man?" "Well," said Gairdner, "you must think it very stupid of me, Sutherland, but I cannot for the life of me remember who it is."

Enough has been said to show that Gairdner was a man in whom the highest intellect and the simplest nature were combined with the purest character and the sweetest disposition. He exercised in virtue of these gifts the greatest influence upon the medical profession of Scotland, and this in the most beneficial way, by elevating its aims, enlarging

its sympathies, and widening its interests. He combined in himself all the personal traits going to make up what Sir Walter Scott has so well termed—" The pleasing vision of the highest talents united with the kindest temper," [1] while upon the minds of all who knew him well there has been left, above and beyond all this, the wholesome example of a life devoted to the good of man.

[1] *The Journal of Sir Walter Scott*, Edin. 1891, vol. ii. p. 177.

GENERAL SUBJECTS

ON THE STUDY OF MEDICINE AS AN ART

An Introductory Lecture to a Course of Practice of Physic, Edinburgh, November, 1853

HAVING indicated the plan of the course, I wish now to offer you a few condensed observations as to the spirit in which you should enter upon the studies of this class, and the difficulties and dangers which you are likely to encounter in becoming accomplished Practitioners or even accomplished Students of Medicine.

Remember that the art of medicine is one eminently requiring the exercise of a calm judgment both as to its means and its ends. It is an art which has in every age been vitiated by the multiplied and lamentable errors of presumptuous dogmatism, honest ignorance, and misdirected ingenuity. The apparatus of nostrums, of vaunted specifics, and would-be-universal remedies, is so great as to make it surprising that almost any disease should resist such a multitude of means. You will find it difficult or impossible, at first, among the discordant testimonies of various ages, times, and persons, to see your way clearly in the labyrinth of opinion; and you will, if I mistake not, be much inclined to throw aside all human evidence as interested or fallacious, and retreat from possible or probable error into universal scepticism as to the therapeutical part of medicine. If you are disturbed by this tendency, so hostile apparently to the true function of the physician, I shall not oppose to it

any direct argument. It is necessary for you to doubt, in order that you may come to believe; and *believe you will*, so soon as you again resume your inquiries amid the anxious and absorbing responsibilities of medical practice, unless you determine to smother both doubt and belief in a most culpable indifference towards your patients and yourself. The history of multitudes of generous minds—I might say of all generous and strong minds, includes the story of much and frequent doubt. I desire, therefore, by no means to discourage in you a rational scepticism. Only, do not generalize it into a formula: do not say to yourselves, "I doubt *everything* I have been taught"; and if you at times hear the evil whisper of Pyrrhonism, do not rest in it. Let your doubt be followed by renewed inquiry; let the anxious craving for secure truth never rest in you. I do not think you will in the end be disappointed; and it is far better that you should begin practice with a few remedies and principles, of which you believe you have sufficient evidence, than with a whole chaos of formulæ which stand equal and undistinguishable before your judgment. I do not hesitate to confess that I greatly respect the sincere spirit of scepticism, when accompanied by earnestness and generosity of character; and I do not think that under these circumstances it is at all likely to mislead you.

It is quite true that there is a class of apparent sceptics in medicine in whom the understanding and the judgment; if not the moral sense, appear to have suffered by their regarding everything from one point of view. There are men who view all things as alike true and alike false;—to whom the venerable dogmas of antiquity, and the passing theories of the hour, are nothing but convenient and empty formulas—convenient, indeed, chiefly *because* they are empty. But these men, though always doubting, are also perpetually interfering; to the great detriment of nature's beneficent operations. Their fertile imagination readily supplied a name for every disease, and a remedy to meet every name. The gap between their real belief and their

assumed principles of conduct is too wide to be filled up; and they have long ceased to desire that it should be filled up. The genuine doubter is anxious for light and oppressed with responsibility; but these men find comfort in the instability of medical theory, chiefly because, as "practical men," they make a point of adopting every new fashion in therapeutics; which they do, not indeed with conviction, but without serious self-condemnation. "After all," they will tell you, as an excuse for flattering the prejudices of their patients—"it is of little consequence what you do—nature takes her own way, and patients get well as soon under one treatment as under another." An excellent logic for the lazy and incapable; and not less so for the confirmed charlatan. Goethe has, with admirable knowledge of the world, put this sentiment into the mouth of Mephistopheles, that subtlest of poetic fiends, when he represents him as donning the professor's robe, the better to assail the virtue of the student. The sentiment is indeed truly devilish, when acted on in the spirit I have indicated. Let it not, then, be yours. For if it were true that all remedies and all modes of treatment were upon a dead-level of inefficacy, then indeed there would be only one course open to you and to me—to retire together from the care of the sick as a profession. But it is not true, and those who put forward this opinion do so, commonly, as a shield for their own indifference or carelessness. The circumstances under which one treatment is better than another are indeed difficult to follow, and no mere dilettante trifling with medicine, or reliance upon hearsay evidence, will enable you to appreciate them. But at the bedside many things become clear which are obscure in the schools, or amid the wrangling of authorities, in the clouded atmosphere of opposing systems. Do not despair, then, because you cannot generalize all that you hear into a consistent whole. There must ever be conflicting opinions, so long as men carry cherished prejudices into the domain of fact; for men are always apt to forget that nature has an inexorable logic of her own, which has no respect whatever for finely elabo-

rated hypotheses. Be you, for your part, content, for a while with small fragments of truth; and, in the end, you will find they are not small, but of very vast importance to your own happiness and that of your neighbours.

Those who perpetually insist upon the uselessness of medical practice, appear to me generally to forget one thing. (I refer at present to the honest doubters, and not to those who make their doubts a reason for succumbing to all fashionable theories and new remedies.) Honest men, who feel strongly the uncertainty of medical science, forget that out of that very uncertainty arises the occasion for a skilful guide in sickness—not necessarily to apply remedies; perhaps, in the majority of cases, to prevent their being improperly applied. For your doubter, when the fever-fit is on him, is the very man who will torture himself to death under the idea that he has not done what it is right for him to do. Is it nothing, under such circumstances, to have the moral support of a calm, disciplined, well-balanced mind, which can apply itself to all the circumstances of your case, measure its danger, calculate, so far as may be, its issues, and prepare you for death or for recovery? They are greatly in the wrong who disparage the physician for doing nothing. He well knows that to do nothing is often his highest art; and regrets to find himself, in obedience to the anxieties of friends, or of the patient, occasionally lending his sanction to practices which his better judgment would lead him to avoid, or to regard with suspicion. Have you ever considered what a responsibility is implied in doing nothing, when fear-stricken humanity calls out for succour? How are you to meet that responsibility if you have not the confidence of your patient, to say nothing of confidence in yourself? How are you to possess either the patient's confidence, or your own, unless you have carefully considered, and honestly doubted, and out of doubt and serious reflection have evolved a well-considered course of conduct?

It is the characteristic of quackery, as it is of routine practice of all kinds, never to doubt; and this from two

causes. In the first place, with many persons, absence of doubt is merely absence of thought. In the second place, it is not profitable, in the direct and tangible sense of the word, to indulge in sceptical reflection. It is not the less, however, your duty to follow out such negative trains of thought. It is a duty imperative, difficult, dangerous; but one which carries with it its own great reward, to a well-educated and honest mind.

While you continue to doubt, then, of many things in physic, you will take care never to *rest* in doubt. Do not suppress the spirit of scepticism; but cultivate by the side of it the habit of earnest inquiry, searching analysis of evidence, strong and determined effort after truth. It is indifference and feebleness of character, or something worse than both, which render doubt injurious to the practitioner of medicine.

A far more noxious error than scepticism is credulousness. The spirit of doubt, besides being the natural portal of sound belief, is essentially honest in its origin; and in its results it is at least not *positively* dangerous. But lightmindedness, or easiness of belief, if not the offspring of dishonesty, is a habit that leads by a short and speedy route into the snare of quackery; and the world is very apt to be with the credulous man, and against the doubter; for which reason the whole host of routine practitioners, not to speak of arrant quacks, are careful to maintain a large appetite for empirical novelties in practice, as well as for specious and popular theories of disease. For these reasons I am anxious to warn you against entertaining too readily either the one or the other. But there is no specific against the credulous temper except the calm and pure love of truth, which should therefore be cultivated at an early period, before "the cares of this world, and the deceitfulness of riches, and the lusts of other things entering in," have time to choke this good seed and to render it unfruitful.

There are many forms of delusion both in theory and practice, the reception of which depends evidently on this lightmindedness and credulous carelessness in the search

after truth. Some of them you may know by their pretensions to exclusive originality; others by the militant spirit in which they oppose themselves to the whole body of recorded medical opinion; others by the vaunting and puffing of a particular name which heralds their introduction. But too many errors also come under the modest guise which truth herself generally assumes, to allow of any absolute rule being laid down for their detection.

There are in the present day many pseudo-medical systems, each of which presents certain alluring peculiarities to those who are not possessed of the stability of mind which springs from a proper intellectual and moral discipline. I might direct your attention to each of these in turn, pointing out its peculiar fallacy, and its relation to the medical art. But this would be to presuppose your acquaintance with that art itself; a supposition I am evidently not at liberty to make at this stage of our mutual acquaintance. Besides, I have always been of opinion that in medicine, as in all things which are of positive and real interest to mankind, it is better to *displace* what is false and fleeting by the sedulous teaching of that which is permanent and true, than to fritter away time and opportunities that ought to be devoted to the advancement of knowledge, in superfluous medical controversy. Those who indulge in frequent polemics are too often persons who are pretty well determined not to be convinced; and I cannot imagine a more ruinous habit for the honest medical inquirer than to be associated early in his career with this description of persons. On the other hand, it would be wrong not to admit that there are cases in which medical controversy, like all other controversy, has been of material service to the cause of truth. It is so then, and then only, when men of cultivated minds have brought them to bear upon the discussion of great principles; and where either mutual respect has prevailed between the combatants, or where on one side, if not on both, the search after truth has been the paramount and distinguishing motive. I shall not, then, condemn controversy; but there is "a time for everything under

the sun." I should deeply regret it, if in after life you recalled this course of lectures as having fostered, to any considerable extent, those jarring and disputatious elements which lie hid, I suppose, in every human character in greater or less measure. I would rather have it said that I had aimed at telling you simply and clearly what I thought to be true and useful; giving you at the same time good and substantial reasons for those things which I felt bound to teach, but trusting to your own judgment, when matured by experience, for the overthrow of all false systems and general theories. The struggle with quackery and falsehood would be hopeless indeed, if it were necessary to attack the monster in detail; for each hydra-head, when removed, seems only to become the parent of another, or haply of a multitude worse than the first; and so long as medicine is imperfect and obscure, so long will there be found men willing to play fast and loose with its most recondite principles, and to hold out, to such as will trust them, the most unwarrantable expectations. Something, however, may be done to inculcate from the first such habits of thought as may dispose you rightly to apprehend the limits of medical truth, and thereby lead you to discriminate between those plausible systems which everywhere seek to arrest your attention, and that less obtrusive, but more valuable body of medical doctrine which ages have combined to build up; which is ever being revised and reformed, but which has hitherto defied the spirit of revolution.

What is, and what is not, the true practice of medicine? My answer to this question is, of course, the entire series of instructions that I shall have to address to you from this place; but I may endeavour to comprise here, in a few sentences, some of the more important characteristics of that medical teaching which, in my opinion, best deserves your attention.

In the first place, the true medical art refuses to be compressed into a formula, imprisoned in an epigram or an aphorism. As the duty of the physician consists of nothing less than the application of the whole faculties of the human

mind, and the entire resources of science to a vast variety of dissimilar problems, it would be strange indeed if any single remedy or any single form of words exhausted his beneficent powers. He has for his purpose to do good to the frail body of man under a multitude of complex conditions; and to enable him to relieve it from the ills which oppress it, "the thousand natural shocks that flesh is heir to," he has at his disposal an armoury of means almost co-extensive with those which beneficent nature has placed at the disposal of man for his support and nourishment. For it may be said of almost everything that works upon the body, that it has a working for good as well as for evil; and that even the most powerful of poisons, when applied as the instructed hand can alone direct them, become the most energetic of remedies. In treating an individual case of disease, however, the wise physician does not limit himself to such remedies; heat and light, air and water, food and clothing, all the natural restoratives of the body, all the habitual stimulants or sedatives of the mind, become the weapons with which he makes war against those evil influences that weigh upon the functions, and of which it is his duty to investigate and to remove the causes, to foresee and to avert the results. To a man who takes this view of his art, medicine is a high and great vocation, giving scope for all the powers of the mind, to all the emotions of the heart. With what an indescribable scorn must such a man look upon those who would reduce his art to a set of technical rules; stamp it with a nickname; degrade it to a mystery and a trade. How must the true physician despise the attempt to catch the ear of the fickle public by any exclusive practice, by any nostrum or any form of words peculiarly his own! With what calm and assured confidence will he take and preserve his high position; secure that while systems and nostrums have their day, the art of medicine remains for ever!

In the second place, the true medical art is too great and too free to be placed under the protection of a great name, or confined within the boundaries of a sect. This,

indeed, follows from what has just been said. For, if no system or special method of cure can be said to impose limits upon the function of legitimate medicine, so no individual, or series of individuals, can fairly pretend to have been the originators or exclusive possessors of the gift of healing. The history of medicine is in this point of view a truly instructive study, for it shows that while many strong minds have attempted to bind up medicine into many permanent systems, the expansive spirit of science has soon burst the fetters imposed on it, and laid in the dust, one after another, those systems and those reputations which have been for a time regarded as all-sufficient.

The readers of Montaigne will remember a celebrated passage, in which this instability of medical systems is made a reason for an all but universal scepticism as to the resources of the medical art. A similar theme has been a common one with satirists in all ages; and every vender of a quack nostrum, now-a-days, knows well how to declaim against the astounding contradictions of medical doctrine. Rightly considered, however, these varieties of opinion, these fluctuating quicksands of medical hypothesis, only show the well-known tendency which the human mind has to outstep the boundaries of its real knowledge, and to indulge in unlimited speculation where that knowledge fails. In the fact that medical science leaves much ground open to dispute, you have a sufficient cause for the crowd of contradictory theories which arrest the attention of the superficial inquirer. The true art of medicine, however, underlies all the fluctuating doctrines of its individual professors; nay, it would not be too much to say that it owes little or nothing to those doctrines, which are in most cases rather to be compared to the parasitic plants that encumber the bark of a venerable tree, than to the juices which nourish its leaves and fruit. The true art of medicine is rarely to be found in the writings of those who have aimed at building it up into a system; for, too often, the clamorous advocates of specious fallacies monopolize the field of medical

doctrine, to the exclusion of the really wise and sagacious men by whom the art has been silently advanced. Truth to say, the great men of medical practice are often not the great writers and great talkers. Our profession is full of "mute, inglorious Miltons," men who never penned a line in their lives, perhaps, in any medical journal or book, but whose influence for good is felt over a wide circle of professional brethren. Nor are "village Hampdens" wanting to us; men who in the obscurity of country practice have studied deeply and thought accurately, and who, though not famous beyond the circle of their acquaintance, have contributed to mould the mind and form the character of a whole generation of local practitioners, and thereby effectually to advance and diffuse sound principles of art. But even among those who figure in the history of medicine we often find that "the race is not to the swift, nor the battle to the strong"; and it is very certain that the true history of medicine as a practical art never has been, and never can be, written. We know the story of medical dogmas; but of the real sources of our traditional experience and accumulated science we often know next to nothing. The best and soundest of the Hippocratic books, the *Prognostics*, is founded upon a collection of clinical records and maxims whose origin is lost in the mists of antiquity. The stupendous industry and enormous pedantry of Galen have impressed, for good and for evil, the whole history of the middle age; but the far greater and truer men who originated the pictures of disease we find in Aretæus and Cælius Aurelianus have been so neglected by posterity that we are ignorant of the very place and time wherein they lived. And so, in comparatively modern times, we find the page of history full of the renown of impudent and drunken charlatans, like Paracelsus in the 16th century, and Brown in the 18th, while many greater, honester and better men are dismissed in a single sentence, or are forgotten altogether.

On the other hand, while the history of medicine has often been unjust to individual fame, it is impossible to deny that it presents a cheering spectacle as a whole. Medical

dogma may have been fluctuating and uncertain, but the great names and great principles which have for the most part guided the course of practical inquiry, have nothing about them of which the careful and conscientious student need, even now, be ashamed. It is, indeed, impossible to contemplate without wonder, without reverence for the human mind, without gratitude for the patient application of so much conscientious effort, the steps by which the science and art of medicine have become what they are. That extraordinary combination of qualities, both moral and intellectual, by which the Greek physicians were enabled, in the first ages of European civilization, to burst the shackles of superstition, and to place the science of medicine upon a foundation of anatomical and physiological as well as clinical knowledge, has happily stood out as an example to all time. The science of medicine has never since stood still; if, in the darkest of the dark ages, the great shadow of Galen somewhat stood athwart its progress, we owe it, on the other hand, to the strong and positive dogmatism of that great man, and to his almost Herculean labours, that medicine, unlike the other departments of human knowledge, was neither overwhelmed by the torrent of Arabian barbarism, nor absorbed into the misty atmosphere of mediæval metaphysics. Throughout the middle ages we observe medicine constantly struggling, and not unsuccessfully struggling, to claim that position as a liberal art, and as a science founded on experience, which had been secured for it in Greece by Hippocrates. Its pretensions were among the first to inspire the Saracens with respect for literary culture; and in return for a host of new remedies which they brought from their eastern home, it inoculated them with that civilization and that love of letters which they had done their best to destroy and deface. The Saracenic medicine was for ages the chief element of the Saracenic culture and civilization. In like manner medicine, almost alone of all the sciences, refused to acknowledge the all-powerful sway of the church, and to be incorporated into the scholastic theo-philosophy; nay, it became, long before the

general revival of letters, one of the most potent instruments by which the human mind was led back to the study of nature on the one hand, and to the records of antiquity on the other. And so, in its subsequent career, medicine has ever claimed a highly independent position, worthy of a liberal art. All attempts, however laborious and well-meaning, to bind it up into a permanent system, have followed each other to oblivion; but the great masters of observation, those teachers who have been the "ministers and interpreters of nature," have their names written on all our hearts. Let the splendid reputations of Ambrose Paré, Vesalius, Harvey, Sydenham, Hunter, Baillie, justify my assertion; compare their present fame, with that of Fernel, or Sennert, or Riolan, or in later times Brown or Broussais (each in his own day of at least equal if not of greater fame), and confess that the true way to advance medicine is not to enclose it within walls of apparent mathematical certainty, and rigid logical completeness; but rather to gather up as they fall, the loose leaves which the Pythian Apollo scatters about in profusion for those who can read them. And so, from age to age, medicine advances, systems and men decay. Amid many changes and conflicts with public opinion, assaulted from without by ridicule and argument, assailed from within by quackery and presumption, the art of medicine presents a certain grandly progressive character, which has rendered it one of the great agents of civilization. In return for the trophies which it has won from each successive age, it has lighted and kept alive the torch of free inquiry in the worst of times and among the most barbarous of peoples.

And this leads me to a third reflection on the true art of medicine, as contradistinguished from many of its false shadows. It is a precious legacy which has come down to us from the past, enriched by many observations and means of cure from a remote antiquity; but also increased by new riches and resources which stretch down in a lengthened line to the age in which we live. It is impossible either to teach or to practise medicine in a true and satisfactory

manner without recognising this its historical character, its development through a long series of ages. And for this among other reasons: there is scarcely a disease which we can name, scarcely a remedy we can employ with entire confidence, that does not stand as a memorial of a long list of obligations to our fathers. And it is in vain to evade this debt—to say that we have made the science of our fathers our own by independent researches pursued in the more correct spirit of modern discovery. It is not so. A thousand facts might be adduced to show that we yet lie under the yoke; that we, confident and self-dependent as we believe ourselves, are yet penetrated to the very core with the medical philosophy, the medical observation, the science, the systems, nay, by the most stupid prejudices and obvious blunders of observation, of some of the ancient physicians. Thus we describe in our text-books a host of fevers, which no man of English birth and education can pretend to have seen. We call these fevers by names culled out of the most ancient medical records, and very frequently, as I shall show you by and by, we use those names in a manner very far from accurate. But with the names we acquire theoretic notions, and practical precepts, sometimes sound and good, sometimes crude and absurd in the extreme. Is it worth while, or is it not, to attempt to know with some approach to accuracy, whence our boasted knowledge and our cherished nomenclature are derived? It is not very long since medical science regarded all delirious affections of the brain, including maniacal insanity and drunkard's delirium, as inflammations, under the impression that they were modifications of the disease commonly called *phrenitis*. A very little attention to the actual descriptions of phrenitis by the ancient physicians would here have saved modern pathology from an error which it took years of improved observation, aided by morbid anatomy, to eradicate. For not only is phrenitis carefully distinguished by the ancient physicians from mere delirium, but it is plain that neither delirium nor phrenitis were associated by them, of necessity, with the idea of inflammation of the brain. Thus,

by a stupid and slipshod use of ancient words, hundreds of lives have probably been sacrificed to an erroneous pathology and practice of which even Hippocrates and Aretæus would have been ashamed.

Again, the stethoscope discovered a large number of kinds of chest-disease, which had either been latent previously, or had been imperfectly known. But no sooner were those diseases discovered, than it became necessary to give them names. They were regarded, perhaps correctly on the whole, as varieties of old and well-known diseases of the chest, and named accordingly. But it was forgotten by many that the old established treatment, set down in the text-books as applicable to these ancient forms of disease, would not apply at all to the newly discovered varieties; and many well-informed physicians, without adverting to this circumstance, congratulated themselves on having brought within the range of diagnosis, and therefore of treatment, several forms of disease before unknown. The consequence was a frightful abuse of active remedies, which has only been corrected by time, and the good sense of the great body of physicians, who refused to yield to the brilliant and seductive lessons of the modern inquirer when they did not correspond with the previous experience of ages, and the results of carefully instituted trials.[1]

Once more:—it happened that surgeons were led, by long-continued and carefully generalized experience, to recognise an important difference between tumours. Some were found to be capable of being removed with complete relief to the local symptoms, and little or no chance of permanent disease in the system. These were called *benign*: others, which had the opposite characteristics, were termed *tumores mali moris*, or *malignant tumours*. Between the two, surgeons acknowledged a doubtful or suspicious class. A careful study of tumours upon this basis of practical experience revealed that the greater number of the so-called malignant tumours contained a special morbid formation,

[1] See a paper by me in the *British and Foreign Medico-Chirurgical Review* for January, 1854.

which was denominated cancer, and was supposed to be the cause of the constitutional vice. The microscope was applied to this deposit, and after various differences of opinion and observation, its distinctive structure was supposed to have been made out. Forthwith the old distinctions were proscribed, by half-instructed zealots, as not in accordance with modern science ; the opinions of practical surgeons were disregarded and despised ; and the microscope was held to be the only arbiter of the nature of tumours, and of the fate of patients afflicted with them. But presently it appeared, by the most unquestionable evidence, that the microscopists had gone a great deal too fast in advance of the surgeons ; and that the latter, adhering to their old and despised formulas and common-place experience, were far better judges of the malignant or non-malignant character of growths than the new school, with all its aids to observation. The microscopists had, in fact, fallen into the error against which I wish to warn you, of supposing that new facts and new methods carry in themselves a dispensation from the necessity of studying older experience. They were in the end obliged to confess, that tumours having, as they believed, no character of cancer to the microscope, might nevertheless be " malignant," in the surgical sense, to the highest possible degree ; and, in particular, that a very large and important class of unquestionably cancerous structures, the *epithelial cancers* (as they are now called), had been arbitrarily and inconsiderately excluded from their natural position among morbid growths, owing to a one-sided and imperfect appreciation of their minuter elements. With this discovery, the boasted microscopic diagnosis between malignant and non-malignant tumours was, of course, set at naught ; and from having estimated much too highly, and proclaimed too loudly, their own services to surgical diagnosis, the microscopists were in some considerable danger of being discredited altogether. Through the influence, however, of some able men, who had not adopted the errors to which I have alluded, the microscope has been replaced in its legitimate position in reference to

surgery, and the schism between the old school and the new may be considered as at an end.[1]

As it is impossible in medicine, that we can learn *everything* by personal experience; and as our necessary debt to the past is so large, it is not the part of a wise physician to show a contempt for authority, any more than to pay it exclusive deference. The truth is, that we cannot have too many sources of information, or too various points of view, in a science so complicated and difficult as that with which we have to do; and as we have reason to presume that there were among our predecessors men as honest and able as any among ourselves, it is a bad sign of our own position in any medical inquiry when it demands of us the sacrifice of our reverence for the past—when we find ourselves disposed to pass indiscriminating censures upon the great men of a former age. We recognise it at once as a piece of bad taste, when any one attempts to raise his own work in our estimation by depreciating that of his living neighbour. How much more should we be careful of the reputation of the dead, who have left to us the precious legacy of their knowledge and experience, in the full confidence that it will be candidly and generously interpreted!

A habitual and studied disrespect for authority in medicine not only incapacitates the mind from profiting by the labours of the past;—it is commonly the fruit of a disposition very unfavourable to the investigation of truth. There are, unhappily, too many prominent examples in the history of medicine, in which impatience of restraint, the pride of knowing and of displaying knowledge, the vain-glorious disposition to grasp at plausible-looking results before they have been properly tested, have led men into a position of isolation, from which they have quickly learned to view

[1] Mr. Paget's *Lectures on Surgical Pathology* in this country; Velpeau's *Traité des Maladies du Sein* in France; the remarkably instructive debate on Cancer in the Académie de Médecine in 1854, in which M. Velpeau's orations, as usual, showed pre-eminent practical sagacity; and the multitudinous labours of Professor Virchow in Germany, have had a peculiarly beneficial influence upon this discussion.

the mass of medical experience as an encumbrance, the mass of therapeutical science as a delusion. From this the transition to quackery is easy : such men are indeed already on the fatal descent. They are already, in some measure, cut off from the sympathies of their brethren ; and they naturally aspire to become the founders of sects, and to seek support among the ignorant or conceited members of the public who are always ready to sympathise with them. Look at the histories of Hahnemann, of Paracelsus, and others of like character. Observe the arrogance of their bearing towards better men than themselves ; the pretensions to almost superhuman knowledge, and to quite superhuman power ; the mingled scepticism and credulity —scepticism as to other systems, credulity as to their own ; and the extravagant assertions with which they invariably think it necessary to bolster up the most fallacious promises ; the perpetual element of discord, and evil-speaking, and malice, in which they live,—and you will agree with me that such a career is not an enviable one, even if it should lead to affluence ; much less so if, as in the case of Paracelsus, it should conduct to poverty and the hospital.

The last character of the true art of medicine to which I shall advert to-day is, that it is doubly founded, first on experience, then on *reasoned* experience. You cannot hope, indeed, either to study or to practise successfully the healing art, until this, its two-fold *rational* and *empirical* character, has been correctly appreciated. The tree of medicine grows up out of the ground, it is true ; its roots lay firm hold of the soil, and may be said with truth to be founded upon the rock of experience. But its branches are raised aloft towards the sky, and are tossed about with every wind of doctrine ; its leaves inhale the breezes and feed upon the atmosphere of human thought. Hence the art of medicine necessarily takes, in some degree, the colour of the science and philosophy, and even of the popular prejudices among which it lives. It has always done so ; it will never cease to do so. The changes it thus undergoes,

the *almost* revolutions to which it is perpetually exposed, have formed, as I have said, the mark of the satirist in all ages. "Where is that solidity and stability of medical truth," he cries, "which should induce me to rely upon it?—when it is known that every new theorist overturns the theory of his predecessor, and that between Asclepiades and Themison, Galen and Paracelsus, Brown and Broussais, the medical art has found itself involved in a perpetual war which has thrown a shade of doubt over all our most valuable remedies, and, in the conflict of opposing principles, has well-nigh proved a war of extermination? Would it not be better to get rid of medical theory altogether, than to adopt it under these fatal conditions of uncertainty and revolution? Would it not be better to be content with the experience of the passing hour, than to construct a body of doctrine for our successors to laugh at?"

The objection is a plausible one, and strikes with some force, as I have already said, at the specious simplicity of those artificially elaborated systems, by which medicine has been in all ages disfigured. But because there have ever been bad reasoners in regard to medical theory, men impatient of evidence, and eager to build up large conclusions upon an unsubstantial foundation, it does not follow that the exercise of the mind is to be condemned in medicine more than in any other art. The difficulties to be encountered only show the necessity of a stricter mental discipline than heretofore in the professors and practitioners of our art. No one who has studied medicine to any purpose will ever think of identifying it with the luxuriant growth of hypothetical imaginations which has clustered around it, but from which a little care, a little knowledge, and a little time and experience, will commonly enable you to separate much that is useful for your guidance, or important as a warning against error.

In the meantime, remember, that in the double aspect of medical science—one face towards fact and experience, another towards reason and thought—you will find not only the source of its revolutions, but the source of your comfort

in the midst of them. Keep in view always that you have no business with the philosophy of medicine until you have made some effort to master its facts. Seek these in anatomy, in observation, in experiment, in reading; above all, seek them in the hospital and at the bedside. If the storm of doctrine be at any time like to blow you away, lay fast hold of the ground which is ever firm underneath your feet. In the wards of the hospital, in the exercise of those little attentions which the most obvious reason suggests and the plainest humanity indicates, you will find repose from the agitations which you are not strong enough to bear, and which often appear to threaten, though they never overwhelm the great body of medical doctrine. On the other hand, recollect that by sedulous attention to experience *alone*, medicine might have become a collection of "wise saws and modern instances"; it might have become such an art as has been practised in every age by old women and mountebanks, who ever take their stand on experience, but whose experience is lost with them, and never passes into the general sum of knowledge. By creeping along the ground, the medical art might in every age have lived out its little life as a simple annual; but it could never have become the great tree which has weathered a thousand storms, and seen the growth and decline of so many men and so many systems.

No, gentlemen, you cannot afford to throw to the winds those manifold considerations, founded on the accurate appreciation of medicine as a science, which guide and confirm the reason in dealing with the facts of direct clinical experience. You cannot throw aside that web of theory by which all rational minds tend to connect together the isolated phenomena submitted to their consideration. And for this, among other equally cogent reasons: that the choice is not between *pure* theory and *pure* experience, neither of which ever led to anything in medicine or in any other science, but between *false* and *true* theory,—between theory founded on a clear and broad view of well-observed facts, and theory which is the expression of ignorance, the work

of ill-regulated imagination, or the slavish reproduction of the opinions of others, always deteriorated in passing from one mind to another.

The same considerations which will induce you to reject a medicine professing to be purely empirical, will also show you the absurdity of that which plumes itself on being in a peculiar sense "rational medicine." This phrase has been used by some good and some learned men; but that does not make it a good phrase. To me it seems, I confess, a very shallow one. The question whether *your* view of medicine is more "rational" than *mine*, is simply the question whether your mind is more clear in its perceptions, more accurate in its judgments, more profound and searching in its analysis of facts, than mine. Nature and truth are the same to both of us, and to both eternal and immutable. To claim, therefore, any peculiar empire over medical doctrine for your reason as opposed to my experience, or to pretend that reason has more to do with your conclusions than with mine, is simply to assume that you are right, and I wrong; a process of reasoning of which no more need be said.

To conclude. My advice to you, in entering on the study of Practical Medicine, may be summed up in a sentence: Seek above all things, in a simple and pure spirit, the plain unadulterated truth. Add to the most ardent love of truth the cultivation of those gentle humanities which enable you to enter into the feelings and to soothe the sufferings of others. Care nothing about systems or about men, in comparison with these; but go on patiently seeking, and studying, and comparing and reflecting, and applying, and you will not fail to become good physicians, wise men, and useful members of society.

INTRODUCTORY ADDRESS

Delivered at the Opening of the Edinburgh Medical School, Winter Session, November, 1856.

It has fallen to my lot this year, Gentlemen, to address to you some words of welcome, of encouragement, and of advice. It is impossible to look round on this assemblage, to remark the old familiar faces, and the many new and eager ones, without the wish to say something worthy of the occasion that has called us together.

If I feel, as I do feel, that personally I am but little fitted for the task entrusted to me, I am on the other hand, strengthened by the knowledge that it is a duty which I have to perform; and that simplicity, directness, and the strong desire of doing good, will stand with you in the place of oratorical cunning.

The duty which I have to perform is partly towards you, and partly towards my colleagues, whom I see around me. On their part I have to give expression to those wishes, those hopes, and those fears concerning you, which must ever be present to the mind of honest teachers. On the other hand, I have to impress upon you that you are received here, not into the society of mere lecturers, but of men anxious and watchful, devoted to your welfare, sympathizing with your aspirations, desirous to remove your difficulties, and not deficient in kindly regard even for your weaknesses. If I can succeed in entangling you thus early within that net of intelligent

sympathies which the study of medicine weaves around all its worthy professors, I shall have brought about the highest result at which I aim, and shall have discharged my duty on this occasion.

A word of welcome, then, first of all. Some of you sit on these benches for the first time; and which of us all does not well remember his first appearance in a medical class-room? Which of you will not join me in saying to the junior student, welcome? He will have, no doubt, his misgivings. The strangeness of the scene, the separation from friendly and familiar faces, may inspire a feeling chequered with something like sadness. Yet it may not be amiss to remind you that life has other emotions, other associations in store for you; not inconsistent with those you have left, but, on the contrary, yielding to them a higher zest; lending to the joys and sorrows of the heart and home that depth and strength and steadiness which arises from their being set in a firm and manly character. You have now to begin the battle of life, to take your places in the world as men of action, or at least to prepare for this great ordeal. And, as the crusader journeyed forth into distant lands, postponing for a season the enjoyments of life and the smiles of his lady-love, till he had done deeds of valour against the Pagan foe, so you, too, seek the class-room and the hospital with no idle or careless intent, but under a solemn vow to show those who have followed you thither with their affections and their prayers, what manner of men you are. And to this ordeal we bid you welcome, not fearing the result.

So much for the beginner. To those of you who have been already initiated into medical studies, and especially those to whom this class-room is a familiar place, we trust that not many words are necessary in the way of welcome. I, for one, doubt not that the return to Edinburgh, to the toils and anxieties of the session, will carry with it some pleasing associations; something of friendships broken and to be renewed; something of work interrupted and to be resumed. The man for whom there waits no kindly

remembrance of these things ought, perhaps, to consider well whether he has not mistaken his calling. With the great majority of you, I am satisfied, the return to work is neither an indifferent nor a painful matter; and we, your teachers, desire to join in the congratulations proper to the occasion; bidding you heartily welcome in the same spirit, grave and earnest, yet hopeful and joyous, in which, we trust, you have already saluted the scenes and the companions of your labours.

A great part of the instruction which you have to receive from your teachers in this school will be conveyed in the form of lectures. Now, lecturing as a means of instruction has its advantages and its disadvantages. The advantages are, that it secures for the teacher a fitting position for the systematic development of his ideas before a large class; that it gives ready opportunities for the exhibition and explanation of specimens, dissections, experiments, analytical tables, etc., and that it does not fritter away the valuable time of the able and attentive student by compelling him to follow the painful efforts of the less gifted or less thoughtful disciple. Against these manifest advantages have to be set grave disadvantages and dangers. Such are the evils which may arise from the want of a proper understanding between the lecturer and the student; which, in the case of the *mere* lecturer, may be pronounced irremediable. Slovenly teaching on the one hand, careless hearing on the other, are sure to follow such misunderstanding. It is a grave and perhaps ruinous injury to you if you fail at the outset to follow your teacher with a certain degree of satisfaction; for the habit of inattention, thus implanted, may never be eradicated. And the misfortune to him is not small; for few men have the power of detecting and correcting their own faults; and unless some other sphere than the lecture-room be accorded to the teacher, the habit of studying the peculiarities, and adapting himself to the wants of his pupils, will in all probability never be acquired.

Under these circumstances, I believe I shall be only

enforcing the convictions of every one of my colleagues, if I say that a more familiar intercourse with you, and especially with the juniors among you, than the lecture, properly so called, permits, is highly desirable as an aid to the instruction there intended to be conveyed. I allude, in the first instance, to the system of *vivâ-voce* examinations, or conversations, as they might perhaps be called, which is now in almost universal use in this school, in addition to the formal lecture. I value this system very highly myself, and I am quite sure that it is, not perhaps indispensable, but, to say the least, extremely useful, both to the student and the teacher. It is therefore always with regret that I observe students detained, whether by want of time, diffidence, or other causes, from joining in these conversations. Let me assure you that it is by cultivating from the first the habit of taking part in them, and in the other exercises of the class, that you will most easily qualify yourselves for passing that final ordeal which has been established as the test of your proficiency; and which too many of you, when the time approaches, view with such vague and exaggerated terror. It is not the experienced swimmer that dreads the water. But if you will save your skin at the first, you must expect to be rather unpleasantly affected, when at the end of your studies you go shivering and naked into that very cold bath which is prepared for you in the College over the way. Be therefore wise in time. Do not trust to the "grinder" in your last session; but accustom yourselves to carry out, all through your studies, the eminently tonic and invigorating habit of ascertaining your own progress, as compared with that of others and with the expectations of your teacher. To recur to my former illustration, you may find the first plunge require an effort, during this dull November weather; but ere long you will go in like a water-dog and come out steaming and glowing, and like a giant refreshed after slumber.

But I have taken a low view of the matter in recommending class-examinations to you as a means of preparation for your final trials as students. They are far more than this. They

tend to fix and confirm your knowledge, to give you opportunities of correcting and arranging it, and, above all, to make sure that it is forthcoming when wanted. They force you into personal communication with others; they form the best of all introductions to your teacher and to your fellow-students; and they lead to many pleasant and profitable associations, which may be followed up with advantage elsewhere than in the lecture-room. Depend upon it, it is not by sitting in the corner of a room, and being preached at for six hours a-day, that you will make yourselves masters even of the doctrine, much less of the practice, of modern medicine. Still less will you fit yourselves in this way for the great business of the world. You should bear in mind throughout your studies that the spirit of medicine is eminently social; that its duties have to be performed *among men*, and that the habit of easy, unrestrained intercourse with others is among the first of the lessons you have to learn. This habit you will easily acquire, if you avail yourselves of every opportunity to make your studies the basis of such intercourse; if, instead of converting yourselves into magazines of solitary erudition, you use your endeavours to interest all around you among your fellow-students in your common pursuits. By attendance on the familiar class-examinations you will be led to make your studies the subject of conversation among yourselves, as well as with your teachers. Some of you will be more, some less advanced in study; some will be more possessed of one, some of another kind of knowledge; but you will all sympathize in each others' difficulties, and I trust you will all be generous enough to be interested in each others' success in overcoming them. Sure I am that the more you rub shoulders in this way the better for you all. The care which you give to each others' progress is, like mercy, "twice blessed: it blesseth him that gives and him that takes." No student can explain the simplest fact in medicine to his fellow without being led to inquire into something which he has formerly overlooked, or without deriving an increased assurance of his own completeness and readiness of informa-

tion. And when you consider how impossible it is for the teacher to exhaust every subject that he touches in a science so wide and so incomplete as medicine, you will readily apprehend that the field for legitimate discussion and conversation outside the lecture-room is one which may be cultivated with the greatest advantage. To this sort of prolific and eminently useful intercourse you are directly led by class-examinations properly pursued. The lecture sows seed, which may or may not fructify, according to the disposition of the soil to receive it. But the examination ploughs deep into the soil, breaks the clod, turns it over and over, and finally, when the good seed has sprung, uproots the weeds which choke its growth, and exclude both light and air from the tender plant. I do not say that it is impossible for a blade of corn to spring up without any of these processes; but without them it is impossible to have an abundant and goodly crop. And assuredly, to the mass of students, examination, or some process whereby the living contact of mind with mind is secured, is not a luxury, but a necessity.

A very important feature in modern medical education is its practical character. This feature is not, perhaps, recognised so fully in your authorized curricula as it ought to be; indeed, in this respect, our practice is in advance of our regulations. I desire, therefore, to say a few words in recommendation of what I believe to be a sound principle. By practical instruction I do not mean, as you might perhaps suppose, instruction in the treatment of diseases only; but instruction, whatever be its subject, conveyed in such a form as to bring the student face to face with the facts of nature as well as with his teacher and his books. Now lectures are by no means to be despised; and neither, most undoubtedly, are books, and especially good books. "A good book," says Milton, in that most magnificent of all his prose writings, the *Speech for the Liberty of Unlicensed Printing*—"A good book is the precious life-blood of a master spirit, embalmed and treasured up on purpose to a life

beyond life." Such books exist in our science, though in this printing and publishing age they are few and far between. Moreover, it is only in the very rarest instances that books of this highest and best quality can be recommended as being also good elementary works for the use of the student. The greater number of those which we shall be obliged to place in your hands are, after all, mere useful compilations, addressed to the memory far more than to the higher faculties, and embodying nothing but the barest summaries of facts and doctrines which every hour is placing in new combinations, or even removing altogether to make room for new ones. Such books may instruct, but can never inspire you. I do not mean to disparage them in saying so; for if they faithfully fulfil even the minor office,—if they prove what they profess to be, good dictionaries and works of reference, you will have cause to be grateful for the possession of them. But as no one, except perhaps a very learned philologist, can learn a language by poring over a dictionary and grammar, so I do not expect that you will become masters in science by any amount of "reading up" in a text-book. Facts, experiments, observations—truth, in a word, as gathered from the living springs of nature herself—these are what the soul of man thirsts after with a never-dying thirst. Books are the faithful and necessary interpreters —the slaves of the lamp, ever at hand when wanted, silently and unobtrusively doing the drudgery of scientific instruction, and yielding up the endless riches of the past to the hand of their master. But he who would use them aright must himself have walked in the enchanted garden of nature; he must have looked on her ever-open volume, to which all others are but the key.

Our modern medical instruction has become fully awake to the fact, that lecturing and listening to lectures, even with the aid which this process derives from examination, is not all-sufficient. It is a great step gained. Time was when this great truth was not recognised in medical discipline—when as yet the hospital, the dissecting-room, the laboratory, the dispensary, were not a part of the generally-admitted

apparatus of medical education. The business of the student in those days was simple; and that man generally succeeded best, other things being equal, who had the most rapid hand at taking notes, the best memory for mastering long names. *Jurare in verba magistri*—that was almost necessarily the rule; and medical schools, as well as the greater world of medicine without the doors of the Universities, were seen dividing themselves into parties upon grounds with which nature and fact had little or nothing to do—where the whole question at issue was an abstraction, and the whole object of the quarrel was a victory for this or that professor over this or that other. No doubt this is a caricature of the good old times; for, notwithstanding grave faults in the system, sensible men were found to overleap its iron barriers, and become good teachers, good learners, good practitioners in spite of all. But the tendency of the lecturing system, carried on to the exclusion of others, was undoubtedly what I have told you; and I have, in fact, given you no very exaggerated picture of many medical disputes in the days of our forefathers. Perhaps there may be found, in a few of those which even now agitate us, enough of resemblance to my rough sketch to enable you to recognise the evil when you meet with it. But, on the whole, times are now greatly changed. Practical instruction is everywhere admitted into your curricula as an indispensable adjunct to lecturing; and those are everywhere admitted to be the best teachers who are able most judiciously and effectively to combine the two, and to feed the uninformed mind, not with vague general doctrines and intangible abstractions, but with the actual lessons of nature and the ideas that spring directly from their contemplation.

The extra-academical school of Edinburgh has in this respect some very important and special privileges which it becomes you duly to appreciate, and which I will take the liberty of pointing out. In the Hospital, for instance, the seat of practical instruction in the more limited sense of the term, you have several special and general cliniques in the medical or physicians' department, and these might be even

increased in number with advantage; perhaps if we can get an additional hour for hospital teaching, as is the earnest desire of my colleagues and myself, they may be so. In the Surgical department you have a general and two special cliniques. You have also that most important department of Morbid Anatomy, the basis of almost all that is exact and sure in modern pathology; to it much time must be given, although as yet none of the Examining Boards have formally required you to do so. How are you to do justice to all these means of instruction unless you take care to begin sufficiently early? It is often said that "walking the hospitals" is of no use until some branches of systematic instruction have been thoroughly learned. I am of a very different opinion. If there is one thing I feel more secure in saying to you than another, it is that you cannot enter the hospital too soon, or leave it too late in the course of your studies. You have little enough time for what you have to do there. I do not mean that you can or that you ought to attend clinical lectures during your first year, although I have often thought that a special series of clinical instructions for the beginner might be organized with advantage; and that much of that education of the senses which in the medical wards demands so much time and trouble, and which at present we are obliged to crowd into the last years of study in a far too hurried manner, might be learned at a much earlier period. It is at least as easy to learn the characters of the pulse, the external characteristics of disease, and even the principal phenomena of instrumental diagnosis dissevered from their interpretation, as it is to understand the shape of a bone, the relations and insertion of a muscle, the nature of a precipitate, or the laws of atomic equivalents. And if you knew how much your future study would be lightened by this foundation, how much would become possible that is now impossible to you, I am quite sure that you would spontaneously accord this additional year of clinical study in the medical department, and that the studies I allude to might be made at least as attractive and useful to the student in his first year as at any subsequent period.

In Anatomy and in Chemistry I need hardly say that the practical departments, under the same teachers as the theoretical, ought to absorb a large share of your attention. How much, I may safely leave it to those gentlemen to tell you.

In Operative Surgery we have always classes during the summer, wherein you have an opportunity of learning what those who go directly into general practice come at once to feel the need of, and what can never be learned except at the expense of human suffering and perhaps life, elsewhere than in some such class as this.

In Practical Midwifery we have also abundant opportunities; and, owing mainly to the exertions of one of my colleagues this most important department has lately been added to the curriculum of the College of Surgeons, as it must soon be, one would think, to that of every other licensing body.

Finally, we have now, I am happy to say, through the assistance of another colleague, succeeded in connecting the course of Medical Jurisprudence with a practical department which is capable of being made of the greatest service to you.

These remarks will serve to show the extent to which the great idea of practical instruction (or instruction carried directly from nature to the ears, and eyes, and mind of the pupil) enters into the plan of this Medical School. I can say for myself, that I feel as if much of my usefulness to you, such as it is, would be lost, should any capricious turn of fortune deprive me of the means of meeting you at the bedside of the sick, where we are all students, as well as in the lecture-room, where I teach and you learn. I am not without hope that the time may one day arrive, when every teacher of a systematic branch in this school may be able to plant his foot firmly down on the practical counterpart of his systematic course; to derive, like Antæus of old, new strength in wrestling with its difficulties from the contact with fact and reality. Nothing is more treacherous or more seductive than the position of the teacher, who

with a large class before him, gives himself up to dogmatize. For a while he may resist the evil influences of his position ; but unless his nature is more than human, he is apt, more and more every day, to put formulas in place of things, hearsay in place of fact, fancies in place of reasoning, plausible theories in place of the results of honest investigation. But take the teacher of natural science to nature herself,—place him with an intelligent and critical audience in contact with her ever-consistent revelations,—and you burst whatever webs of sophistry he may have unwittingly woven around his own ideas. The mere lecturer—the dogmatist, preoccupied with his opinion, and contemptuous of yours—is gone ; in place of him you have a student like yourselves, candid, humble, inquiring, and truth-speaking, so far as it is in him to be. Hence we trust the botanist most in his garden, and the geologist most on the hill-side, and the comparative anatomist most in his museum ; and for the same reason we ought to see that the great departments of medical science are taught after this truly useful manner—giving to every systematic and doctrinal teacher, as far as possible, his practical field of instruction ; to the anatomist his dissecting-room, to the chemist his laboratory, to the physiologist and the pathologist, to the teachers of medicine and surgery respectively, the fullest command of all appropriate means of practical education.

The only way to obtain the full benefit of practical instruction is to begin early. Do not wait until you are half through your curriculum, for instance, before you enter the hospital. Take my advice, and go at once in search of a perpetual ticket. I don't think it of so much importance whether you begin in the medical or in the surgical department ; but go there, even during your first year, if not regularly, at least very frequently. Learn to use your eyes and your ears ; learn the habits and demeanour necessary for dealing with the sick ; learn, in short, everything that you can learn at this stage of your progress. You will take twice as much interest in your other studies when you have seen, even dimly, to what they tend, as if you went

to them without such preparation. And so with all the rest. I believe, myself (though I desire not to commit my colleagues on this point) that the best preparation for many of their lectures would be a little practical knowledge of chemistry, of pharmacy, of anatomy—just so much, of course, as to clear the way; a little knowledge of the appearance of the bones, for instance, before attending lectures on the bones, and so on. It is not always easy to carry on your studies in this manner; but where you find the facilities ready to your hand, you should by no means neglect to use them. That is all which I feel I have a right to say upon a subject which is necessarily one of detail; and in regard to which each teacher must advise you in his own department.

And now let me say a few words as to the spirit in which you are to go about your work. That you should be instant and watchful for every opportunity of acquiring knowledge, is too evident to require much illustration. However long the time may be before you, I never yet found the student who at the end of his curriculum said that he had more than he required. Besides, nature is a coy mistress, and does not always show herself in the wished-for mood. You cannot get your illustrative case when you want it; you cannot go into the hospital or dispensary, and say, "show me this or that broken bone, this or that inflamed organ." You must lie in wait for knowledge, and take events as they come; looking at the same thing many times over, with new information, new experience, more enlarged mind each time. The sooner, therefore, that you master the elements the fewer will be the incidents which will pass by you unimproved. The lazy student, who bungles his anatomy the first year, is thereby disqualified for much of his surgery the second; if his chemistry has been neglected, the evil falls forward on the materia medica,—and so on. By the end of the third year he has sufficiently come to himself to be wandering about in a maze of confusion in the hospital, wondering when he will be able to see and understand

things like his neighbours. The best thing that can happen to such a man is to be rejected at his examination, and to begin again ; or to stop altogether, and take to something else with more spirit and method. Do not, therefore, put off any portion of your studies to a more convenient season ; do everything thoroughly as you go along, and put the whole force of your mind into the work of to-day. Be systematically in your usual place at lecture ; and if unavoidably you miss anything there, make it up in reading as soon as possible, and do not make your absence a reason for not attending the examination, but rather for going there the more punctually. The habit of steady application is not only a good habit in that it promotes work, but also in that it permits of ease and rest, which are never so sweet as when they have been earned by honest, hard, good work. The student who has been at his classes and over his books all day long, will seldom, or at all events will not wisely, grudge himself the night's rest ; still less will he run the risk of being confounded with those who turn the night into the day, and, as a necessary consequence, the day into the night.

But you will need resources beyond the sphere of books and lectures and hospitals. Your profession is one demanding a great deal of general mental culture ; it is, moreover, an eminently social profession ; and lastly, the body must be attended to, for it would be lamentable, while studying the very structure of that complex machine by which the work of your life is to be done, to allow it to fall into decay for want of proper tending. I do not hesitate to recommend that for these purposes you should make a clear break in your professional avocations on the Saturday afternoon. I was at a meeting the other night, of the working man's half-holiday association ; and I fully coincided with the sentiments of some of the speakers, that we, who work by the brain instead of the hands, are entitled to the highly honourable designation of a *working class*. Let us, then, have our Saturday half-holiday too, and from one, or at least two o'clock on Saturday afternoon till nine

o'clock on Monday morning, let us "throw physic to the dogs," and give the time to exercise, to amusement, to society, and to worship.

Let me conclude my advice to you on this occasion by exhorting you to pursue the profession you have chosen, in an earnest, grave, decorous, and, above all, a religious spirit. You are not to regard the science and art of healing as a mere means of earning daily bread—though that, in itself, is no real degradation to any science or any art; nor yet merely as an intellectual pursuit, nor as a means of moral culture, nor as an instrument of practical beneficence; but over and above, and including all these considerations, you are to place the conviction that the work of your lives is a work imposed on you by God—a *calling* in the real sense of the term—one which requires of you no less than the devotion of your best energies, as, indeed, it presents to you the widest of fields for their development. The spirit which I now recommend to you may be expressed in the words of St. Paul as applied to the Christian calling—"Take heed that ye walk worthy of the vocation wherewith ye are called." I will only suggest one or two considerations which should induce you to take this matter seriously to heart.

The first is, that there is a special sacredness in the art of medicine itself. You will ere long be placed in the chambers of the sick and dying. Your life will be spent, as it were, in sight of the very gates of eternity; and amid the most affecting and confidential intercourse with your frail fellow-mortals, you will be charged to watch over the flickering flame of life. Can you be careless whence comes that life, whither it goes, and to what purpose it is devoted? The very instruments with which you work—sharp knives, sharper and more deadly medicines—are suggestive of an awful responsibility. Will you use these instruments with your own reputation and your own glory solely in view? Or will you use them reverently, as you shall answer to God, who gave them to be for good or for evil, for a blessing or for a

curse, according as you shall direct them? From your lips, one little word, winged and irrevocable, may carry healing and comfort, or inflict torture worse than death. Will you and dare you speak that word without a thought of one whose ears are always open, and to whom, as the Great Physician, you must account for it, as well as for every word and every act of your professional career? No; your art is sacred; you cannot think it otherwise.

But put aside for a moment the sacredness of medicine as it deals with your fellow-men. Let me suppose that you are studying medical science without any immediate view to the responsibilities and the cares of practice; that the pure love of knowledge, and the desire of intellectual and moral gratification are all the rewards you seek. Still, I say, think of the end. You have talents, means, opportunities of no common kind. To what purpose will you devote them? Many men have toiled that you may know; the science of two thousand years has unrolled her ample page before you, and you are "rich with the spoils of time." Where will you deposit your treasures? Will you bury them in the earth, or will you lay them up in heaven? Will you consider your knowledge and your powers as your own, or will you become the faithful stewards of Him to whom you owe them all? Will you seek chiefly wealth, fame, personal distinction, those luxuries of the body and of the mind? Or will you be content with little of these, so you can find the way to render your gifts available for man, and return your talent with increase to God who gave it? Is your study of medicine to be merely an innocent, or, at most, a less hurtful form of self-indulgence; or is it to be the devotion of a life to important objects, and the systematic pursuit of these objects through good report and evil report, for the good of man and to the glory of God? Are you to be the spoiled child of science, or the heroic and religious man, to whom science is but the armour for the battle of life? All depends on the spirit in which you begin; and it is now that you have to make the choice of motives which may influence your whole life.

Science fame, honour, riches—all these give a certain stimulus to exertion. Many discoveries and inventions, useful to humanity, as well as striking and beautiful in themselves, have sprung from the love of these. They are, therefore, not to be despised, and still less to be treated as adverse to the religious spirit. On the other hand, nothing can be more injurious and degrading to the practice of medicine, nothing more lamentable in the followers of our noble and truly humanizing vocation, than that they should be wanting in that which can alone regulate and direct aright these comparatively selfish and ignoble impulses. I will do no more than hint at the shocking prostitution of our art which arises from the desire of wealth too exclusively pursued. A mercenary doctor, in whom the "*auri sacra fames*" has eaten out alike the human heart and the love of knowledge, is indeed a creature abhorred of God and man. Happily you will not be exposed during your studentship to this temptation; and I trust that the spirit of the patient and disinterested scholar will prove a protection, in the earlier part of your career, against a vice which is rarely the vice of the young, and perhaps still more rarely that of the young Physician or Surgeon than of most other members of the community. But you may require to be warned, and you cannot be too early warned, that the search for scientific truth itself, and still more the appetite for reputation and precedency in that search, is capable of intoxicating and preoccupying the mind to a dangerous and vicious degree; capable of absorbing that native generosity of disposition which is rarely wanting in the well-educated youth; capable of undermining the moral principle even in the firmest of characters; of blasting the charities of life even in the mildest. I have seen some of the very ablest of our students display in the mere struggle for College prizes, all the evidences of a selfish and unscrupulous nature; and I have thought, with pain and humiliation, of the greater struggle which awaits such men in the world, when there will be almost nothing to check, and everything to encourage the virulence of animosity, and the meanness of jealous self-seeking. Surely,

of all the infirmities of noble minds, the distempered and insatiable ambition of which I speak is one of the most lamentable. To how much offence, to how much real injury, to how much petty and humiliating irritation does it give rise! How cruel and haughty is it in prosperity! How abject and wretched in adversity! How unjust to others! How little satisfied with itself! Truly the man who is beset by this absorbing and jealous passion will live to say with the preacher—"Vanity of vanities—all is vanity!" Nor will the most eminent qualities and the most splendid success save him from this bitter conclusion.

I have brought before you this one form of vicious self-indulgence for the purpose of impressing upon you the truth which I wish to convey—that it is possible to fall very far short of your duties as men, even while you seem to yourselves to be straining every nerve towards distinction and success as students. The vice which I have held up to your aversion is one to which ill-regulated and ill-balanced, rather than sensual and degraded natures are prone. But no other vice, not even the worst of those "fleshly lusts that war against the soul," marks more distinctly the absence, or the feeble and indistinct development, of the religious principle. You have indeed duties to perform to yourselves, and you err grievously if you steep your souls in the lethargy, and abandon your bodies to the destroying influence, of dissipation and sensual indulgence. You have, moreover, duties to perform to society, and you are lamentably wrong if through sloth or perverseness, or even thoughtless frivolity of disposition, you neglect them. But over and above these social and personal duties, comprehending them all, and therefore superior to them all, stands the great duty of *self-sacrifice*—of *devotion*, in the only genuine sense of that much abused word. For devotion is not a sentiment, or a creed, or a formality, as some would have it, but a deed—a practical recognition of the great Creator and Sovereign of all, by the life-long dedication to Him of that which is his. You are *devoted*, or *devout*, which means the same thing, if you are busy and studious; but

you must also be more anxious to study to a right end, than to reap a present reward in the praises of your fellow-students or even of your teachers. You are *devoted*, if you preserve an unblemished reputation ; but you must also have been more desirous to be, than to seem, good, otherwise your virtue is the virtue of the hypocrite, and such will be your reward. You are *devoted*, if you seek, by fair and honourable means, a distinguished place among your fellows ; but you must not only see that you deserve, before you attain, such a place ; you must also be sure that you look to distinction only as a means of greater good, a fulcrum for that moral lever by which you are to work on and on, through time and through eternity, the work that is given you to do.

Self-sacrifice—or rather self-devotion—is the mark of the religious character, as selfishness is the sure sign of the opposite. I trust I do not exceed my duty in this place by saying, that we look for the manifestation of a religious character, after this manner, in each of you. We do not inquire, and we care but little, in what form you clothe your religion, as a sentiment or as a system ; what creed you adopt, to what church you adhere. But to find you, or to make you, earnest men ; to keep you ever mindful, by precept and example, that your art is a business, and not a mere pastime ; a God-given business too, and not a mere money-making machinery, an arena for intellectual gladiatorship : this we conceive to be no less our duty than teaching you the details of the art itself. We may perform this duty feebly and inadequately ; but a duty it is, and as such we recognise it. Do you, on your side, not fail to give your thoughts seriously and often to this religious aspect of your vocation : let the consideration of it preserve you from idleness, which is the waste of your time ; from dissipation, which is the abuse of your body ; from over-work, which is the ruin of your mind. You are the appointed keepers of these precious gifts of God ; keep them well, that you may render a good account of them. Be not vain and self-confident, for this is to value yourselves above your

work; nor abject and mean, for this is to degrade yourselves below it. Find out what you can do, and do it cheerfully and quickly; for the measure of your ability is also the measure of what is required of you. Seek distinction, not to rest upon it, but to strive beyond; the more honour, the more work; the greater the praise, the higher the task. Let your object be to prove your powers to yourselves, not to display them before others; so shall you be careless of unmerited praise or neglect, not through indifference, but because no one can take away the object for which you strive. It is a little matter that another man's work is better than yours; it is much, that your own be as good as you can make it.

If you study in this spirit, you will stand clear of all baser motives, so far as they are opposed to this; you will put away jealousy and evil-speaking, "malice and all uncharitableness"; you will be helpful and generous, modest and truthful, careful of the reputation of others, not fearful or anxious about your own. And so may God send you this spirit, and help the good work, both with students and with teachers!

ON THE MEDICAL ART, CONSIDERED IN CONNECTION WITH POPULAR EDUCATION

Read before the Scottish Educational Institute, April, 1856.

I VENTURE to ask, for a little while, the attention of the Scottish Educational Institute to the present state of the Medical Art. I do so, not for the purpose of glorifying my art by a narrative of its improvements, and still less of depreciating it in your estimation by exhibiting its frequent revolutions of opinion, but with the view of bringing under your notice the intimate connection which may be traced between its progress and that of the general human mind; between the origin or the diffusion of sound medical principles, and the state of education, and of morality, in the general community. That such a relation exists, few, perhaps, will be found to deny; but that it is generally understood or appreciated, can hardly be maintained by any one who has studied the art of medicine and its history with that earnest and serious attention which they deserve. We very often hear the art of Medicine spoken of as if it were a thing apart, subject to conditions and laws of its own, and standing aloof from the general current of human sympathies and activities. Medical men are regarded as a kind of priesthood, drawing their inspiration from some mysterious revelation of traditional philosophy, which cuts them off, for good and for evil, from the aspirations, the knowledge, the follies, the vices of the hour. People rarely reflect that

the faults and the virtues of medical men are in reality only their own faults and virtues, modified by accidental circumstances. Yet nothing is more certain, than that medical men are nurtured on the same intellectual and moral diet with the rest of the educated population, and only differ in possessing a technical knowledge of things relating to disease. To accuse the medical art, therefore, wholesale, as some people are fond of doing, is substantially to criminate the age which has produced, and tolerates it. You can no more expect to have a real and an exalted art of healing among a degraded or half-educated population, than you can expect to gather grapes of thorns and figs of thistles. I will not, indeed, assert, because I do not believe, that it is impossible for the medical art, or for individual medical men, to be to a certain extent independent of the circumstances in which they find themselves; but in the main it is true, that from a barbarous age you can have nothing but a rude and ignorant medical practice,—from a superstitious age, an irrational practice,—from a corrupt age, an immoral practice,—from an age of imposture, a practice of quackery.

Surely this view of the medical art is worthy of your attention as educators of the people. If I have correctly stated the position of my own profession, you cannot refuse to admit that the responsibility for the future fate of one of the first, one of the most necessary, and one of the noblest of the arts, devolves indirectly upon you. Whether that Art shall be a blessing or a curse; whether it shall be practised purely and independently, or under the slavery of humiliating superstitions and dangerous fanaticisms; whether it shall open men's minds, or close them, to the truth; whether it shall be permitted to learn the lessons of experience, or be chained to the chariot wheels of some victorious philosophic theory; whether it shall be constrained to follow, or allowed to lead, the dense mass of unenlightened public opinion;—are questions the answers to which will be found mainly in the use that you, the teachers of the public and of the future professors and practitioners of medicine,

make of your high function. If you, in the exercise of your vocation, bring up a generation strong and robust in thought, informed with rich and varied knowledge, penetrated with a noble self-respect, and therefore charitable and tolerant; zealous for truth, and therefore not afraid of criticism; full of generous human feeling, and therefore devoid of pedantry and exclusiveness;—you may be sure that the Medical Art will not fail to reflect these characteristics. On the other hand, if you sow the seeds of narrow prejudices and small bigotries; if you inoculate the rising generation with the love of word-catching and puzzle-headed false philosophy; if you teach it to prefer sect or party to the claims of truth, the errors or passions of the multitude to the dictates of conscience guided by reason, the *vox populi* to the *vox dei*;—then indeed you may count on the results of your training of the youthful mind, in a deteriorated practice of the Medical art. For there is no subject of general human interest, except, perhaps, religion, in which poisoned waters of a false or narrow education turn so soon and so surely to bitter fruit as in the art of Medicine, of which Cicero has said that its genuine exercise brings men nearer to the gods than any other.

As I wish neither to magnify nor to vilify my art, so I will now say of the position of the medical practitioner towards society at large, precisely that which I think may be truly asserted, and may recommend itself to your sober judgment without rhetorical artifices or special pleading. I do not venture to assert that the medical practitioner, considered an individual member of society, is either better or more enlightened, apart from professional qualifications, than his neighbours. I would fain hope he is at least no worse; and considering the immensity of his responsibilities, the amount of his unremunerated or partially remunerated labour, the paucity of honours or prizes held out to him as the rewards of a lifetime of exertion, perhaps I might fairly argue that, on the whole, he more than earns his place in our social system, and has a right to hold up his head among the benefactors of mankind.

But what it concerns us chiefly to observe at present is, that he is, above all other men of the learned professions, *the child of his age*; inasmuch as he is what he is by its free choice and favour, and not by the force of law, the voice of the state or of the church, the exclusive preference of sect or of party. I apprehend that you consult a physician or a surgeon not merely because he bears a diploma, has studied in this or that University or College, follows this or that mode of practice, but because, after due consideration, and consultation with mutual friends, you have reason to think he knows better than you do the issues and remedies of disease; while his personal character and professional reputation are such as to command your individual confidence. Having thus determined in his favour, you admit this man to your family circle; you instal him as administrator of some of your most important interests; you intrust him with the most sacred confidences, and furnish him with the most ample materials to make or to mar the comfort of your home. No Spanish inquisitor has greater power to torture, if he so will, your body or your mind; no father-confessor, be you the most devoted child of Mother Church, can hint so many of your secrets—and that, too, without the breach of any formal oath, perhaps without even incurring any serious accusation. This great and inevitable responsibility you intrust, under the pressure of calamity, to a man whose only claim to your confidence is his reputation for special knowledge, for calm and correct judgment, for truth and purity of character; aided, perhaps, by some personal impressions derived from a slight and passing acquaintance, or, at most, from casual association with him in comparatively indifferent concerns.

No one will deny that the trust here pourtrayed is one which ought to be, nay, which must be, exercised under the influence and control of public opinion. To maintain the contrary is to exempt the art of medicine from that wholesome discipline which all other arts tending to the good of man, undergo; and to which most of them owe whatever adaptation they have to the spirit of modern times, and to the wants

of modern society. It is not possible, even if it were desirable, that the practitioner of medicine should now-a-days be a hierarch or mystery-man, as he was in remote antiquity, and is still in savage nations. He cannot always with a good grace retreat from the public inspection even in cases where he has some right to do so, and where the inherent difficulties or technical nature of his duties requires a large and generous confidence. Judges and advocates, patients and their friends, popular lecturers, sanitary reformers, and patent pill-venders, betray a most provoking familiarity with the technicalities of medical science, and a determination to tear the veil from its mysteries. Instead of dispensing his oracles in scattered leaves of mystic import, eluding the sense even of the devoutest worshipper, Apollo seeks now-a-days the aid of fashionable publishers, and issues his opinions to all and sundry in octavo and duodecimo volumes innumerable, beautifully printed, illustrated with wood-cuts, and with all technical terms carefully explained. The Pythagorean *ipse dixit* no longer avails; the sage must render up to every questioner some reason for the faith that is in him. A hysterical lady has her desk full of prescriptions of various authors; she deciphers their most crabbed latinity, knows exactly what is the base, what the adjuvant and what the excipient in each; and ends by constructing a theory of disease, and of her own disease in particular, out of odds and ends of science about anti-spasmodics, and emmenagogues, and anti-periodics. If she says in English that she has a pain, she must, as an old physician has wittily remarked, get it back in Greek that she has neuralgia. If she is troubled with megrims, nothing but the doctrine of hemicrania will serve her turn; if her stomach is disordered, the whole theory of dyspepsia must come under review, and bile, or something else equally accommodating, must get the blame of the mischief. If you prescribe any ordinary medicine, you are told that she has been taking that at her own hand for the last three weeks; if you order something more recondite, she says that a great physician in London or in Dublin gave her

the same thing two years ago; your only chance, if you wish to show exclusive knowledge, is to ransack the stores of the chemist for the last organic compound with a name of six syllables, or to operate with metals more unfamiliar than even Cerium, and Titanium, and Palladium, which are all of them down in her vocabulary long ago, and about which she knows——just about as much as you do, or rather more. She has tried Homœopathy, and knows exactly the theory of the action of Arnica 20; she has tried Hydropathy, and can tell precisely how the capillaries of the skin react upon those of the mucous membrane, and how the peccant matter of the blood is to be eliminated without the use of such poisons as rhubarb and cream of tartar. Mesmerism has come to her aid, and she has got relief from it for a time; besides, a clairvoyant in Paris has given her invaluable information about the neurilemma of her great sympathetic nerve, and made her aware of many sufferings which no physician had previously revealed to her but which she has had for ages without knowing anything about it. In short, she is a very learned lady, and, like an old bird, is not to be caught with chaff.

This daily increasing tendency on the part of the public to know, or think they know, the grounds of medical opinion, is not, however, to be disposed of by a caricature. It is a grave and indubitable fact, and must needs be dealt with as such, whether we think it a movement for the better or for the worse; whether, in particular instances, it proves an obstruction or an assistance to the mutual good understanding of physician and patient. It is plain that against a tendency so inherent in human nature, no mere use of Latin prescriptions and scientific terms can ever prove an effective or permanent barrier. No titles or licences can convey a monopoly in dogmatism; and, happily for the moral health of the profession, its more learned and esteemed members give no encouragement to bombast, and are but sorry practitioners in the art of mystification. The real adepts of the present day have little temptation to "stand upon the ancient ways" and cover the deficiencies of their

arguments with the mantle of Galen and Avicenna. The tide has set in for novelty, and those who are anxious to take it at the flood will find that fortune generally favours, for the time at least, all persons who sedulously proclaim a new doctrine, and treat with studied contempt the trammels of authority. From Morrison's pills to Homœopathy, from Dr. Samuel Dickson to Professor Holloway, from the water-cure to the latest Palingenesietherapie or Kinesitherapie of our transcendental friends across the German ocean, the cry is still that old friends are to be done away; that a new revelation of medical truth is to supplant the science and the traditions of centuries, and that the reign of Hippocrates, of Sydenham, and of Cullen, is at an end. When confronted with the strongest statements of personal experience, or the best established facts and principles, and asked to reconcile these with the particular new system which happens to be in the ascendent, ambitious innovators reply, after the manner of Sganarelle in "Le Médecin malgré lui" when charged with misplacing the liver and heart,—"Oui, cela étoit autrefois ainsi, mais nous avons changé tout cela; et nous faisons maintenant la médecine d'une méthode tout nouvelle." From this secure, or at least not very easily assailable position, of absolute and entire irresponsibility to medical traditions, our would-be medical lawgivers discharge all sorts of missiles at what they call "old medicine"; and having improvised a system, unencumbered by antecedents and reckless of consequences, they find great comfort and not a little sustenance in hunting for flaws in the ancient doctrine. Well! well! let them live their day. They are the woodpeckers of science. The grand old Hippocratic oak will outlive them; the insects that nestle in its bark will become their lawful prey.

But can the art of medicine, rooted as it is in the past, remain calm and unmoved in the midst of these vagaries of the popular sentiment? Are we who practise that art to maintain a contemptuous indifference to the effervescence which is going on around us in the public mind? Or are we rather to make a bold appeal to intelligent public opinion,

and to seek for ourselves and for our art a position more elevated and secure than hitherto? Clearly, in either case there is danger: but the one is simply impossible; the other only difficult, and, because difficult, full of hope for the future to the man who loves, and has faith in, his art. No man of noble instincts, and of genuine courage, can hesitate for a moment between an advance amidst hostile opinions, which call him into contact, perhaps even into collision, with the stirring movement of the human mind; and a retreat upon mere authority, which leaves him high and dry on the rock of antiquity, but removes him from the pleasant and warm current of human sympathies, as well as from the boisterous surges of human passion. In medicine, as in all other arts which are closely interwoven with the delicate fabric of life, which minister directly to a man's comfort, happiness, existence, an isolated position is an impossibility. But in this very fact lies hid a difficulty, which, when overcome, will constitute the real triumph of medicine. "The physician must not only be ready himself to do what is right," said Hippocrates, in the close of that noble first aphorism of his; "he must also bring it about that the patient, and those who attend him, and all manner of external influences, co-operate." The physician is no mere lawgiver; he is a man dealing with men, and not permitted even to choose the objects of his ministrations, as most of those do who decry him and his business. He has to accomplish his ends, not by abstract rules of art, but by the quick intuitions of an intelligent and sympathetic nature, with experience and learning to guide it. He cannot afford to lose sight of a practical conclusion amid the mists of theory; and must often avail himself of imperfect knowledge, and even of prejudices, when he is satisfied that their tendency is not mischievous, but in the right direction. In every case it is his to see that the right thing—that is, not the best thing in the abstract, but the best under the circumstances—is done. He cannot therefore, in all cases, explain the grounds of his judgment, even to those who are well-informed in the principles of medical science; much less to those who only

fancy themselves to be so. To these he must assume the manner of a dogmatist, and demand that implicit confidence to which his character, intelligence, and skill entitle him, and which he will generally receive, if he seeks it not from the love of mystery, but from the necessities of his position. But to possess this confidence in a high degree he must be on good terms with his fellow-men, and must have acquired the habit of seeking and finding the way to their hearts, as well as to their heads. It is plain that the man for this work must be a man of humane and generous, not of hard and angular nature. He must be an integral part of his age, and must yield neither to the intellectual conceit of appearing above it, nor to the licentious sophistry by which some unworthy men contrive to accommodate themselves to the time and place in which they live, by denuding themselves of everything personal and characteristic, which might happen to give offence. The true physician must enter into the aspirations of his time, combat its prejudices, examine its fair and reasonable judgments, profit by its hard-won experience. But he must, if he would be a true physician, be the representative of its wide and varied humanity, and not of its sectional or party feelings, still less of its quackeries, its delusions, and its cant. Now, to lay hold upon the sympathies of the present age, with its wide and varied, though superficial culture, with its restless intellectual activity, and its expansive beneficence, is no mean and no easy task for the medical art. And to do this without the aid of superstition, or of a false and plausible philosophy by which to explain all medical mysteries, is a task for each individual practitioner, well worthy of the best. I do not believe that we need shrink from this great ordeal, on account of anything in the nature of the medical art itself. But it is reasonable to entertain a fear lest, by the imperfect education of medical men on the one hand, and still more of the mass of the population on the other, the day may yet be far distant when an enlightened and reformed art of medicine shall be established in full sympathy with an intelligent and sound public opinion.

It is much to be regretted, for the sake of the public as well as of the medical profession, that the use of remedies, and the nature of the benefit to be expected from them, is not somewhat more clearly understood, and more carefully studied by those chiefly concerned. That the most gross and absurd ideas are abroad in the world upon this subject cannot be doubted by almost any one who has undertaken the responsibility of treating disease. Many persons appear to suppose that the cure of a disorder under the hands of a physician ought to require an expenditure of his resources invariably in proportion to the violence of the disease. "*Ad extremos morbos, extrema remedia.*" Nor is it ever a popular view of the subject that the physician should honestly believe and frankly confess his conviction that nature should have her own way. Patient and friends are apt to be equally dissatisfied not only unless *something be done*, but unless it be done brilliantly and suddenly. Under these circumstances, alas for the poor doctor who knows the truth and is simple enough to recommend waiting on nature. The patient knows better. Somewhere or other there is a screw loose; it is the doctor's business to tighten it, and then the machine will go on as before. We expect the human body to be set right when it is out of order on the same principles as a steam-engine or a watch. We look to the physician not only to understand all the internal machinery, but to have an absolute control over it. This would be quite justifiable, quite correct, if the physician were the engineer or the maker of that complicated piece of mechanism with which he has to work; if besides watching its movements, and oiling its wheels, and freeing it from extraneous clogs and impediments, he could also stop its pulses at pleasure, take it to pieces, renew lost, and patch up worn-out parts, and then set it a-going again as if nothing had happened. This power, and this alone, would put the physician on a level with the engineer.

It is now upwards of 2000 years since the Father of Physic embodied a great truth in the striking expression, "Our natures are the physicians of our diseases." Yet this truth,

accepted and acted upon by almost all great physicians, has generally been scouted by the public at large; and the whole tribe of quacks have aided and abetted them in despising or distrusting the healing influences of nature, and pointing a finger of scorn at physicians who knew better, or who did not choose to pander to popular prejudice. What could follow from this, except a system of over-drugging on the one hand, and of inert practice, disguised under the pomp of apparent science on the other? That such results have followed, we all know. Only the other day I found in a Homœopathic Journal one of the partizans of infinitesimal globules accusing ordinary physic of being a system of scientific trifling with disease, using only palliative remedies, and based on the rule of "let ill alone";—the very same charge in different words, which Asclepiades of Bithynia brought against the Hippocratic practice when he called it a "waiting upon death." Compare with this the fulminations of hydropaths, gymnasts, *et hoc genus omne*, against the "drug-doctors" with their poisonous and deadly means of cure; and it will be seen, that in whatever direction medicine may move, it has little chance of escaping the generous and highly disinterested criticism of those who, as Hippocrates said, "make an art of vilifying the art."

If any one shall assert that the medical profession itself is partly to blame for these misconceptions on the part of the public, I shall assuredly not go out of my way to deny the imputation. Indeed, I have already pointed out that the medical art is, in every age, and almost of necessity, formed and moulded more or less in accordance with public opinion. I may *think* what I please; but I must, after all, *practise* to some extent, as you, the patient, please. At least, I must refrain from prescribing what is especially abhorrent to your prejudices; and unless you are more than usually inaccessible to considerations founded on your natural good opinion of yourself, I may possibly succeed in turning to account our mutual good understanding; for it will not be difficult to persuade you that in patronizing my little pill, or my stupendous system, you show a great intel-

lectual and moral superiority to the vulgar herd who are content to be treated as their forefathers were treated before them, or nearly so.

It is a well-known fact that quackery finds its victims most readily among the "honourable of the earth"; the wealthy, the highly educated, the refined, the self-indulgent; all those, in short, who believe themselves, rightly or wrongly, to be a good deal better than their neighbours. In general it will happen that a strong-minded, or a prejudiced, patient will rule his doctor as much, at least, as his doctor will rule him. And whether the practice resulting from this reciprocal influence will be better or worse than it would have been under a strict medical autocracy, will depend very much less upon the degree of intelligence than of candour and moral rectitude which enters into the copartnery. Now, you have only to consider for a moment this necessary dependence of medical practice upon public opinion and public prejudice, to understand the great temptations to which it exposes the virtue of the individual medical man. He is well aware from the first, or soon comes to know, that the path to honourable success is both long and toilsome; while, by a certain amount of adroit management and jesuitical conformity, he will oftentimes be able to make a sudden leap into fame and fortune; and that in such a case success will, like charity, cover a multitude of sins.[1]

There is only one cure for these aberrations of the medical profession, and it is to be found in the cultivation of a better understanding, and a more genuine moral sympathy, between the public and their medical advisers. Even as it is, the man whose head and heart are sound, and who is neither con-

[1] The reader of Goethe's *Faust* will remember, that in the strange scene between Mephistopheles and the student, when the former dons the doctor's cap, and reads a lecture upon the capabilities of the different professions, medicine is distinctly in the ascendant in Satan's good opinion. And it must be admitted that this great poet, who has given to literature the most perfect of devils, presents his studies of human wickedness with a minuteness and impartiality worthy of the spokesman into whose mouth they are put.

ceited nor perverse, is generally saved by a certain wholesome instinct from the clutches of the rapacious pretenders who prey upon the most grossly ignorant and the most highly educated alike, when suffering under bodily disease. And in like manner the physician who keeps his honour untainted, is pretty sure in the end to have his merits recognised. But this consummation would be greatly assisted were men generally aware how much more of moral than of intellectual, or even professional excellence, is necessary for the irreproachable performance of the physician's daily duties. In particular it ought to be known, that the desperate search after a *remedy*, at any cost and under any conditions, is utterly opposed to the cultivation of a sound moral relation between the physician and his patient. In the latter, it begets a habit of dissatisfaction and fault-finding if the cure is delayed. In the physician, on the other hand, it most directly encourages that fatal tendency to over-drugging, or of deception under the form of *placebos*, from which we have seen the recoil into homœopathy and countless other systems of magnificent nonsense. All this might be avoided if people could be made aware that the real value of medical services is generally in the inverse ratio of their pretension and self-assertion. For the cure of diseases belongs in general, not to the physician, not to any earthly power, but to the supreme Artificer, who rules all the action of the bodily machine for life or death, for health or sickness. The physician stands by, the earnest watcher of nature's process; he removes whatever of external hindrance is in the way, and endeavours by simple, mostly palliative remedies, by regulated diet, by attention to sleep and waking, and to the due performance of all the physiological functions, to rescue the patient from those dangers to which he would inevitably expose himself when unassisted, and when suffering under the vitiated tastes and feelings that accompany disease. He relieves, moreover, the troubled mind of undue anxiety, and, on the other hand, is careful to direct the fool-hardy and thoughtless sufferer by the path which nature points out to him; he guards

the man wrapped up in the daily toils and unhealthy drudgery of life against the injury to which his anxiety for his family, or his avarice for himself, are exposing him ; he steels the over-excited nerves of the hypochondriac or hysteric by wholesome medicine for the soul as well as the body ; he preaches a solemn warning to the unhappy voluptuary, by holding before his view the precipice which he is approaching ; he foresees the end, whether for good or evil, and prepares for it by counsels of hope, tempered with caution, or of resignation without despair. And even when in the discharge of his duty he has foretold the inevitable fate, when he knows that the irreparably damaged organs are incapable of supporting much longer the fluttering pulses, and feeding the smouldering fire of life, the skilful and humane practitioner will take care to cherish and turn to the best account the small and frail remainder of those mysterious powers which are soon and surely to be returned to their Giver. In the worst event, he can often keep the actual passage to the grave from being embittered by positive bodily pain, or still more by that mental restlessness, which those alone can conceive who have experienced or witnessed the craving for human help, even amid the deadly fear that it may be unavailing.

All this the physician can often do, not the less effectively that he does it unassumingly, that he does not vaunt *his own* remedy, *his own* peculiar method, *his own* infallible or precious specific, or that of his sect, like the charlatan or the enthusiast. There are in fact, or there ought to be, no such thing as *sects* in physic at all. The methods of practical medicine have been radically the same in all ages and times, nor is there any essential discrepancy, other than in matters of detail, between the earliest and the latest. In the writings of truly great physicians we find no disposition to compress the spirit of practical medicine into one or two dry and unprofitable formulas ; no vaunting of single remedies or prescriptions ; no presumptuous affectation of superiority to the habits of thought, the knowledge and the experience gathered for and transmitted to them in the

progress of centuries. Such men have always used the materials at their hand with the tact of men of the world, and with the enlarged views of men of science; and by that habit of just and clear intuition which can never be analyzed and reproduced through any education or any system, they have given to the world and to their successors an example which in some instances has not perished with them. But it is important to observe that, in the case of all the greatest masters of the art, the movement they communicated to their age was of the nature of an example, almost an inspiration, not of a body of doctrine, or an all-embracing philosophy of disease and of cure. This is eminently true of Hippocrates, of Aretæus, of Sydenham, of Mead, of Heberden, of Corvisart, of Laennec, of Hufeland, of Abercrombie. With these men, systems and philosophies were quite of secondary importance. Their business was to leave an example of honest work and careful observation, not to impose a yoke on the minds of men. It would seem, indeed, to be an essential law of medical history that no dogmatic exposition of mere opinion or speculation can hope to live long apart from the personal influence of its propounder. The doctrine of Galen is a great exception; but then Galen lived in the most slavish and corrupt age that the world has seen. His system endured because, under the imperial despotism of Rome, men soon ceased to originate thought; while every active element of civilization was fast crystallizing into the form of arbitrary laws, as a necessary protection against the sea of barbarism which was surging even into the courts and palaces of the Cæsars. It may be truly said, in this case, that the exception proves the rule.

I have said that the spirit of true medicine admits of no *sects*. It is, in the most genuine sense of the term, absolutely catholic and free. It is wide enough in its boundaries to embrace every real utterance of free thought, and to welcome cordially all who are content to follow honestly and cautiously the teachings of nature and experience, without desiring to trammel and bind up the spirit

of knowledge in chains of their own making. Unlike the professions, either of the church or of the law, it appeals to no human standard of orthodoxy, and acknowledges no authority except that of God's own truth, as it appears written in the book of nature. The days are long past when it was necessary, according to the inimitable satire of Molière:

> Essere in omnibus consultationibus
> Ancieni aviso
> Aut bono
> Aut mauvaiso.

The days are past when sages decided questions of practice according to the fundamental propositions of Aristotle's logic, applied to Galen's Anatomy and the Aphorisms of Hippocrates the divine. There is now no appeal but to nature and fact in the determination of disputes. All opinion, all dogmatism, all doctrine in medicine, counts for the opinion of the individual, the dogmatism of the individual, the doctrine of the individual: no more. And no opinion is sufficiently wayward or irrational to shut its possessor out from the catholicity of medicine, provided he does not shut himself out by attempting to erect his opinion into the standard of a sectarian distinction, or by assuming that it contains the whole of medical truth; by setting up, therefore, on his own behalf, that worship of formulas, that standard of orthodoxy, which true medicine repudiates as opposed to its essential freedom.[1] With respect to all such

[1] If a man says to me—"I have reason to think that so and so is true, and here are the facts on which I found my inference"—he is entitled to respectful attention; and, if the facts be important and his statements credible, to careful investigation of his opinions at my hands. But if he says to me—"The *only* true law or the *only* effective practice in medicine is so and so; and all who resist this are committing murder by wholesale"; if his whole writings constitute a continuous libel upon the past, and a presumptuous anticipation of the future, what answer can be given to such an outrageous form of appeal? The only answer is to disown it in the name of the medical art; not as a heresy of opinion, but as an offensive utterance of base calumnies. The writings of Hahnemann are, as I have shown in the *Edinburgh Essays* for 1856, fully entitled to the description just given; and (setting aside all the absurdities of his

sectaries, indeed, modern medicine is extremely intolerant; but it is on account of their own exclusiveness, not on account of any real narrowness in the spirit of medicine. It excludes them—it cannot but exclude them—from its sympathies; but it excludes them because they claim to exclude it, to give it the law, to abrogate the past, to dictate to the present and the future.

The spirit of modern medicine acknowledges no orthodoxy; it can therefore by no means acknowledge any heterodoxy. Its followers must of necessity be eclectic in their mode of study; they must not accept one age, and deny another; accept the present, and deny the past; accept the Greeks, and deny the Arabians; accept Cullen, and deny Boerhaave. The criterion of medical truth is not the present against the past, or the past against the present, but the present added to and harmonised with the past. We are accused of pushing this conversation too far. It may be so in some instances; but that it is so in general I think no impartial inquirer will assert who considers that this conservative spirit is the sole barrier against the flood of quackery, imposture, and fanaticism, to which the very freedom of medicine necessarily lays it open, but from which no restrictions in the present day could possibly protect it. These things are the price that we pay for *Freedom*; and no man who knows the value of Freedom will grudge the price. But the very absence of restraint upon individual opinion imposes in a peculiar manner upon the age the right education of that individual opinion.

I return, therefore, to the position from which I started; that on the sober, truthful, and at the same time free spirit

dogma), it is a strange proof of the ignorance and perverseness of the public judgments in matters of medical science, that a character so utterly worthless as that of this notorious adventurer should have enlisted the sympathies of any considerable part of mankind. It is of a piece with this idolatry of the man who sold borax under a false name, that the deluded partizans of the sect should now-a-days be ready to discover all manner of excuses for the most frightful forms of medical immorality, even when proved beyond dispute in a court of law.

which may pervade society at large, must depend, to a very great extent, the liberation of the medical art from those vices which uniformly beset it in barbarous, ignorant, or immoral ages; and that on the tone imparted by this and other similar institutions to popular education, it will depend whether Society performs its part to Medicine, and Medicine to Society. That Medicine shall not advance, is impossible and inconceivable; but it may be subject to trammels and checks from the ignorance, or to bigotry and error from the wilfulness of its professors, from which they can only be saved by an enlightened public opinion; and this it is your duty, as educators of the public, to form.

THE GRADUATION ADDRESS

Delivered in the University of Glasgow on July 22nd, 1897.

LADIES AND GENTLEMEN,—I had occasion some years ago to encounter what I have selected as the text of this little sermon, or address, in a form which I venture to think is susceptible of improvement—demanding, in fact, what, if it were a Biblical text, we should call a " revised version." As it is only, however, a very common French proverb, we may take it upon ourselves now to adopt it with the rendering most suitable for our purpose. I was standing, on the occasion referred to, in the reception-room of the British Medical Association, when one friend, locally familiar with the ground, was inquiring for tickets for some evening entertainment or other, while another friend, a very distinguished ethnologist, and I were awaiting the result. The first friend was by this time pretty well merged in a crowd, all alike clamorous for tickets, on which he ejaculated incidentally, " Ah, everything comes to him who waits." " I beg your pardon," I remarked, " you will not say that if the tickets are all done and you find you cannot get them." " How do you reconcile that proverb," said the ethnologist to me, " with Shakespeare's ' tide in the affairs of men that, taken at the flood, leads on to fortune'?" "Oh," I replied, " I do not see any difficulty there ; but our friend has quoted the proverb wrongly." " How so?" " The proverb as I understand it in French, does not promise anything at all to the man who merely waits, but everything to the man

who *knows how to wait.*" And the distinction here is all-important; for Shakespeare's "tide" is just as likely as not to submerge the man who, like Dickens's Micawber, is merely waiting for "something to turn up," while it "bears on to fortune" only the man who "knows how to wait"—who is not merely waiting, but is ready and fully equipped and armed to take the advantage of the tide when it comes. The proverb, therefore, is all right in its real and original form, and Shakespeare is also all right; and both of them contain a good deal of the philosophy of human life and of human effort which it may well be worth while for all of us to consider a little on the present occasion. For, graduates, I am afraid it must be admitted, for most of you at least, that a good deal of "waiting" will have to be done, now or later, before you can expect to reap the highest rewards of your profession. It will not be given to many of you, as to a very excellent hospital assistant of mine now in Johannesburg, to be pitchforked at once into a share of a practice worth several thousands a year, and likely to be largely increased. You will have to wait, most of you, perhaps all of you, for considerable success in your profession; and you may have to wait longer than you quite like. But as this waiting has got to be done—as it is part of the game, in short, and, as I hope to show you, a very important and even beneficent part of it—my lesson to you is that it is as well to study how you can play this waiting game scientifically and practically, according to the laws of the game. My lesson to you is that you should not fret or worry over this, but rather be well advised about it beforehand; in other words, and in the words of the proverb, that you should "know how to wait."

"*Tout vient à point à qui sait attendre.*" That, I believe, is the true original form of the proverb, and, as you will easily see, it contains a far deeper philosophy of life than the current English version, even if we admit that the latter has any meaning at all for us, or is not absolutely misleading. "Waiting" is here set forth as a part, and a very important and necessary part, of the art of success in life;

it is to be studied as a branch (a Frenchman might say) of the *savoir vivre*. It is to be accepted, not in a spirit of restlessness and impatience, still less in the mere happy-go-lucky mood of a Mr. Micawber, but rather as a part of every earnest man's destiny, and in the full confidence that when it is so accepted and so used by a man working in the right way and in a corresponding temper of mind, the opportunity will come, the tide will rise, which "taken at the flood" (as Shakespeare says) "leads on to fortune." And so the immortal dramatist, he who was "not of an age, but for all time," is found to be in perfect accord with that possibly very far inferior authority unknown, who devised and popularised the science of "getting on" in life in the very epigrammatic phrase with which this little address began.

There need be no hesitation, then, in recommending to you, as young graduates entering on a medical life and medical responsibilities, that you should try, above all things, to "know how to wait." That is the very plain and obvious condition, the first step towards ultimate and honourable success in medicine as well as in every other high pursuit. The great statesman, for example, is he who can "take occasion by the hand, to make the bounds of freedom wider yet"; but he must be content to wait for the occasion. The greatest general or admiral at the head of the strongest army or fleet will be the most careful to see that success is not marred by an inopportune moment for an engagement. Why, then, should you, with all the world before you, and all your lifetime to work in, not "know how to wait"? Why risk all that should be most valuable in your life in one ugly rush—character, experience, efficiency in the end—because, owing to faults of temper, or even the laudable desire to "get on," or the apparent and immediate need thereof, you feel tempted to satisfy all your ambitions at once? "Waiting"—in the sense of the proverb—is not idleness, and in our profession of all others it has no need to be an unprofitable or degrading occupation. There is plenty to do for all of us, if we can only accept our just position and act in a subordinate part with frankness and sincerity.

It is even a duty of the first order to keep ourselves, body and mind, in a state of preparation for higher duties; and therefore no one can for a moment think of "waiting" in the sense of a folding of the hands or of what is called across the Atlantic "loafing around." No; he who "knows how to wait" will strive to keep himself always ready to use favourably the occasion when it comes. He will instinctively beware of letting his faculties go to rust; and you, graduates, will naturally seek such occupation as will conform to the large and valuable discipline which you have already undergone. But you will, nevertheless, "know how to wait"; and sooner or later the favourable occasion, or occasions, will come. And then, just because you have so waited and so kept yourselves in readiness for them, you will reap your reward.

There is nothing very profound in all this, you will say. No; but still it requires to be thought about seriously and often. Many a promising career has been blasted, many a life practically thrown away; still worse, many an honourable beginning converted to quackery and baseness by not knowing or not believing that all will come right to him who knows how to wait. The temptations of a medical career are not small—it may even be said that to ill-regulated or over-ambitious minds not guided by principle, and easily swayed by self-interest, they are apt to be overwhelming. Let me advert very briefly to only two out of the many.

1. There is the making haste to be rich—not, perhaps, necessarily a fault (much less a crime) in itself, but certainly an unbecoming attitude of mind in an early medical career, and one which is not easily reconciled with the principles of your higher and nobler vocation. "They that will to be rich," says an old and venerable book, "fall into temptation and a snare, and into many foolish and hurtful lusts which drown men in destruction and perdition." It can hardly be disputed that the temptation and snare here referred to receive many illustrations in connexion with the profession of the healing art. Nor is it at all an exaggeration to say that the making of money, if advanced

to the rank of a first principle and practised without any scruple or any restraint of honourable tradition, is by far the easiest part of success in the art. The vast fortunes made by the most glaring quackeries, the immense sums spent (it is to be presumed profitably) in the advertisement of remedies which are either worthless or set forth in terms that can only be described as evidently mendacious, are the standing proof that the art of medicine, if pursued as a mere trade, is at once the easiest and the worst, or the most contemptible, of all possible trades. It may, therefore, nay, will often, occur to you in your early career that the restraints upon your freedom of action arising out of the necessary and honourable tradition of centuries, and out of the almost unspoken influence of all that you have been taught in this University, are somewhat in the way of your success, if the "tide which taken at the flood leads on to fortune" is to be merely a tide of fees. I know not if it is an absolutely true story, but it is one which might be true, and almost looks like the truth—that of the bonesetter in Paris, who, on account of some questionable transactions, was visited by the police as practising irregularly without a diploma. The story goes that a perfectly regular diploma was immediately produced; but that the supposed culprit earnestly requested the police agent not to betray him, and explained that he had carefully concealed and deliberately suppressed his diploma, "because," said he, "I once practised on the strength of it and was starving; and now, by behaving like a charlatan, and freely advertising as such, I am already a rich man." The moral of the story is that in the making of money, as in other things, you must "know how to wait." The money will come, unless you are very unlucky, with the gradual increase of reputation and in the faithful, quiet, honourable discharge of higher duties; but to make it a first consideration is almost the sure road to a fatal lowering of tone, to the alienation of your more honourable brethren, and in the end to "destruction and perdition" in a medico-ethical, if not also in the apostolic, sense. In the course of a pleasant excursion not long ago with a number of

medical brethren, most if not all of them old pupils of mine, and as good fellows as I can desire my old pupils to be, one of the best and not least successful said to me jokingly, "You never taught us how to make money." I at once admitted the truth of the remark, but it was because in my opinion that part of a medical education may, nay, ought, to be severely left to itself. The money-getting side of your art is very easily, perhaps too easily, learned by those who desire it above all things; but it is not to be looked on as the end, much less the beginning, of your professional efforts. This medico-ethical disability (if such it be) is not one that attaches to our calling exclusively, or even especially. It is true that we have to live by our profession; but at the same time we are bound, as in all high and noble callings, by a much higher law—we are bound in every instance and at every stage of our career so to live that the higher is not sacrificed to the lower. It is not too much to say that every man worth his salt in the medical profession makes up his mind from the first, and advisedly, to do far more for nothing, or for the love of it, than he can ever do for fees. One cannot but regret that it should be needful to place this high doctrine thus clearly and definitely before young and generous minds which are not, as a rule, given over to the opposite view. But the sheer necessities of your early struggles in some cases, and in all cases the hardening influence of a kind of trades unionism that is now being at many points insidiously introduced into professional life, has a tendency to displace its purer and nobler traditions, and to subvert the teaching of that grand old Hippocratic oath, which in a modified stage is still the substance of the declaration that each one of you has adopted to-day as the rule of your life. I will not go into details, because time will not permit. I will rather take an illustration from another calling, which lies ready to hand. In the current number of the *Quarterly Review* there is an article under the title "On Commencing Author," in which you may read as follows: "There are numerous motives operating upon the true man of letters . . . urging him to exercise his

vocation, and to exercise it to the very best of his power without being paid for it at all. We do not say that he will not ask for his cheque, or thankfully accept it, but we do say that the motive which prompted him to write, if he is likely to be worth his salt in literature, is a motive with which the cheque has nothing whatever to do." The last word that can be said in this place, on this branch of the subject, is that you should love your profession as such—love it so well and with such a whole heart that you cannot think of it as a mere trade, under whatever apparent compulsion of necessity; and to do this, which in the end will be your true advantage and benefit, even (in most cases) from the pecuniary point of view, you must above all "know how to wait."

2. The second great temptation which tends to make shipwreck of a medical career, especially in its earlier stages, is an uneasy personal vanity or egotism which makes itself, as it were, the centre of the medical universe, and is never done inquiring, "Why am I not as well off as A, or B, or C, who may have been more lucky, but is certainly not better or abler than other people"—or than the complainer, to wit? It is very easy to have this feeling within limits, and perhaps impossible to avoid it altogether; but it is also very easy so to cultivate and coddle it under one or another specious disguise that it becomes a perfect passion for demeaning others, a perpetual cover or cloak for malice, hatred, envy, and all uncharitableness. I hope you will most of you avoid yielding to a weakness which is often that of the strongest and most active minds, and which will often claim to be even a virtue in respect of its bold nonconformity or want of respect for persons, in denouncing what it pleases to hold as abuses. The only criticism, however, I am concerned at present to pass upon such persons is that they are usually glaring examples of men who do not "know how to wait," and who therefore allow their talents, their time, and all that should be most valuable to them, to be squandered in personal and for the most part petty disputes and differences. The result is, that instead of everything coming to them

à point, as in the proverb, they find themselves from first to last handicapped in the race of life by personal animosities and antagonisms, many of which are not shared in even by the persons, of more quiet and even temper, whose peace of mind it has been intended to disturb. I do not say that it may not occur to any one of you to have to take a part, as matter of duty, in controversy; but as a rule controversy, and especially personal controversy, is to be avoided, and is always particularly to be avoided in the case of young aspirants who, from want of experience if not from their own overweening vanities and conceit, may be too readily brought to mistake very small issues for large ones, and so to nurse, as it were, a vindictive soreness at what they deem to be the neglect of their own high ideals. The broad and large common-sense and the generally peace-loving disposition of the medical profession usually discounts the pretensions of such erratic and quarrelsome members of it, and relegates them sooner or later to the category of men who stand in the way of its normal business, because they have never learned "how to wait."

You will see, I trust, from these examples of the snares and possible pitfalls that beset the path of the young graduate waiting for employment and reward, that the temper and spirit in which that *waiting game* (as I have called it) is to be played may have a very large and important influence on your future happiness and success. If the game of life is not a mere haphazard, if it is at all like (say) a game of chess in its intricacy and its demands on your skill, you will not succeed in it in such a way as to be satisfactory to your friends or yourselves unless you give some attention to the laws of the game; and by these I mean not only those obvious and comparatively well-defined rules the violation of which will involve you immediately in disaster, but those laws also—moral, spiritual, and physical—the study of which, and the acting on which, are absolutely necessary to secure a well-founded and enduring success. A false step in the beginning may have consequences to you which can never be repaired. But (what is perhaps of even

greater importance), in medicine more than in most other callings, a steady and satisfactory progress depends much more upon *character* than even on the most brilliant talents; and character, though it may easily be lost by a single false step, can only be built up gradually by many acts of personal devotion, and by the application in detail of all that you have learned for the relief of suffering humanity during these years of your probation. Hence it may be said with perfect truth that the more modest *rôle* at the beginning, is often not only the safest, but also the most successful (in the best sense of the word) in the end. The subordinate position, while it enables you greatly to extend your experience, and to keep all your powers and your acquirements well in hand for future use, is often much to be preferred to the more ambitious programme, which by giving you a nominal independence would also place you in a position of comparative isolation, with comparatively slender resources that can be turned to future account. A man who "knows how to wait" will not despise the day of small things; but he will say to himself, Whatever happens, I must above all things be ready: I must, in any case, strive to keep what I have learned and earned in the way of skill and experience; and I can only do this, by steadily persevering in getting more. So that the question of immediate success, whether in the way of emolument or of reputation, is often merged in the larger question, How am I to keep my powers intact for a larger success later on—to maintain the discipline of mind and of body, of fingers, and of ears and of eyes, the familiarity with practical methods and processes, which up till now has made me what I am, a legally qualified practitioner and graduate?

It is at this point, graduates, that I venture to commend to you—not for the first time, as you will find from the other little printed address which I have caused to be handed round among you—the claims of the thesis for the degree of M.D. (such of you as mean to go on by-and-bye to that higher degree) to your careful consideration from the very commencement of your medical career. Under the

newest regulations by ordinance, the thesis has ceased to be, what it long was, the only academic test employed in respect of the higher degree; but it remains, and probably will remain, the chief test for honours or distinctions under the new system as under the old. But however it may be in this respect worthy of your early attention, there can be no doubt, I think, that the gradual maturing of the faculties towards a well-considered and, if possible, original thesis on a subject absolutely of your own selection forms an admirable way of keeping your faculties in good repair, of proving your powers to yourselves, and in the end of making them apparent to others without even the faintest surmise of self-advertisement in a bad sense; for the thesis that bears any trace of such a destination is pretty sure to be rejected. So highly do I (and this, I think, is true also for all of us in the medical faculty)—so highly do we all appreciate the value alike to you and to your Alma Mater of a thoroughly good thesis that we heartily wish it were within the power of the University to offer, as has long been the case in Edinburgh, a certain number of gold medals as the reward of very high distinction in this respect. I may add to the expression of this wish (not now for the first time), that without any certain knowledge I am also not altogether without a hope that through the goodwill of some benefactor proceeding upon a recognition of the desire in question it may ere long be accomplished.

I have hitherto considered the French proverb or epigram with which we began from the point of view which was probably that of its unknown author—viz. as referring to worldly success only, or chiefly; a high kind of worldly success, it is true, and such as we, your professors, wish for all of you in your art; certainly not such a success as forms in and by itself a degradation of the art of healing. But I should like before I close to lift the argument for a moment into a yet higher region, and to show you, if possible, that there is a very grand and noble interpretation of the proverb, a spiritual sense of it, in which the "waiting game" may be played with absolute security and with a success, in the

highest sense of the word, that none of the "chances or changes of this mortal life" can ever diminish or take away. In one of his most intricate and, I will add, difficult arguments in the Epistle to the Romans (Chapter viii., v. 28) St. Paul has formulated for us a proposition which, although I do not much approve in general of texts as detached from their surroundings, I will take the liberty on this occasion of so detaching; inasmuch as while the context is of the nature of an argument, and one which has led to much controversy, the few words I refer to are really of the nature, not of argument, but of deep spiritual experience, and have a very curious parallelism, so to speak, with the wording, not to say also the meaning, of the French proverb. "We *know*," says St. Paul (not as admitting any doubt of it at all) "that all things work together for good to them that love God." Listen again to the Frenchman: "*Tout vient à point à qui sait attendre.*" Is there any real relation or connexion between these two expressions? I think there is; and if you will bear with me still for a few minutes I will endeavour to show you that the relation is not only real, but that it goes to the very root of the whole matter.

Huxley, in one of his early discourses upon the laws of Nature, has a superb—I would say a noble, even a sublime—image which ought, in my opinion, to have gone one step further, but which, even as he has it, might have saved him from the remarks of some who have confounded his agnosticism with irreverence. He represents man as playing for his life at a game of what we ignorantly call hazard, against a beneficent and pitying angel, who as a matter of duty will always oblige him inexorably to keep the laws of the game whether he loses or wins; but who, so far from being vindictive or adverse in feeling to man in this trial of his powers, "would as soon lose as win." Now, this is Huxley's way of saying, in what he regards as scientific (though at the same time highly imaginative) language, what our Frenchman has said in the proverb; and it is also, I think, a reflection in some degree (though, of course, without the theistic and emotional element) of the burning words of

St. Paul. The point which is common to all three is the idea that human life, its successes and its failures, are in some way or other under the influence of *laws*; and even Huxley, who was no materialist, would assuredly have admitted (though he does not say so) that these laws of our being extend over the mental, moral, and spiritual region as much as over the material. Play the game, he says, according to the rules; for if you do not, ignorance of them will not protect you, but if you do you *may* win, and, in any case, will have done your best. Be sure, says the Frenchman, that if you can only *wait* and be ready, the occasion will come, and with it all things will come to a point for you. Love God, says St. Paul, and all things are yours; for "all things work together for good" to such as love God and conform to His sovereign will. Of course, there is an element here that is not found directly in the other two. It is what we may call the definitively Christian or religious element. The French proverb here is carried up to its highest terms in the form of a great and far-reaching spiritual experience.

But in another and, perhaps, an even more remarkable phrase used by St. Paul elsewhere, the sublime idea here set forth is carried on a stage further. Of himself and Apollos he says, in speaking of the foundation and planting of the Corinthian Church (1 Cor. iii. 9), "We are God's fellow-workers; ye are God's husbandry, ye are God's building." This working together of all things for good, therefore, admits of active as well as passive co-operation with God, and those that work on these lines in the love of God are συνεργοὶ τοῦ θεοῦ—labourers together with God (as the Authorised Version has it); God's fellow-workers, more literally and more strikingly, in the Revised Version. Here, one can see at once, is the implied condition of the *synergy*, the working together for good, that is pointed to in Romans. In doing God's work, doing it consciously and along with Him, we cannot fail to have Him with us. Thus it is that "all things work together for good to them that love God" and are willing to co-operate with Him in His work. Success in life, as this view of the matter puts it,

depends on what you mean to do and how you mean to do it. If your only objects are money, reputation, a crowd of patients, and large fees you may very easily fail ; nay, you will certainly fail in some degree, for the desire in these cases increases usually beyond the means of its satisfaction. But if you desire, humbly and reverently, to work the work of God, and be a " fellow-worker " with Him, in what profession can you accomplish this object more consciously, more surely, or more inevitably than in ours ?

Natura non nisi parendo vincitur. In that saying of the great Chancellor, Francis Bacon, is contained the whole secret of medical as of all other real success. Whether it is the healing of the broken bone, or of a wound, or of a fever, it comes round again to that. By obeying the laws of nature, by working consciously along with them, by adapting our efforts to their conditions, we, too, become " fellow-workers with God." In the material as in the spiritual world it is the same law which governs the whole. God is the great Worker—the great Healer. It is through knowing His laws, and acting on them and through them, that we can best, or indeed alone, co-operate with Him. And in so working with God, even if we have to wait for it, success is assured. " *Tout vient à point à qui sait attendre.*" Only, for success in the highest, in the only real sense, it is necessary to " love God " as well as to know His laws. Then, indeed, we shall find that " all things work together for good," and the " waiting game "—even if it comes to that—is assured of success.

Few things in the personal history of great men are more pathetic and at the same time more encouraging from this point of view than what we have come to know of the blindness of John Milton. It came upon him, not suddenly it is true but completely, in his forty-fourth year. " What a fate ! " writes Professor Masson, in detailing all the circumstances with his usual pious care and unwearied diligence. " All his intentions, all his projects, all his hopes of further activity and usefulness, turned into mere gropings through a future of blackness. His secretaryship, to which

he had been accustomed, the duties of which he had come to like, how on any terms could he give up that? Then, apart from his secretaryship, the cutting short of his own cherished studies and the fond occupations of his life hitherto, his miscellaneous pursuit of knowledge, his commerce with the books he had gathered about him and with the libraries at hand, his unfinished controversies"—all this and more, it will be admitted, forms a most discouraging picture of calamity and possible ruin in the very middle of a great career. But Milton was one of those men who know how to wait. He tells us so himself in those two admirable sonnets to Cyriac Skinner, in 1655, when the calamity had already been borne for three years. He had a good many more years to wait before he had quite accommodated himself to the new conditions. He had to go into hiding at the Restoration, and then to live in comparative obscurity after he had regained his freedom. Altogether, it was a very poor case, speaking from the point of view of the worldling, for the man who under Cromwell was probably the most powerful as well as most renowned of Englishmen, Cromwell himself excepted, and whose "*Defensio pro populo Anglicano*," with its bitter controversial sequels, were not only in some degree the causes of his blindness, but also of his exultation; as he himself says:

> "Liberty's defence, my noble task,
> Of which all Europe talks from side to side."

Yet it is none the less true that for us, and for all time, the real greatness of Milton was, in a very genuine sense, the outcome of his blindness and comparative obscurity, for to these we owe "Paradise Lost" and "Regained," "Samson Agonistes," etc., while the "Defensio" is now only rescued from oblivion by its being the work of one at no time unconscious of his own great powers, but who had assuredly not taken a just measure of them when he wrote in early despair of his great calamity just alluded to:

> "That one talent which is death to hide,
> Lodged with me useless"—

and when, in the calm anticipation that his time of activity and usefulness was ended, he brought this wonderful sonnet to a close in the line :

"They also serve who only stand and wait."

I hope most sincerely, ladies and gentlemen, that none of you will have a trial like that of John Milton. But if it should be so, you will draw more encouragement and some small degree of hope, it may be, from his noble example. At all events, your course is now clear. You are graduates of this ancient University, and, as such, fully entitled to practise the art of healing throughout Her Majesty's dominions. We desire to accord you our heartiest congratulations, and to part with you in sincere good-will and with every disposition to help you in your work. What that work is to be we do not presume any longer to dictate. But whatever it may be, we trust that you will apply yourselves to it in the spirit of Longfellow's noble lines, which form the conclusion to the " Psalm of Life," and with which I, too, will close this address :

"Let us, then, be up and doing,
With a heart for any fate ;
Still achieving, still pursuing,
Learn to labour and to wait."

A PLEA FOR "THOROUGHNESS"

Introductory Address delivered at the Yorkshire College, Leeds, at the opening of the session, October 1st, 1899.

GENTLEMEN,—Some few weeks ago I read in a newspaper somewhere a paragraph, which, whether written in jest or in earnest, I thought at the time might be made the text for some remarks more or less suitable to the present occasion. You will perhaps laugh at me, or suppose that I am laughing at you; but at all events I will pray you to laugh *with* me, when in the presence of this august assembly I venture to read the said paragraph. Here it is:

"AN AUTOMATIC DOCTOR.—A Dutch apothecary has just taken out a patent for a novel automatic machine in the shape of a wooden figure formed like a man. The figure will be covered with compartments labelled with the names of various ailments. The sufferer has only to place a piece of money in the compartment upon which the name of his illness is inscribed, and forthwith will appear a pill or powder suited to his case. The machine is constructed upon the same principle as the ordinary automatic chocolate and bonbon machine. A Dutch paper predicts a brilliant future for this very original doctor."

Now, I am not going to ask such of the young men now before me as are applying themselves to medical study, "Is this *your* ideal?" That would be not only too absurd, but even (but for its absurdity) insulting to an audience whom I am desirous above all things to attract and to please. But

I am entitled at least to ask, Why is this *not* an ideal form of medical advice and prescription for a considerable proportion of those who may require such aid in sickness? In the attempt to answer this question, I believe we shall find a few nuts to crack which may occupy us profitably for some portion, at least, of the time usually devoted to an address such as this. Think, in the first instance, of the immense comfort and convenience of the proposed arrangement. Why, you might have these machines at almost every railway station, always ready for use, just as you pop your penny into the slot when in quest of a cheroot, or of the wax vestas to light the same! Only, let us hope, for the sake of the poor doctors who are not automatic, that the "piece of money" to be automatically administered—the fee, in fact—would never degenerate lower than half-a-crown! With this proviso, one seems to see how the thing might work tolerably well in some cases. There are a good many of the "thousand natural shocks that flesh is heir to"—perhaps even a majority of them—very easy to name and very well defined indeed—to the sufferer. And some of these are, moreover, very easily put a stop to, if you take them at the very beginning. "Principiis obsta"—here you have it all made clear to the most ordinary capacity; no trouble, no nervousness, no hesitation nor procrastination, no embarrassing or uneasy confidences with a man of skill; only half-a-crown in the slot, and there you are! Liver, stomach, heart, lung, brain, biliousness, colic, blood-spittings, rheumatism, neuralgia—all under control, without a moment's delay, and by the most approved remedies in each case, at every railway station, and in every public place! Surely there is something to be said, *prima facie*, on behalf of such an arrangement as this!

I am pleading, you observe, the case of the "automatic doctor" as against the living, flesh-and-blood doctor, in these words; and some of you who are readers of Carlyle may be reminded (*longo intervallo*) of the severe, almost savage, irony (worthy of Dean Swift) of his proposal to substitute "cast-metal parsons" for our spiritual guides and

instructors. In both cases the suggestion proceeds upon the lines of economy and convenience; and further, on the assumption that the automatic is nearly, if not quite, as good for certain purposes as the living article. But it might further be maintained, in the case of the "automatic doctor," that it is by no means a necessary part of the case that the machine-made should entirely supersede, or even replace, the living doctor of medicine. If some more or less fastidious persons object to be prescribed for anonymously, why should there not be an acknowledged, even a distinguished, living authority behind the dummy? or even more than one? Fancy a well organised group of "automatic doctors" (perhaps with some distinctive uniform) literally doing the work of Sir William Jenner or Sir Andrew Clark (so far, at least, as prescriptions go) in every railway station in England, by carrying the most famous and successful of their acknowledged recipes, ready made up, in the most economical way possible, to almost every sick person who can define his own ills to himself sufficiently to profit by their skill thus imparted. I am assuming, of course, that there will be a copyright in the recipes thus used, and that the law will protect the authors of them from piracy; so that an automatic doctor who may represent a great physician by using his recipes shall do so only by previous arrangement with him. Can there be a doubt, do you think, that very considerable professional incomes might be made in this way? and that hundreds, nay, thousands, of comparatively poor persons would eagerly avail themselves of the help that could thus be had at every man's door, without a visit to London or from London, and a correspondingly heavy professional honorarium?

You will allow that I have argued the case of the dummy or automatic doctor at least as well as the newspaper from which I derived my information; and, indeed, so as to present it with every possible advantage and in the most really professional form attainable, or perhaps imaginable. I will now proceed to inquire a little more seriously what it is that hinders a manner of treating disease like this from

being habitually adopted—or, at least, admitted frankly as in accordance with an enlightened medical opinion? For we may take it as certain that the "automatic doctor" carrying the authority of Sir William Jenner or Sir Andrew Clark is just as remote a contingency as would be Carlyle's "cast-metal parson" doing the work of either Canon Liddon or Mr. C. H. Spurgeon. Why is it, in other words, that in matters of health and disease there is not only no possible substitute for the living man as a counsellor, but that even the living man, to satisfy the just expectations of an enlightened public opinion, must undergo the training, acquire the experience, and generally go through the hard and life-long discipline alike of intellect and of character, which you, my young friends, are now only beginning to appreciate, and which finds its final expression in some of our greatest physicians and surgeons?

Had it been even possible to formulate adequately any considerable portion of the medical art in the manner required for merely mechanical prescription, not only would this have been done long ago, but the progress of the ages would have given such stability to the results that the treatment of disease in this nineteenth century would have been mostly stereotyped (as one might say) beyond the possibility of change or revolution. Instead of this we have the curious fact evidently before our eyes, that in no previous age of the world has so little importance been attached to formulæ, or so much to the labours, experience, and opinions of individual men dealing with disease, as in the present. I am not speaking, observe, of disease as matter of doctrine, or as treated of in books, or in the lecture-room, but disease as occurring in the sick chamber or hospital. You will find it, I believe, to be the simple truth that at no time in the world's history have the medical advisers of the sick been so little hampered by traditions as at present; at no time have the methods, the formulæ, the remedies of past ages undergone more revolutionary changes, and yet at no time have the living influence, the character, the personal status of the physician and surgeon been more adequately maintained

than precisely in this latter end of this nineteenth century of ours. So much is this the case that if you take up a book which represents the traditions of even less than half a century ago, you will find these traditions almost entirely superseded, and even hopelessly out of accord, on many points of the first importance, with the matured opinion of almost every one of our greatest masters of the art at this present hour, either in England or in any part of Europe. Not one in a hundred of the medicinal formulæ so copiously scattered over the pages of Copland's Dictionary, for instance, obtains any considerable amount of favour or support to-day; not one acute disease, probably, is treated as he and the greater number of his contemporaries would have treated it; nor is there a single article, perhaps not a single page, of that immense and voluminous work, on which the merest tyro of to-day would not be amply justified in writing numerous emendations and improvements, founded on the instructions and experience of a four-years' medical curriculum; and yet medical opinion, as such, and especially individual medical opinion and influence, far from losing in weight, owing to this prodigious amount of destructive criticism and revolutionary change, are of wider scope and further reach than ever before. On all this, considered as historical fact, I have had to expatiate a good deal of late years in various addresses and papers, some of which have lately been collected into a volume.[1] I am therefore the more disposed in the present instance, to forego the argumentative defence of this position, and to assume it as proved, in order to make its consequences instructive, if possible, for you.

One consequence of this state of things I have already foreshadowed, or rather have affirmed—viz. that *real knowledge*, by which I mean something quite different from learning, or hearsay evidence of mere opinion about disease and its cure, counts for more in the individual man, even in the most ordinary general practitioner, than it ever did before. Not so very long ago, it was possible for a man with

[1] *The Physician as Naturalist*, 1889. Glasgow: James MacLehose and Sons.

a very ordinary amount of industry and of scholastic attainments so to saturate himself, as it were, with the doctrine of a particular teacher or school as to command a certain amount of attention and respect for opinions enunciated in terms of that doctrine, however remote they might be from the facts of a case under observation at the moment. The fever in this man (an actual patient) was, on very slender evidence, adjudged to be a malignant, or a putrid, or a low nervous fever ; it arose from this or that depravation of the humours, this or that *materies morbi*, this or that entirely supposititious or hypothetical change in the blood, in the nervous system, in the solids of the body, according as the man speaking to you was a humoralist or a solidist. A very few obvious indications sufficed here for the diagnosis—*i.e.* to enable the fever to be placed in the nosology, and so to get a name. The treatment followed almost as a matter of course from the name, or rather from the theories or hypothesis connected with the name, according to the school or system of doctrine in which the observer had been trained. Now, I am far from saying that this method of viewing and of treating disease is altogether extinct, even at the present day ; but I am free to affirm that it has been very much modified. Any one of you, gentlemen, after only a four-years' curriculum, will be in a position to form a far more direct, sounder, and less ambiguous judgment upon many cases of fever and acute disease than any man who wrote at large upon them, even in the first third of the present century ; because you will have to apply tests, and to use methods of observation, and physical means for the detection of local changes, which were then almost unknown. Do not let this assurance, however, make you conceited or contemptuous of our forefathers : there is still plenty of room to say that neither they nor we have anything like a complete knowledge of any fever or any acute disease.

When I hear a man talking at large and dogmatically about " biliousness " (a term of Abernethy's, which neither Abernethy nor anyone else has been able to explain further than that it requires blue pill), or about " congestion of the

brain," or anæmia of the same, which he cannot have seen, and does not know at all as really existing, or about "irritation of the mucous membrane" (so beautifully vague that it may mean almost anything), or about "hyperæsthesia" or "hyperæmia," or, to use a more modern phrase, "neurasthenia"—I know that he is in the stage of mental progress represented by those old theoretical views of the fevers to which I alluded a moment ago. He is using words, not to set forth knowledge, but to conceal and gloss over essential ignorance; his diagnosis, and prognosis, and treatment will probably all follow the same lines, and be based upon some intangible theory or current hypothesis of his own or someone else's invention, and not upon the plain facts and carefully studied details of the case before him. Therefore it is that I urge upon you young men to get into the habit of steering clear, if possible, of all such mere vague and misleading phrases. Try to think of diseased phenomena as men do who have and who use (up to a certain point) the means of *knowing them as facts*, and not merely *speculating about them*; men armed with the stethoscope and ophthalmoscope, and laryngoscope, etc., and with chemical and microscopical tests at hand, and the thermometer, sphygmograph, etc., to give the exact indications of fact, which should place you out of the bearing altogether of these survivals of ancient, sometimes even of mediæval theory, which may for a time usurp the place of medical science, but are no more closely related to real knowledge—that is to say, the true *science*—of disease than the atoms of Democritus and Lucretius, or the Vortices of Descartes, are entitled to rank as physical science in the modern acceptation of the words.

Now, the attitude of mind which I am commending to you (for as a mere attitude of mind it is of more value to you than oceans of "cram," or even of crude information about disease, in respect of your future career) must be cultivated; and it must be cultivated early if it is to rule in any considerable degree your education as medical students or practitioners. This is all the more necessary

to be said—indeed, to be insisted on,—because the opposite attitude, which for distinction's sake I will call the "automatic" attitude, is still widely prevalent, and is much more easily learnt. It did not require the suggestion of an "automatic doctor" to teach us that much of our casual prescribing is, as it were, or was not long since, done by machinery or by rote.

It is very easy to lay hold of one or two leading symptoms or complaints and to call them a *disease*. It is very easy then to take down a little book of *formulæ*—it may be the recipes of very distinguished physicians; to look out the appropriate *name* of the disease supposed to exist, and then have a shot at it with the first *recipe* that comes to hand. That is what I am calling the automatic method. But the true physician knows that the diseases of our poor humanity in their ever-varying aspects are not thus to be formulated; and further, he knows that what he has got to treat, and to cure, if he can, is not the *disease*, as regulated and named by custom, but the *man who is afflicted by the disease*—the whole man, and not only a part of him; all his disordered functions or altered structures, and not only the particular point where he feels the pain. Therefore it is that the "automatic method," easy as it is in many cases to learn and to apply, will not do even for this nineteenth century, and will be thoroughly exploded in the twentieth. Easy? Yes, but just because it is easy it is correspondingly fallacious. It ignores, or sets aside without due consideration, all that a more carefully disciplined modern physician places in the front rank of importance—hygiene, the inquiry into causes, and treatment of the highest order depending upon these. It recognises only the disease and its assumed antidote—and thereby for the most part fulfils Voltaire's well-known sarcasm, that the art of medicine consists in putting a number of drugs of which one knows little into a body of which one knows even less. There is a fatal facility in this method, which has made it specially appropriate for quacks in all ages. To use the words of Hamlet to his faithless friends, Rosencrantz and Guildenstern, when the

latter declares his inability to play upon the pipe—" 'Tis as easy as lying : look you, these are the stops. Do you think that I am easier to be played upon than a pipe ? "

I do most earnestly trust, gentlemen and students of this medical school, that, between those two contrasted attitudes of mind as respects disease and its cure, you will decide early, and be safely guided by your teachers as to which you are to choose. For upon the choice you make depends almost entirely (apart from individual moral and intellectual characteristics) whether you will be found really advancing the art and science of medicine, or whether you will be of those who sink deeper and deeper into the ruts of routine practice and retrograde theory as life goes on. But I cannot persuade myself for a moment that in a school which can boast of three or four generations of such distinguished and progressive surgeons as the Heys and the Teales, and which even to-day and yesterday has, or has had, such leading spirits as Clifford Allbutt and Wheelhouse, there can be a moment's doubt as to your decision. With such traditions and such examples before you, it is morally almost impossible that you should become, or wish to become " automatic doctors." I will only in a few words point out to you what is, in my opinion, the most important kind of discipline for enabling you to tread successfully the straight and narrow way that leads, not to the attainment of a high and varied medical culture (as to which you must, in the main, depend upon your teachers and on the authorised curriculum), but rather to the state of mind which alone makes such attainment possible. For you must take it from me that at every step in your career the " automatic doctor," in one insidious form or another, will stand athwart the path of progress, and may even, with some of you, tend to stifle the nobler impulses by proving how very successful he is—with the public. From the merely lucrative point of view, there are but few even of our most successful physicians who could compete with the highly automatic money-making machinery of Holloway's pills and ointment, or old Dr. Jacob Townsend's sarsa-

parilla. From any other than the mere lucrative point of view, and happily, in the end, with most of you (I trust), even from the point of view of a reasonable worldly prosperity, it is most undesirable that you should yield, in the very slightest degree, to this insidious form of temptation.

Now, it is at this point that I find it needful to have a very few words with my friend, Mr. Wheelhouse, not, I trust, inappropriate to the present occasion, concerning some of the remarks which he made here a few weeks ago in that most admirable address of his, as president of the British Medical Association. I do not suppose at all that we differ greatly in essentials, or perhaps even in details; certain I am that we have both one object at heart, the advancement of our profession, alike in its essential usefulness and in the good opinion generally entertained of it; which, indeed, forms a part, and a large part, of its usefulness to the public. But some of the remarks in that address appear to me to be open to an interpretation which possibly is not the one intended, but which at all events I desire not to allow to pass current without a *caveat*. It seems to be assumed or suggested that, in order to keep a medical education upon what are supposed to be practical lines, it is necessary that every student, before getting his name on the Medical Register, shall have had opportunities of seeing, handling, and in a certain sense treating, almost every kind of common disease and every ordinary type of infectious fever. By parity of reasoning, it would equally be necessary, as a security to the public, that every man admitted to the Register should have done every minor, or even possibly every ordinary major, operation, so that nothing should come to him hereafter to find him quite devoid of previous experience. Now, while I am as anxious as Mr. Wheelhouse or anyone else can be to improve, in every reasonable sense, the practical side of a medical education, I venture to say that what I have now put into words of my own as being suggested by the address referred to is not, to my mind, even a good practical ideal. Moreover, if any

such ideal is to be carried out, or attempted, it will be absolutely necessary for the General Medical Council to go back on their late resolutions with respect to the "covering" of unqualified medical assistants; for, without some sort of "covering," lawfully or unlawfully, how is this extent and variety of practical experience which is regarded as essential ever to be attained by the majority of students or of undergraduates? It will be conceded that in order to make an experienced as well as a properly instructed practitioner, it would be requisite that he should have had opportunities of setting fractures and reducing dislocations; of performing venesection; of passing the catheter, not only in the normal subject, but through strictures and other difficulties; of treating and observing throughout, with a certain amount of responsibility, cases of measles, pneumonia, typhus and typhoid fevers, phthisis, small-pox, rheumatism with and without cardiac complications. Why not also of performing operations in strangulated hernia, tracheotomy, ligaturing of arteries and veins, and other cases of emergency which, according to the programme just referred to, ought, if possible, to form a part of the training necessary to fit a man completely for all the necessities of general practice?

I apprehend that it is only necessary to put the case in this form in order to show that there is a fallacy somewhere —I do not say in Mr. Wheelhouse's argument, but in the conclusions to which I suppose his argument will, if carried out logically, apparently lead. I will therefore here affirm that my own view of what is essential to a practical medical education differs in some respects from that which I have presented above, using (it may be erroneously) the name of Mr. Wheelhouse. Speaking from my own experience as a teacher, I hold that the hitherto unsolved problem of a perfect medical education is to be sought in a direction nearly the opposite, or converse, of that just indicated—viz., *not* that the student or candidate for a diploma should have seen and done personally almost everything, or even many things; but that he should have seen and done comparatively a few things in such a way as to give a proper

foundation or leverage, as it were, for attempting successfully others which have necessarily lain outside the possibilities of a young man's experience. And accordingly I have come to regard it as of far greater importance that every case observed or dealt with in any way by the student, or in his presence, should be *thoroughly* observed, and its treatment *thoroughly* understood, than that a much greater number and variety of cases should be so observed and so treated as to beget habits of imperfect observation and loose thinking—in short, the habits which in their developed form give origin to the " automatic doctor " aforesaid.

This general principle being affirmed (for it would be out of place, I think, to go further on the present occasion), I am content to leave the practical questions arising out of it for settlement by wiser heads than yours or mine, with only one additional remark. It has been several times affirmed by way of criticism on the present curriculum of the schools that a young man may have gone through it all, most carefully and well, and yet never have actually seen a case of measles. Well, that is a misfortune, and to the extent of my own personal influence (under the difficulties arising inevitably from recent sanitary legislation) I have always done my very best to remedy it, or even to render it impossible, in the case of my own students. But I hold it to be far less of an evil that a man should go out into practice, not having seen a case of measles, than that he should have seen, and handled, and treated hundreds of cases of all manner of common diseases, after the frisky manner of Mr. Robert Sawyer and Mr. Benjamin Allen, the undying literary types of the *ancien régime*, under the system of compulsory apprenticeships. And I sincerely trust that, if any attempt is made (as seems likely) to restore to the medical curriculum whatever may have been good in that system, it will be under such wholesome regulations as to time, place, and manner as will effectually give substance and reality to the instruction afforded, and avoid perpetuating in a new generation, or perhaps in two or three of them, the routine practice, and the slovenly and hasty diagnosis,

which could alone make the suggestion of an "automatic doctor" a possible thing.

These, however, gentlemen, are considerations not for you (at least in this stage of your career), but for your seniors, and especially for the General Medical Council, and Mr. Wheelhouse as an influential member thereof. I trust I have not in any way exceeded the bounds either of duty or of courtesy in submitting to you and to him these remarks (somewhat longer, perhaps, than I intended) on this most important subject. What remains for you to do is to avail yourselves fully of the splendid opportunities that lie open to you in this medical school and in your magnificent hospital, for laying the foundations, both broad and deep, of a medical discipline that will stand the strain and support the weight of the responsibilities that will by-and-bye be laid upon you. How this is to be done in detail I have already considered in several of the addresses included in the volume I spoke of, and of which this is in part the echo in a somewhat different form. The gist of it all is that the instincts of the naturalist, the love of nature and of fact, the reverent study of the laws of biological science, and of the physical sciences which underlie the phenomena of living as of all other matter, form the salt which redeems all our professional work alike from barrenness and from falsehood in principle. You first encounter these laws and phenomena in your study of chemistry and anatomy, zoology and botany. I advise you to lay the foundations well *there*, and by no means to lay the flattering unction to your souls that any of these are *useless* studies, because they seem not to be immediately *practical*. Apart from their manifold applications in practice, they are of untold value as a discipline. And, as I have insisted elsewhere, this discipline at an early stage of your medical career is of special importance, because it is of a kind almost overlooked in much of your earlier school education. What you learn in these scientific departments—always supposing that they are taught practically, and not *only* in books and lectures—is to come to a closer grip with Nature and with

fact, and not to be deceived and deluded either with false shows or learned philosophies and philosophisings. And this is a kind of knowledge, *the knowledge of things as opposed to words and abstract ideas* (for so Michael Faraday called it), which is often greatly deficient even in educated minds, and the want of which is felt at every stage of a medical career. Let *thoroughness*, therefore, enter into all your dealings with these earlier studies—those of your first and second year. It is not merely that you are gaining *information*, but that you are *forming habits*.

Then, as regards the application of the habits so formed to your studies in medicine and surgery proper, I will ask you to-day to receive, and to ponder, only one or two suggestions from a teacher, some of whose early pupils are now grey-haired practitioners. I do not propose to speak of doctrinal instruction, the value or even the correctness of which depends largely on the teacher, but only of clinical instruction. In this, as in everything else, there is a right way and a wrong, and the difference between the two ways consists almost entirely in what I have called *thoroughness*. If you learn *thoroughly* to know even *one* case—diagnosis, prognosis, treatment, and all about it—in a humane as well as a scientific spirit, you may rest assured that you have got something which will remain with you all your life. If you learn, otherwise than thoroughly, to know after a fashion a great many cases, you have also got something that may remain with you as a life-long possession ; but that something is simply a bad habit of working, while in the other case it is a good habit, and, over and above this, a valuable addition to your positive knowledge. I can remember now single cases seen by me forty years ago, and some of them are so printed in the memory that they have all the vividness, as well as the value to me, as though they were of yesterday. Now, I am going to say to you what may seem to be, but is not, a reflection upon particular teachers—or pupils. There is a way of perking and pluming one's self upon a *diagnosis*, as who should say, " See how rapidly and surely I can do it ; a single glance at the expression, a

moment with the stethoscope, a word or two of conversation, and the thing is done, and the whole case is before my mind." This, according to my experience, is a very fatal bad habit, both for the teacher and the taught. No doubt rapidity of diagnosis comes with experience, and is not without a certain value; but in teaching, the first and the last lesson is always, or ought to be, *thoroughness*; and thoroughness, in the sense in which I am using the word now, is not consistent with aiming at these lightning-flashes of intuition. A true diagnosis risks nothing; it aims at being exact, not at all at being wonderful or rapid. Therefore it is never a thing to make a show of, or to boast one's self about; and he that does it in that boastful spirit is, in the very act of doing so, setting a bad example and teaching a bad habit.

What shall we say, then, of those out-patient rooms where twenty, thirty, or forty cases are disposed of in an hour, not only as to diagnosis, but also as to treatment? What, again, of those immensely busy practices where the mere multitude of the cases, or the distances to be traversed, make it well-nigh impossible ever to *complete* a diagnosis? Can these be considered as fit fields for gathering sound clinical instruction, or for maturing the crude experience of young aspirants whose chief desire is, according to the programme I mentioned a little while ago, to have personally seen and treated almost every common form of disease? I do not stop, as you will observe, to argue out these questions, for I have no time to do so; but I state, without the slightest hesitation, my own sincere conviction that for sound clinical teaching and for the formation of correct ways of observing, and thinking about, and treating disease—such habits, in short, as will be serviceable for a lifetime—the first and absolute rule is *thoroughness*; there must be no hurry. An hour, or even more, spent over one case (always supposing that the state of the patient admits of this) is out of all calculation more valuable than five minutes each over twelve cases, or even ten minutes each over six cases. And therefore I say again that the maxim

which should guide our superiors in laying down the law of practical instruction is *Non multa sed multum*; not a great multitude of cases just seen, or very superficially observed, but a smaller or much more moderate number, the deliberate and careful observation and management of which in detail may form, as Tennyson sings, the stepping-stones on which to rise to the higher levels of a more rare, or a more difficult, or a more advanced experience.

Gentlemen, I have done. I had a good deal more that I should have liked to say to you, but the lapse of time warns me that there will be some of you at least waiting anxiously for the end of my sermon. It now only remains for me, as an outsider, to wish you all possible success and all possible happiness and efficiency in the studies of this medical school during the present session. I have endeavoured to indicate to you only the broad lines on which such success and such efficiency are to be sought. It is for you, so far as lies in you, to make the application.

God bless you all! Farewell!

THE TWO DISCIPLINES IN EDUCATION

An address delivered at the opening of the Medical Session, Mason University College, Birmingham, 1899.

Not very many weeks ago, I had the opportunity of spending a long summer day in the country with a friend who, if I were to name him to you, would be recognised by many here as one of the foremost men, and one of the greatest authorities, in medical science; and along with this old friend, we had one of the very foremost physicians of the American continent, a guest for the day like myself, in a most beautiful country within easy hail of London, amid the downs of Surrey. The occasion was one which might have led to discussion of some of the points I intend dealing with in the present address. I do not remember that there was much, if any, discussion of that kind among us, and yet I have something like an instinct that you may take what I shall deliver to you as being substantially in harmony with the views and opinions of these eminent colleagues. One thing, however, I may here mention as having taken some hold upon my own mind, that of a life-long teacher of medicine. This was a lecture given by our host, in the afternoon, to a mixed audience of all ages and ranks, within an "educational museum" that owes its existence in this sequestered village to the energy and restless fertility of resource that characterise my friend alike in his professional career and in his relaxations—if so they may be called. This lecture was one of several carried on at intervals during

the summer and autumn, under the auspices of a local committee with whom the museum is vested as a trust. It goes without saying that the lecture was no common one—it could not have been so, considering the lecturer—but as it is with the method more than with the matter of the discourse that I am concerned at present, I will venture to use it as an illustration, without too many details. One thing, however, particularly struck me, in the very commencement of this altogether remarkable lecture. It had been announced that the first twenty minutes or half hour would be given to some common object or objects freshly provided, while the remainder of the discourse was to be upon "Whales." In this first portion the key note, as it were, was struck by presenting to an intelligent little maiden of perhaps twelve years old, sitting near the lecturer, a severed branch of a fir tree just imported from the adjoining woods, and asking her—"What is that brown object growing in the cleft of the branch there?" "A pine cone?" "No, it is not a pine cone, though it may look a little like it. You see it has some of the green leaves growing out of it. Now, take this knife, and cut what you supposed to be a cone down the middle." This cut being performed revealed an insect inhabiting the woody growth, and this led to a few minutes' discourse on these animal parasites, or galls, in their more general relations. Then followed an exhibition of potato blooms, so recently recovered in our fields after many years of almost entire absence; with some remarks on the possible causes of these changes; but, as I have said, the details of the lecture being of less consequence for my present purpose than the method of it, I will pass over what was said upon this subject. Interest being thus thoroughly aroused, alike among old and young hearers, by this dealing with common objects, the main subject of the lecture as announced was proceeded with, and proved to be a most philosophical, though by no means too abstruse, discourse on dried specimens in the museum, of the structure of certain whales and their congeners, the dolphins, narwhals, etc., etc.; following out the lessons to be derived

from the modifications of the skeleton and the dental apparatus in different tribes of animals more or less resembling each other, as viewed in the light of Darwinian principles by a thorough and experienced student of nature. It was a lecture, which, from plain and simple, very visible and very tangible facts, presented in the first instance objectively (to use a comparatively modern philosophic word), extended up to very general and somewhat recondite laws of nature; but above all, it was a lecture which had "Nature" for its groundwork and its basis, observation of Nature for its primary method; and which thus tended to quicken, or educate, in an equal degree, the faculties of observation and those of reflection.

THE METHOD OF SCIENCE.

In the course of hearing this admirable exposition, and noting at the same time, the very vivid and unfeigned interest displayed in it by old and young alike, I was led into some considerations which were not by any means new to me (as you will presently find), but which I have since thought might be turned to account as a fitting basis for the present address. Observe, once more, that it is the method, and not the matter, of this teaching on which I desire your attention to be fixed, as students of medicine. It would be going too far, perhaps, to affirm that the method here in question is the exclusive method of medical science or of any other science. But it is, in a broad and general sense, the method of *Science*, as distinguished from *Literature* and *Philosophy*, which, in days not very long past, well-nigh monopolised the field of education, and seemed to many generations of our ancestors to comprehend, if not to exhaust, all the available faculties of the human mind. My desire is, not to disparage literature and philosophy, much less to insist upon these, or any department of them, being supplanted by newer methods in education; but to show that what I have already called the *objective* method has, and ought to have, a quite definite place in all education, from the infant

school up to the University ; and that to refuse or deny to it this proper place is practically to arrest the growth of certain faculties which are at least as important in the evolution of an educated human being as are those which have for ages been subjected to the disciplinary methods commonly called scholastic, inasmuch as these methods have had almost the undisputed possession of the schools, and therefore of the minds trained in them. My argument will be, not that the scholastic discipline is bad of its kind, but that it is, or at least has been, defective ; not that its results, so far as attained, were otherwise than most important and valuable ; but that other results, which should have grown up side by side with these, and in perfectly harmonious association with them, have been artificially withheld and denied the opportunities of development which they might have had, and would have had in a complete and well-balanced educational system.

This complaint is not new. The late Professor Huxley, in the course of a long and busy life devoted in almost equal proportions to science and to controversy, omitted no opportunity of urging the claims in education of the kind of discipline here referred to ; and in his case the brilliancy of his own literary gift, and the luminous powers of exposition which he applied to certain philosophical topics, forbade the idea often—and perhaps in some cases justly—entertained of the mere scientist, that in the riding of his own hobby he would willingly ride over, or at least would be utterly careless of, everything else that came in the way. Moreover, in his lecture—" On a piece of Chalk "—Huxley has left to us a practical and outstanding illustration of the kind of wide scientific outlook that may be based even upon a very common object, and of the singular attractions which such an exposition may have—and ought to have—for all minds that have not been already fatally distorted by overmuch and exclusive training in book-lore, and the merest pedantry of current educational systems. And almost, or quite a generation before Huxley, when, indeed, that brilliant and combative genius may have been (for aught I know)

among the early entranced child-listeners at the Royal Institution, Michael Faraday was experimentally unfolding and expounding the "Chemistry of a Candle" to audiences which, although the simplicity of the treatment was such as to fascinate children in their Christmas holidays, and was in the first instance intended for them—came to include also many of the most gifted statesmen, philosophers, and adult persons of social eminence, in London.

FARADAY ON ENGLISH EDUCATION.

These lectures at the Royal Institution, by one who is confessedly one of the greatest scientific investigators of the present century, were probably responsible in some degree for the first effective protest against the one-sidedness of the scholastic methods as then pursued; for in 1855, Faraday (although with characteristic modesty he confessed himself to be "not an educated man") affirmed, in presence of Prince Albert and a large assembly of highly cultivated and socially notable persons, his clear conviction that the most highly educated minds in this country were often entirely undisciplined in the merest elements of the knowledge of *things* as opposed to *words* and abstract ideas, and that these minds, even in mature age, remained impermeable to truths which to one with any scientific discipline at all were all but axiomatic.

Faraday was not only assured by his own experience as a teacher that very young minds were open to receive, and ready in apprehending, the data and inferences resulting from well-conducted experiments, but he had also had before him the fact that in the most highly cultivated classes in English society persons were to be found on whom experimental and physical evidence failed to make any impression at all, when brought into contact with an *a priori* conviction, however foolish and absurd. The crude impostures of table-turning, spirit-rapping, and other quasi-supernatural manifestations were at this period in full flow of their fashionable popularity; and Faraday, who was, withal, a man of

the most sincere and profound religious faith, maintained without hesitation that these materialistic communications, alleged to be from the spirit-world, were, as such, wholly unreal and misleading. As regards some of them, he proved to demonstration, by physical experiments of a remarkably simple kind, that this was so. But still the great world, and the fashionable world of society went on believing, and furnishing converts to wholly exploded fallacies. Faraday was thus led to the conviction that there was a grave deficiency in the scholastic discipline of very high-born and distinguished Englishmen in his day; and in 1862 he obtained the opportunity of explaining and impressing this opinion more at large before the Public Schools Commission of that date.

When addressing the British Medical Association from the President's Chair, in Glasgow, in 1888,[1] with special reference to the demands of medical education, I had occasion to cite this testimony of Faraday at some length, with the view of showing that although some response had even then been made to his modest representations, supported as they were generally by the Public Schools Commission already referred to, and still further by a later Commission on Scientific Instruction in Public and Endowed Schools in 1875, there was still much to be done before the lessons read to the first of these Royal Commissions by this great master in science could be said to have had their just effect. Faraday was himself fully aware of the difficulties, and even dangers, of prematurely urging, or rather forcing, the claims of science-teaching; and no one could be less open than he to the reproach of having made revolutionary proposals in favour of science, to the detriment of the older and more established scholastic discipline. Others were no doubt much more impulsive, or less restrained, or less unwilling than he was, speaking always as he did in the character of one who was "not an educated man," to carry war into the camp of the humanists. In the admirable "Romanes

[1] *The Physician as Naturalist.* Glasgow: James MacLehose and Sons, 1889. Also in *British Medical Journal*, 11th August, 1888.

Lecture" recently published by my former colleague, Professor Jebb,[1] I find generally that the sixties—*i.e.* the years immediately following the report of the Public Schools Commission—are referred to as being the critical moment, "a moment in this century when the attack upon the humanities was somewhat formidable." It is very satisfactory to note that, as the result of his whole investigation of the subject, Professor Jebb considers that the humanities have successfully weathered this storm, and are now, towards the close of the century, in a stronger and more secure position than they were before. "In the sixties," he writes (p. 35), "considering the strength of the attack both from without and from within, the position of humane studies was certainly more seriously imperilled than it had ever been before. . . . The danger was lest the powerful alliance between insurgent men of science and disaffected humanists, aided by the legions of Philistia, should force on a movement for imposing restrictions in a spirit altogether favourable to the new studies, but unfriendly to the old; with the result that classical studies might be so narrowed, so hampered, so maimed, as to lose nearly all their educational virtue: and, after languishing for a time, might gradually die out of the schools."

The other side of the question is stated not less candidly, though not, perhaps, with the same fulness of knowledge as regards scientific results and methods, by Professor Jebb, as follows (p. 37):—

"In the last thirty years the position of the humane letters, relatively to other studies, has been altered in several important respects. The study of the natural sciences is now firmly established in schools and universities; it can no longer be said that a haughty and exclusive humanism keeps them out of the educational field; indeed, there are not a few seats of learning where they hold a clear predominance. . . . This establishment of the modern studies is, so far as I have seen, viewed by humanists generally with

[1] *Humanism in Education.* By R. C. Jebb, Litt.D., Hon. D.C.L. Macmillan and Co., 1899.

cordial satisfaction. The spirit of humanism, indeed, wherever it is not a narrow pedantry, is one which welcomes every accession to the domain of sound knowledge. Meanwhile, the claims of humanism itself, sifted by a period of controversy, and illustrated by larger views of liberal education, which now prevail, are usually stated with more discrimination than formerly, and are more willingly and more widely acknowledged."

THE "ATTACK" ON HUMANISM IN THE SIXTIES.

This is surely a very satisfactory result, so far, of a controversy in which, as I have all along held and maintained, the two parties that have appeared as jealous rivals in the educational field, ought in reality to have been the best of friends; inasmuch as each of them was pursuing an end quite necessary in itself, and one which, rightly understood, so far from excluding, or being incongruous with the other, ought rather to have been considered as giving it the support and assistance required in working out a common purpose—the all-round evolution, or education, of the faculties of the human mind. But I am by no means convinced that the "attack" (to use Professor Jebb's expression) on humanism, which is attributable to the introduction of the methods of natural science into education, has by any means exhausted itself, even now. All the more, therefore, I am grateful to the Romanes lecturer for making it clear to me and to all men that the danger which was so seriously apprehended in the sixties, has been averted, or (as I should put it) shown to be really no danger at all; and that everything in humanism that is most of its essence, and therefore of most abiding value, has not only survived this "attack," but has become stronger and more broadly and truly human, through the trials to which it has been exposed. Partly by getting rid of certain pedantic and deeply engrained defects of method, and partly by the development of quite new kinds of interest in the old and traditional studies, the classical and other pursuits which constitute humanism have been strengthened,

rather than weakened, by the intrusion of the new or more modern elements in education. As Professor Jebb himself puts it (p. 34), "the humanistic studies have, during this century, become wider and more real. They have gradually been drawn out of a scholastic isolation, and have been brought more and more into the general current of intellectual and literary interests. So far from losing strength or efficiency by ceasing to hold that more exclusive position which they occupied two or three centuries ago, they have acquired a fresh vigour, a larger sphere of genuine activity, and a place in the higher education which is more secure, because the acceptance on which it rests is more intelligent."

DANTE—PETRARCH—ERASMUS. THE ITALIAN RENAISSANCE.

It would be very unbecoming in me to attempt to add anything now to the force of these words, carefully set down as the result of a life-time of experience and research by perhaps the greatest of our living humanists, a friend and (I am glad to recall the fact) an ex-colleague of my own in the University of Glasgow. But I may be permitted to remark that this hopeful and tolerant attitude of mind, towards the changes in our educational policy that have been gradually taking place since the early sixties, has been by no means universal; and that the admission of the new methods and new forces of scientific training into our educational system has been very grudgingly, and often inadequately conceded by those who, themselves trained on the older lines, and being officially masters of the situation, have treated all innovations as hostile, and their authors as enemies to culture instead of friends in disguise. Perhaps there may also have been a spirit of undue aggression and even contempt on the other side, among those who have written and spoken as if the so-called "modern" studies were all-in-all in this nineteenth century of ours. To such over-zealous partisans the *apologia* of Professor Jebb may be commended, as a temperate, careful, and I need scarcely say thoroughly well-informed historical and argumentative

statement of the claims of "Humanism in Education." He attributes to Petrarch the honour of being the pioneer in "that great movement which we call the Renaissance." Dante, who was thirty-nine years old when Petrarch was born in 1304, was the last great (and perhaps the greatest) figure of the middle age "standing in the borderland between the darker ages and the revival, when he shows us a keen intellect and a sublime imagination moving within the limits, and obedient to the forms of mediæval thought." "At the further verge of the Renaissance" (and at quite two centuries' distance from Dante) we have "Erasmus, the life-long antagonist of the schoolmen, who makes so vivid to us the contrast between the intellectual atmosphere of scholasticism and that which the humane letters had created" (p. 6). And in these two centuries no one surely will now attempt to deny that the human mind awoke as from a slumber, and entered on a new course of life and activity, as a consequence of the Italian revival in the 14th century. "Petrarch's ideal of humanism," says Professor Jebb (p. 7), "as a discipline which aims at drawing out all the mental and moral faculties of man, pervades the whole course of the Italian Renaissance."

That this ideal was not always attained; that it was "often obscured by affectations or puerilities," and "not seldom belied by evil living"—a kind of revived Paganism, in short—Professor Jebb frankly admits. But, all the same, it was "a real force, which comes out more or less in all the greater and nobler of the humanists." To all this, considered as the history of a great movement which began more than five hundred years ago, and has had an abiding influence on the educational methods of all European countries, one can readily assent, while maintaining, with Faraday, Huxley, and all the great scientific authorities of the present century, that the discipline of humanism requires now to be supplemented by a newer and different discipline; and this, not merely, and not chiefly, on account of the utilitarian value of the latter, but in obedience to the very principle attributed to Petrarch, that education, in the large and

liberal sense of the word, should be such as to "draw out all the mental and moral faculties of man."

It is with a view to this distinction, that I have ventured to give this Address the title of "The Two Disciplines," and I hope to show you, before I have done, that without at all disparaging the first, or older discipline, that of the Renaissance, commonly called Humanism; the second, which finds its objects in the physical facts of nature, and its methods, generally, in those of what we call physical and natural science, must be firmly upheld and pressed home, until it permeates every phase of education, as I said before, from the Kindergarten School up to the University.

WHAT THE RENAISSANCE FAILED TO DO.

The great men of the Italian Renaissance, owing to the very fact that they were so exclusively and completely preoccupied by the revival of learning, and mainly of classical learning, could not be expected to perceive that learning, even if we include in it philosophy as well as literature, and mathematics as well as philosophy, does not by any means exhaust the cycle of human faculty. It would have been indeed marvellous if, in an age when astronomical science, founded on genuine and on the whole exact observation of nature thousands of years ago in Chaldæa, and methodised (though erroneously) by Ptolemy, had degenerated into astrology; when chemistry (whatever there may have been of it) had passed into the visionary search for the philosopher's stone and the elixir of life; when biology, pursued at least on a large basis of observation by Aristotle, had become absolutely inept, and a mere repetition of verbiage attributed, and often ignorantly attributed, to the ancients; when anatomy, founded at least on something like personal observation in Galen's time, and carefully pursued by Erasistratus and others in the Alexandrian school, had altogether perished, and was, indeed, forbidden by authority; and when the science and art of medicine, so necessary at all times for the preservation of life and health, had become a mere craft,

with such learned equipment as we read of as Chaucer's "Doctour of Physike";—in such an age as this, it would have been more than human if the pioneers of the Renaissance had ever thought of placing the discipline of science, or of any kind of observation of nature, upon their programme of an all-round education of human faculty. In their revival of classical learning, they had the support and patronage of popes and of Medicean princes, even when the trend of the new studies seemed to be in the direction of sheer paganism alike of thought and of life. It was safe enough to denounce, or to ridicule, the pseudo-sciences of the time; and this (according to Professor Jebb) the humanists did, and did effectually, especially as regards astrology; for "from Petrarch onwards," he says (p. 15), they "made open war on this flourishing imposture." But the discipline of true science (or of natural and physical phenomena) was a very different thing from this negative attitude towards its counterfeits; which may, however, be placed to the credit of the humanists. From no pope or Medicean patron in those days could any effectual assistance have been expected, either in genuine and fruitful investigations of the *arcana naturæ*, or in teaching the methods of "science" as we now understand them. The fate of Roger Bacon, in the century immediately preceding Petrarch, and of Copernicus and Galileo later on, remain for us as standing monuments at once of the dangers that waited upon the pursuit of physical science in mediæval times, and of the small share that the movement called the Renaissance can claim in advancing these pursuits to a place in the education of humanity.

A NOTABLE PHRASE OF J. C. SCALIGER.

There was, however, at least one humanist, of the latter period of the Renaissance, who by a single brief epigrammatic phrase, has managed to shew that he, at all events, was not wholly insensible to the value of a discipline such as Faraday advocated and practised, and such as we are

now considering in this address. The Scaligers, though they bear a name of distinction, were not, perhaps, in the front ranks of the humanists, and certainly did not do very much, in their day, for the advance of science. But the elder of the two, Julius Cæsar Scaliger, was trained as a physician, and, I believe, practised as a physician; and in writing to another and perhaps more eminent physician, Jerome Cardan, somewhere about the year 1557, in the course of a long, desultory, and somewhat unreadable dissertation [1] "De Subtilitate" he has formulated the following very notable phrase, pointing, as he seems to think, to something wanted in the educational tendencies of his time. The special subject in view is Rock Alum, and the fallacies current in respect to it; and I am certainly not here to allege that the reflections of Julius Cæsar Scaliger on Rock Alum have any kind of resemblance to those of Mr. Huxley "On a Piece of Chalk." But it is something, surely, to have compressed into seven brief words of Latin an idea so foreign to the humanism of the Renaissance as the following:—*Rerum ipsarum cognitio vera e rebus ipsis.* If Julius Cæsar Scaliger had been Michael Faraday, and if he had lived in the nineteenth century instead of the sixteenth, he could hardly have given a more complete and condensed expression than we find in these words to the defect in the purely humanistic programme of education on which Faraday so strongly insists in his evidence before the Royal Commission in 1862. What Faraday deplored as being defective in the most highly educated Englishmen of his day was the knowledge of *things* as opposed to *words and abstract ideas of things*—or, in the phrase of Scaliger, a true cognition of the things (or objects) themselves, taken directly from the things themselves. The argument of the highly educated grammarian of the 16th century was that the want of this direct knowledge of things led to some curiously distorted views about rock alum and many other natural

[1] Exotericorum Exercitationum Liber xv. de Subtilitate Exerc. 104. Sec. 6, "de Alumine Rochae." I quote from the edition of Francof. 1612; but there are various editions from 1557 onwards.

phenomena. The contention of the "uneducated" philosopher of the 19th century was that the "want of judgment in natural things," and the "great deficiency in the power of giving the reason why" were such among the educated class as often to defeat the ends of justice in courts of law, and to lead to the most humiliating exposures of confident ignorance and presumption in matters of fact; and all this in the case of persons who had received every advantage, and who, in all kinds of learning and in the discipline then provided in the schools, stood in the front rank of Englishmen. And here it was, that the almost unique experience of Faraday—unique at least at that time—came to his aid; for he had found personally, in the course of his lectures in the Royal Institution, that the elementary ideas of the "judgment in natural things" were not only easily conveyed into the minds of children, but had a peculiar fascination for them. "The young mind," he says, "as I find it . . . is very observant and asks most acute questions. I do not find it, generally speaking, backward to understand a statement I make in simple language; and if I tell him this or that . . . and then shape it into a question, he can generally answer me. I must confess to you that I find the grown-up mind coming back to me with the same questions over and over again. . . . Their minds are not prepared to receive or to embody these notions, and that is where you want education; to teach them the A B C of these things. . . . I never yet found a boy," he says in another place, "who was not able to understand by simple explanation and to enjoy the point of an experiment."

I am repeating, Gentlemen, in these quotations from Faraday's evidence, and in some of the remarks attached to them, the very words that formed part of my Address to the British Medical Association in 1888. But I think you will concede to me that the words are worth repeating. Here is a master in science, if ever there was one; but one who, nevertheless, proclaims himself "not an educated man" according to the conventional views of education in this country. He does not, however, express any resent-

ment at the current discipline, or any desire to attack or destroy it. "I should be very sorry," he says, and evidently with conviction, "that the introduction of science should be an injury to any other branch of knowledge." All the same, however, he insists with the confidence of one who thoroughly knows what he is speaking about, on the disastrous effects of an omission which he finds in the highest and most coveted English disciplines of his own day. And this omission is practically identical with that foreshadowed by the man of the Renaissance, Julius Cæsar Scaliger, in 1559—*Rerum ipsarum cognitio vera e rebus ipsis.*

THE TWO DISCIPLINES CONTRASTED.

In thus insisting on this old and half-forgotten expression, I do so not because it had any considerable effect at the time, or because it had much apparent significance in relation to the career either of Scaliger or his correspondent Cardan; but because it formulates for us now more exactly and more briefly than any other words I can devise, the kind of discipline and method which in the commencement of this address I had it in view to illustrate, and which it was Faraday's object to introduce more largely into education. The essential difference of the "Two Disciplines" (as I have ventured to call them by way of contrast) is that one is based upon ideas which have already received expression in human speech; while the other is concerned chiefly with objects, that is, with phenomena occurring in external nature. The former discipline accordingly is (whatever else) necessarily preceded by and mainly founded on linguistic studies, and keeps close to them throughout, inasmuch as language is not only the vehicle of ideas, in respect of their intercommunication; but (as Max Müller has so cleverly put it) the λόγος is at once the word and the thought; the one being in a practical sense inseparable from the other. Hence the trend of this discipline is towards *humanism*, *i.e.* towards the discovery and highly elaborated study of the best models and examples alike of thought and of

expression; the noblest and most inspired ideas of the most outstanding men, clothed in the most appropriate and (if it may be so) beautiful, but at all events fitting and luminous, words. The men of the Renaissance found this kind of perfection in the literatures of Greece and of Rome, and directed all their energies to the revival of these, and to living over again, and thinking over again, the best ideas of classic antiquity. Nor is there anything to be said against this noble humanistic ideal, as it presented itself to Petrarch in the 14th, or to Professor Jebb in the 19th century, except what the latter, indeed, frankly admits. Humanism may be "obscured by affectations or puerilities," it may be "belied by evil living," founded on the too admiring familiarity with Pagan practices and habits of thought; or it may, as a discipline, become much too technical—as sometimes in its grammatical developments, to be a good all-round education, even for a humanist. For, as we read in the Romanes Lecture (p. 27), "the modern specialist in certain branches of classical study, may come perilously near to passing out of the province of humanism."

λόγος AND φύσις.

Such, then, are the leading characteristics of what we may call, historically, the older and, until recently, paramount scholastic discipline, founded mainly on language, and including in its widest range philosophy and mathematics, which are also, as studies, only to be compassed through verbal symbols and definitions underlying their whole argument. This discipline begins in the individual man with the actual A B C, or rather with the first spoken words of the child, and is methodised and expanded in the school and university either into humanism of the classic and renaissance type, or into some other variety of literary and philosophic evolution founded on this, but more largely permeated by modern ideas. Having in view the contrast I am about to present to you, and also the large and comprehensive interpretation placed upon the word by Max

Müller, I am disposed to indicate the whole of this discipline in one Greek word, and call it the discipline of λόγος.

But now let us revert for a moment to the other discipline, which occupied our attention in the beginning of this address—and which, instead of calling it by the ambitious title of the discipline of science (although it deserves to be so-called) I prefer now to consider under its simpler and more universal aspects, as the discipline of φύσις, *i.e.* of nature and natural phenomena. I have already employed this expression for it in my former address, and have had occasion to point out that by a very ancient tradition embodied in the name *physician*, the medical art and its professors of the higher order have been all along supposed (sometimes it is to be feared erroneously) to be specially imbued with this discipline, or at least to have a quite special interest in it as the only fruitful method of medical science. It is, moreover, true at least of Hippocrates, the Father of Greek medicine, that he anticipated by some two thousand years the well-known expression of Francis Bacon, by indicating the true position of the healer as being ὑπηρέτης φύσεως—the servant (or minister) of nature. And, if further evidence were required, the very foundations of what we now call Biology, pursued in relation to the art of healing, came to be spoken of at a very ancient date as Physiology—the doctrine, or λόγος of φύσις—a designation which it has retained till our own day. There is thus a reasonably good traditional ground for the position in which we stand to-day—in this flourishing medical school of a nascent University—pleading for the more ample recognition of the discipline of φύσις, in relation to general education.

It is perhaps worthy of note here, that in the natural order of evolution of human faculty as regards the individual, the discipline of φύσις is first, that of λόγος second. The latter commences with the first words spoken or apprehended by the infant, followed up in time by the A B C. But the A B C of φύσις begins in the cradle, in the very first hours of conscious life. The whole existence of the infant is a discipline—sometimes a painful, oftener a joyous one, in

the wide school of natural phenomena. His very primordial instincts, the play of hands and feet and lungs and voice, tutored and guided by experience, are a part of that natural discipline. To learn that fire burns, and that water cools and cleanses; that iron is hard and heavy, while wool is soft and light; that colours, sounds, tastes, and smells differ, and may mean good or evil, help or hindrance, in particular circumstances; these are the first steps in the discipline of φύσις and are learned, as a rule, with great ease and confidence, though not without mistakes, from experience, long before the A B C of the other discipline has begun to be thought of. In the admirable kindergarten schools of recent times, moreover, the discipline of φύσις is often attended to and aided by well-contrived efforts and experiments, increasing both knowledge and power without any sense of task-work, or fatigue of body or mind, and with the clearest proof of enjoyment on the part of the child. But, so soon as the discipline of λόγος comes in, monopolising the attention and making drafts upon the intellect in the shape of memory and task-work, the more natural discipline is apt to be thrown aside or even severely repressed, as being unprofitable in a scholastic sense. At the very least, it fails to be methodised and expanded, and is not turned to account either in a scientific or any other direction, as the discipline of λόγος is when it carries off all the tangible rewards, and in fact becomes exclusively what is called "education" on the lines of humanism, as we have just seen.

Fortunately for the human race, however, the discipline of φύσις is not confined to the schoolroom. The fisher learns a good deal of it in this commonest of sports in studying the habits of the *salmo fario*, or even of the minnows in the pool in childhood, and does not dignify what is so learned with the name of "science." The child addicted to bird-nesting, the collector of flowers, or of insects, of sea-weeds, or any other natural productions, acquires a hold thereby upon one or other corner of abounding "Nature," which may or may not be wisely cultivated. In every kind of sport, in almost every occupation pursued in the country,

φύσις will have her votaries and, up to a certain point, her successful students. A Gilbert White will indulge to the full his wonderful instinct for the observation of nature even in a quiet country parsonage, and will give us the "Natural History of Selborne." A Hugh Miller of Cromarty, combining a fine literary instinct for "humanism" with native scientific genius, will tell you years afterwards that the best part of his early education at the parish school was on the days when he "played the kip," *i.e.* the truant, and devoted himself to long walks, taking in slowly the lessons which afterwards came out in "The Old Red Sandstone," and "The Testimony of the Rocks." But such brilliant aberrations of scholastic discipline only bring to mind the old Latin proverb—*Naturam expellas furcâ tamen usque recurret.* Faraday's principle is that the great natural aptitude which most boys display for observation and experiment ought to be fostered, and not hampered, in a general scheme of education. No doubt, the more recondite and abstruse generalisations of "science"—especially those which involve mathematical considerations—are beyond the scope of most boys at school, as they are of most men even at a later period. But that is no reason for rejecting the discipline that arises from much more simple and easy demonstrations of φύσις. The lectures "On the Chemistry of a Candle" were of this description, and they have been published. They were intended to shew, and did shew, that most important scientific generalisations, resulting from experiment, and actually demonstrated in presence of a juvenile audience, could be made perfectly clear to the minds of school-children, if presented in simple language by one who was a master in the subject.

FARADAY'S DIFFICULTY—TO FIND THE TEACHERS.

It was at this point, however, that Faraday did not fail to foresee a difficulty which is even now, perhaps, not unlikely to endure for some time to come. Where are the masters to come from? In his own days he said, "I am often asked

what men to recommend to spread a knowledge of natural physical things. I cannot recommend such; such men hardly exist at present. They want the A B C of science, not the X Y Z; they want the first elements." Faraday was well aware that a mere scheme for teaching "science" among a lot of what are commonly called "modern subjects" would not work. "It is not the matter," he said, "it is not the subject so much as the man. You want men who can teach (science) and that class has to be created." Faraday knew well (though he did not put it exactly in these words) that "science" (so-called) might be taught out of a text-book, by rote, like anything else, and in such a way as not to have the smallest relation to the discipline of φύσις. The one essential point in that discipline, as distinguished from that of λόγος, is that the teaching must be real of its kind, founded on experiment and observation directly, and not doctrinal. It must be, in short, *rerum ipsarum cognitio vera e rebus ipsis.*

Now in the actual teaching of a medical school in these days, gentlemen, we do our very best to make this kind of teaching yours as far as we can. You are taken into the dissecting-room and are expected to learn for yourselves (with assistance of course)—to make yourselves practically familiar with the anatomy of the human body by the work of your own hands and senses. You have to undergo a similar discipline in the chemical and physical laboratory, and again in the physiological, and again in the pathological laboratories, and in many others. In the great practical departments, you have your clinical work in the hospitals, which, if well managed by competent men, ought to be all a part of the discipline of φύσις and nothing else. A certain amount of dogmatic instruction by lectures and text-books, it is true, is absolutely indispensable, and I am not here to disparage it, as I have long practised it myself, and have fully discussed elsewhere the need that still exists for it.[1]

[1] Two Lectures. I. Lectures, Books, and Practical Teaching. II. Clinical Instruction. University of Glasgow, Session 1877-78. [The first of these Lectures was printed in part in *The Lancet*, November 17th, 1877.

But every systematic teacher of any eminence or effectiveness will tell you, that if he were deprived of his practical service, his teaching, even as a systematic and more or less doctrinal lecturer, would inevitably degenerate. Here again, therefore—*rerum ipsarum cognitio vera e rebus ipsis*—is the true note of the discipline of φύσις. If general doctrines and conclusions must needs be taught, they should be taught as much as possible in the light of instances; and the more these can be freshly provided and brought before the senses of the learner, the better.

But the question which I am here to discuss to-day, though it is one of medical education, is also one of general education, as this will, I trust, come to be largely moulded and dominated by the Midland University of the future. Would not medical education be greatly improved, and rendered easier as well as better in quality, if the well-nigh lost discipline of φύσις were to be restored, in some appreciable measure, to the "grammar school"? Nay, would not the discipline of letters itself be improved, were it to become a fundamental principle in all education, from first to last, that λόγος and φύσις should go hand in hand, each of them, of course, in a measure suitable to the advancing intellect and general growth in faculty of the child or adolescent? My argument is that this ideal, though perhaps difficult of complete attainment now, and for some time to come, is not impossible, nor yet in any true sense injurious to the older discipline.* I take it on the authority of Professor Jebb that the "attack" on "Humanism in Education" in the sixties has not been really disadvantageous to the latter, but rather, by purging humanistic studies of pedantic excrescences and methods, by drawing them out of a scholastic isolation, and bringing them more into the general current of intellectual and literary interests, has actually strengthened their position by making them "wider and more real." Now, if all this be true as regards the higher

The second has been mostly used for distribution to my clinical classes, as an exposition of the method to be pursued. Both Lectures are now out of print.]

education, why should not the same principles, and with a like result, prevail also in the grammar school? Observe that I purposely avoid the use of the words "science" or "scientific teaching" here, because we may be dealing (as Faraday said) only with the A B C, and not with the X Y Z; and further, because "science" may be crammed out of a text-book after a fashion that makes it no more a discipline in φύσις, than are the rules of prosody, or the genders of Latin nouns, or the declination of Greek irregular verbs. But when the proper kind of teachers can be obtained (and that is the greatest difficulty now, as it was in Faraday's time) the discipline of φύσις will make as great and as beneficent a change in the grammar school as it has already, in part, done in the Universities and seats of the higher and more advanced learning.

On this point I find little to alter in the language I employed in 1888, a few sentences of which you will perhaps excuse me for repeating, as I cannot find any other or better terms now for expressing the changes which I have in view. "Our present programme of school discipline is still too largely founded on the idea that at six or seven years old, as the case may be, a child may be withdrawn, in a great measure, from this strictly natural, wholesome, and all-round education of the faculties (*i.e.* that of babyhood and of the kindergarten school), to have his nose buried for ten long years in books and papers, and mental and other arithmetic, till his little eyes become myopic, and his little brain suffers a corresponding deformity. Athletic sports, as a counterpoise, are no doubt good, and industrial training better, in some cases, especially among the children of the poor; but they do not supply the missing link, which can only be found in the early discipline of φύσις, pursued in the very spirit of Faraday." To which I should only wish now to add, in parting with that great man on the present occasion, a few words from his evidence on this very point, in reply to a question as to what he thought of the ordinary grammar school discipline in this respect. "It does not blunt the mind," he said, "but it gives the growing mind a

certain habit, a certain willingness to accept general ideas of a literary kind, and to say that all the rest is nonsense. . . . It takes up the general impression that a certain kind of knowledge, what I call the real knowledge, the knowledge of things, is of no importance."

A QUESTION FOR THE GENERAL MEDICAL COUNCIL.

I will close with a single remark, which may have the effect, considering that my audience is chiefly concerned with medical education, of showing you that the higher orders of schools are, even now, awakening to the demands that may be made upon them in this respect; and that some, at least, of the medical examining bodies are not indisposed to encourage this movement in advance. I was obliged to separate myself at the last meeting of the General Medical Council from (I believe) all, or most, of my Scottish and Irish friends, and to vote in a minority with the representatives of the English Corporations. The occasion was this. You are aware that the medical curriculum has been, within the last decade, extended to five years; and even with this extension, it is still overcrowded and likely to become more so as the demand for special practical subjects, to be included in the curriculum, increases. Now, Mr. Huxley many years ago suggested, in the interest of course of scientific education as he conceived it, that the strictly medical training of the university and medical school proper might be lightened by getting some of the early discipline, in elementary physics, chemistry, and biology into the schools, to be pursued side by side with the more ordinary scholastic discipline, before the usual age of commencing strictly medical studies. Although this suggestion was not taken up at the time, at least to any very appreciable extent, it now appears that a good many English schools of the higher kind have, by engaging science masters, and providing sufficient laboratories and apparatus, been placing themselves in a position to claim that this kind of instruction may be communicated to their pupils to such an extent as

to enable them to pass either the whole or some portion of the first professional examination. The English Colleges therefore determined to recognise, after due investigation, such schools as appeared to them to be capable of giving, or rather to be actually giving, such instruction as is real and valuable in these subjects. The Irish Colleges, on the other hand, objected to this new departure, on the ground, apparently, that "grammar schools" are not, and should be practically debarred from becoming, schools of "science" in any such sense as should form part of a medical curriculum. Observe, that it is conceded on all hands that five complete years after registration as a medical student must be devoted to professional studies conducted at a medical school. The attempt, therefore, to anticipate by a year the acquisition of some of this preliminary discipline does not in any way encroach on the time or attention to be given to more strictly medical or technical studies; but rather, by clearing the way for these in advance, permits the years passed at the medical centre to be more fully occupied by the studies proper to the medical art. I am far from assuming that no errors have been committed, or that there is not a good deal to be said on both sides; but from what I have already laid before you in this discourse, you will be prepared for the statement that I could not approve of anything that would tend to discourage the teaching of elementary science in English schools; and on this ground I voted with the minority on this occasion. I think the future is with the English colleges in this matter, and I trust they will persevere.

I have now only, Gentlemen, to congratulate you on your appearance in a school so well furnished in all its departments as Mason College, with whose origin and early progress one of my oldest friends, the late Dr. Heslop, was so closely associated that my interest in it may be said to date back to a period when I was still an Edinburgh man, an extra-academical teacher and hospital physician, and not a Glasgow University professor. I most cordially hope, for you, that the great scheme of a Midland University

with which this school is ever to be associated, may be successfully carried out; and meanwhile, I would join in the welcome extended to you by all your teachers, and in the anticipation of a prosperous and successful session.

THE EDINBURGH ROYAL INFIRMARY IN THE 'FIFTIES

WITH REMINISCENCES OF SOME MEN ENGAGED IN ITS MEDICAL WARDS

An address delivered to the Royal Medical Society, at the opening of their Session, on 20th October, 1893.

GENTLEMEN,—It would under any circumstances be an honour, and therefore at once a duty and a privilege, for any professor of medicine to respond to the invitation of the Royal Medical Society of Edinburgh by addressing you, at the commencement of a new Session, on matters more or less germane to your daily work and occupations. But, when the professor alluded to is one who may be said to have spent in Edinburgh considerably more than the half of an average human lifetime, and to have laid there the foundation of whatever reputation attaches to his present position and office, the sense of obligation in so responding to your wishes becomes indeed overwhelming. In my case, too, it is enhanced by the very marked courtesy which has been extended to me on many occasions since I became professor in Glasgow in 1862 ; by the great privilege of sitting at your hospitable table, and otherwise, too, of being allowed to associate with the younger spirits which go to make up, and to keep up, as I am happy to believe it is kept up, the reputation of this greatest and oldest of undergraduate Associations. For if it be true, as the great Roman satirist has told

us, not perhaps out of the fulness but surely out of the most sacred depths of his heart, that the utmost reverence is due to the young, it is no less true that maturity, age, and experience may receive a stimulus, and often, too, a new and more fruitful direction, from contact with younger minds, and from sympathy with youthful hopes and aspirations. Therefore it is not of constraint, but very willingly, that I am here to-night. The duty, indeed, was clear from the first; but the anticipated pleasure and profit in this meeting has been all along more clear even than the duty; and I shall indeed have failed in my purpose if I leave this great assemblage without the sense of having won from you far more than I can pretend to give you—more of sympathy, more of enthusiasm, more of the *vivida vis animi* tending to the encouragement of every good word and work.

It is one of the great privileges of the *teacher*, properly so called, whether of medicine or of any other art or science, that he is able, if he is worthy of it, at all times to enjoy, as well as to profit by, this association with younger minds. That great man and great teacher, the Master of Balliol, who was laid in his grave the other day amid an assemblage of the greatest and noblest in the land, probably owed more to this indefinable quality of mind, than to his scholarship, his literary work, or even to his noblest monument, the translation of Plato, admirable as that is allowed by all to be. His true monument, after all, is not the visible and tangible one; not the letters written on stone, or with ink, but (as St. Paul said to the Corinthians) those written with the Spirit, and graven "in fleshly tables of the heart" of the many to whom he ministered—the young undergraduates who afterwards grew up into great statesmen, philosophers, historians, who kept stored up in their hearts the remembrance of their old preceptor. That is a kind of reputation which any man may be proud to leave behind him, and which, in Jowett's case (it seems very notable), has all but extinguished, or reduced to very small dimensions, the fires of controversy that at one time raged around some of his work. And it is because of my overwhelming sense of the

importance of this element of sympathy, that I shall invite your attention to-night, not to any elaborated discourse or lecture, such as might be appropriate in a classroom ; not to the exposition of a thesis, or even the defence of one already expounded, but to some of those personal reminiscences and suggestions which only a man advancing in years is qualified to place before the young, but which may be easily invested with some degree of human interest, and may perhaps carry (only not too obtrusively) a moral with them as well.

When I tell you that with the exception of Professor Tait, who dates from 1860, and the further doubtful exception of Sir Douglas Maclagan, who writes to me that he was appointed in August 1862 (and therefore may have become a professor just before I migrated to Glasgow), there is not now in the University a single professor or high official, who occupied a corresponding position when my own appointment in Glasgow took effect, you will at once feel that if I am to speak to you at all of the personalities of this great school and University, as I once knew them, I must needs take you back a whole generation, and speak of things and of men known to most of you (if at all) only by tradition. And when I add, that since my graduation in 1845, there have been three professors of botany appointed, two of institutes of medicine, two of practice of medicine, two of anatomy, three of chemistry, four of natural history, one of midwifery, one of materia medica, two of surgery, two of clinical surgery, two of general pathology, and one of medical jurisprudence, you will readily appreciate the fact that the movement in the *dramatis personæ* during that pretty long interval of years has been such as to afford ample scope for a few remarks on the differences between *then* and *now*. Such, then, is the general scheme I have proposed to myself to be dealt with in this Address, not exhaustively, but only in the way of a few cross-lights on the manner of teaching and of learning in those days, with which my own experience brought me most in contact. I hope at least to escape one of the penalties of contemporary criticism, by speaking

mainly, if not exclusively, of those whose work is accomplished, whose reputation is fully established, and who have passed—so far as we are concerned—" to where beyond these voices there is peace." Nor is this quotation from Tennyson to be taken as a merely conventional one. For I can assure you that—whatever it may be now—*peace* was by no means the characteristic of the men of those days, in our medical world of Edinburgh. We, of the then younger sort, felt that we were born into a world of giants, and we were justly proud of them ; but every one was obliged to admit that they were very combative, not to say quarrelsome, giants. It will be possible now, I trust, to allude to them without kindling again any of the old fires, or at least without offending by harsh judgments any of those who, like the professor who is now addressing you, delight in pious memories of the past, and of the great men from whom he received so much of his own best instruction.

But before saying anything more about men, I should like to have a word or two with you as to the *genius loci*. Edinburgh is not, in some important particulars, the same place as I left in 1862 ; the College and University, especially as regards the medical department, have entered upon a new existence, in new and greatly improved buildings ; and, greatest change of all to me, the old Royal Infirmary, where so many had studied medicine and surgery during four or five generations at least, is now gone completely, and all that I have in the shape of a material relic of it is this stethoscope, which I owe to your good friend and mine, Professor John Chiene, who had the happy inspiration of getting a number of these instruments made out of the old and well-seasoned rafters of the dear old institution, and presenting them to those who would value them as keepsakes. This one, at any rate, as long as it lasts, shall remain on the table of my consulting-room, and will mutely appeal to me to remember the days of old, when as successively a student, dresser, clinical clerk, house surgeon and physician, pathologist, and lastly as physician and clinical teacher, I had

the good fortune to be attached to that noble old Hospital, whose fame all over the world was very great, till it was eclipsed by the splendid and palatial structure which now occupies the head of the Meadows. It would certainly be wrong, and would savour too much of a *laudator temporis acti*, to deplore, under these circumstances, the loss of the good old building; but one may be readily permitted to bear a late but sincere testimony to the good work that was done in it, the love it inspired, the *esprit de corps* it generated, and the faith we all had, as young men, that everyone connected with the Edinburgh Royal Infirmary was bound thereby to excel, either in the practice or the teaching of his art. You will find this feeling reflected with sufficient distinctness in both the admirable articles on the Royal Infirmary which have found a place in the recently published volume of *Edinburgh Hospital Reports*, on the production of which it may be permitted to me (although to a very limited extent a contributor to its pages) to congratulate the editors, and the school generally. The article by Joseph Bell, in particular, as regards the surgical side, conveys in most vivid language, and by many graphic touches, the feelings most of us had in those days, and it enables me to add here that my dear friend Joseph Bell was the last of my resident assistants in 1862, coming to my wards directly from those of Mr. Syme, of whom he was a favourite pupil and most enthusiastic admirer. I can recommend you all to read this article, not only on account of its own merits and eminent readableness, but because what I have to say here must necessarily bear more on the medical side, as the one with which I was chiefly, and almost all along, associated. I should like, however, to put on record one fact as regards my comparatively brief surgical experiences, because it shows very strikingly the change that has come about since those days by the adoption of more practical methods, both of teaching and examination. I was already a graduate of several months' standing when, acting for a short time under that excellent surgeon and kind-hearted genial man, Dr. James Duncan, I found myself *for the first time* endeavouring

to apply a long splint. How it was done on that occasion I know not ; probably very badly. But I remember well the flush of shame that came to me with the consciousness that as a surgical resident I was expected to show to the students what I really had never learned to do on my own account ; and the reason was not far to seek. It was not that I had been careless or indifferent, but that I had had no opportunities. In Handyside's wards, where I officiated as dresser, the fractures were all put up by Walter Dickson (afterwards a most distinguished naval surgeon and a most conscientious man), who preferred to do all his work as much alone as possible, so that none of us ever did anything or saw anything done (except by the rarest of chances) that was not the direct outcome of the daily visit at noon. There was nothing like regular instruction in surgical mechanisms, no, not even in bandaging, which for the most part we learned from the nurses. I need not point out how all this is changed nowadays. It is perhaps a still more striking fact that, in 1845, I was permitted to become M.D., without having ever attended a woman in labour, or ever witnessed, even as an onlooker, the process of a normal delivery ! Although I did not personally suffer from this great omission, as my early bias towards the work of a pure physician, and my duties as a pathologist, kept me from first to last quite out of the way of midwifery as a professional occupation, yet I cannot but allow that the students of forty years ago, though many of them became good surgeons and good gynæcologists, had many more chances than they have now of going out into the world in a discreditable state of ignorance of both these departments of practice.

But, as you have already apprehended, it was in the medical wards of the Royal Infirmary that I first found my true vocation. In these, whether as acting under older men, or as associating with equals or juniors, most of my lifelong friendships were formed. A few months ago, when trying to acknowledge an act of great kindness done to me by many of my old students, I was led into using an expression which some of you may think an odd one, but

which is not too strong, nor in any way unsuitable for the idea I wished to convey—that Edinburgh is a city which to me is *full of ghosts* ; not the ghosts which gibber and squeak and rap upon tables, and declaim nonsense in the names of great men departed, but those which still live on in the memory of survivors, and are taken to their heart of hearts, not in the darkness, but in the broad daylight ; not amid associations of terror, or even of vulgar curiosity, but in the blest communion of spirits which is too real to require what is vulgarly called " materialisation." Of these, in my case, and all of them closely associated with that old Royal Infirmary, are Warburton Begbie, Murchison, Sanders, Haldane, Wilson Fox, Heslop, afterwards of Birmingham, and many others less known to you by repute ; all of them, I believe, my juniors ; all of them enthusiastic workers in the domain of clinical medicine, who attained afterwards the greatest distinction in their several spheres, mostly becoming energetic and successful teachers as well as consulting physicians. What a privilege it seems *now*, to a man wearing towards the close of a fairly long life, to have known and valued and loved such men ! and to have known them all in strict amity and self-forgetfulness, with never a note of distrust, or of envy or uncharitableness on either side, during long years of loving fellowship ! Warburton Begbie stands first in the list, and I had almost said first in my heart ; [1] at all events, no one here will dispute that he rose to be (with the possible exception of Abercrombie) the most largely employed consulting physician that Edinburgh ever had ; and she has had, as you know, very many of the highest reputation and worth. Well do I remember Begbie *in the making*, for I was at all times his nearest associate in the wards of the Royal Infirmary. From the time that he became a resident medical officer, just after I had completed my two years' service in the like capacity, I had constant opportunities of knowing the steps by which his great reputation was slowly

[1] See "A Tribute," etc., addressed by me to the Class of Practice of Medicine in Glasgow, at its next meeting after Dr. Begbie's death, and reported in the *British Medical Journal*, March 11, 1876, p. 311.

built up. He inherited an admirable tradition from his father, who again was a favourite pupil of the Abercrombie already mentioned. But, almost from the first, though not declining family practice altogether, James Warburton Begbie worked on the lines of a consulting physician. He lived in and for his work in the Hospital, and threw himself so thoroughly into it that it seemed as if he could not possibly have enough of it. No one in my day, not even dear old Alison, was so unwearied in placing himself at the disposal of all, for gratuitous advice ; and while the public advantage of this was, I think, more than doubtful in some respects, no one had the heart to criticise a service that was so ungrudgingly performed, and that went so clearly to the making of a great physician. Nor did he, as some do, speak lightly of pathology in order to exalt therapeutics. I count among the treasures of my library a copy of Bright's *Hospital Reports*, which bears the following inscription in Begbie's handwriting : " Presented to Dr. William T. Gairdner, by the Gentlemen attending his first Course of Lectures on Pathological Anatomy, Session 1848–49." This, it is to be understood, was a purely voluntary course, and a work of supererogation on both sides ; for at that time no instruction in pathological anatomy was required for the degree, and the course of general pathology in the University was wholly divorced from the dead-house, so that no " slovenly, unhandsome corse " needed to " come between the wind and his nobility " who affected to study pathology under the Professor Henderson of those days.[1] Such men as Warburton Begbie, however, could not be contented with this way of dealing with a great subject, and accordingly it was my privilege, in the first year of my pathologistship, to conduct a class for the *élite*, wherein I taught them what I myself had learned in the two preceding years from John

[1] It must not be inferred from this that Henderson was not himself well instructed in pathological anatomy ; but only that he did not teach it in a practical fashion. He was a very good clinical teacher, and had quite a high reputation for physical diagnosis, until he spoiled all by taking to homœopathy.

Hughes Bennett. From this time onwards, till 1862, Begbie and I were ever most closely associated; more closely, perhaps, than ever, when after the lapse of a few years we both became physicians to the Royal Infirmary, and, in a sense, rival teachers of practice of physic and of clinical medicine. The senior ordinary physician of the time I refer to was William Robertson, a man of the very highest ability and accomplishment, who had returned from Vienna laden with all the latest treasures of clinical method, and especially with the views and experiments of Skoda, which then appeared to be making a revolution as regards percussion and auscultation. Begbie was a close friend of Robertson, had been (I think) his resident physician, and was not only sincerely attached to him personally, but always spoke of him as the Abercrombie of the future, the man who was bound to be the great consulting physician of Scotland. Such, I believe, was the opinion of not a few others, although I had not at this time the intimacy with Robertson that Begbie had, and that I also had a few years later. But Robertson had one great defect, which, from this point of view, impaired his chances, and correspondingly advanced ours. He either could not, or would not, teach. I think the will was wanting more than the power; for everything that he did, whether in writing or in speaking, was characterised by a thoughtfulness, a lucidity, and a completeness, that ought to have brought him into the front rank of physicians; and no one excelled him, or could excel him, in the thoroughness of his clinical work. But he was constitutionally unambitious, and did not even like to appear as aiming at high things; and thus it came about that he not only came short of expectation as a consultant, but by his excess of modesty led, indirectly, to a change in the whole arrangements of the extra-mural clinical teaching, amounting to what was really, at the time, a revolution, and one the consequences of which remain to this day. This incident is scarcely even alluded to in the otherwise admirable "Sketch of the History of the Royal Infirmary and of the Development of Clinical Teaching," by Professor Grainger Stewart,

which I have already noticed as the companion paper to that of Dr. Joseph Bell in the *Hospital Reports*, and as I am now the only survivor of those chiefly concerned, and therefore the only man who can give you the facts at first hand, I may as well occupy a short paragraph of this address with them.

It happened thus. Up to (I think) the year 1855, the established practice and rule in the Royal Infirmary of Edinburgh was that the senior ordinary physician and the next in order—there were always four of them—had the privilege and the right of giving clinical lectures *alternately*, but never both at once. The two junior ordinary physicians were kept out of it altogether, or, at least, were not allowed to issue tickets for a clinical course. The professors who engaged in clinical teaching were, in like manner, on duty *one at a time*, the wards passing by rotation from one to the other. Thus there were two courses, and two only, of clinical medicine always provided, one intra- the other extra-academical. The intra-academical rotation was in periods of three months, and I myself thus attended the lectures of four clinical professors at different times, viz. Graham, Alison, Christison, and Henderson, while I acted as clinical assistant for a longer or shorter time to at least two, if not three, of the extra-academical teachers. Dr. George Paterson and Dr. A. Halliday Douglas, who are both still with us,[1] are the last of these that I remember; and Dr. Douglas's term of office must have expired just before the change I am now about to explain, thus bringing Dr. William Robertson and myself to the front as the two seniors, constitutionally entitled to open our mouths to the students. But Robertson, as I have already said, declined so to open his mouth; nay, when I urged it upon him in every way I could think of, and with all the force derived from a perfectly genuine admiration, and a strong desire that he should assert himself, and take his share of the clinical work, as by rule appointed, he still resolved to abstain, and with the most entire amity

[1] Dr. Paterson died, I believe, in Edinburgh just two days before this Address was delivered; the fact being unknown to me at the time.

and good-will allowed me to pass, as it were, over his head in the matter of teaching. I was thus confronted suddenly with the question, Should I take upon myself the whole of the duty hitherto accruing to the two seniors? or would it not be, in all respects, a more wholesome precedent for the future to enlarge to the utmost the area of the bedside teaching and work, by getting the managers to pass a new rule, and so admit the two juniors also to a share in the clinical lectures? It appeared to me that the latter was clearly the better plan, and I proposed it accordingly to Warburton Begbie and Keiller, who were nominally the juniors, though the latter was a much older man than either of us. As all of us were fast friends, and all alike willing to work together, the arrangement of details was easily accomplished. Keiller, who was by previous training a gynæcologist, and who only became an ordinary physician with a view to this specialism, was to give lectures on diseases of women only, while Begbie and I undertook the more general work, and the lectures were to be arranged so as to admit of the students attending all three series, if so disposed, under a joint admission card. Such was the scheme submitted to the managers, and actually carried out, with a quite wonderful success, during all the remaining years of my Edinburgh life, and, I believe, still adhered to after 1862.

I think you will find, gentlemen, that whatever changes in detail have taken place at a later period, all have been based on the principle here first affirmed in practice, that the area of bedside instruction should be kept as wide as possible, and that the services of every capable teacher should be brought into requisition as a matter of course, no monopolies of clinical teaching by any part of the staff being encouraged or even permitted. It may, indeed, now seem to you strange, that this principle has not been in operation all along. The enormous development of clinical resources, and of clinical teaching, within the last quarter of a century, has been a notable part of the evolution of medical education altogether. I claim no credit for the part I have taken

in it, either in Edinburgh or Glasgow, but in the history of the Edinburgh School and Royal Infirmary I trust it will be recorded, that when, being already a Lecturer on the Practice of Physic and a hospital physician, the privilege of clinical teaching fell to my lot, I was anxious then, as at all times since then, to share it with others, so that all the members of the staff should, as far as possible, be partakers in the good work.

Perhaps it may enliven a little this part of my discourse if I briefly indicate a fact which there may be in this room a few persons old enough to remember. In the old Royal Infirmary there was a stair in one of the side-wings, which mounted from the waiting-room door, by three flights of steps, corresponding respectively with Wards 3, 6, and 9, which were all under my charge at the earliest period of my physicianship. At the middle of the flight leading from Ward 3 to Ward 6 there was developed a remarkable acoustic peculiarity, which, being constantly remarked upon, and demonstrated *en passant*, to groups of students who even then attended my wards, obtained for this particular part of the Infirmary the nickname of " Gairdner's Corner." The peculiarity in question was a very loud booming sound or echo, which was always produced when the stamp of a foot upon the stone step was sufficiently emphatic, and when the numbers around were not too great. This phenomenon, which was always identical in its musical pitch, so closely simulated the " amphoric echo " heard in a large pulmonary or pleural cavity, that it was invaluable as an illustration of physical diagnosis. And further, the delicacy and complexity of the adjustments which contributed to its perfection, were set forth by the fact, that, although all the three successive flights of stairs were, to the eye, precisely alike, the booming echo was much more prominent in the lowest one than in either of the other two ; and in the top-storey hardly existed at all. This was long before the day when Helmholtz's resonators were brought into play as illustrations of physiological acoustics, and perhaps, therefore, a familiarity with the phenomena of " Gairdner's Corner "

has not been without a certain educating value to successive classes of students.

I desire to add to these somewhat desultory jottings a few words only about another member of the group above mentioned, Charles Murchison, the author, as all of you must well know, of that noble and monumental work on the *Continued Fevers*, which, although first published so long ago as 1862, may be said to be among those treasures of medical literature which can hardly ever grow old. Murchison was from the first a most painstaking and exemplary worker in Hospital, greatly esteemed and loved by all of us; but I do not think that the comprehensiveness of method, and the philosophical ordering of large accumulations of facts and statistics, which afterwards distinguished him, had grown into prominence while he was with us in Edinburgh. I remember his telling me, long afterwards, that it was Sir James Y. Simpson who first aroused him to a sense of what he owed to his own reputation, by putting it to him somewhat sternly whether he was going to let the years slip idly away without doing something to reflect honour on the school which had trained him, and from which he held a degree. "It brought the tears to my eyes," Murchison told me; and no doubt, coming from that most indefatigable worker and man of genius, whose whole career was, and is, an abiding example of the doctrine he preached, the words of rebuke thus uttered became an inspiration. I must say, however, that Murchison's method, and his work in detail, were alike his own. No department of medicine, in my opinion, has ever been so dealt with, as Murchison dealt with the continued fevers. Working on the lines of Sir William Jenner, and in the same hospital, there was necessarily an initiative derived from the great and invaluable labours of his predecessor; but in carrying out the principle of distinguishing these fevers in all times and places, viewing them in every possible relation to each other amid every kind of change of circumstance and environment, Murchison's work was absolutely original and unique. I had almost said (but that is perhaps too much to say of any man's work)

that it is a *perfect* work, in respect of freedom from grave errors or omissions. With many temptations to haste, and large opportunities for theorising, his grip of the facts is such that there is hardly any corner of the whole vast subject that is not illuminated with the light of truth. Philosophical insight and practical sagacity go hand in hand throughout.

I remember a story told of Murchison in those days, which I did not, indeed, hear first from himself, but which, on the appeal being made to him, he admitted to be true. He was serving under a physician well known to all of us, a man of many strong and some admirable qualities, but in whose doings and sayings the " personal equation " was often unduly prominent, so as to make him not seldom a terror to his subordinates. A man had died very suddenly in the ward, and (it was believed) from having swallowed, by accident or design, a liniment containing Fleming's tincture of aconite. Murchison, who was in charge, had only just seen the patient alive. An inquisition was made next day into the circumstances, and in presence of a numerous class. The symptoms of poisoning by aconite, conium, opium, etc., were discussed, and the only skilled witness available was called on formally to give evidence. " Mr. Murchison, did you carefully observe and report the condition of the pupils, the pulse, the respiration ? " " No, sir." " Then," said the professor, " you missed a great opportunity ; what were you doing ? " " Oh," said Murchison very quietly, " I ran for the stomach-pump ! "

Murchison's last years were a great lesson for all of us. He had, and knew that he had, a grave cardiac lesion ; and later on, examining his own urine himself, he found it albuminous, at least occasionally. This latter circumstance he examined into with great care and deliberation, exactly as if he had been concerned with another man's case instead of his own ; and I got all the details of it from himself in London, not long before his death. In the autumn of 1872, he came down to Scotland and consulted his friend and contemporary, the late Professor Sanders and myself, then

both on holiday at Arrochar. The lesion was aortic regurgitation. There was no getting over the fact, and no wish to disguise it. But the calm, clear way in which he inquired, how long we could give him to live, *and work?* was very striking to both of us. And work he did, up to the very last. There was no note of failure or of infirmity, so far as the outer world was concerned. He performed all his accustomed duties; was regularly, I believe, at his hospital, and presided with great acceptance over the Pathological Society of London. He also had a large and rapidly-growing consultation practice, which must have cost him a great deal in labour and anxiety. I saw him in London not many days, I think, before his death, and he assured me that he had never done so much work in this way, and had never done it so easily, as in the past winter. The death was absolutely sudden, so much so that a patient had left his consulting-room only a few minutes before. It took place on April 23, 1879.[1]

It would have been a great pleasure to me to have prolonged this Address by some more reminiscences of men who were very near and dear to me, both in their professional careers and in private and personal life, whose familiar faces gave lightness to the old Royal Infirmary in the 'Fifties. But I must consider your time and patience, and strive not to grow wearisome. One man of somewhat younger standing than either Begbie or Murchison is, in my mind, on an equal platform of friendship and of high professional character with either of them. But he had only just risen to be an ordinary physician of the Infirmary in 1862, and his career, in so far as it was really famous, together with all his great reputation as a clinical teacher, was made between that time and his too early death at the age of 53, when, however, he had unquestionably begun to occupy the position in Edinburgh consulting practice from which Begbie,

[1] See the obituary notices published at the time; and also a few words spoken by me from the Chair of the Glasgow and West of Scotland Branch of the British Medical Association, June 26, 1879, *British Medical Journal*, 1879, vol. ii. p. 193.

too, had been prematurely removed. This was William Rutherford Sanders, the father of one of your own late most distinguished presidents, and, like the other two, a lifelong and intimate friend of mine; so that I may well say that to have enjoyed in early manhood three such intimacies is surely enough for one man. But my appreciation of Professor Sanders was written out with such detail, in the obituary notice, *Edinburgh Medical Journal*, 1881, p. 939, that I will content myself with referring to this, and dispense with further words now. Two of the men of older standing also, on the medical side, engaged from my pupilage onwards my warmest affection and respect; but in their case also you can easily find in your admirable library all that I could tell you here about them. Of dear old William Pulteney Alison, indeed, one could say no more and no better than that which was conveyed in a humorous form by my equally dear friend and fellow-student, James D. Gillespie, that we all expected and hoped to see the wings growing out of his shoulders, that were to lift him into the diviner air that was alone good and pure enough for him. But in this I have already forestalled myself, by having written, at the request of the then editors of the *Edinburgh Journal*, a rather long obituary notice or biography which you will find, if you desire it, in the volume for November, 1859, or in a much more recently published volume entitled *The Physician as Naturalist*, published in 1889 by MacLehose of Glasgow. Finally, of Sir Robert Christison, that marvellous veteran, professor, author, experimentalist, jurist, physician,—equally eminent as a president of the Royal Society and as a captain of University Volunteers,—I have had the most ample opportunity of detailing reminiscences elsewhere, owing to the flattering invitation I received from his two medical sons to write a chapter in the second volume of his biography, edited by them,[1] and to this I am content to refer such of you as care to know a little about the old Royal Infirmary in the 'Forties, as well as in the 'Fifties.

[1] Published in Edinburgh by William Blackwood & Sons, 1885, 1886.

These, gentlemen, are a few, and only a few, of the many friendly "ghosts" which haunt your beautiful city, and which, to me, haunted the old Royal Infirmary as long as it lasted, and endure with the memory of it now. I trust I have not trespassed upon your patience too much in summoning them up for you this evening, and if I have left others, equally or more prominent to my own or to the general mind out of the canvas of my picture on this particular occasion, it is not without a purpose. I said to you that there *was* a moral attached to this Address, but that I did not wish to make it too obtrusive: "There were giants in those days"—giants both of medical and surgical prowess; but, as I said before, they were very combative, not to say quarrelsome, giants. The Edinburgh of the 'Fifties was a difficult place to live quietly in. It was nearly always full of "wars and rumours of wars." A new D'Israeli the elder, if he had been of our profession, and lived among us, would have had abundant scope for any number of volumes on *The Quarrels and Calamities of*—Doctors! I hope, and I believe, that it is much more peace-loving nowadays. And although there is not one of us, I am sure, professor or otherwise, who would wish to be measured intellectually against any of these giants, it may not be unprofitable for you to know that there were even in those days some, not altogether undistinguished, who preferred the paths of peace and quietness, and could work together, and even in rivalry, without making the world resound with their grievances. To you, gentlemen, is committed the future of the profession in Edinburgh, and in many other places. Cultivate it, fulfil your own part in it, with as much power and vigour as may be. But also, I pray you, do not fail to set forth as your ideal, that you will live and work in the spirit of Begbie and Murchison and Sanders, rather than in the midst of strife. Controversy, no doubt, and discussion must needs be, where opinions differ, but much depends upon the men, and in these men I have endeavoured to set forth to you worthy examples, not too far removed from us to be still useful to us by their traditions, thus handed down to you

by a friendly survivor. I have only one thing more to say, and I think I have said it in this room before. *Eminence* may come to some of you, or it may not; but *character*, in the sense of what makes or mars the individual man, in the daily work of the world, is what you cannot help making or unmaking, step by step, every day of your life, and in every station to which you may be called. And character, though it may be easily tarnished, and even ruined, by inadvertence, cannot be made at a rush. These men all worked well, because they worked slowly and patiently, with constant and self-sacrificing devotion, rather than because they worked brilliantly. None of them aimed merely at notoriety; none of them even attached much importance to it. Not one of them was capable of taking an unfair advantage, or of establishing himself in any position, or doing any work, at the expense of his neighbour. Therein lies my little *moral*. I hope it comes home to you, without offending anyone. I will only illustrate it further by one more fact. In the retiring-room attached to my classroom I have a photograph, which I hope may last as long as I am there to look at it, of the eight clinical assistants who were attached to my wards in the summer of 1862, the last clinical session passed in that grand old Infirmary. One of these eight men is now a most distinguished professor in this University, and therefore known to you all. One, the head of the group, who was the resident, is not, indeed, a professor, but I think I am justified in calling him rather more than a professor, for is he not Sherlock Holmes—*i.e.* Dr. Joseph Bell? A third member of the group is now at the head of a very important department of medical work under Government. A few, you will not be surprised to hear, have passed away through some of those trap-doors that Addison described in the bridge in his well-known "Vision of Mirza." One, I grieve to say not the least distinguished member of the group at the time, has undergone penal servitude for an offence which was not only "infamous in a professional respect," but infamous in any respect. One cannot bear even to think of such things, were it not

for the moral they carry,—how easy it is, often, by the mere turning of a hair, to lose or to ruin *character*. I will conclude this address in the words of one whom I may be permitted to call (now when he is, I believe, in his eighty-fifth year) the grandest old man in our profession on the other side of the Atlantic—an LL.D. of Edinburgh, and a very dear friend of mine these many years—Oliver Wendell Holmes. No doubt many of you know the lines well, but those who are most familiar with them will be most ready to acknowledge their aptness in this connection.

THE TWO STREAMS.

Behold the rocky wall
That down its sloping sides
Pours the swift rain-drops, blending, as they fall,
In rushing river tides!

Yon stream, whose sources run
Turned by a pebble's edge,
Is Athabasca, rolling toward the sun
Through the cleft mountain edge.

The slender rill had strayed,
But for the slanting stone,
To evening's ocean, with the tangled braid
Of foam-flecked Oregon.

So from the heights of Will
Life's parting stream descends,
And, as a moment turns its slender rill,
Each widening torrent bends,—

From the same cradle's side,
From the same mother's knee,—
One to long darkness and the frozen tide,
One to the Peaceful Sea!

OLIVER WENDELL HOLMES
In the *Professor at the Breakfast Table.*

MEMORIES OF COLLEGE LIFE; ESPECIALLY IN THE SIXTIES, AND EARLY SEVENTIES

Contributed to "The Book of the Jubilee" in commemoration of the four hundred and fiftieth anniversary of the foundation of the University of Glasgow, 1901.

I HAVE been requested to furnish some personal reminiscences of the University of Glasgow for a volume intended to be issued on the occasion of the ninth semi-centenary of the University. My qualifications for this task may be said to consist mainly in the fact that for quite thirty-eight years, 1862-1900, I held the office of Professor of Medicine, and have thus been very closely associated with the inner life of the University both in its old quarters in the High Street, and in the new and splendid educational palace at Gilmorehill. It would seem to be most fitting that I should dwell chiefly upon the earlier period of this record; for Lord Kelvin, with Sheriff Berry, the Master of Balliol, Professor Blackburn, and Professors Ramsay and Young, are now the only survivors of those who sat with me in the Senate-room of the old College, or who were lecturers within its walls from 1862 onwards. It will probably be accepted as appropriate that I should refer in this place, for the most part, to those who have passed away. Yet in the case of Lord Kelvin, at least, it would be very pleasing to me to enlarge on the affection, as well as the admiration, that he inspired among all his colleagues,

during a much longer tenure of office than any professor in my time, with the possible exception of the late Sir Robert Christison in Edinburgh. But of Lord Kelvin everything has been said that can be properly said, at the magnificent gathering for his jubilee some years ago; and it will be more fitting that this brief record should be framed upon the general principle of speaking of the dead rather than of the living; of forms and faces that have passed from observation, rather than of those that are still present with us.

On leaving Edinburgh for Glasgow in 1862, I had been for ten years an extra-academical lecturer on Practice of Medicine in the former place; and for some years previously (1848), at first Pathologist, and then Physician, to the Edinburgh Royal Infirmary. It seems necessary to mention this, not only as bearing on the circumstances under which my professorial life was begun, but in order to do justice to the good feeling and sympathy with which I was received, not only in the Senate, but also elsewhere in Glasgow; and particularly among the students, at the very outset of my long career as a teacher of Medicine in the University. It might well have been otherwise, for there was at that time a pretty strong feeling that Edinburgh had had too much to say in Glasgow appointments; and I believe the members of Parliament for the city were strongly urged to resist this particular appointment being given to any but a local man. It speaks well, however, for the generous hospitality of Glasgow that, the appointment once made, the holder of it for so long a term can thus testify with a grateful heart to the warmth of his reception at first, and the uniform personal kindness and consideration with which he has been favoured, alike by old and young, during what, at the time of his demission of office, amounted to almost exactly the half of his natural life.

My predecessor in the Chair of Practice of Medicine was Dr. John Macfarlane, who held the office for ten years, having been appointed in 1852. Of him I will only say that he was a very eminent and, I believe, very greatly

respected physician, who, beginning life with a bias towards surgery rather than medicine, had attained at the time of his appointment the reputation of the most largely-employed and trusted consulting physician in Glasgow and the West of Scotland. His appointment as professor however, came to him too late in life; too late, I mean, for academic efficiency, as otherwise it was a most fitting recognition of his distinguished position in his profession. I had only a slight acquaintance with Dr. Macfarlane, and none at all with him, of course, as a teacher; but, unless all the reports that have reached me are untrustworthy, he was not a successful lecturer; while, as a teacher, he aimed at being a lecturer, and nothing else. He did not even (as his immediate predecessor, Dr. William Thomson, did) occupy a position in the hospital, which would have been easily found for a man of his eminence and popularity had he desired to make his large stores of experience in the healing art available for bedside instruction. In this respect, however, Dr. Macfarlane only reverted to the traditions of his own early academic period, when a professor of medicine was considered to have done his whole duty to his pupils and to the University by a six months' course of lectures. Even of Cullen, the greatest physician who ever occupied the chair, I doubt if there is any evidence of his having taught *clinically* until after he went to Edinburgh University in 1756, and probably not for some years after that time. The Chair of Medicine in Glasgow University may not have been exactly a sinecure, but it was certainly a very easily held academic position in the days of Charles Badham, M.D. (1827-41),[1] when Dr. Macfarlane probably may have attended the University as a pupil (if not indeed under his predecessor, Dr. Freer).

The professors who occupied chairs in the Medical Faculty at the time of my entering it in 1862 were—1. *Botany*, G. A. Walker-Arnott, LL.D. (appointed in 1845); 2. *Natural History*, Henry D. Rogers, LL.D. (1857); 3. *Chemistry*,

[1] See the amusing sketch of Dr. Badham's Professoriate, by Dr. James Finlayson, in the *Glasgow Medical Journal* for May, 1900.

Thomas Anderson, M.D. (1852) ; 4. *Anatomy*, Allen Thomson, M.D., LL.D. (1848) ; 5. *Physiology*, or *Institutes of Medicine* (as it was then called), Andrew Buchanan, M.D. (1839) ; 6. *Materia Medica*, John A. Easton, M.D. (1855) ; 7. *Forensic Medicine*, Harry Rainy, M.D. (1841) ; 8. *Surgery*, Joseph Lister, M.B. (1860) ; 9. *Midwifery*, John M. Pagan, M.D. (1840). In addition to these, the venerable Dr. Wm. Mackenzie occupied, with European distinction (though without a seat in the Senate), the post of Waltonian Lecturer on the Diseases of the Eye (from 1828). With all of these men I had most pleasant, and with some of them most profitable, personal relations ; several of them bore the reputation of being admirable teachers, as well as skilled practitioners in their various departments ; one or two were pre-eminent, as Allen Thomson (whose pupil I was at an earlier stage, but whose reputation went on, even increasing, not only as an anatomist and teacher, but as one of the most influential members of the Senate) ; and Professor Lister (now Lord Lister) who laid the foundations of his immense reputation and world-famous labours on behalf of surgery and humanity, in the wards of the Royal Infirmary, Glasgow, and in the class-room of the Old College in the High Street. He is the sole survivor of the group above named, and I am sure he will corroborate my statement that we were, on the whole, a band of brothers, not without our little differences of detail, of course, but with one accord working in the interests of the University, and of our students, and with about as little friction, or obtrusion of personal differences, as any like group of men engaged in the service of a great institution.

When I left Edinburgh, and for some years before, it would be vain to deny that this state of (on the whole) harmony and mutual goodwill was by no means characteristic of the Faculty of Medicine in the University of my native city. I have been accustomed to say, that "there were giants in those days" ; but they were very combative, not to say quarrelsome giants. In the profession at large there was then (as always) I hope, a great deal of kindly

and delightful intercourse, and an almost unbroken high standard of professional duty and honour; but the perturbations produced in medical circles by the "wars and rumours of wars" surrounding the names of three or four (or perhaps more) of our natural leaders was such as to make the path of younger men difficult, and the entire avoidance of partisanship, if not enmity, almost impossible to most of us. One effect of this upon my own mind, in passing into a new and more quiet medical atmosphere, was to impress it upon me as a kind of religious duty to avoid (as far as possible) everything tending to mere personal disputes, and, *per contra*, generally to "follow the things which make for peace, and the things wherewith one may edify another." It is a difficult attitude to maintain, and is sometimes apt to be construed as a weakness or "want of backbone" in those who occupy prominent positions; but on the whole, my chief regret in looking back over a long career is that, under the pressure of circumstances and of fancied duty, I was led sometimes in early life to depart from it, more than I can now justify to myself.

Passing now from the consideration of the Medical Faculty for a time, I should like to say a word about two great officers of the University under whom it has been my good fortune to serve before the election of the present Principal in 1898.

Dr. Thomas Barclay, D.D., had held the office of Principal for four years previous to my appointment; having been elected in 1858, and retaining the office till his death in 1873. I had been acquainted with him more or less for a number of years before his appointment as Principal, when he was minister of the parish of Currie, in Midlothian. In those days he and the late Rev. Dr. Robert Lee, of Old Greyfriars, used frequently to divide, on disputed points, they two against the whole of the rest of the brethren. But as both my father and I held the minority to be in the right on most of these occasions, the future Principal suffered no disparagement in our eyes thereby. At an earlier period, when he was the minister of Lerwick, in his own native

Shetland, Dr. Barclay had attracted the attention of Sir Henry Holland, in a way which the latter has placed on record in a charming volume, published in 1872, under the title of *Recollections of Past Life.* Visiting the Shetland Islands in 1840 (Sir Henry writes in a footnote, p. 58), "I heard an admirable sermon from the minister, Dr. Barclay. The following day this gentleman, with two or three other friends, accompanied me in a boat excursion to the Isle of Noss, wonderful from the wall-like cliffs with which it overhangs the sea. Passing round the rude promontory called the Bard of Bressay, our boat was caught by one of those gusts of wind which sweep suddenly and impetuously through these isles, and for some minutes we were in danger. All others lost their presence of mind; but Dr. Barclay, deemed one of the best boatmen in Shetland, seized the tiller, and by his firmness and skill brought us into safety. . . . Some years afterwards, having been previously translated to a ministry in the east of Scotland, he became a candidate for the office of Principal in the Glasgow University, then vacant. Lord Murray wrote to me begging that I would see Sir George Grey, Secretary of State for the Home Office, with whom the appointment lay. I did so immediately, and put the matter to him in its simplest form, expressing my conviction that a man who could preach such a sermon on Sunday, and on the next day save a boat from being swamped by his firmness and promptitude, was one eminently fitted for the government of young men, and of a great College. How far this contributed to it I know not; but Dr. Barclay received the appointment, which he has ever since held with high honour and usefulness."

There is perhaps a slight touch of irony in this; but Sir Henry Holland was a very good judge of character, and knew that Sir George Grey had access to other evidence of Dr. Barclay's qualifications than those which were thus humorously set forth. Be this as it may, I am able to confirm the impression conveyed that from this all-round wisdom and experience, as also from his marked indepen-

dence and firmness of character and his whole-hearted devotion to his office, Dr. Barclay, as I knew him, was an excellent Principal. He was not eloquent, or even an orator at all, in the same sense as his successor. But no one could doubt for a moment his sincerity, his knowledge of affairs, his admirable tact and sagacity, and his goodwill to all concerned in University work. His predecessor, the Rev. Dr. McFarlan, has always been described to me (for I never saw him) as a man of a remarkably fine presence. Dr. Barclay was not exactly that; but, though cast in rather a plain, or even rude, mould, he had a dignity all his own, and in the portrait by MacNee, which hangs in the Senate Room, with the long flowing beard and the quick observant eyes overshadowed by shaggy eyebrows, one seems almost to find something of the features of a John Knox in the nineteenth century, though he himself would not have acknowledged the resemblance. As one very closely associated with him in his later years, I can testify to Principal Barclay's having filled his great office in such a way as to win the respect and sympathy of us all. He had many private anxieties and distresses, but he faced them all with a courage and unselfishness which won admiration, well expressed by Dr. Caird in the memorial sermon preached in the College Chapel on the Sunday after Dr. Barclay's death. He was an admirable linguist, being especially expert in the Scandinavian languages; and was one of the very few men who could, I believe, decipher Icelandic and Runic inscriptions. He had also several other refined tastes and accomplishments, learned in early life, but which, from the entire absence of any love of display, he had allowed, in a measure, to fall into desuetude. In my medical intercourse with him, I had one very striking evidence of his clear-sightedness and force of character, which I will mention here, as it contains a lesson which I had occasion repeatedly to convey to my own pupils.

From the time when he was the minister of Currie parish, at least, Principal Barclay had been subject to severe asthmatic seizures, which yielded for a time to residence

on two occasions for some months in Egypt, but recurred later on, accompanied by bronchitis, which was very disabling while it lasted, but which only brought out more clearly in his case the physical and mental vigour of a robust and, on the whole, imperturbable character, tending to the optimistic under many adverse conditions, of which these purely personal ones were by no means the most distressing to him. "I think my lungs must have been made of leather," he said to me after having suffered for about 30 or 40 years in this way, when I told him that he had come through all these attacks with wonderfully sound organs. In the earlier periods of this almost life-long illness he was attended by Dr. Craig, of Ratho, who had as his assistant at the time one of my best pupils, a very devoted and intelligent young man, long since dead, as is also Dr. Craig himself. They sent me on one occasion a prescription, with the remark that the medicine indicated in it had been found of very great service in Dr. Barclay's asthma, but that neither of them could understand the *rationale* of the prescription. This was not very wonderful, as it was one of those excessively complicated instances of polypharmacy in which (*more Anglico*) fourteen or fifteen different and more or less active substances were combined in one inextricable blend, so as to defeat as far as possible all reasonable efforts to discover the *modus operandi* of any particular constituent. Yet it was a very favourable example of this particular kind of prescription, "elegant" in its form (to use the conventional word), and, as regards its effects quite worthy of the great reputation of the late Dr. Jephson, of Leamington, from whom it emanated as a purely personal compliment, after his retirement from practice on account of blindness. Dr. Barclay had met Dr. Jephson at the house of Sir Wm. Gibson Craig, of Riccarton, and this prescription, with a number of very shrewd and clear-sighted directions as to diet, rest, and general hygienic precautions, were the results of the only one conference with the great English physician of the Midlands. At my suggestion, but after his own thorough and philosophical method, Dr.

Barclay thereupon commenced a series of experiments in his own person on all the separate ingredients in this prescription which could by any reasonable interpretation be supposed to be its active principles, not discontinuing the complex form, but substituting from time to time simpler and simpler combinations, until in the end it was conveyed to him, and through him to me, that *iodide of potassium* was the agent that in all probability contained the curative virtues of the entire prescription. This was for me at the time, as a young teacher of medicine, quite a new therapeutic fact, and from that time onwards I rarely omitted an opportunity of preaching iodide of potassium (as well as prescribing it) in bronchial affections, together with the lesson conveyed by the whole investigation as regards simplicity in therapeutics *versus* polypharmacy. Dr. Barclay retained his belief in this remedy to the end of his days, and used to take two or three grains of it three times a day, as a potent factor in helping him through his attacks, though it could not intercept them entirely.[1]

Dr. Barclay died in 1873, being, I think, in his 87th year at the time. His death was in one sense sudden, that is, unexpected as to the precise moment (it was during a service in the College Chapel, and I had seen him immediately before) ; but the long struggle with an ever-recurring disease, and with many and great personal anxieties, had completely exhausted a physical constitution originally of great vigour, and a mind which, almost to the very last, was receptive and intelligent, as well as sympathetic in no ordinary degree. No one, of course, will for a moment think of comparing Principal Barclay with his great successor,

[1] Although this is not a suitable place for further discussion of this subject, I may be permitted to add that the properties here attributed to the iodides were quite unrecognised in the great work of Pereira, and, so far as I can discover, were unknown to the medical profession at large at the time of the incident here referred to. It is perhaps not easy to say how far the oral teaching above referred to may have indirectly influenced professional opinion, but the cardinal fact of the Rev. Dr. Barclay's logical analysis of Jephson's prescription has never been stated in print (so far as I know) until now.

the Rev. John Caird; but those who were privileged to know both men intimately, and who were also aware of the widespread feeling of regret and sympathy on the occasion of his death, will certainly not fail in according to Principal Barclay the credit due to a brave man, cultured and generous in spirit, who during the whole period of the removal of the University buildings from the High Street to Gilmorehill presided over the deliberations of the Senate, and won the respect of all by his impartiality, his practical wisdom, and his absolute devotion to the work of his great office.

Of Principal John Caird, I find it difficult here to speak without repeating words which have been recorded elsewhere,[1] of one who was a member of the Senate as Professor of Divinity from 1862, and who after Principal Barclay's death in 1873 was with one accord hailed as the only fitting head of an institution of which he was all along the glory and honour. All the world knew of his great power and splendid career as a preacher; but no one outside of a very limited circle was in a position to know fully the simplicity, the earnestness, and the almost singular modesty, graciousness, and personal charm which lay behind his great gifts—of eloquence and philosophic insight. It seems almost like a contradiction in terms to affirm that one who was so much before the public was, at the same time, of a peculiarly retiring, sensitive, not to say shy, disposition; and yet it really was so. Nothing was more abhorrent to Dr. Caird than the least thought of personal glorification in his efforts in the pulpit, or, indeed, in any kind of public appearance. He would travel any number of miles to give help in a good cause or to assist a friend, perhaps in a remote country parish; but always under the implied condition that nothing was to be done out of the ordinary course in the way of making known his presence. When the great call was being made for funds to enable the University to change its habitation, and to shine out in a new and more stately home on Gilmorehill, the suggestion was offered in several

[1] See the short biography of Dr. Caird prefixed to the issue of his Gifford Lectures, by his brother, the Master of Balliol, vol. i. p. cxxxiv *seq.*

worldly-wise quarters that a good deal of money could be raised if Dr. Caird would go round to the great centres of population and preach the gospel of academic rehabilitation in a series of well-advertised sermons suitable for the occasion. But I should be glad to believe that the suggestion never was conveyed to him; for, had it ever reached his ears, the reply to it inwardly (however outwardly expressed) could only have been that of the apostle Peter to Simon Magus. It came, accordingly, to be clearly understood that while Dr. Caird would go anywhere, or to any person or body of persons, from ministers of State downwards, to lend his influence to deputations, he must not be asked to preach or in any way to take part in a worshipping assembly, the object of which was, directly or indirectly, the raising of funds for the building scheme then being promulgated. I do not know exactly where he drew the line—in the matter of ordinary or extraordinary "collections"—in places where he preached; but his instinct in this case, at any rate, was sure and clear.

At a later period he willingly officiated once a year in the College Chapel, when it was understood and announced that a collection was to be taken on behalf of the Western Infirmary, but this he accepted as part of his clear duty as Principal, and to give the students (and especially the medical students) an opportunity of quietly contributing on a small scale to a work of public benevolence closely allied to and yet not strictly a part of their academic work. On these and all other occasions the simple announcement on the notice-board in the quadrangle that "The Principal" was to preach next Sunday--the announcement being made on Tuesday—sufficed to carry the news all over the town, and secure an enormous congregation. And it was the same everywhere else. Even when in an emergency he occupied, without notice, an unfamiliar pulpit, the news seemed to spread abroad, no one knew how, and to secure an overflowing congregation. Nothing could be more striking, and indeed more touching, to one accustomed to the ways of the "young barbarians" who are wont

to give undue emphasis to their animal spirits in University public celebrations, than the rapt attention and eagerness with which most of those noble "University Addresses"[1] were listened to by the undergraduates. They were in every instance read by Dr. Caird carefully from the paper, and were not at all in the pulpit manner, nor surrounded by the reverent ceremonial of public worship. They were also very long, rarely less than an hour, and would have been much longer but for the careful and determined suppression of parts which afterwards appeared in print. They dealt, it is hardly too much to say, with the "omne scibile" as far as it can be done in generalisations belonging to the nineteenth century. Being so much outside his own familiar region of thought and inquiry, they must have cost him an enormous amount both of reading and of hard thinking. Yet the impression given throughout was that the audience—town and gown alike—were fascinated into silent attention, and unwilling to let a single word escape them throughout a long discourse. Whether the matter of the discourse was of the more abstract or of the more concrete kind the effect was always the same. Those who knew beforehand that the address of the season was to be on Erasmus, Galileo, Bacon, Hume, felt sure that in these types there would be an interesting and profitable discussion of great general principles. Those, on the other hand, who had come to hear a dissertation on "divine philosophy" had a clear assurance that in the hands of Principal Caird such subjects as "The Unity and Progressiveness of the Sciences, History, Art, etc.," would be brought into close relations with life in various departments, so as to justify the words of Milton :

> "How charming is divine philosophy;
> Not harsh and crabbed, as dull fools suppose,
> But musical as is Apollo's lute."

[1] *University Addresses: being Addresses on subjects of Academic Study delivered to the University of Glasgow.* By John Caird, D.D., LL.D., late Principal and Vice-Chancellor of the University. Glasgow: James MacLehose & Sons, 1898. (The posthumous selection was made by the Master of Balliol.)

The very last of these remarkable discourses, April 13th, 1897, though shorter than some of the others, shews no falling-off at all, I think, in the vivid power and sterling eloquence characteristic of the whole series, the fruit of life-long meditation, and at every point full of strong human sympathies and diversified interests. This address, which (as the Master of Balliol tells us in the preface to the volume) "contains the last words which he was able to speak in public," caused not a little anxiety to some of us, who had occasion to be medically responsible for its delivery. Principal Caird had been struck down with paralysis two years before, in the middle of his second course of Gifford lectures. But, although his right leg was enfeebled, more or less (the arm almost escaping, contrary to the usual rule), the mind had been throughout clear and serene. When he was medically advised, however, not to encounter hard philosophic reading for a time he at first took to novels, and then, after getting tired of them, to Gibbon's history, which I believe he read again straight through. He even desired, after a time, to resume delivery of the Gifford lectures, three or four of which were completed in MS.: but in this matter he submitted to medical direction. The fear was, not that he could not think, or even speak, as well as ever, if he could be assisted to the rostrum; but that he might grow nervous in face of an audience, as even in his best days he sometimes apparently became, if emotion got the better of him. At last, after two years' waiting, he was known to be getting restive, and was applying his mind to a discourse on the contrasts between "General and Professional Education," with which he was greatly desirous of breaking the long silence. Dr. Tennent, who had watched and advised the Principal almost from day to day, was disposed to think that he might venture it, and after consideration and consultation I came to be of the same opinion, as the disappointment might have been more injurious to him than the effort, and he was sure, in any case, of a generous and sympathising audience, who would greatly prize the effort, and overlook any possible or

probable defect. The address, as it now stands, is a comparatively brief one, but I do not suppose that anyone will allege that it is inferior, or anything but a most fitting close to a noble series. But I had hardly left the hall where it was delivered when a medical friend who had been present said to me, somewhat reprovingly—"You should not have allowed the Principal to do that: did you not observe that he is becoming aphasic?" For the moment, I was staggered; for indeed there had been some hesitation and apparent difficulties of expression; but, as I afterwards ascertained, these were entirely due to the deficiency of light enough for reading his MS.!

Dr. Caird died more than a year after this, on the very day (1st August) on which he had completed 36 years of University service, 25 of which were in the great office of Principal; and it is safe to say that no man ever held that office with more unique distinction, and at the same time with more entire freedom from contentiousness and self-seeking. The peace which he enjoyed in his own breast he inspired in others, and I am sure that all my colleagues during his reign will concur with me in a feeling alike of pride and of gratitude in having served under such a chief.[1]

[1] With reference to the sometimes considerable length of Dr. Caird's sermons, I have heard it again and again remarked that he was the only preacher who could occupy the pulpit in the College chapel for approximately an hour without the audience giving signs of weariness, or anyone complaining that he had said too much. In these degenerate days this is a pretty severe test of oratory, in the pulpit or out of it. I do not feel absolutely sure that even Dr. Caird would have fared equally well with an English audience if he had much exceeded the customary half-hour; but I once had a long and interesting talk with a dignitary of the Anglican Church (who still survives), and who, being one of her late Majesty's chaplains, set it forth to me as a serious grievance that, while the Queen would allow Dr. Caird to preach to her for an hour at Balmoral, they who were accustomed to officiate at Windsor were strictly limited to twenty-one minutes at the outside! So that it was perhaps a matter of latitude as well as of longitude! And this suggests another criticism that was said to have been passed upon one of Dr. Caird's sermons by someone

Brief allusion has already been made to the fact that it was during Dr. Barclay's tenure of the Principalship that the movement was carried out by which the College of the University was transplanted from the site it had occupied for over 200 years among the slums of Glasgow to the magnificent position from which it now dominates the West End, amid open parks, and on an eminence on which Gilbert Scott's great building sits proudly four-square to all the winds that blow. The details of this great enterprise have been elsewhere and often recorded, and cannot fail to be fully commented on at the ninth jubilee of the University. But it may be permitted to one of the medical professors who witnessed this movement from almost the very beginning, to state here that the life and soul of this great undertaking were to be found in Dr. Allen Thomson, my first teacher of anatomy in Edinburgh (extra-academical), previously to which he had occupied the Chair of Anatomy at Aberdeen. In 1842 Dr. Thomson was appointed to the Chair of Physiology or Institutes of Medicine in Edinburgh, and, six years later, to the Professorship of Anatomy in Glasgow, which he retained with the greatest distinction for 29 years (1848-1877), being during the greater portion of that long period by far the most influential single member of the Senate, and certainly, both among the undergraduates and with the citizens at large, one of the most honoured of all the professors in my time. This exceptional position he owed partly, of course, to his high reputation, his unquestioned efficiency as a teacher and as a philosophical anatomist, but also to a certain tact and persuasiveness, a moderation of judgment and adaptability which, without his appearing at all to dominate, gave him very great and real power to direct the opinion of men very far removed from his own special pursuits. In this latter respect Dr. Allen Thomson was probably without a rival among the

who was rather disturbed by doubts as to the theology thereof. "Don't you think" (it was cautiously inquired by one hearer in conversation with another) "that it was rather broad?" "Oh, yes," was the rejoinder; "it's just as broad as it's long!"

members of the University Senate ; and, speaking from the point of view of the Medical Faculty, I can only wish that he had had even " more power to his elbow " among the brethren of the Faculty of Arts, so as to have carried out, in a broader sense, when we were about it at any rate, some of the ideas which must have been present to his own mind as to what in its new form we have now got to call the University Extension Movement.

But in the sixties the Faculty of Arts was very closely controlled by our conservative Chancellor of the Exchequer, Professor Hugh Blackburn, who still, happily, survives, and who, I think, will hardly contradict me when I affirm that the prevailing conception of the requirements of a professoriate were a lecture-room (with a chair, of course !), a retiring-room, and a few other unconsidered trifles in the way of blackboards, chalk, diagrams, etc. ; subject to the admitted exceptional needs of Natural Philosophy (which under Professor Wm. Thomson, now Lord Kelvin, could hardly be denied a claim to some kind of laboratory accommodation) ; Chemistry also, and Anatomy, which, if it was to exist at all for teaching purposes, must needs have a separate museum and a dissecting-room. Even this last claim, however, was rather grudgingly conceded by some of our colleagues in the Arts and Divinity Faculties, of whom perhaps the most representative of all was a reverend colleague long since deceased, a most amiable man personally, and in particular a great friend of Dr. Allen Thomson's. This gentleman once buttonholed me in the High Street to expound to me in detail his theory of a real—or rather an ideal—University. In the Medical Faculty (he said in effect) there should be only two chairs ; one dealing with the most profound generalisations in physiology, and one corresponding with it in pathology. It would be too much to suggest that this worthy man's ideas, in this extreme shape, had won any converts, but when he went on to explain that all the other chairs, especially the practical ones, though possibly useful in their way, were quite *un*academic ; that hospitals and laboratories of all sorts (from the ideal University

point of view) could only exist on sufferance, and that, above all, Anatomy was to be kept at a distance because it required a dissecting-room, I could not help thinking of Hotspur on the field of battle, and the lordly gentleman with the pouncet-box! But the outcome of it all was that those of us who saw in the University of the future a great congeries of institutes, all furnished, so far as required for teaching and research, with laboratories of one kind or another, and yet bound up into a whole by linguistic, philosophical, and scientific chairs of the old kind, representing the discipline of humanity in a broad and general sense, had to contend at every step for the means and the space demanded, and likely to be demanded more and more, by the expansion of our ideals. Even Dr. Thomson had to be content with an odd corner in the noble pile at Gilmorehill, instead of crossing the road (as he might have done) and occupying a fine and ample piece of ground which had been included in the original purchase, but which (alas!) was adjudged to be sold again owing to fiscal necessities.

And thus, before many years had passed after the migration from the High Street, the needs of the medical school were already under discussion, and the anatomical department, in particular, had to be remodelled, by bursting out of bounds into those unsightly tinned-iron excrescences at the north-eastern aspect, which most of us knew so well, and regretted so much, until they were removed in favour of a still further and more complete reconstruction, in connection with the plans for the new engineering laboratory. Everyone knows now the growth of opinion on this subject that has given rise, under the present Principal, to elaborate statements of the needs of the University in the way of practical and laboratory facilities in many departments, as well as in the medical faculty; and it is a noteworthy fact that some, at least, of the Arts professors have been among the most forward to foresee and to promote extensive changes, as to the urgency and necessity of which their predecessors were very hard to be convinced, even under the gentle persuasion and politic management of

Professor Allen Thomson. The academic status of Anatomy is not only no longer in question, but every great scientific department of human knowledge claims to be taught upon lines similar to those which have long been recognised in Anatomy, Chemistry, Botany, Clinical Medicine, Clinical and Operative Surgery. The whole method and spirit of University instruction have undergone a revolution since the migration in 1870; and the resources and means provided at that date on Gilmorehill have been more and more felt to be antiquated, and latterly altogether inadequate.

My own personal attitude towards this revolution was never in doubt; but, at the same time, I have felt all along that it carried, in my own special department, so much reason and practical necessity on the face of it as to be little likely to be brought into controversy. From first to last I have held, and have preached by example as well as by precept, that a professor of medicine—*i.e.* of what has been technically called *Practice of Medicine*—must be a hospital physician; and that his hospital work and instruction form the one and only kind of laboratory instruction which can be attached to his chair so as to keep his systematic instruction by lectures vivid and fresh and well up-to-date. Ten years of teaching so conducted in Edinburgh before my appointment in 1862 had so riveted this conception in my own mind, that I should have been very seriously disturbed, and even afflicted with a sense of positive disaster, had anyone expected me to follow the precedent of my immediate predecessor (and I believe of all *his* predecessors except Dr. William Thomson) by endeavouring to lecture on systematic medicine without a hospital appointment. Fortunately, a vacancy occurred in the staff of the Royal Infirmary towards the end of 1862; and although the managers as a body, and according to the constitution, owed no kind of allegiance to the University or its teachers, they were kind enough to consider my application favourably; and so (as Professor Lister had, two years before, received a similar appointment on the

surgical side) he and I were alike able to conduct our work in teaching without serious impediment, in its clinical aspects, as well as by lectures, till the time (1874) when we were required by the University to transfer our services to the Western Infirmary.

There was, however, a peculiarity in the arrangements which was far from satisfactory, and which shews how little organisation had at this time entered into the idea of clinical teaching. No physician and no surgeon of the Royal Infirmary had, at the time I am speaking of, what could be called a clinical *class*. The pupils entered their names in a book, and signed from time to time their attendance, not on any particular teacher, but simply as pupils of the Infirmary at large. Two of the four physicians, and a like number of the surgeons, were told off in rotation to give the instruction by two "clinical lectures" in the week; and, for the rest, all was confusion or happy accident, every pupil going to the wards just as much or as little as he pleased, and without reference to any particular course of tuition at the bedside—the very seat and centre of true clinical teaching. It was impossible, on this happy-go-lucky plan, to feel that, either as regards the students or their teachers, the great resources of the Royal Infirmary were being fully appreciated or wisely employed. And accordingly, as soon as the Western Infirmary was placed in a larger measure and in a more permanent way, at the disposal of the University, new arrangements had to be introduced which did not settle into their present form without considerable discussion, but from which, I venture to think, all parties will now admit that very great advantages have been derived.

The hour of the Royal Infirmary visit and of the clinical lecture (co-called) in those days was 8.30 a.m., and as my systematic lecture in the High Street on Practice of Medicine was due at 10 a.m., considerable importance was, of course, bound to be attached to punctuality in beginning the visit or the lecture, as the case might be. I have a very vivid recollection of the bracing effect of these early hours on one accustomed to the later hours of the Edinburgh *régime*,

where the visit hour was at noon and the systematic lecture at 3 p.m. It was, however, rather too much of a feat of mental and physical dexterity, so to speak, to compress a visit (statedly to three wards under ordinary conditions), a clinical lecture on certain days, and a systematic lecture immediately following, with a rush down the High Street intervening, into the brief period of two hours and a half daily for at least five days in the week. It was done, however, somehow; and as I was a very willing worker in those days, and, owing to being a new man in Glasgow, had a sufficiency of time on my hands, I often returned to the hospital alone after eleven o'clock to finish what I regarded as an incomplete ward visit. This was all very well under ordinary conditions, as aforesaid; but presently, and especially in the course of 1864 and 1865, typhus fever —the great epidemic scourge of Glasgow (now happily nearly extinct)—grew to large proportions, and the way adopted in dealing with it was to charge it on as extra duty to the two physicians who were supposed not to be engaged in the clinical "lectures" at the time, and, only when the excess became manifestly overwhelming, to appoint a special physician *ad hoc*. Then, indeed, the life for the first time began to seem a burden to me; and, although I had no fear personally of typhus fever (having passed through a most severe attack in 1884 while still an undergraduate, and being always deeply interested in the subject), it occurred to me that some consideration was due to the limits of human endurance, and something, perhaps, to my double office as Professor of Medicine and Physician to the Royal Infirmary. In virtue of the former office I was a manager myself under the charter of the Royal Infirmary, but I did not choose to avail myself of any privilege on that account; and at the beginning of the winter session of 1865 (I think) the managers received from me a written request, as from one of their ordinary physicians, to be relieved from fever duty during the winter, on the ground that, although I had already done without complaint much more of that special work than any other physician acting

at the time, I really had no power of doing justice to two large wards of fever cases, in addition to my ordinary hospital and academic duties. Then, indeed, occurred the nearest thing to "upsetting the apple-cart"—as a famous South African magnate expressed it—that has happened to me in the course of a long career in connection with hospitals (much longer now than any man in Scotland, or, I suspect, the whole three kingdoms). The *supreme* and all-powerful manager of the Royal Infirmary at that time, being also chairman of the House Committee, was a man of great beneficence and public spirit, but of a pretty stiff backbone, and rather addicted to making himself *felt* (not to put it too strongly) whenever the medical staff appeared to him to be "putting on too much 'side'"—to use a slang phrase for once. To this gentleman, accordingly, my only too humble petition appeared like the attempt of a recently-appointed physician to get a triumph over "the Board," and that, too, in the name and by the power of the University, an institution which the management had never acknowledged in any formal way as entitled to control clinical instruction.

In the correspondence that followed, extending well up to Christmas, in answer to my representation, it was conveyed to me by the Secretary that, in the opinion of the Board, the Royal Infirmary had the first claim upon me, and that all other duties must give way to those already intimated to me, which the managers did not see their way to modify, and therefore they adhered to their previous requirement. My reply to this was very brief—to the effect that while I should continue to act in the fever wards during the Christmas holidays, I should decline to enter them after the 1st of January, and must throw upon the managers the whole responsibility of providing attendance after that date! Tableau! The immediate result was that Professor Allen Thomson had to be called in (he was also a manager under the charter of the institution) to patch up a *modus vivendi*, which he did all the more readily because I believe that many of the members of the Board

had by this time come to see that to push the quarrel to extremities would be generally viewed as an act of mere oppression, and that it was not worth while to administer a snub to the University or to a professor thereof, because he, *qua* physician, declined to accept more work than he could adequately undertake without giving up his professorship. The result, not long after, was that Dr. Perry was appointed to be special Fever Physician ; and so the situation was saved. After this, my personal relations with the managers of the Royal Infirmary (as with those of the Western later on) were always satisfactory, and their behaviour to me was generous in the extreme.

The one anxiety which beset us throughout the progress of the negotiations for the transference of the College to the West was, whether we could absolutely rely upon having the new infirmary, proposed from the first, ready and amply provided in time, for the opening. The anxiety was not without grounds, as the event proved. But at the time that the subscription for the new buildings was started it was all, so to speak, *in nubibus*. It was true that, in respect of the £120,000 promised on behalf of the Exchequer (in the event of a like sum being locally subscribed), there was a distinct engagement that (I think) £24,000 was to be set aside for the building of a hospital. But everyone who had studied the subject knew that £24,000 would go but a very small way, and was, indeed, a quite inadequate sum for building an infirmary which could be relied on for the clinical instruction of many hundreds of medical students. It was necessary, therefore, at once to organise a separate fund for the erection, furnishing, and support of the Western Infirmary ; and, although the general body of the citizens was probably in favour of this, as a valuable and much-needed addition to the charities of the city, there were not wanting some very influential persons who claimed that the Royal Infirmary (of which we are all alike, I trust, justly proud) should be, and should remain, the one and only infirmary for the sick and hurt within the bounds of Glasgow. It was only too easy to see

and to feel the possibility of disaster in this direction, for the Royal Infirmary is three miles from Gilmorehill; and any wreckage, or even great insufficiency, of the scheme for the new Western Infirmary would have meant nothing less than paralysis, if not extinction, for the medical school. On the other hand, it was always possible, or even probable, that in the event of success the clinical requirements of the medical school of the University would be more adequately provided for, and its claims more formally recognised than ever before. It is impossible here, of course, to explain the difficult and complicated, though, on the whole, satisfactory arrangements which had to be gone into in reference to a matter that was of such vital significance for all of us in the Medical Faculty. But Professor Allen Thomson was always confident, and always active and helpful. And we all believed in Dr. Allen Thomson.

Nevertheless, as if to give us a taste of the risks which we in the end escaped, it was found at the last, in 1870, that the Gilmorehill buildings were ready, while the Western Infirmary was not ready—no, nor yet for three years thereafter. Thus it fell to our lot, after all was done as regards the migration to Gilmorehill, to organise a temporary and makeshift arrangement, which, now that we can look back on it quietly through the vista of years, seems like escaping disaster by the skin of our teeth. The systematic classes, from Anatomy onwards to Practice of Medicine, were all to be conducted in the new University buildings; while the clinical work, both in Medicine and Surgery, had to go on during this interval as formerly, in the Royal Infirmary; the hour of visit, however, having been several years before (at the instigation of the late Dr. J. G. Fleming) altered from half-past eight to nine, while my class of Medicine was taken an hour later than before, viz. at 11 o'clock. At 10.40 a.m., accordingly, a service of swift omnibuses was provided at the Royal Infirmary gate, to convey all the students who had a class at 11 o'clock to Gilmorehill. The students, I am glad to say, entered into the spirit of the whole arrangement with

diligence and punctuality ; and on the whole the good work went on with surprisingly little detriment, though it was no small strain upon the learners and also upon some of us teachers, from 1870 to 1874, when the Western Infirmary was at last opened for the winter session.

But our anxieties were by no means at an end when the building, admirably planned and appointed as it was for the purposes in view, was ready for the opening, in November, 1874. By an almost inconceivable procrastination in some quarter or another, not a single patient had been admitted to the wards on the very day when the college was to resume its educational work, when the medical students were flocking into their class-rooms from all quarters, and when the makeshift arrangements above referred to had become impossible owing to the final severance—under direction—of all the medical and surgical officers representing the University from their posts in the Royal Infirmary, and their formal election to corresponding offices in the Western Infirmary! Fancy the searchings of heart with which all of us thus stood on the very threshold of a clinical course, due to a day, and even to an hour (according to precedent), at the very opening of the winter session, but in imminent danger of being starved or wrecked by a failure in the very material of bedside instruction, or even by imperfect knowledge on our part of what cases were, or might be, producible for clinical teaching! The suspense was great, but fortunately it was not very long maintained. The Western Infirmary was opened without a trace of ceremonial of any kind, and it is not too much to say that within twenty-four hours the beds then existing (but they have been more than doubled since) were full, so great had been the pressure going on for weeks for admission to the new institution. It was hard work at the beginning of the new session, to have so many new cases to learn and to teach from all at once, but it was done ; and so far as my own share of the experience went, there has never been from that time to my retirement last summer any want of abundant material

for clinical teaching in the Western Infirmary. Nor can I express too strongly the deep and abiding sense I have, as representing an important chair in the University, of the kindness and consideration we have received from the managers, of which a new and most signal evidence was afforded in the large and complete pathological laboratory erected for the late Professor Joseph Coats, and continued to his successor, Professor Muir.

Here perhaps these desultory reminiscences of an old and retired Professor had better be brought to a close. Bearing, as they do, on a particular aspect of a great subject, they are nevertheless a part of the annals of the University of Glasgow, and of its great "flitting," that might easily be overlooked, and are therefore not without a fitting place in the volume of its Ninth Jubilee.

A TRIBUTE TO THE MEMORY OF THE LATE J. WARBURTON BEGBIE, M.D.

Being Remarks addressed to the Class of Practice of Medicine in the University of Glasgow, February, 1876.

SINCE we last met here, on Friday, I have lost a very dear friend, and the medical profession one of its most distinguished ornaments. You all know to whom I allude—Dr. Warburton Begbie—a man who had been recognised by all classes in Scotland, and beyond Scotland, as the ablest and kindest of physicians, and whose premature death is, with me, the sad ending of a friendship extending over more than a quarter of a century, as to which I can now say that nothing in it, save the sad ending, is fraught with any sorrow, but, on the contrary, with all the best associations and tenderest memories. I have tried in vain to put in writing, for your benefit, some indications of what Dr. Begbie was to me and to those who knew him best. Language fails; and feelings that cannot be fittingly expressed here rise up in the presence of so recent and so overwhelming a calamity. But these few words, spoken just as they arise to my mind, you will take as being the sincere, though inadequate, tribute of a warm personal friendship to one whose name was known to multitudes, and whose life was a lesson to all of us.

Dr. Begbie inherited from his father an apparently robust constitution, and a tall, graceful, and somewhat stately presence; to these, he added an exquisite charm of manner

that was peculiarly his own, and which no one could or did resist, because it was so obviously founded on a quick sympathy and a genuine desire of doing good. He may be said also to have inherited, or, at least, derived from his father, the traditions and connections of a very successful family practice in Edinburgh, one which extended back in direct succession to the days of Abercrombie, and which brought with it a great deal of curious practical information in details, as well as valuable personal associations. But it was obvious, almost from the first, to those who knew him intimately, that the younger Begbie was destined for other things than family practice; and, though he did the duties of this, as he did everything, thoroughly and well, his heart was set upon hospital practice, and his aims were those of a consulting physician. I remember his father, not long after I left Edinburgh in 1862, telling me that Warburton did not really care to extend his family practice, but preferred his hospital and his lectures, with such chance of consulting practice as was then open to him. It was in 1855 that I was first brought into formal relations with him, on equal terms, in the Edinburgh Royal Infirmary, as physician; previously to that time, we had been just two years apart as students, and in most of our occupations separate, though much within sight of each other in the Infirmary and elsewhere. It fell to me, in 1855, as the senior physician willing to undertake the duty, to deliver the authorised extra-academical course of lectures on clinical medicine; and it at once occurred to me to make it, what it had never been before, a co-operative course, so that the efforts of all the physicians willing to teach might be engaged, and the students might have a larger field. Dr. Warburton Begbie and Dr. Keiller were thus associated with me from the first in clinical teaching, and the method then initiated prevails in Edinburgh to this day. From this time till 1862, my dear friend and I were always side by side, doing essentially the same work, and, to a considerable extent, teaching the same students, in hospital and elsewhere. It is with a sense of profound thankfulness that I tell you what is simply

the fact, that no single occasion arose during all that time on which there was anything less than the most complete confidence and goodwill between us. He was a man with whom it was eminently safe to act as a colleague, because he was entirely above every kind of suspicion of taking any undue advantage. The same transparency of character, and freedom from everything like intrigue or *dodging* (to use a vulgar but expressive phrase), attended him in the practice of his profession ; and I cannot doubt that it was the main secret of the great confidence reposed in him by his professional brethren. Among his many contributions to medical literature, there is not one that can be said to be written with a view to exalt himself, or to obtrude unduly claims to success in treatment, or, indeed, any other kind of success. And yet his success, both as a teacher and as a physician, was most marked from the first, and it was founded on genuine, thorough study of and sympathy with his work. He was a most laborious, as well as a most successful, hospital physician ; indeed, I often thought at the time that he was overstraining his energies, even then, by the unmeasured way in which he gave his hours to work which was not directly remunerative, and which must have greatly invaded the time required for family practice, and for other occupations. But he had what appeared to be a just confidence in his strong, well-knit frame and vigorous mind, and he gave himself with all the enthusiasm of a finely touched nature to the work of his life, never resting in his desire to know all that could be known, and do all that could be done for the relief of disease. The first lesson of Dr. Begbie's life, undoubtedly, taken as a whole, is simplicity of aim and steadfastness of purpose. He knew well what he was fit for ; knew, therefore, where his work lay, and he did it thoroughly with his whole heart and mind. This success, therefore, was a genuine success ; not a factitious reputation, founded on a mere show of superficial acquirements. And his success as a teacher and lecturer was not less real and genuine. Most people, indeed, regarded it as a great misfortune, when, in consequence of what I

believe to have been a misunderstanding of his wishes, he was not reappointed to be Physician to the Royal Infirmary in 1865. His retirement as a lecturer followed soon afterwards; and the extraordinary increase of his consulting practice showed how well he had laid the foundation in the goodwill of his brethren. The second lesson of his life, therefore, is faithfulness and loyalty of personal character, which, added to severe and thorough study of his profession, and the personal gifts to which I have already alluded, made him what he was. There is one other lesson, which, perhaps, I ought not to insist upon here; but I will venture to allude to it, because, without it, my brief memorial would be incomplete. Dr Begbie was not only an accomplished and a humane physician, he was also a religious man. Let me say exactly what I mean. He was a Free Churchman, as we all know; and he was so, as he was everything else, genuinely and conscientiously. But I do not think of him as a Free Churchman. His religion was not a special form, but an unseen influence; it was woven into the web and woof of his character, not displayed upon the surface of it, in patches. He did the work of Christ, and he did it in the spirit of Christ. I call him, therefore, much more than a Free Churchman—a Christian and a Christ-like man. Underneath all his outward acts, underneath even the feelings he showed to others, lay a great and abiding sense of responsibility. He used the talents committed to him unsparingly in the service of his fellowmen, but he used them also after the manner expressed by Milton, in one of the finest of his earlier poems: he did his work, from first to last

"As ever in the great Task-master's eye."

OLIVER WENDELL HOLMES

A Letter addressed to the British Medical Journal, November, 1894.

My Dear Editor.—I have read with full appreciation your notes of an interview last year with dear old Oliver Wendell Holmes, and am at once glad and proud (as any of us may well be) to find myself lovingly remembered as one of his old friends on this side of the Atlantic ; but it is only right to make one correction. It is certainly not the case, and could not, possibly, I think, have been intended to be conveyed to you that Dr. Holmes met me in Paris in the days of Louis Philippe, for at the time referred to I was more likely to have been puzzling over the multiplication table and vulgar fractions, or over early Latin lessons in school, than over the statistical method of studying typhoid fever, under Louis at La Pitié. There was a Dr. Jackson, jun., who was a well-known pupil of Louis at that time, and was then and afterwards quite affectionately intimate with Dr. Holmes. I think it not improbable that my name may have slipped into your notes in this relation alongside of his. But, be this as it may, the true origin of my long friendship and correspondence with Oliver Wendell Holmes may possibly interest your readers ; and certainly there is nothing in it which can do aught but add to the feelings of affectionate admiration with which he is regarded by the entire medical profession, not to say the whole civilised world.

The real date of my first correspondence with Oliver Wendell Holmes was 1856. In the immediately preceding year Dr. James Jackson, then only known to me by reputation, and the father of the James Jackson, jun., aforesaid, published a small volume entitled *Letters to a Young Physician just Entering upon Practice.* Being at the time a young physician myself, although old enough to have established a connection with the *Edinburgh Medical Journal*, I read this work with a strong feeling of youthful enthusiasm, and was only too happy to find, or make, an opportunity of commending it to the notice of others in a review, which I can read even now without any sense of its being over-strained, although pervaded in every line by the feelings of deep respect and gratitude excited by the perusal of this work of an evidently venerable, but to me little known, author.[1] Certainly the last thing that ever could have entered my mind in penning this anonymous tribute was that it would bring me into friendly, even affectionate, relations with one who, if not quite indisputably first of American humorists, is at all events inferior to none in the unique character and high spiritual quality of his humour.

Oliver Wendell Holmes was at this time a man in middle life, and had become very closely connected by marriage with Dr. Jackson. How it was discovered that I wrote the review in question I do not now remember; but the next post brought me a letter of acknowledgment from Dr. Jackson, and the immediately succeeding one a photograph of the old man, sent to me with his signature and with a delightful letter from Dr. Oliver Wendell Holmes, who revealed to me in it the warm affection and almost unqualified admiration inspired throughout the profession in America by the character of the senior. That portrait of Dr. Jackson is now in my retiring room at the University.

His son, James Jackson, jun., had been previously known to me by reputation as a follower of Louis, and as one who had died too early to have established a fame such as

[1] *Edinburgh Medical Journal*, April, 1856, p. 934.

might have been anticipated from his communications to the Society of Medical Observation in Paris. He was, I believe, well known to the late Dr. Walshe.

In 1861 *Another Letter to a Young Physician* was published by Dr. Jackson, senr., with some further medical papers, including an important one on the last illness and death of Washington.

But my intercourse and correspondence with the older man may be said to have been wholly taken up and absorbed by Oliver Wendell Holmes, with whom, to my great satisfaction and advantage, I have ever since continued to correspond from time to time, the last letter received from him being certainly not quite two months before his death.

Even at the above-mentioned date of my first letter from him, the name of Oliver Wendell Holmes was well known as that of an author of a quite unique charm, who was thoroughly loyal to his profession while cultivating literature with a rare amount of success.

I cannot now remember what it was in his writings that had impressed me, as I suppose the famous "Autocrat" could not possibly have been known to me till some years later.

But I know this, that as the star of Lowell had not yet risen, at least on our side of the Atlantic, the literary quality of the humour displayed in the writings of Holmes, and the large grasp of contemporary thought, combined with medical and physiological illustration, struck me as being a quite new phenomenon alike in literature and in medicine; and having been up to that time accustomed only to bits and scraps in newspapers of Josh Billings, etc., as samples of American humour, I was the more drawn to this unique phenomenon, and my personal relations with the author, as above indicated, were more than maintained by an ever-increasing familiarity with his works, on which, however, I will not at present enlarge, as my opinion on them is simply that of the whole world.

It has been well observed in many quarters that to know Dr. Holmes, even through his works, is to know him and to love him personally; but I venture to think that the rela-

tions into which I was thus accidentally drawn with him in correspondence were of a kind which justified his allusions in the interview which you record, quite apart from the apocryphal Paris incident.

For more than a quarter of a century I continued to correspond with Dr. Holmes from time to time without having ever had the chance of seeing him ; and when he came to this country in 1886 I was most anxious to have it arranged that he should come to Glasgow and be in the hands of a few friends there for a visit to the Clyde and the land of Burns. The American consul here at this time was Francis H. Underwood, LL.D., a man of fine literary instincts, a personal friend of all the Boston men of eminence, and one of the early contributors to, if not editor of, the *Atlantic Monthly*, a man whose personal popularity in Glasgow would have alone secured the success of such a visit.

But it was not to be. The magnetism of London society and the necessity of coming down to Edinburgh, though but for a single day, in order to receive the degree of LL.D., exhausted all the time that could be devoted to Scotland, and I had to content myself on this occasion with the pleasure of being present at the graduation in Edinburgh, and afterwards meeting Dr. Holmes in the evening at the house of Professor Crum Brown.

In the autumn of 1891, however, I went to the United States for the first time, and was, like you, privileged so far as to spend the greater part of a day with Oliver Wendell Holmes at his beautiful summer residence at Beverley Farms.

I did not make any notes of that interview at the time, and have, therefore, not much in detail to say about it ; but it was peculiarly delightful to find the dear old man actually awaiting me at the station, full of gentle kindness and astonishing animation ; full, also, of beautiful reminiscences and little revelations of his innermost self, which will remain with me, I trust, to the last.

At this time Mrs. Sargent, his widowed daughter, who accompanied him to this country in 1886, was dead, and he

lived with his son, the eminent Judge Holmes, referred to in your memoir, who was with us at lunch at Beverley Farms on that occasion. The conversation naturally had a very wide range, and I cannot make even an attempt to reproduce it; but it left on my mind the same consistent impression which Oliver Wendell Holmes produced upon everyone alike in his works and in his conversation, of a mind rendered cheerful and serene, not to say youthful, even in extreme old age, by a kind of optimism which could only be the result of a well-assured and loving trust in the best and highest spiritual things. In him this element of what may be very properly called "faith" was so steadily maintained as to keep him easily at a spiritual level far above the small vexations which disturb inferior natures. Some of the memoirs of him lately published seem to me to assume too easily that this happy temper of his was a result merely of his fortunate surroundings. I will venture to indicate, without going into any detail, that this was by no means so. Oliver Wendell Holmes was a happy man to the last, in spite of some very disturbing domestic circumstances (for which no one was to blame), but which in many high-strung natures might have tended to quite the opposite result.

There was a curious similarity in this respect, as in some others, between him and my equally dear friend Dr. John Brown, the well-known author of *Rab and His Friends*; but the temperament of the two men, alike in one thing—lovableness—was very different, and the effect of calamity upon them differed accordingly. Both of them, however, will remain to all who have personally known them, and to a multitude of admirers who have not had that privilege, among the noblest, as well as the most lovable men of genius that our age has produced.—I am, etc.,

W. T. GAIRDNER.

P.S.—One little incident of the conversation that afternoon is perhaps worthy of being recalled and here set down, because it appeared to me at the time so absolutely characteristic; and there is no one else in a position to record it.

In the course of a long drive in his little open carriage, in which he took me to see some of the noble elm trees of which he had sung the praises in his works, we found ourselves not far from Salem, the birthplace of Nathaniel Hawthorne, with its terrible memories of witch persecutions in the seventeenth century. I had just been visiting a very old lady (all but a centenarian) well known to him, whose marvellous vitality and restless mental activity contrasted, in a way that almost seemed "uncanny," with the bodily frame wasted and decayed to the last possible degree consistent with life. I ventured to say to him: "It is fortunate that Mrs. —— did not live in Salem in the middle of the seventeenth century; for some of your ancestors or their friends would certainly have had her burned for a witch." He looked me in the face, half seriously and half in jest, and said at once: "Is she *not* one?" I could not help thinking at the moment of Elsie Venner, with the impossible rattlesnake theory so elaborately worked out through the aid of the "scientific imagination," till it seems to the average reader to be gravely propounded as an outcome of the laws of heredity.—W. T. G.

MEDICAL SUBJECTS

CONTRIBUTIONS TO THE PATHOLOGY OF THE KIDNEY

From the Monthly Journal of Medical Science, 1848, *vol. viii. pp.* 715, 795, *and* 860, *and* 1849, *vol. ix. pp.* 14-96.

Introduction.—On the Normal Structural Anatomy of the Kidney.

There can be no doubt that Van Helmont[1] *regarded disease of the kidney as the cause of dropsy, and that Cotunnius*[2] *discovered the renal secretion in cases of dropsy to be coagulable by heat; it is unquestionable, however, that Richard Bright*[3] *is justly entitled to the entire honour of determining the connection between dropsy, albuminuria, and disease of the kidneys. Other observers, such as Blackall*[4] *were certainly engaged in investigations upon the subject; but none of them attained to more than a vague conception of the association between the presence of dropsy and the renal functions. In his earliest work, Bright clearly showed that albuminuria and dropsy are the result of structural changes in the kidneys. He depicted indeed, with wonderful accuracy, the pathological alterations which we still recognise. To his original contribution, he added much valuable information, in the first and fifth volumes of Guy's 'Hospital Reports.' In these different works, he showed that*

[1] *Oriatrake, or Physick Refined*, London, 1662, p. 507.

[2] C. Darwin, *Zoonomia*, 1894, London, vol. i. p. 316.

[3] *Reports of Medical Cases*, London, 1827, vol. i. *Guy's Hospital Reports*, 1836, vol. i. and 1840, vol. v.

[4] *Observations on the Nature and Cure of Dropsies*, London, 1814.

albuminuria might be elusive, and dropsy absent; and he described almost all the symptoms and complications of kidney disease—even suggesting an explanation of cardiac hypertrophy, which has not yet been refuted. It is perfectly true that his conception of the pathological processes was warped by the theories of his time, in accordance with which he regarded the various forms of kidney disease as merely stages in the development of a deposit. The observations of Christison,[1] *and the labours of Rayer*[2] *prepared the way for an explanation of the renal lesions, based upon reaction to irritation. The latter of these authors, indeed, gave an adequate description of the process, as being of the nature of inflammation.*

Such was the general state of pathological and clinical information as regards renal affections when Gairdner published the following paper, and it therefore is not merely of interest as being his first contribution to medical science, but as being one of the earliest investigations into the pathological relations of the kidneys. In the article itself will be found reference to the labours of other workers in this field, and it is unnecessary to make any further remarks upon them.

It will be observed that the author employs a terminology which differs in certain essential particulars from that of our own age. He speaks, for example, of "exudation," and the phrase, as used by him, evidently corresponds sometimes to what we would now term "degeneration," while, at other times, it just as clearly corresponds to "desquamation."

The author gives, by way of introduction, an excellent description of the normal histology of the kidney, and thereafter proceeds, under the heading of "Pathological Anatomy," to deal with exudations—crystalline, or saline; oleo-albuminous; and purulent—into the tubes. In the discussions of these different aspects of the subject, Gairdner corrects some of the errors of Johnson as regards albuminuria; while he fully acknowledges the justice of this author's views regarding fatty changes. Exudations into the Malpighian bodies, and into the stroma occupy but little space; and these are followed by what is termed by the author "Partial distribution of the oleo-albuminous exudation." This appears to correspond to what we would now term infarct, in certain cases, and sclerotic atrophy in

[1] *On Granular Degeneration of the Kidneys*, Edinburgh, 1839.

[2] *Traité des Maladies des Reins et des Alterations de la Sécrétion Urinaire*, Paris, 1839-44.

others. Dealing with the lesions which affect the vascular system of the kidney, Gairdner describes marbled or waxy kidney, which he, for the first time, brought prominently into view in this country, and discusses in the most interesting way whether the chronic processes constituting so large a proportion of Bright's Disease are usually preceded by acute processes. A discussion of the changes occurring in the tubes themselves forms the third part of the paper, and the processes of desquamation of the epithelium, and obliteration of the tubes, are fully described. The formation of cysts is explained, and this undoubtedly is one of the most original sections of the paper, which ends with a reference to dilatation and thickening of the tubes.

The paper concludes with a series of short remarks on the principal physical characters ascribed to diseased kidneys, which will be found to be extremely terse and definite.

When it is considered that this important contribution appeared within three years of the graduation of its author, and when he was just at the threshold of his twenty-fifth year, it is impossible not to be struck with admiration at the indefatigable industry, scientific insight, and philosophic generalisation which he exhibits.

THE researches of modern anatomists have shown, that the secreting substance of the kidneys may be considered as essentially consisting of the ramifications of a mucous membrane, which is disposed in the form of minute tubes, clothed internally with epithelium, and closely embraced by a network of capillary vessels. These tubes, which in the cortical substance have an exceedingly irregular and tortuous distribution, pass through the pyramids, forming straight converging ducts, which unite together two by two, and finally open on the surface of the papillæ, where their epithelial lining becomes continuous with that of the calyces, pelvis, and ureters. In every part of their course, these tubes are accompanied by vessels, the ramifications of the renal artery and vein being distributed copiously in the substance of the pyramids, and over the external surface of the organ ; while the cortical substance is chiefly occupied by the capillary plexuses surrounding the tubes, and by the Malpighian bodies, which have been ascertained to

consist of globular tufts of vessels arising from the arteries, but whose anatomical relations and function are still the subject of much discussion.

The labours of anatomists having been thus far successful in elucidating the normal structure of the gland, it became necessary for pathologists to follow in their steps, and by the same modes of investigation to trace out the changes induced by disease in those structures which had been found to be the most essential anatomical elements of the kidney. This branch of the investigation was first taken up by Valentin,[1] Hecht,[2] and Gluge,[3] who from 1839 to 1842 published various microscopic observations on Bright's disease and other morbid conditions of the kidney. These became extensively known in Germany, and gave rise to several other contributions of much interest, but appear to have excited no attention in this country till after the publication of the papers by Dr. Johnson and Mr. Simon of London, in the *Medico-Chirurgical Transactions* for 1846 and 1847. It is proper, however, to state, that in 1842, Mr. Goodsir addressed to the Medico-Chirurgical Society of this place a communication on the anatomy of the kidney, and the changes in Bright's disease, which was, so far as I am aware, the first contribution to the literature of that subject in this country ; but of which an abstract only was published in the *Monthly Journal* for that year.[4]

The researches which I am now about to publish, were at first undertaken, and indeed had proceeded a considerable way, with a very imperfect knowledge, on my part, of what had been previously observed by others. In consequence of this circumstance, I was led to the minute and careful investigation of some minor points in regard to the normal

[1] Valentin's *Repertorium*, Bd. II. 290.

[2] *De Renibus in Morbo Brightii Degeneratis*, Berolin. 1839 ; and in Casper's *Wochenschrift*, 1839.

[3] *Observationes Anatomico-Pathologicæ.*

[4] Dr. H. Bennett informs me that he has, for several years, explained in his classes the views on stearosis of the kidneys contained in Gluge's work ; which he had also verified himself.

anatomy of the gland, which I might perhaps have neglected had I possessed the confidence which the examination of Gluge's drawings would have given me. Nevertheless, as no one appears to me to have observed and defined with sufficient exactness the *standard of health* in the human kidney with relation to microscopico-pathological researches, I have thought it might be desirable to introduce the strictly pathological part of this inquiry by a few observations on the healthy structure, premising that I mean to notice only such points as are important in reference to pathology, and by no means to attempt anything like a complete account of the normal anatomy of the gland.

1. *On the Vascular System of the Kidney.*—It is well known to all who have given attention to the subject, that nothing is more variable, even in cases where no disease can be suspected, than the vascularity of the kidney. Nevertheless, there is no doubt that in some instances its abnormal character is most important as leading us to detect disease of the organ. It is therefore of some consequence to understand the conditions under which the most marked alterations of vascularity take place.

The veins of the kidney are disposed chiefly in two situations, viz. on its surface, and in the substance of the pyramids. The cortical substance contains exceedingly few. On the surface, they form a peculiar network, visible with the naked eye, and known to anatomists from the time of Ferrein, forming, by their intersections, the boundaries of small pentagonal or hexagonal spaces, in the interior of which the natural pale colour of the cortical substance appears, about the size of a very small pin's head (half a line). The larger veins are scattered over the surface, and tend to a stellate distribution. In this situation, the venous injection is liable to the greatest irregularity of distribution and amount (as has been well described by Rayer). The veins may appear in the highest degree distended, or, on the other hand, perfectly anæmic, in correspondence with the degree of fulness of the general venous system ; and no change is more frequent than the distension of veins on the

posterior side of the organ, from gravitation of the blood. On the other hand, great irregularity of injection, amounting to marbling of the surface, and great increase in the size of the stellar vessels, are generally tokens of disease, as they are the result either of partial obliteration of the venous network, or of the extrusion of the blood from it through over-distension of the loops of tubuli, which form the intervening pale spaces.

The engorgement of the capillaries and Malpighian tufts give rise to two conditions: first, a generally diffused heightened colour of the cortical substance; and, second, increase and greater distinctness of the vascular striæ running from the base of the pyramids to the external surface. This latter species of injection often exists, to a great extent, without any corresponding injection of the rest of the kidney, and, in some instances, the red points composing the striæ are so much increased in size as to form considerable petechiæ (one line in diameter or upwards), in which case the petechiæ usually extend to the surface, occupying the intervening spaces of the venous polygons above mentioned. This appearance was supposed by Rayer to occur from simple hypertrophy and vascular injection of the Malpighian bodies; but Bowman,[1] who has shown that the Malpighian bodies do not exist on the surface of the kidney, has also given a better explanation of such petechiæ, which he holds to arise from rupture of the Malpighian tuft, with extravasation of blood into the neighbouring tubes. He argues that the petechiæ are of irregular form, and of much larger size, than the Malpighian bodies have ever been observed to acquire. He gives, also, a figure, representing the occurrence of a similar appearance from artificial injection at the surface of the kidney. In this figure the loops or knuckles of the tubuli are seen filled with injection, presenting themselves at the surface, and surrounded by the venous network. The correctness of this explanation cannot be doubted, and it is therefore evident, that the occurrence of these petechiæ must be considered as invariably morbid.

[1] *Philosophical Transactions*, 1842.

The blanched and anæmic state of the cortical substance is a very frequent condition. In many cases, no trace of red colour is to be seen, and the vascular striæ and points are wholly imperceptible; in others, a uniform light rose colour mingles with the pale yellowish tint peculiar to the renal substance, and the vascular points indicating the Malpighian glands are faintly visible. These conditions may coexist with abundant vascularity on the surface, and in the pyramidal portions of the kidney; and they have, since the publication of Dr. Bright's researches, been looked upon by all pathologists as of the greatest importance in estimating the healthy and diseased states of the gland, particularly in reference to the granular degeneration. Rayer points out that inflammatory and other diseases of the kidney, are the most frequent of all causes of decoloration of the cortical substance; but he has also noticed the occurrence of anæmia as an independent lesion in the kidney, and says, with great justice, that both in the anæmic and hyperæmic conditions of the organ, the partial character of the vascularity is much more decidedly indicative of the presence of a morbid product than its absolute amount.

I have had numerous opportunities of examining, microscopically, kidneys in which the cortical substance was decolorized, both where this occurred independently, and where it was connected with abnormal deposits in the organ. In such cases, the Malpighian coils of vessels, which, in a strictly normal specimen, may be observed filling the capsule, particularly towards its circumference, with red injection, are pale, bloodless, and compressed, sometimes maintaining their rounded form,—at other times, more or less angular. Along with this condition of the Malpighian bodies, I have generally observed distension of the urinary tubules, either by morbid deposit, or by the accumulation of their own secretion. In the latter case, the kidneys have usually been above the normal size, and of more or less diminished consistence. On the other hand, in cases in which the kidneys have been about or under the usual size, and firmer in texture than ordinary (without morbid

deposit), I have several times observed the amount of vascular injection to be greater than usual.

In considering these phenomena with reference to their cause, it is not difficult to show, that from the anatomical constitution of the kidney, the fulness of the urinary tubules must of necessity induce, as its first consequence, compression and emptying of the Malpighian vessels. For whether we adopt the view of Bowman, who asserts the capsule of the Malpighian body to be the dilated extremity of the urinary tubule—or that of Gerlach,[1] who regards it as a diverticulum—or of Toynbee,[2] who considers it as a separate membrane retaining the tubule and the vascular coil in contact with each other, we find that the close connexion of the Malpighian vessels with the urinary tube is maintained by the majority of modern observers (although denied by Reichert and Hyrtl). If this be admitted as probable, then it follows that fluid pressure arising within the tubules must fall back upon the Malpighian vessels. Moreover, from the exceedingly firm character of its fibrous investment, the kidney cannot be suddenly increased in bulk without considerable pressure being exerted on its substance; so that, as a consequence of the anatomical disposition of the gland, the sudden engorgement of its secreting tubes must necessarily be followed, even in health, by the diminution of its vascular supply. I shall afterwards have to adduce numerous instances of the occurrence of this in the pathological states of the gland.

On the whole, it appears from the analysis of the variations in the vascular system, that the most interesting of these, in reference to pathological inquiries, are those of the Malpighian bodies; and that the varieties of the superficial venous plexus are of little consequence, excepting in the case where it is so unequally filled as to give rise to mottling or marbling of the surface.

2. *On the Tubuli Uriniferi.*—The tubes, within which the urine is secreted, are composed of an extremely delicate, translucent, and brittle membrane, the exterior of which is

[1] Muller's *Archiv*, 1845, No. IV. [2] *Med. Chir. Trans.* vol. xxix.

in contact with the capillary vessels, and the interior with a layer of nucleated cells. That these cells are intimately connected with the function of secretion has long been considered probable; and the researches of Goodsir comprise observations extending over so wide a series of secreting structures, and so apposite, as almost to amount to demonstration,[1] that the epithelium of the ultimate glandular ducts is the immediate agent in the process of secretion. Hence the pathological alterations of these structures have become of peculiar importance.

In the kidney of the human subject, the appreciation of the normal characters of the tubuli, and of their epithelium, is a task of no small difficulty. It is not always easy, especially in hospitals situate among the population of large towns, to find organs which can be relied upon as furnishing a standard of health; and, even in those which present no obvious marks of disease, the variations observable in a minute examination of the tubes, are so frequent and considerable, as to present the greatest difficulties to the unpractised observer. With the view of familiarizing myself with these variations, I examined, during nearly two months (with the kind concurrence, and frequently also the valuable aid of Dr. Bennett), all the kidneys, with few exceptions, which were removed at the *post-mortem* examinations in the Edinburgh Royal Infirmary. The following results of this inquiry may be useful to those engaged in similar observations, by preventing the mistake of healthy for diseased conditions.

The lining membrane of the tubuli, which is the homogeneous or basement membrane of Bowman, is never seen in the fresh and healthy kidney uncovered by epithelium-cells, the nuclei of which are ranged, at pretty nearly equal distances, over its internal surface. In certain diseased states, and also as the effect of maceration, there may some-

[1] This is peculiarly evident from his observation on the testis of the Squalus Cornubicus, where the actual process of secretion may be said to take place under the eye.—*Anat. and Path. Observations*, No. V. *and Trans. Royal Society of Edin.* 1842.

times be seen in the kidney considerable portions of tube having a perfectly homogeneous character, and perfect transparency, with no appearance of structure. Much more commonly the tube is seen destitute of epithelium-cells, but retaining in its walls a few scattered oval nuclei, about one-third smaller than the nuclei of the epithelium-cells. These are the young epithelium-nuclei of Bowman, the germinal centres of Goodsir. They appear imbedded in the substance of the membrane, and are very rarely separated from it even when, in diseased conditions of the kidney, the tube has ceased altogether to perform its function. Observations illustrative of these facts will be given in a succeeding part of this memoir.

The membrane of the tubuli appears to be possessed of considerable elasticity, so as to be capable of accommodating itself to the greater or less amount of secretion within them. In no case is it thrown into folds when the tubes are *in situ*, even when the calibre of the tube is very much narrowed. In the strictly normal kidney, however, the diameter of the tubuli varies much less than might be supposed, being generally, in all parts of the organ, from 1-25th to 1-15th of a millimetre. This is no doubt owing to the constant nature of the secretion, and the freedom with which it escapes as it is secreted, on account of which the tubes are not, like the ducts of the mammary gland, subject to alternate distension and relaxation.

The epithelium-nuclei of the tubule are, as above stated, in the normal state arranged at somewhat regular intervals on the inner surface of the membrane, the intervening spaces being occupied, and entirely filled up by the cell-walls, which, when *in situ*, assume an irregularly polygonal form from mutual pressure, according to the amount of distension of individual cells. The cavity of the tubule appears to be entirely filled up by these cells and by the secretion which distends them, and which, when freed, filters away between them.

The size of the nuclei is pretty constantly from 1-120th to 1-100th of a millimetre. They are circular, and have an

extremely clear, well-defined edge, which is perfectly smooth when the kidney is fresh; but occasionally, from putrefaction or other causes, becomes slightly irregular, destroying the circular form of the nucleus. They appear quite flat by every arrangement of the light, and when seen sideways become oval or nearly linear. By transmitted light they have a slight uniform shadow, and present one or two central dark points, which, however, are not constant in their occurrence and position. I have not observed the nuclei to present the phenomena of endosmosis and exosmosis. The addition of water produces little change on them; acetic acid generally makes them clearer than before, but rather by dissolving away surrounding obscurities than by any change in the nucleus itself.

The cell-wall is extremely delicate, sometimes indeed so much so as to be scarcely visible, even with the most careful management of the light; but if a current be produced in the fluid, when the nuclei are floating free on the field of the microscope, the presence of the cell may always be recognised, even when it is most delicate, by its preventing the complete approximation of the nuclei to one another. In a certain proportion of the nuclei, also, it appears to be absent even in the most healthy kidneys; and I have frequently seen organs presenting no other apparent change, in which the proportion of free nuclei was so large that it was difficult to find a complete cell among them. Of this circumstance, and also of the extremely different degrees of tenuity of the cell-wall, where it is present, I am not able to offer an explanation, further than that the latter seems to have a relation to the rapidity of development of the cell; inasmuch as when the cell-development is evidently sluggish, and the tubes obstructed with granular matter, the cell-wall is, in the majority of instances, denser than usual.

The size of the entire cell varies considerably; it being sometimes but little larger than the nucleus, while at others it attains a diameter of 1-50th or even 1-40th of a millimetre. Its shape, when free, is spherical; within the tubule, however, this shape is modified by the pressure of surrounding

parts. When floating free in fluid, the cells frequently roll over, showing the position of the nucleus, which is attached to the side.

The fluid contained in the cells of the tubules, being in fact their own secretion, is, in the strictly normal state, perfectly transparent. Nevertheless, it is exceedingly common to find it clouded and rendered opaque by a minutely molecular deposit, which may be so abundant as entirely to obscure the nucleus, or may even appear distinctly granular, being at the same time scattered over the field of the microscope, and resembling very closely some of the morbid deposits to be hereafter noticed. This molecular shading of the cells is, in the great majority of cases, owing to a deposit of lithate of ammonia, which is removed almost instantaneously by the addition of an excess of acetic acid. Such a deposit, when moderate in quantity, can scarcely be called morbid, as it takes place from the cooling of the urinary secretion under the most various circumstances, and without any other trace of the presence of disease.

It is not yet certain whether the act of secretion implies the disappearance and subsequent removal of the cell-wall, or whether the cell gets rid of its contents by a process of exosmosis, in the same way as by endosmosis it receives them from the vessels. But if the former view be correct, it is clear that the effete particles must be removed by the urine in a molecular form or in solution ; as no epithelial debris of any kind can be detected in the tubes of a perfectly healthy kidney, and the existence of such debris is one of the most unequivocal and ordinary signs of disease. It is not at all improbable that the molecules of effete secreting epithelium may constitute a considerable part of that impalpable sediment which subsides from normal urine, and which is so fine as frequently to present, even under the microscope, nothing but a cloud of almost invisible molecules.

Whatever be the destiny of the cell-wall, the nucleus must be regarded as a permanent structure, whose function is

the perpetual renewal of the membranous cell-wall, and of its secretion. Accordingly, the nuclei have a greater power than any other part of the organ of resisting decomposing agencies; and they are never observed in the urine except when the tubes are the seat of disease.

3. *On the connecting Tissue or Parenchyma, and the general Structural Arrangement of the Kidney.*—Toynbee, who ascribes great functional importance to the parenchyma, describes it as consisting in part of peculiar cells, similar to those within the tubes, to which blood-vessels and nerves are distributed, and which he supposes may have the office of effecting some change in the blood preparatory to the secreting process. Bowman and Goodsir describe the different anatomical elements of the kidney as connected together by a delicate fibrous tissue, which forms a sustaining skeleton for the organ.

The general arrangement of the tissues of the kidney is readily seen by making careful sections through the cortical and tubular substance with Valentin's double knife. Where such a section is made through the cortical substance, the tubes are seen sometimes in section, and sometimes presenting to view more or less of their sides, enclosed in the areolæ of an extremely delicate and lax fibrous tissue, which is so disposed as completely to fill up the interspaces. Here and there a Malpighian body is seen surrounded by its capsule, and enclosed in an areola two to four times the size of that of the majority of the tubules. By rubbing the section between plates of glass, some of the tubes may often be displaced, leaving the areolæ clear and empty, and displaying the fibrous network unaccompanied by the other tissues. These appearances are represented in the woodcuts below, although it is extremely difficult to delineate this very delicate tissue without some degree of exaggeration.

When the section is made through the striæ of the cortical substance, a divided blood-vessel is occasionally visible, and, whether filled with blood or not, is known by the large amount of fibrous tissue which enters into its walls. In the pyramids a similar structure is seen when they are cut

across the axis of the tubes; but, as might be expected, the fibrous tissue is much more abundant, from the greater number of large vessels included in the section.

When the capillaries are distended by natural or artificial injection, they are seen to form a close network round the tubes, running in the midst of the intertubular areolar texture above mentioned, and almost filling up the intertubular spaces. Indeed, so much of these spaces do they

Fig. 5.

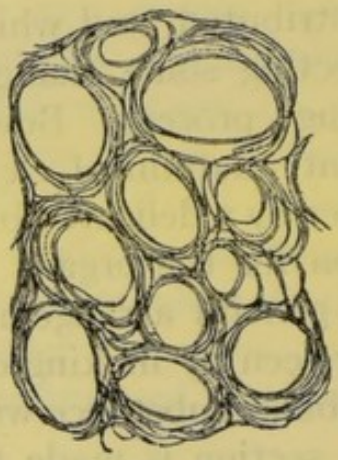

Fig. 6.

FIG. 5. Section of the cortical substance (by Valentin's knife), treated with acetic acid, showing the tubes and a Malpighian body, with the intervening delicate areolar tissue. The nuclei are seen scattered over the field, being brought out in strong relief by the acetic acid. Magnified 180 diameters.

FIG. 6. Portion of a similar section, from which the tubes have been squeezed out. The areolæ are seen empty. Magnified 180 diameters.

occupy, that I think it not improbable, that the whole, or nearly so, of the delicate fibrous tissue alluded to, is made up of the walls of the capillary plexus of vessels. Mr. Goodsir and Mr. Bowman, however, seem to consider it as an independent structure, the former regarding it as analogous to the capsule of Glisson in the liver.

As to the parenchymal cells of Mr. Toynbee, I have not been able to observe them either in the healthy or diseased states of the kidney, and must hesitate about admitting their existence, especially as they are not described with such minuteness as to enable us to distinguish them from the epithelium of the tubes.

Part I.—On the Pathological Anatomy of the Kidney.

In the following observations, I have endeavoured to keep constantly in view the connexion between the pathological elements and the normal structures; and also to reconcile, in as far as possible, the microscopic appearances with those visible by the unaided eye. In describing the latter, I have availed myself extensively of the works of Bright and Rayer; and the reader, to whom those works are accessible, will find in the present memoir numerous references to their excellent plates, which will serve to prevent misapprehension as to the appearances referred to.

I. EXUDATION.

The exudations from the blood-vessels of the kidney, being at once the simplest and the most common of its structural changes, and being connected, either as cause or effect, with most of the other more complex conditions, require to be considered first of all.

Exudations into the substance of the kidney give rise to a great variety of external appearances. These have of late years, especially since the observations of Dr. Bright upon their connexion with some of the most severe and fatal diseases, been objects of much interest to the pathologist; and have been represented and described in the works of Bright and Rayer, with a completeness and accuracy which has scarcely a parallel in the anatomy of any other organ in the body. Nevertheless, the cultivators of a more minute pathological anatomy have found the lesions of the kidney involved in greater mystery and confusion than those of any other organ whose structure and functions are equally well understood; and although some of the best modern observers have endeavoured to bridge over the chasm between our physiological and pathological knowledge, much remains still to be done.

Exudation may take place from the blood-vessels into all

the tissues of the kidney. Its most common seat is the interior of the tubes; but it also occurs frequently within and around the Malpighian bodies, and in the intertubular tissue, the tubes being quite clear. I have also seen it infiltrated through all the tissues in the form of a homogeneous mass, which contained within it the whole of the anatomical elements of the kidney.

1. *Exudation within the Tubes.*—The process of secretion in the kidney being in fact a normal process of exudation from the blood-vessels into the tubes, and one which, from its extreme complexity, is liable to very frequent derangement, it is not surprising that, of all the lesions of the kidney, the presence of foreign matters in the tubes should be the most frequent. The greater part of such exudations, however, are either soluble in the urine, or readily carried away by it, and only come under the notice of the pathological inquirer in connexion with the alterations in that fluid. Of this kind are the albumen in Bright's disease, and the sugar in diabetes, besides a number of soluble substances, which, although connected with morbid processes in the kidney or elsewhere, find a ready exit from the system, and do not remain to block up the tissue of the organ.

When, from any cause, the secreting cells of the kidney receive from the blood substances which do not remain in solution, obstruction of the tubes very readily takes place; much more readily indeed than in most other glands, owing to the remarkably tortuous course and narrow caliber of these tubes, and their complete occupation by the normal epithelium. If the abnormal character of the secretion continues, the obstruction is progressively increased; while, at the same time, there accumulates within the obstructed tubes a quantity of insoluble exudation, which modifies the appearance of the gland, and interferes materially with its function.

The appearance of the kidney, as altered by the presence of exudation in the tubes, is subject to variations depending on the amount of engorgement, and its partial or general character. I have already stated that the almost invari-

able effect of the repletion of the tubes throughout the kidney, is a corresponding diminution in the fulness of the vessels of the cortical substance; particularly of the Malpighian vessels and the capillaries surrounding the tubes. Indeed, when the accumulation is considerable, the Malpighian vessels are rarely to be traced with the naked eye, as they generally are in a healthy kidney. I have shown above the anatomical reason of this phenomenon.[1]

However characteristic of morbid exudations into the tubes, this paleness of the kidney on section is by no means peculiar to such diseased states. It sometimes occurs as a consequence of general anæmia, and much more frequently in organs turgid from retained secretion, arising from accidental causes. Such kidneys are also frequently very soft and easily torn, often œdematous, and present a remarkable similarity in their general appearance to some of the earlier stages of exudations. In these cases the microscope is of the greatest service in enabling us to form a positive opinion; and I have so frequently been enabled to correct my own first impressions, as well as those of others, by this means, that I have ceased to repose confidence in the judgment of the unaided eye on kidneys of this description.

[1] I take this opportunity of stating, that on this point I am compelled to differ decidedly from the views of Dr. Johnson of London. It appears to me, that he has fallen into an error both of observation and theory, in ascribing the albuminous urine of Bright's disease to secondary congestion or rupture of the Malpighian bodies, caused by the distension of the tubes from accumulated fat. Not to insist further on the anatomical argument, his view is opposed by every one of Bright's own plates, which show the cortical substance uniformly pale and bloodless, with the exception of Plate V., which would probably not be admitted by Dr. Johnson to be a case of Bright's disease at all, inasmuch as it presents none of the appearances of a fatty kidney. In Rayer's work, also, every plate representing the section of a granular kidney (See Plate VIII. Figs. 3 and 5—Plate IX. Fig. 8) presents a pale uninjected cortical substance; and this concurs with the description of Rayer, Bright, Rokitansky, Christison, etc. It will be seen hereafter, that my views of the relation of albuminous urine to the fatty and other degenerations of the kidney, are different from above.

The consistence of kidneys containing exudation in the tubes is very various, depending chiefly on the amount and character of the morbid deposit. The colour also varies considerably according to the kind of exudation. When this is very white and opaque, it presents itself in marked contrast to the intervening tissues, giving to the kidney on section a minutely and irregularly speckled appearance, which extends through the cortical substance, and sometimes affects also the tubular cones. It is also seen very distinctly in the intervals of the superficial venous polygons, when these have not been obliterated by pressure. This form is admirably seen in some cases where the exudation consists of salts deposited from the urine. On the other hand, when the exudation approaches nearly in colour to the kidney itself, it is frequently distinguished with great difficulty, the organ presenting a uniform paleness, without any further apparent change.

The volume and weight of kidneys containing exudation in the tubes, are frequently much increased; but this circumstance is so much under the influence of accidentally coexisting diseased conditions, that I prefer to leave it to be treated of afterwards in a separate section.

The above remarks indicate the appearances produced by exudation uniformly diffused through the tubes of the kidney; but this, though common in the slighter forms of the affection, seldom persists when the abnormal deposit has become such as to crowd any portion of the organ. It then tends to accumulate in certain sets of the convolutions in which the urinary current is least active. These, becoming partially blocked up, and ceasing entirely to secrete, are thrown aside from the general outward current of secretion, and become a centre of attraction for further deposit, just as the eddies and still water at the sides of a rapid stream receive from it the foam and floating bodies brought down from above. In this way more and more of the adjacent loops of tubuli are filled with the abnormal deposit, and become added to the former nucleus, until the masses of exudation, thus imprisoned within tubules through which

no secretion passes, form irregularly rounded bodies in the cortical substance, visible to the naked eye, more or less prominent on the surface of the organ, and usually of an opaque yellowish colour. These are the granulations first described by Dr. Bright, and figured in his 1st and 3rd plates, and in Rayer's work (Plate VIII. Figs. 1, 2, 5, 6, and Plate IX. Figs. 1, 5, 8). The admirable descriptions of these bodies by the last-mentioned pathologist are now well known in this country, and supersede the necessity of further detail in this place.

In 1842, Mr. Goodsir described the granulations of Bright as formed by the accumulation of secreted matter within the tubes. In Germany, Gluge,[1] Hecht,[2] Eichholtz,[3] and other observers, have given descriptions of the structural relations and composition of the granulations, which, though differing in detail, concur in representing them as formed within the tubes; and in the excellent work of Lebert on microscopic pathology, a description of them will be found, which, although short, is not surpassed by any of the others in accuracy. In this country several recent observers have taken up the same view. In a paper by Mr. Toynbee in the *Medico-Chirurgical Transactions*, vol. xxix., there are excellent plates of the anastomoses and convolutions of the tubes, and the granulations in Bright's disease, which afford valuable aid in the understanding of this subject.

The peculiar seat of the renal granulations is the cortical substance; the flow of urine through the pyramids being too constant to permit of the accumulation there of exudation in large quantity. The tendency to form granulations is generally first displayed in the neighbourhood of the surface, and also in the deep-lying convolutions between the pyramids; in both of which situations the tubes are remote from their orifices, and the pressure from behind is consequently small.

It may easily be understood, that the tubes involved in a granulation are in general permanently lost to the kidney

[1] *Atlas der Pathologischen Anatomie.* [2] *Op. cit.*
[3] Müller's *Archiv*, 1845.

as secreting structures ; for, having ceased to perform their function, and the stream of secretion having been diverted into new channels, the re-establishment of the former ones is in the majority of cases impossible, and the useless granulations become absorbed and obliterated. The mode in which this occurs will be hereafter described.

The special characters of intratubular exudations next fall to be considered. Excluding tubercular and cancerous deposits, which are rare, and in regard to which I have no new observations to offer, these may be considered under three heads, viz. (*a*) Crystalline or saline matters deposited from the urine ; (*b*) Oleo-albuminous or granular exudations from the blood-plasma ; (*c*) Exudations forming pus.

(*a*) *Exudations consisting of Crystalline or Saline matters deposited from the Urine after secretion.*—I have already alluded to the fact, that the urate of ammonia, which so frequently occurs as a sediment in urine out of the body, is no less frequently deposited from the urine contained within the tubes of the kidney. This occurs in most cases simply as a *post-mortem* appearance, consequent upon the cooling of the body ; and, when it is small in amount, it is only appreciable by the microscope. Occasionally, however, it is found in such quantity as to present to the naked eye the appearance of a distinct deposit. In such cases the cortical substance, which, when otherwise healthy, generally retains its normal vascularity, appears occupied by a white or yellowish-white opaque deposit, which presents itself also in a very marked form between the vascular striæ of the pyramids, particularly in the half nearest the cortical substance, where the vessels are more abundant than towards the apex. Such a deposit, which in reality is consistent with a perfectly healthy state of the organ, might easily be mistaken for a diseased condition.

The distinguishing character of this deposit is its ready solubility in dilute acids, such as the acetic or nitric. Under the microscope it presents the appearance, when within the tubes, of a fine molecular shading, which entirely obscures the nuclei. That part of it which floats free on the field

of the microscope, may be observed to be composed of fine molecules and granules, which, when large enough to have a defined edge, may be observed to be amorphous or angular, sometimes approaching the circular form, but never accurately rounded. Sometimes these granules cohere together in the form of opaque masses, dark by transmitted light, and of irregular form. The addition of a drop of acetic acid produces instantaneous clearness, unless the deposit be very abundant, in which case more must be added.

In a case in the *Medico-Chirurgical Transactions of London*, vol. xxix. p. 272, in which one kidney was deficient and another very extensively diseased, Mr. Busk describes a deposit in the tubes of "a semi-opaque white granular material, soluble or rendered transparent by acetic acid, and presenting none of the characters of oil." In this respect, and also in the small opaque specks and white striæ described as existing in the cortical and pyramidal substances of the kidney, the case has many points of resemblance to those which I have given above; and, in as far as appears from the description, I should incline to consider this a deposit of the same kind, occurring in a kidney otherwise diseased and atrophied. Mr. Busk seems to have considered it albuminous in its nature; but albuminous deposits, when amorphous or granular, are not generally found to present the ready solubility in acetic acid which is described in this case, and is always found with urate of ammonia.

Although the deposit of urate of ammonia in the tubes is of little or no pathological importance in the majority of cases, yet it occurs so frequently, and presents an appearance so much like other deposits to the unaided eye, and in some cases readily mistaken even in a microscopic examination, that I am satisfied it must occasionally have been a source of erroneous impressions to pathologists. At least I am conscious, in my own case, that I must have been frequently misled, before I was aware of the importance of applying the test of acetic acid to every deposit occurring in the kidney. This test is particularly necessary, when, as

not unfrequently happens, the urate of ammonia deposit is mixed with a certain quantity of fatty granules ; in which case, an idea of the relative amount of the two forms of exudation can only be obtained by the removal of one of them in the way described.

Crystalline deposits within the tubes are of much greater rarity than the amorphous urate of ammonia. I have repeatedly seen in the tubes of diseased kidneys, small groups of perfectly circular bodies, with a clear distinct edge, of a yellowish colour, and varying in size from 1-200th to 1-80th of a millimetre. As they did not present the

Fig. 7.

Fig 8.

Fig. 9.

FIG. 7. Some of the white semi-fluid matter from the cavity in the kidney. It is seen to be composed of molecules and granules, interspersed with free nuclei, the debris of the epithelium cells. Some of the granules are aggregated into masses of irregular form. The perfectly spherical granules are composed of fatty exudation, which is present in limited quantity. The nuclei are slightly and uniformly shaded in their interior. (250 diameters.)

FIG. 8. Fluid scraped from the surface of the cortical substance in the same kidney. It differs from the last in containing entire epithelium cells, which are obscured and filled with granules and amorphous exudation. Part of this, as in Fig. 7, is composed of perfectly spherical fatty granules.

FIG. 9. The same. The urate of ammonia has been removed by acetic acid, leaving only the fatty granules and epithelium cells.

peculiar glistening refraction which distinguishes fatty granules and globules, I was disposed to consider them as crystals, more especially as they were very similar to those described by Dr. Golding Bird (*Urinary Deposits*, Fig. 8, p. 72) as crystalline urate of ammonia. I have lately seen reason to alter this opinion, having found them to resist the action of acetic acid, and to present characters by no means compatible with the supposition of their crystalline nature. I have now ascertained, from observing their generation in urine, that they are in all probability formed out of the body as a product of decomposition ; but as my observations have not yet led to any precise knowledge of

the mode or circumstance of their development, I shall for the present do no more than record their occurrence.

A less questionable form of crystalline deposit has been observed by Gluge in the kidney of a dog, whose bladder contained a sanguinolent urine. The kidney presented marked capillary injection, and was considered by Gluge as being inflamed. The tubes were in some places crowded with semi-transparent crystals of very irregular form (see *Atlas d. Path. Anat.* Lieferung 10, P. II. Figs. 5–7). Gluge has not stated the probable nature of these crystals.

FIG. 10. Irregularly crystalline deposit, probably of uric acid, in a tube from diseased kidney. Some of the crystals are seen loose. (250 diameters.)

(*b*) *Oleo-Albuminous Exudations from the Blood-plasma.*—I employ this term as including, in one extended series, the whole of those exudations recognised both by German and English pathologists as fatty in their nature (the fatty granules, globules, and corpuscles of authors), together with many of those which have been distinguished as more properly inflammatory, such as the inflammation globules, granular corpuscles, or exudation granules and corpuscles of different writers. The necessity of a classification founded on the elementary structure and chemical composition, rather than on the accidental structural varieties of such exudations, is every day becoming more apparent. On the one hand, the use of the term "fatty" by pathological anatomists, though in itself most descriptive and apposite, has frequently been the means of vitiating their conclusions, and even their descriptions, when employed, as it has very frequently been, with the preconceived view that it is applicable only to chronic changes. On the other hand, the observations contained in the present memoir will be found to add new links to the chain of evidence which has been accumulating for several years past, that the so-called "inflammation" or "exudation" corpuscles, masses, and granules, are by no means characteristic of

acute inflammatory processes. This conviction, which from the first induced many of the most eminent and well-informed pathological writers to withdraw the original term "inflammation globules" employed by Gluge, and to substitute for it those of granular cells, or exudation corpuscles (see the works of Henle, Vogel, and Hughes Bennett), has received a most complete confirmation from the valuable researches, lately published by Reinhardt,[1] on the nature of the granular corpuscle; in which it is proved by numerous and well-founded observations, that the corpuscle in question arises, in many instances, from the deposition of granules, consisting of fatty and protein elements, in the natural epithelium cells of different organs; and that its origin is not only in some cases independent of the inflammatory process, but frequently a purely physiological change, as in the *membrana granulosa* of the Graafian vesicle of the ovary. The occurrence of the granular corpuscles in almost every species of pathological product, is mentioned in the systematic works of Lebert and Vogel; and the readers of the *Monthly Journal* need scarcely be referred to the observations of Dr. Bennett on cancerous structures, for numerous proofs of their formation in connexion with this form of chronic disease.

The fatty nature of the granules occurring in inflammatory products is known to most histologists, and is easily proved by observing the reaction of ether. Vogel[2] states, that they are composed partly of fat and in part of protein, and carbonate or phosphate of lime (the mineral elements are, however, very variable in amount). The relations of the oil to the albumen or protein, in these and other structures, has been minutely studied by Ascherson and Hughes Bennett,[3] who have shown that an albuminous membrane surrounds the oil granules, and prevents them from coales-

[1] *Archiv für Phys. Pathologie*, by Virchow and Reinhardt, No. I. 1847; analysed in *Monthly Journal*, February, 1848,—Retrospect, p. 6.

[2] *Path. Anatomy*, Dr. Day's translation, p. 157.

[3] See the paper of the latter "On the Structural Relation of Oil and Albumen"; in the *Monthly Journal* for September, 1847.

cing, as they would otherwise necessarily do; and that the formation of emulsions, where oil exists in a minute state of division, depends on a similar arrangement.

I have myself had repeated opportunities of observing the formation of granular corpuscles in diseased structures. In pneumonic exudation, in which they can be traced very frequently from their earliest stages, I have invariably found them to be formed from epithelial cells, according to the law laid down by Reinhardt. I have likewise satisfied myself, by repeated observations, that in some of the forms of so-called pneumonia, the quantity and size of the fat globules and granules is such as to constitute a true fatty degeneration of the lung, in the same sense in which the term has hitherto been more familiarly applied to the corresponding lesions of the liver and kidney; and that these lesions present no structural difference from the more ordinary forms, except the greater number and the larger size of the globules which accumulate in the tissue. Finally, in the case of the lung these views have been fully borne out by chemical analysis. In a series of researches by Guillot (*Gazette Médicale*, No. XXIX. 1847), it is shown, that in all diseases of the lung giving rise to obstruction of its tissue by exudation, there is an increase in the relative quantity of fatty matter, which, in the adult healthy organ, is about six per cent., but in the diseased states (such as pneumonic or tubercular infiltration) rises frequently to fifteen, and sometimes to fifty per cent. No similar analysis has yet, so far as I know, been applied to the kidney; but considerations, deduced from histological observations, give the strongest reason to suppose that in this respect, as in others, an analogy would be found between the pathological conditions of the two organs.[1]

I have entered thus far into the general pathological anatomy of this form of exudation, with the view of recon-

[1] Rokitansky enumerates inflammatory exudations as among the circumstances under which fat is deposited pathologically.—*Handbuch d. Path. Anat., and British and Foreign Med. Chir. Review*, Jan. 7, 1848 p. 287.

ciling my own observations with those of previous writers on this subject ; as also to explain my adoption of a classification which annihilates, or, at least, very much modifies, distinctions which many conceive to have a pathological significance corresponding to their practical importance. However satisfactory it might be to point out the inflammatory and non-inflammatory lesions of the kidney as presenting strongly marked pathological distinctions, I am satisfied that such an attempt would fail, from not being founded in nature or truth ; and I am confirmed in this view, by the free admission, on the part of the most skilful pathological anatomists, of the extreme difficulty of making the distinction in question in the case of the kidney. Indeed, in considering the terms inflammatory and non-inflammatory as being more applicable to the modes of invasion of diseases of the kidney than to differences in their pathological anatomy, I am only following out the ideas of Rayer, who included most of them under one pathological name (Nephrite), and then distinguished them into acute and chronic.

The application of these principles to the explanation of various well-known diseases of the kidney, will be treated of in the sequel.

Oleo-albuminous exudations are distinguished by their being partially soluble in ether, which leaves an amorphous residue insoluble in cold mineral acids. The amount of this residue relatively to the whole mass, differs much in different cases, and indicates the relative amount of the protein element ; it is sometimes in minute quantity, but is never entirely absent. The form assumed by such exudations is that of granules or globules, which are perfectly spherical, and present a dark distinct edge. These spherical bodies vary in size, being sometimes exceedingly minute, at other times as large as 1-60th of a millimetre, or even much larger ; the variation is generally considerable in a single portion submitted to examination. Owing to their powerful refraction of light, they present a brilliant white centre and a dark circular rim, which is darkest towards the external edge.

The composition of these granules and globules has been already adverted to. The oil which forms their central portion is probably derived from the serolin, and the protein envelope from the fibrin or albumen of the blood-plasma. When the albuminous element is in large quantity relatively to the oil, the granules found are small, verging into the minutely molecular appearance; when the reverse of this is the case, they occur mostly in the form of large globules, of which the investing membrane is thin and readily ruptured by pressure. The prolonged action of acetic acid

Fig. 11.

Fig. 12.

FIG. 11. Fatty granules and globules as seen in fluid scraped from the cortical substance of a very pale and soft kidney (slightly granulated). The existence of a large number of free nuclei of epithelium cells, as at *b*, *b*, indicates the imperfect formation or disruption of these cells. Some of them are however, seen entire, as at *a*, and contain fatty granules in different proportion. In this exudation the oil is relatively in large, the albumin in small, quantity. (250 diameters.)

FIG. 12. Fluid scraped from cortical substance in kidney of patient. The entire epithelium cells (*aa*) are in much larger proportion than in the last case. Some of them are nearly clear, others filled in different proportions with granules, constituting the forms of cell described by authors as inflammation globules, exudation cells, fatty corpuscles, granular cells, etc., etc. Free nuclei (*b*) and free fat granules are also seen in considerable numbers. The proportion of oil is in this case also relatively large. (250 diameters.)

also frequently dissolves the membrane, and allows the contained fatty particles to coalesce. Caustic potass dissolves, after a time, both the albuminous and the oily element.

The mode in which the fatty granules or globules are disposed within the tubes of the kidney, next demands attention. The existence of fat in this situation was first distinctly recognised and described by Gluge as a diseased condition of the kidney, to which he gave the name of cirrhosis [1] or stearosis, with the view of distinguishing it from the states which he has described as inflammatory.

[1] Gluge uses the term cirrhosis, both in the liver and kidney, to denote the pale and yellow rather than the granulated state of the organs.

In Germany, although many authors have written upon this subject, none appears to have in any way added to Gluge's later researches in his *Atlas der pathologischen Anatomie*. In this country, the memoir of Dr. Johnson of London is the only one, I believe, yet published on this subject.

According to my observations, fatty exudations from the tubes present themselves under two different aspects: *First*, free molecules, granules, and globules, intermingled with the cells and nuclei of the secreting structures; *Second*, similar granules, etc., enclosed within the cell-wall, between it and the nucleus. In regard to the first of these forms it is not necessary to repeat what has been already said; but the second demands a few words of explanation.

Dr. Johnson is certainly the first who has stated distinctly, and kept constantly in view, the fact of the accumulation of fatty granules in the glandular epithelium of the kidney, having been guided in doing so by the facts previously stated by Bowman with respect to the liver. In regard to these facts there is no doubt; nor is there any doubt that their disregard by the continental writers on the

FIG. 13. Section of the cortical substance in a granulated kidney; the tubes are filled with granules and molecules of fatty or oleo-albuminous exudation. A Malpighian body is also occupied by exudation. The granules are mostly of small size. The tubes appear somewhat shrunk, the fibrous tissue having contracted around the deposit. (250 diameters.)

kidney, has introduced much needless complexity into their descriptions.

It never happens that the whole of the tubes and cells of a kidney are equally the seat of fatty deposit. Even in

the most diseased kidneys some healthy cells and tubes can generally be found; and, on the other hand, it is not unusual in kidneys apparently quite normal, to meet with a few cells containing fatty granules in greater or less

Fig. 14.

Fig. 15.

FIG. 14. Exudations consisting of fatty globules of considerable size, in tubes of same case as in Fig. 12. (250 diameters.)

FIG. 15. Scattered granules in tubes of the pyramidal substance, in a kidney in which many of these were obliterated; *a a*, cells from cortical substance of the same kidney; *b*, nuclei from do. (250 diameters.)

number. This is, however, a departure, though a trivial one, from the strictly normal state of the cell.

The general description of the appearances produced in the kidney by the occurrence of deposits in the tubes is applicable with peculiar force to the oleo-albuminous exudations, which are of all others the most persistent, and the most frequently disorganizing in their effects.

The progress of the granulations of Bright is connected with other changes of a different character, such as atrophy of the surrounding tubes, obliteration of vessels, etc.; and will therefore come under consideration in the other sections of this memoir. Enough has been said for the present to show their connexion with the fatty exudations in the tubules.

(*c*) *Exudations in the form of Pus.*—The occurrence in the cortical substance of deposits having all the external characters of pus, is not very uncommon. Their most usual form is that of small abscesses, rarely exceeding the size of a pea, and frequently much smaller, sometimes confluent, and irregularly disseminated through the cortical substance. They are generally surrounded by more or less deep vascular

redness; this, however, is limited to a narrow rim around the deposit; the remaining portions of cortical substance being either natural in appearance, or paler than usual. These appearances are well delineated in Plate II. Figs. 1 and 2, of Rayer's work.

The formation of abscesses having a distinct limiting membrane, or surrounded by condensed tissue, is, in the kidney, of extremely rare occurrence. I have already related a case where a cavity of this sort was found; but the appearances of the contained matters to the naked eye and under the microscope had no resemblance to those of pus.

Fig. 16.

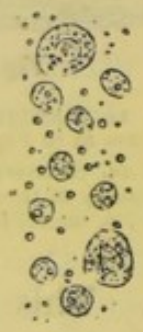

Fig. 17.

FIG. 16. Pus and fragment of tubule from small abscess in the kidney, 250 diameters. *a a*, Granular cells of the size and appearance of granular epithelium. *b b*, Smaller pus corpuscles, presenting a very granular structure (most of them have been represented too flat by the engraver). *d d*, Corpuscles like the preceding, but surrounded by a cell with clear contents and an exceedingly delicate wall. The tube is seen to be filled mostly with the bodies *b*.

FIG. 17. The corpuscles treated with acetic acid.

In the *Monthly Journal* for February 1848 (p. 589), Dr. Bennett has described and figured pus corpuscles, which appeared as granular nuclei, surrounded by a delicate and transparent cell-wall. These corpuscles he has found in abscesses of the lung and kidney, and also in grey hepatization of the lungs; and he thinks that at a later stage of their formation the cell-wall disappears, leaving the nucleus as the mature pus corpuscle. From having seen Dr. Bennett's previous demonstrations, and had my attention turned to the subject, I had no difficulty in recognising the cells as identical with those described by Dr. Bennett. But what appears to me worthy of attention in the present case is, 1st, the coexistence of these bodies with those

which are undoubtedly the granular epithelium cells described in a previous part of this memoir; 2nd, the existence both of these bodies and the smaller pus corpuscles *within* the tubules, where they appeared to take the place of normal nuclei and cells; 3rd, their existence in the lungs, in cavities formed by dilatations of the smaller bronchi: in short, their formation both in the lung and kidney in connexion with a mucous surface.

It is well known to microscopic observers, that the pus formed on the surface of mucous membranes seldom presents the clear and definite reaction with acetic acid characteristic of normal pus corpuscles. It has even been at different times supposed that the pus corpuscle is formed from the epithelium cell; and though this doctrine is undoubtedly attended with many difficulties, and has never been held by pathologists generally, it seems to be worthy of further investigation in cases like the present. The resemblance in size and form of the bodies to a granular epithelium nucleus, and to granulated and non-granulated epithelium cells, appears, especially when taken in connexion with their position *within* the tubules, and the complete absence of normal epithelium, to be somewhat more than an accidental circumstance.

The symptoms in this case were referrible to the abdominal and pulmonary lesions described; but they were attended from the first by a marked typhoid depression, and a continued languor and exhaustion, which justified a suspicion of idiopathic fever superadded to the local disease.

In several other cases which have occurred in the Royal Infirmary of purulent deposits in the kidneys, a similar typhoid state has existed; in all, however, there have been numerous other lesions, and sometimes abscesses in other parts of the body, indicating a general tendency towards the formation of pus. The blood in these cases presented no unusual appearance.

2. *Exudation within the Malpighian Bodies.*—The granular (oleo-albuminous) form of exudation above described as so

frequently occupying the tubes of the kidney, is also occasionally found within the capsules of the Malpighian bodies. When in large quantity in this situation, the tuft of vessels which normally fills the capsule is completely compressed and shrunk, in most cases invisible. Where the exudation is in smaller quantity, however, it frequently adheres to the interior of the capsule and the exterior of the tufts, without materially affecting their form.

Exudation in this situation is generally accompanied by similar exudation, in greater or less abundance, within the tubes. The anatomical relations of these parts, as now generally understood, would, indeed, entitle us to expect that the pathological conditions of the one should be shared by the other. While, however, many cases of this sort have occurred to me, I have met with a still larger number which confirm the statement of Dr. Johnson (*Med. Chirurg. Trans.* vol. xxix. p. 4), that the exudation within the tubes often occurs to a very great extent, without the Malpighian bodies being at all involved. More rarely a limited amount of deposit occurs within the latter, when there is comparatively little within the tubes. The cause of these differences is very obscure; nor does the examination of it appear to promise any results of importance, in the present extremely imperfect state of our knowledge as to the special functions of the Malpighian bodies.

3. *Exudation in the Intertubular Tissue.*—In cases where oleo-albuminous exudation is in small quantity, it frequently appears to be disposed without any distinct relation to the tubes; and, where it is in very large quantity in the tubes, it sometimes appears in the interstices of the areolæ, as is represented in Fig. 13. In kidneys which are the seat of firm opaque granulations, a section of these frequently presents a dark opaque mass, covering a large portion of the field of the microscope, and showing no trace of arrangement; the deposit must, therefore, either have broken up the structure entirely, or completely occupied every vacant place. In all these cases, however, it is extremely difficult

to determine by actual observation that the exudation is external to the tubes; and I am not a little disposed to doubt the occurrence of this condition, or at least to consider it as secondary to the complete occlusion of the tubes by exudation.

4. *Partial Distribution of the Oleo-albuminous Exudation.* (*Plaques Blanches de Lymphe Plastique,* Rayer.)—I have already described the formation of granulations as dependent on the accumulation of deposit in particular groups of tubules in the cortical substance. In such cases, however, the affection is probably at first general; they are very different from the form now to be described, in which the deposit is quite limited in extent, and isolated.

There are occasionally met with on removing the capsule from the surface of a kidney, irregular patches of a paler colour than the rest of the organ, sometimes a little elevated, sometimes depressed below the general surface. Their boundary is quite abrupt, and they are frequently surrounded by a well-marked rose-coloured areola, extending more or less into the surrounding substance. On making a section of these patches, they are found to penetrate into the cortical substance, and sometimes even a certain way into the pyramids. The vascular areola, when present, extends round them in every direction, and is found on examination to consist of highly injected Malpighian bodies and capillaries, with or without extravasation. The colour of the patches varies from yellowish-grey to a gamboge-yellow; their consistence is generally firm. On microscopic examination, they present a large amount of exudation, varying from the molecular to the large granular form. In some cases the tubes may be seen filled with exudation; in others, they appear to be in great part obliterated. In one case I found the Malpighian bodies quite free of exudation; they preserved their usual arrangement, and were readily discoverable by a simple lens on the surface of the section. The parts of the kidney not involved in the deposit, generally present no abnormal appearance.

Various illustrations of this species of deposit are to be found in Rayer's work. (See Pl. I. Fig. 6, Pl. V. Fig. 2. Pl. XXXIV. Figs. 2, 6.) He has figured it in various stages and under different names, as *Néphrite simple* and *rhumatismale*, and *Hémorrhagie*. Its origin and progress are very obscure, and it has not been satisfactorily connected, either with other morbid states, or with any peculiar symptoms. I have seen it in connexion with fever, with puerperal convulsions, with erysipelas and dementia, and in several cases where no account of the symptoms could be procured. Dr. Bennett possesses a most remarkable preparation and drawing of a case in which such deposits were most extensively present, and left very little intervening sound tissue. The affected kidney had a most singularly variegated appearance.

II. LESIONS AFFECTING CHIEFLY THE VASCULAR SYSTEM OF THE KIDNEY.

In passing to the consideration of the morbid changes which occur in the vascular system of the kidney, the conditions of sanguineous congestion and extravasation on the one hand, and anæmia on the other, would fall to be described first in order. But the simple hyperæmic and anæmic states of the organ have been noticed so fully in the anatomical introduction, that little more remains to be said on this subject.

Congestion followed by permanent obliteration of the Capillaries of the Cortical Substance.—Under this head I have to describe a form of lesion in the kidneys which, although certainly of less frequent occurrence than those characterised by exudation, is of a pathological and practical interest in no way inferior to any other.

The appearances most characteristic to the naked eye of this form of lesion, are those so admirably figured and described by Rayer as the second form of his "néphrite albumineuse." The kidneys are generally increased in size,

sometimes very remarkably so. Their consistence varies; they are sometimes more flaccid than in the natural condition, but always preserve considerable tenacity. The surface is either quite smooth, or more or less depressed and furrowed. The venous vascularity assumes to a considerable extent the stellate form; the polygons are mostly absent; and the extreme irregularity and abruptness of distribution of the superficial veins gives to the surface a variegated or "marbled" appearance, which is quite characteristic of this stage of the affection. (See Rayer, Plate VI. Figs. 2, 3, 5; Bright, Plate II. Fig. 1.) Occasionally, also, amid this unequal injection there are to be found scattered petechiæ, indicating recent extravasations of blood into the tubes. On section the cortical substance has considerable volume, and presents a smooth, glistening, almost semi-transparent appearance, which cannot be better distinguished than by the term *waxy*. It may partake in a slighter degree of the variegated character of the surface; more commonly it is of uniform appearance, and of a yellowish or fawn-colour, sometimes verging into a pale flesh tint. The vascular striæ of the cortical substance are generally to be traced by a more or less distinct injection, and a few injected Malpighian bodies, or petechiæ of extravasation, are sometimes dispersed through the section. (See Rayer, Plate X. Fig. 3.) In other cases a little further advanced, both the striæ and the Malpighian bodies are nearly destitute of blood. (Rayer, Plate X. Fig. 1; Bright, Plate II. Fig. 1.) The pyramids frequently retain their normal vascularity; sometimes, however, they are of a pale colour, and their bases are indistinctly marked,—a condition which indicates the progress towards a further disorganization.

When a kidney in this condition is carefully and minutely injected, the greater proportion of the cortical substance remains impervious; the injection, however, can frequently be made to penetrate as far as the cortical striæ, and even to some of the Malpighian bodies. (See Rayer, Plate X. Fig. 2; Bright, Plate II. Fig. 3.)

From these circumstances it is obvious that the lesion above described consists in an obliteration or obstruction of the capillary system of vessels throughout the organ, and a partial obliteration of the veins on its surface. There is also every probability that this condition is secondary to one in which there is a high degree of congestion of the organ. The extravasations, the occasionally injected Malpighian bodies, and the highly injected, though partially distributed, stellar veins, leave no doubt that the state of congestion described as the first form of albuminous nephritis by Rayer, is really the antecedent of the present or second form.

To any one who is familiar with the *marbled* and *waxy* kidney here described, there can be no difficulty in recognising a further stage of the same lesion, in which the organ is perfectly pale both on the surface and on section, with the exception, perhaps, of a very few stellated superficial veins. The kidney in this stage (the transition to which seems to be represented in Rayer, Plate VI. Fig. 4) is still heavy and voluminous ; it acquires additional firmness and elasticity, and assumes much of the general appearance of a true non-vascular texture. It varies from a light yellow to a fawn-colour, which extends to the pyramids, the bases of which become still more confused and intermingled with the cortical substance than in the marbled kidney. The capsule is frequently more firmly adherent to the external surface than in health.

From the pale and yellow appearance of the kidney in this stage, it is very apt to be mistaken, even by a practised eye, for an extreme degree of the fatty degeneration. A well-marked example, indeed, will hardly give rise to this error, if attention be directed to the degree of firmness of the organ, the peculiar lustrous character of the cut surface, and the entire absence of the opaque granulations of Bright, or of that dull tint which distinguishes the excessive degrees of the fatty disease. The appreciation of these characters is, however, more difficult where, as sometimes happens, exudation is also present ; and the distinction which has

escaped the acute observation of M. Rayer, has undoubtedly been overlooked by many other observers.[1]

The microscopic characters of this lesion are chiefly negative. There is not unfrequently an entire absence of exudation; indeed, in the most marked cases of the lesion, I have seldom found even the slightest trace of any abnormal deposit. Occasionally, however, there is a very minute quantity of fatty exudation in the tubes, generally in very small granules, and scattered throughout the organ. The tubes are either natural, or in the advanced stages pass into some of the states hereafter to be described. The capillary vessels surrounding the tubes are not visible, and in their place there is fibrous tissue, which in this form of lesion always appears somewhat exaggerated. The Malpighian bodies are also frequently seen in process of obliteration, and surrounded by dense capsules of fibrous tissue. The epithelium is frequently altered in character, but its changes follow no fixed rule.

The absence or scantiness of exudation, taken in connexion with the extent of degeneration appreciable by the naked eye, are amply sufficient characters to distinguish this lesion from the extreme stages of the fatty disease. . . .

In this kidney, we have the waxy degeneration accompanied by a certain degree of exudation, although not to such an extent as to give any special character to the lesion. The petechiæ are interesting in connexion with the scorbutic diathesis which existed; nevertheless, the state of the superficial veins, and the injected state of some of the Malpighian bodies, along with the fact of extravasation into the tubes, appear to indicate, as in the former case, a recent condition of vascular excitement. Whether the exudation occurred as a consequence of this condition, or of the general tendency to fatty degeneration, as shown in the liver and heart, is open to question. The former view appears, from

[1] Plate VI. Fig. 4 of Rayer's work, is probably an example of the waxy pale kidney; Plate VII. Figs. 2, 3, 4, of the fatty disease. The distinction is sufficiently evident even in the plate. All of these are referred by Rayer to the third form of "Néphrite Albumineuse."

the character and partial distribution of the exudation, to be not improbable.

Connexion of Congestion with the Process of Exudation.—I have already stated, that in general the degree of fulness of the vessels stands in an inverse ratio to that of the tubes; and that accordingly an amount of exudation, so great as to produce distension of the tubes, generally produces a corresponding depletion of the vascular system. I have likewise shown, by reference to the works of Rayer and Bright, as well as by original observations, that paleness of the cortical substance is one of the most frequent characters of kidneys containing exudation.

A moderate or small amount of exudation, however, sometimes occurs in organs which present considerable vascular injection, and sometimes even a distinctly hyperæmic condition. Such cases occur not unfrequently in the latter stages of heart disease. In only one or two cases have I found the vascularity much greater than usual; in these the kidneys were of large size (weighing from six to eight oz.), and contained very little exudation. In one instance they presented on the surface the petechiæ indicative of extravasation into the tubes (as in Bright's fifth Plate); in this case there had been albuminous urine and dropsy during life, with obstinate hæmaturia during two months before death; there was also intense dysentery and disease of the liver. The kidneys were otherwise diseased; but the exudation was in small quantity, and in very minute granules. In another case (a woman who died of convulsions in the eighth month of pregnancy, and in whom an apoplectic clot was found in brain), the kidneys weighed four and four and a half oz.; they were firm in texture, and of a tawny orange colour, pretty generally diffused. This was found to proceed from the presence of blood in the tubes, intermixed with small fatty granules, which were in considerable abundance. There was, however, little capillary injection, and the Malpighian bodies were mostly bloodless. The urine in this case had not been examined.

The analogy between the results of exudation in the kidney, and those in the lung and other organs, would naturally lead to the idea, that, as in the red hepatization of the lung, the deposition of granular exudation is preceded and accompanied by vascular turgescence, so in the kidney there may be a form of the exudative process in which active congestion of the organ plays a part. Almost all pathologists since the time of Dr. Bright, who have written on the diseases of the kidney, have dwelt more or less strongly on the forms of renal disease, accompanied by acute symptoms and a congested state of the organ, as being the precursors, in some instances, of the more ordinary changes, in which no increased vascularity occurs; and Bright himself has indicated "an inflammatory state of the kidney" as a probable cause of the structural changes which he described and classified. (See Bright's *Reports*, vol. i. p. 3.) Christison and Rayer concur in describing a state of congestion as the commencement of many of the chronic changes, and have established the coincidence of such a state, in some instances, with symptoms of acute dropsy and albuminuria. Even supposing the opinions of these authors as to the connexion between the hyperæmic and anæmic changes to be, as they probably are, founded partly on theoretical views as well as on direct observation, they must be admitted to be of great weight in a question which requires for its solution so large a field of experience.

From the comparatively small number of cases of acute disease of the kidney, and more especially of acute dropsy, which have occurred in the Edinburgh Royal Infirmary since my attention has been turned to this subject, I am unable to add any observations bearing more directly upon this question than those to which I have already alluded. Whether, in the two cases above mentioned, the evidences of vascular excitement, and the exudation in the tubes, can be considered as anything more than accidental concomitant circumstances, I am not prepared to say without further opportunities of investigation; but if these cases can be considered as examples of a state of the kidney analogous

to red hepatization of the pulmonary texture, I am satisfied that this state is of much rarer occurrence, or at least more rarely fatal, in the former organ than in the latter.

I have alluded incidentally to Dr. Johnson's views on this subject. He considers the deposit of fatty granules in the kidney as being invariably a chronic process, never preceded, in any case, by congestion or extravasation. On the other hand, admitting that these states are frequently found in connexion with fatty exudation, he considers them as secondary results of the distension of the tubuli uriniferi. (*Med. Chirurg. Transactions*, vol. xxix. pp. 4, 8, 9.) I have already shown that this view is opposed both by anatomical considerations and by pathological facts; and, in particular, that the confirmed fatty degeneration of the kidney is usually accompanied by nearly complete depletion of the vessels. The comparative rarity of congestion in connexion with fatty exudation, under all circumstances, and its greater frequency while the exudation is still small in amount, sufficiently show the inadequacy of Dr. Johnson's hypothesis to explain the phenomenon. The assertion of the exclusively chronic nature of the deposit in the kidney is probably founded on an imperfect theoretical view of the nature and origin of fatty exudations in general.

On the whole, the supposition which appears to harmonize best with the analogies of other organs, and also with what has been hitherto observed in the kidney, is, that the oleo-albuminous or fatty exudation is sometimes preceded and accompanied by a congestive stage of short duration, in the course of which extravasation of blood into the tubes may occur. When, however, the exudation has accumulated within the tubes to such an extent as to cause fluid pressure by obstruction, the vascular system of the organ is emptied of its blood in a degree proportionate to the amount of distension; and, as the exudation continues to increase, the stage of congestion is rapidly superseded by the development of the pale yellowish colour so frequently mentioned in connexion with the fatty degeneration. This view appears to be strongly supported by the cases formerly

mentioned, in which the oleo-albuminous exudation occurs in scattered whitish patches, surrounded by a distinct vascular rim. On examining microscopically a section of such patches, I have observed the line of demarcation between the congested and the depleted Malpighian bodies to correspond accurately with the boundary of the exudation, so as to render it probable that the congestion, originally present throughout the diseased portion, had been superseded by the presence of the abnormal deposit.

Whether any connexion exists between the development of the congestive form of exudation and the presence of acute symptoms, is a question for further clinical experience, united with careful pathological investigation, to determine. As it is evident that the earlier stages of disease in the kidney have been hitherto to a great extent overlooked, both at the bedside and in the dissecting-room, it is highly probable that many of those affections which have been considered as most obviously chronic in their nature, may in reality be the advanced stages of processes more or less acute, which have not been fatal in the first instance, or which, if fatal, have not presented lesions appreciable by the unaided eye.

III. LESIONS OF THE TUBES AND EPITHELIUM.

Some of these lesions have been already fully described under the head of exudation (I.) ; but there remain others which are not less important in themselves than those formerly alluded to, and which are very frequently found in connexion with them.

Imperfect Development of the Epithelium Cells and Nuclei.— The natural condition of the epithelium cells has been fully described in the anatomical introduction to this memoir. The size of these cells, and the thickness of the cell wall, has been stated to vary within certain limits in organs apparently healthy. The size of the nuclei is less variable than that of the cells ; but in all kidneys, whether healthy or diseased, the nuclei which are most closely adherent to the basement

membrane are less perfectly circular, and of considerably smaller size, than the majority of those lining the tubes, and surrounded by complete cells.

Notwithstanding these differences in the normal condition, the physical characters of the epithelium are capable of affording important information as to the diseases of the kidney. In very many pathological conditions of the organ, the nuclei occur in various places almost wholly devoid of cell walls. They may be more abundant or more scanty than usual; and often appear in great profusion, huddled together in confused masses, and mixed with shreds of membrane and amorphous molecular matter, not soluble in acetic acid. This appearance of debris, which no doubt results from disintegration of the cell walls, most frequently occurs in kidneys which are abnormally soft and large, and from the cut surface of which an unusually large amount of turbid whitish juice may be scraped. It is usually impossible to obtain a satisfactory microscopic section; the cohesion and elasticity of the organ being so much impaired as to present no resistance to the pressure of the glasses. Such softened and altered kidneys occur frequently in fever and other diseases, and have appeared to me frequently to occur with deficiency in amount, or alterations in character, of the urinary secretion. I have not been able to arrive at any very definite conclusion, as to how far the disintegration in question may be the result of post-mortem change. However this may be, it is clearly abnormal, and ought always to suggest careful examination, as it is undoubtedly often connected with other morbid characters.

A more unequivocal pathological change (often occurring along with the above) is the small size and altered form of the nuclei throughout the organ. I have frequently observed the majority of the nuclei to be not more than half the usual size (some of them being even less); in this case they have always been destitute of cell walls, and have presented a more or less oval or slightly angular form. Sometimes they float scattered and solitary in the field of the microscope;

at other times they appear aggregated together, either by two and three, or in much greater numbers. When a few of these aggregated nuclei are observed, it can usually be seen that they are not actually in contact, but are inclosed in a very delicate and transparent filmy substance, which is readily twisted about in all directions by currents in the fluid, but which, nevertheless, has sufficient tenacity to prevent the nuclei from being torn asunder. Occasionally, in the midst of this connecting substance, obscure marks of cell walls can be observed around the nuclei ; and, from repeated observation of these varieties, I am convinced that this transparent and homogeneous film is nothing else than the nascent or undeveloped cell membrane, which has separated from the basement membrane along with the half-developed or young nuclei above detailed. These aggregations of young nuclei are sometimes mingled with the amorphous debris of effete epithelium, or with granules and molecules of oleo-albuminous exudation, or of lithate of ammonia, which communicate to them a dark and confused appearance. Not unfrequently also these masses, when freed from the tubes, retain more or less of their form, and present so exactly the appearance of the casts of the tubuli seen by Franz Simon, and many other observers, in the urine, as to leave no doubt of their identity with these bodies.

Desquamation of the Epithelium.—The changes above described are generally accompanied by an extremely rapid generation of nuclei, which are separated from the basement membrane in an imperfect state, and carried away along with the urine. I shall not at present enter into the subject of the changes in the urine, further than to say, that the appearance in that fluid of the immature nuclei and cells, as well as the aggregations above mentioned, forms one of the most sure and undoubted signs of a diseased condition of the urinary tubules. There is reason also to think that disease may be detected in this way, long before the kidney has undergone disorganization at all evident either to the unaided eye or to the microscope.

The analogy of the anatomical changes now described, with those which occur in diseased mucous membranes in general, deserves to be adverted to in this place. Henle was the first to show satisfactorily that the essential phenomenon of mucous catarrhs and inflammations is usually the increased formation of epithelium cells in various stages of growth, and their separation along with an increased quantity of fluid secretion. The same fact has also been clearly elicited by Lebert, in his examination into the microscopic character of expectoration. (*Physiologie Pathologique*, vol. i.) The analogy of the phenomena in these cases, with those presented by urine in the various disorders of the kidney, in which desquamation takes place from the tubuli, cannot fail to be appreciated, when it is remembered that the basement membrane of the tubuli is essentially a mucous structure, differing only from mucous membranes in general by its anatomical arrangement, and by its containing none of the accidental or non-essential parts of mucous tissue.

The desquamation of the epithelium of the tubuli uriniferi, has been recognised by several continental pathologists as an important characteristic of renal disease. Vogel (*Icones Histologiæ Pathologicæ*, p. 108), in particular, has furnished important observations on this subject; and several other authors might be cited as having alluded to the different forms and sequelæ of this morbid process.

In the memoirs of Dr. Johnson and Mr. Simon, published simultaneously in the London *Medico-Chirurgical Transactions* for last year, the desquamation of the epithelium and its anatomical results are described, from independent observations, as characteristic of the inflammatory affections of the kidney, and as distinguishing these from the chronic *fatty* degeneration of the organ. According to Mr. Simon, the latter is the exclusive result of scrofulous disease; while the desquamative disease is the consequence, in general, of a rheumatic or other *inflammatory* diathesis. Dr. Johnson describes the desquamation as occurring both in an acute and chronic form, to which he gives the names of *acute* and

chronic desquamative nephritis; and these are held to be true inflammatory affections, giving rise to organic changes of a peculiar kind. In the description of these changes, and of their relations to the different forms of renal degeneration described by former pathologists, there are considerable differences between the memoirs of Dr. Johnson and Mr. Simon, some of which will be presently adverted to.

In detailing the results of my observations on the anatomical changes in the kidney, I have avoided as much as possible all speculations as to the pathological causes or symptomatic phenomena of the lesions described. This branch of the subject I hope to take up at a future period; in the meantime, however, it is necessary, to prevent misconception, that I should repeat as regards the alleged scrofulous and rheumatic forms of renal degeneration, what I have formerly said in reference to the inflammatory and non-inflammatory,—that the application of such names to the anatomical changes in diseased kidneys is subject to this objection, that none of the lesions which I am engaged in describing can be correctly considered as having an exclusive connexion with any specific pathological cause. In particular, I believe that there is no disorder of the system in general, or of the kidney in particular, with which the desquamative process is not liable to be connected, and that its connexion with the oleo-albuminous exudation is the most frequent of all.

I have therefore thought it necessary to exclude all names having reference to such vague pathological theories; believing that the lesions of an organ must be made the subject of purely anatomical consideration before its pathology can be rightly apprehended.

In some cases of desquamation of the epithelium, it is scarcely possible to recognise any departure from the usual condition of the kidney, either with or without the assistance of the microscope. The degree of vascularity is very various in different specimens, and the epithelium thrown off is so quickly resupplied, that there is no very observable change in the microscopic condition of the tubules. In

one very intense case, in which ten pounds of very watery urine, loaded with an epithelial sediment, were passed daily for some weeks before death, the kidneys were small, flaccid, and bloodless; many of the tubes were quite full of nuclei heaped closely together; some of the nuclei were undersized; the cells, when entire, were much compressed and angular. (See Fig. 19.) In another instance, where urine was passed in large quantity and full of epithelial debris, during the last two months of life, the kidneys were found in an opposite condition, viz. large and congested, and with a firmness and smoothness of section like the

Fig. 18.

Fig. 19.

FIG. 18. A portion of a tubulus uriniferus normally filled with cells. It presents an obviously cylindrical form, and the nuclei are disposed on the internal surface so as to leave considerable intervening spaces. The cells and nuclei from such a tube are unaltered in form. (250 diameters.)

FIG. 19. A tube crowded with nuclei and compressed cells. Some of the cells altered in form, and nuclei are seen mixed with debris, outside the tube. The nuclei are somewhat smaller than those of the healthy tube. (250 diameters.) Figs. 18 and 19 are from the kidney mentioned in the text.

first stage of the waxy degeneration formerly described. In this case the condition of the tubuli was in most parts quite natural; in some, however, there was extravasated blood, and in others the epithelium had accumulated to an abnormal extent. In both these cases there was imperfect development of the epithelium; but cases have occurred to me, in which this character was by no means well marked. The crowding of the tubes with nuclei, although frequently found in the earlier stages of desquamation, is not invariably present; and I have seen the tubes gorged with epithelium, in a case where none had been separated with the urine for weeks before death.

So long, therefore, as the epithelium is freely regenerated, the kidneys may preserve a tolerably healthy appearance even on minute examination. The principal characters of the disease in this stage are derived from the urine. After prolonged disease, however, further changes take place. The epithelium becomes more sparingly generated, and is thrown off in the coherent masses above described leaving the basement membrane in portions bare, or with a few scattered oval nuclei, much smaller than those cast off, adhering to its inner surface. (See Fig. 20.) In the microscopic examination of organs in this condition, there are frequently seen films of such exceeding delicacy and transparency as to be only visible by very careful management of the light. They preserve the shape of the tubules, and contain no nuclei or structures of any kind. Similar films are occasionally seen in the sediment of urine. They are probably thrown off from the denuded basement membrane.

Obliteration of the Tubes.—The basement membrane, which, with the few closely adherent oval nuclei above described, is now the sole remaining structure of the tubes, soon undergoes a change. (See Fig. 20.) It loses the cylindrical form proper to it in the fresh and natural kidney, and becomes flattened by the pressure of the surrounding parts. Its cavity is thus obliterated, and what was a tube assumes the appearance of a transparent riband, dotted here and there with small oval nuclei, which, when seen at the edges, appear to be enclosed between two layers of membrane. These riband-shaped portions of membrane appear to preserve considerable tenacity and elasticity; by their greater density, and by the constant presence of the small oval nuclei so often mentioned, between their layers, they are in most cases readily distinguished from the delicate films which have been referred to above. They are very various in diameter, but are always inferior in this respect to the normal tubes; and they appear to break up spontaneously into smaller portions, each of which contains from one to six, or even more, nuclei. These portions are of various sizes, from 1-10th to 1-4th of a millimetre in length,

and from 1-120th to 1-30th in breadth. They are usually broadest in the middle, and taper to a point at both ends. The smallest of them contain only a single nucleus, and present an appearance in every respect like that of the young fibres of areolar texture, or those fusiform cells which have been called *fibro-plastic*. I think it probable that the whole of the diseased basement membrane ultimately splits up into fibres of this kind.[1]

While these changes are proceeding, the capillary vessels, which have ceased to be subservient to secretion, are usually obliterated. The consequence of this double obliteration of

Fig. 20.

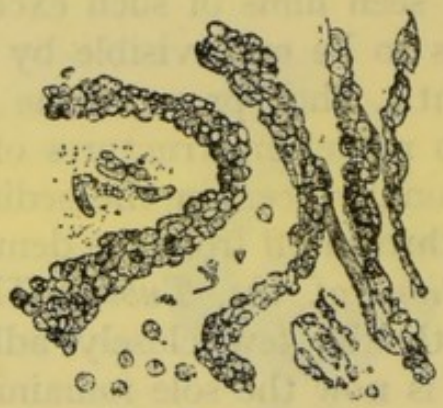

Fig. 21.

Fig. 22.

FIG. 20. Fragments of tubes from which most of the cells have separated, and which are undergoing the change mentioned above. Some small nuclei are seen adherent, and others free. (250 diameters.)

FIG. 21. Debris of epithelium separated from the interior of the tubes, and presenting a mould of their form. (250 diameters.)

FIG. 22. Delicate films presenting the form of the tubes, and probably separated from their interior. Along with them a few nuclei and cells, which, however, are not imbedded in the membrane, as in Fig. 20. (250 diameters.)

vessels and tubes, is a considerable degree of atrophy in the diseased parts; and, as the atrophy takes place at first chiefly in the cortical substance, great irregularities of the surface generally supervene. Thence arises the appearance so well described and figured by Dr. Bright (Plate III. Fig. 2), in which, from the atrophy of the cortical substance,

[1] I have never seen any reason to believe, with Mr. Simon, that the tubes in diseased kidneys burst from over-distension, discharging their contents into the inter-tubular tissue. The separation of the epithelium from the tubes, under the pressure of glasses, takes place to a considerable extent even in healthy organs, and much more in disease; but it is the result of the manipulation, not a pathological appearance.

the bases of the pyramids "are drawn towards the surface of the kidney."

When oleo-albuminous exudation supervenes on the above derangement of the tubes, or when desquamation supervenes on the former (circumstances which I conceive to be of very common occurrence), the exudation most commonly takes the form of the granulations of Bright, which are deposited chiefly in the diseased tubes; and the atrophy proceeding around these, they become salient, and the surface generally irregular, giving rise to the tuberculated state of the surface, so common in all the later stages of the granulated kidney (Bright, Plate III. Fig. 1; Rayer, Plate VII. Fig. 6; Plate IX. Fig. 8). As the atrophy, however, proceeds, the granulations are gradually absorbed; and when the kidney has become extremely contracted and irregular, they often in great part disappear.

The atrophied portions of the kidney are usually exsanguine and of a tawny or drab colour; they have considerable hardness and toughness. Examined microscopically, they appear to consist of fibres and fusiform cells in great abundance, and of more or less granular exudation, according to circumstances. According to Henle, Eichholtz, Gluge, and others, these fibres are in great part new formations; Johnson and Simon consider them as nothing more than the compressed parenchyma of the gland, from which all the other normal elements have disappeared. I look upon them as formed in great part by the breaking up of the basement membrane of the tubes (as above described), as well as from the parenchyma and obliterated capillaries. It is not improbable, however, that, in addition to these elements, some new fibrous tissue is formed.

The extreme stage of the atrophied kidney is nearly the same whether exudation have existed or not.

Microscopic Cyst-formation.—It occasionally happens, on examining the section of a kidney with the microscope, that we see scattered through some parts of the section a few small clear vesicles of nearly circular or oval form; they are either of a very pale straw-colour, or nearly colour-

less, and are perfectly clear and translucent, with a very distinct shadowed margin, which causes them to stand out in bold relief from the other textures composing the section. Their diameter is usually from 1-40th to 1-15th of a millimetre, but in this respect they vary considerably; sometimes they appear to lie in the tubular areolæ, and at other times to be unconnected with these. Very rarely they have appeared to contain a few granules; most commonly, even when there is granular exudation around them on every side, they contain nothing but clear fluid. Their

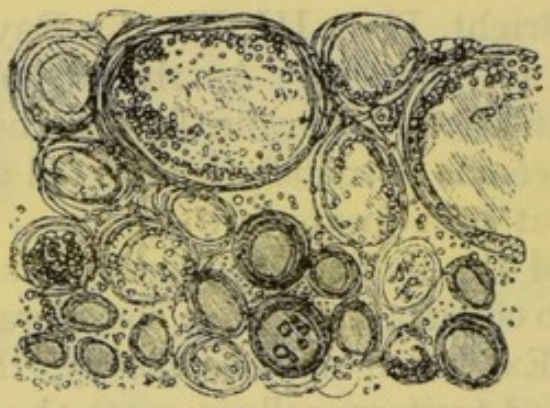

Fig. 23.

Fig. 24.

FIGS. 23 and 24. Vesicles described, dispersed amid the normal elements of the section of the kidney. There is a considerable quantity of granular exudation in the Malpighian capsules and tubular areolæ. One vesicle in Fig. 20 contains two or three granules, but all the rest are quite clear. In Fig. 23, a tube contorted, and here and there apparently much constricted, is seen to pass from the left (below) to the right (above). One of the vesicles lies over a constricted portion, and two others are seen in contact with the tube below.

refractive power is not so great as that of oil, while it is much greater than that of the spherical cells of the tubes. Hence their distinct and characteristic shadowed outline. (See Figs. 23 and 24.)

These bodies (which, however, have never appeared to me to present distinct nuclei) are probably the same with the "nucleated cells or vesicles" described by Mr. Simon, as resulting from the extravasation of the epithelial cells into the intertubular tissue, and as progressively enlarging so as to form the cysts visible to the naked eye, which are so common in diseased kidneys. To these structures he attaches great importance in the pathology of the kidney, conceiving them to be the invariable result of the desquama-

tive disease when of long standing; the kidney being, in Mr. Simon's opinion, changed more or less into an aggregation of microscopic cysts, which either undergo absorption, and lead to atrophy of the organ, or increase in size and monopolize its texture. Thus, according to Mr. Simon, the serous cysts so common in the kidney result from an enormous development and hypertrophy of extravasated epithelium cells, which assume the character of the vesicles he describes, and acquire the power of increase and endogenous development.

Whether the bodies described by me above are the same with the vesicles of Mr. Simon, I have some difficulty in determining; but they are the only objects I have seen which correspond at all closely with his description, unless, indeed, it were possible to suppose, as Dr. Johnson appears to hint,[1] that he may have mistaken the normal disposition of the tubuli for a cystic structure.

However this may be, I am satisfied that the vesicles above described are exceptional productions, and by no means invariably connected, as Mr. Simon describes his vesicles to be, with the progress of the desquamative degeneration. They are seen in comparatively few cases; on referring to four, of which I have drawings or memoranda, I find two to have been congested and waxy kidneys, with slight exudation, one to have been a soft and desquamating kidney, also with slight exudation, and one a granular kidney, with numerous cysts, from the size of a pea to that of a hazel-nut. On the other hand, I have examined organs in every stage of desquamative disease without finding these bodies, the production of which cannot therefore be an essential step in the degeneration and atrophy of kidneys so affected.

The origin and progress of these vesicles is very obscure. It is not improbable that, as Mr. Simon asserts, they are transformed into the larger cysts visible to the naked eye; though I confess that I have not been able to trace the

[1] See first part of article Ren, in Todd's *Cyclopædia of Anatomy and Physiology*, just published and not yet completed.

intermediate steps of their progress in a satisfactory manner. On the other hand, their origin from extravasated epithelial cells seems exceedingly improbable ; indeed, I have already stated that I do not think the epithelium ever becomes extravasated. Moreover, the vesicles in question have all the appearance of being formed *within* the tubes, although they afterwards become separated from them.

From the occasional appearances of alternate distension and constriction presented by the tubes when undergoing obliteration, I am induced to believe that cysts may be formed by the occlusion and isolation of portions of tube which have not yet lost their power of secretion. Whether the vesicles in question are formed in this way, can only be determined by close and repeated observation ; and I have not been able to obtain demonstrative evidence on this point.

The larger cysts in the kidney present very strong evidence of being formed in connexion with the secreting membrane. In one instance I found their inner surface to be lined at some points with tesselated epithelium, in the form of pentagonal or hexagonal flattened cells, with circular nuclei ; in another case there were oval nuclei without any distinct cells, and a large number of free oil-globules of considerable size. The existence of oil in these cysts has also been observed by Dr. Johnson. Other products of secretion are also occasionally found. On one occasion I found several cysts in a kidney otherwise healthy in appearance, which contained a turbid ochrey-coloured liquid, presenting under the microscope numerous minute crystals of uric acid. Mr. Simon mentions having found on two occasions xanthic oxide in considerable proportion. I have more than once observed them to contain blood in large quantity, and I have likewise found them full of a matter like stiff glue.

The occurrence of cysts in kidneys presenting a generally healthy structure is so frequent, as to lead to the idea that they must be in such cases the result of disease which has been arrested before any considerable disorganization has

taken place. Many of the cases of partial atrophy of the kidneys figured by Rayer (see Plate V. Figs. 5, 6; Plate XXXV. Figs. 8, 9, 10) are probably due to the rupture or obliteration of these cysts.

Before leaving the subject of cyst-formation, I may state, that in one instance I have observed the Malpighian capsules to be occupied by distinct cysts. This case will be presently alluded to.

Dilatation and Thickening of the Tubes.—This condition, although by no means a very frequent one, is important as being characteristic, so far as I have observed, of the extreme stages of what I have called the "waxy degeneration." I have scarcely ever seen it unaccompanied by entire obliteration of the vessels, and by enlargement and increased density of the kidney. The organ has the dense, resistant feeling of fibro-cartilage, and both cortical and tubular portions have the light yellow colour, and the appearances are those of the waxy degeneration in its last stage. The striæ of the pyramids appear to radiate indefinitely towards the surface, and meet the cortical substance in digitations, instead of being marked off by a sharp semicircular line, as occurs in the healthy kidney. When examined with a simple lens, or even the naked eye, the pyramidal striæ are seen to pursue an unusually sinuous course; this is peculiarly the case where they pass into the cortical substance. Moreover the pyramids are unusually broad at the bases; and the length of the straggling digitations is sometimes so great, that I have measured fully an inch and a half between the extreme end of the striæ and the corresponding papilla. Nevertheless the cortical substance is not usually diminished in quantity, being developed to a great extent between the pyramids.

This condition I have ascertained to proceed from dilatation and thickening of the tubuli uriniferi throughout the organ. The dilated tubes are usually twisted and varicose, as may be seen by inspecting a section of the pyramids with a low power. When examined with a higher power, the section presents an appearance very similar to some tumours

(of the fibrous or fibro-cystic kinds) ; viz. a number of compressed areolæ, enclosed by fibrous tissue, and presenting an appearance of irregular concentric rings of various distinctness (an effect apparently due to the peculiar refraction of light by the thickened membrane). The nuclei are obscured or invisible, owing to the thickness of the intervening wall, but nevertheless exist in considerable numbers. The Malpighian bodies and capillaries are usually obliterated. The kidney has in fact become, like the tumours whose structure it resembles, a true non-vascular texture.

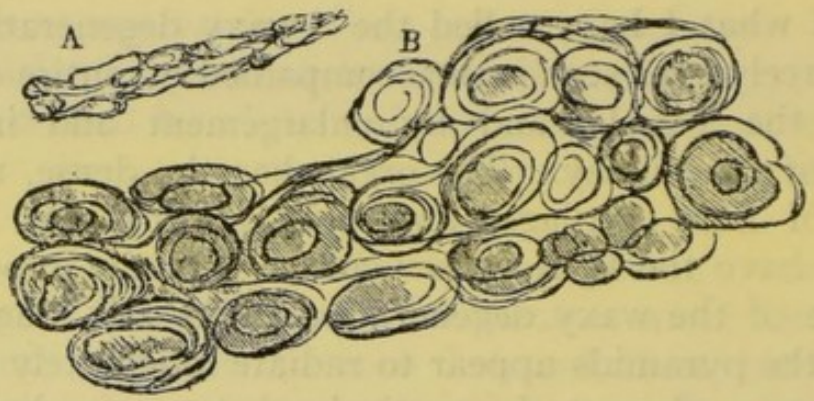

FIG. 25. A. Tubes in the pyramidal substance of the kidney in the case of Campbell. (Observation XIV.) (40 diameters.) B. A similar section (as described above). (170 diameters.)

The explanation of the peculiar extension of the pyramidal striæ towards the surface in these cases, is to be found in the fact, that even in the normal condition the convoluted tubuli have a general disposition from the bases of the pyramids towards the surface, in the direction of the striæ of the cones. This is evident from the facility with which the gland tears in that direction ; although in the normal state this disposition is masked by that of the vessels, which, passing in straight lines through the cones, break into a complicated network of capillaries at the bases of the pyramids. In the present lesion, the vessels having disappeared, and the course of the tubes being strongly marked, their disposition towards the surface becomes manifest, and the abrupt line of demarcation between the cortical and pyramidal substance, caused by the presence of the vessels, is obliterated.

The above transformation of the Malpighian bodies has not escaped the attention of Rayer, who says that in an advanced stage of the *Néphrite Albumineuse* " the glandules of Malpighi resemble small serous vesicles, mingled with others a little larger, which still later became true cysts." (See Rayer's *Atlas*, Pl. IX. Figs. 6 and 7.) In the case just detailed, however, there was no enlargement of any of the Malpighian bodies.

CONCLUSION.

With the view of enabling the reader to place the foregoing observations in relation with the descriptions found in systematic pathological works, I subjoin the following short remarks on the principal physical characters usually ascribed to diseased kidneys.

Increase of Size and Weight—Hypertrophy.—Enlargement of the kidney occurs chiefly in consequence of three conditions; 1st, from sanguineous engorgement; 2nd, from distension of the tubes by secretion or exudation; 3rd, from permanent dilatation and thickening of the tubes. Of all these causes, the second is by far the most common. The last is characteristic of the waxy degeneration formerly described.

The quantity of liquid in the tubes is at all times subject to so much variation, that it is difficult to say what amount of increase of weight may be thereby occasioned without the existence of any positively morbid condition. It is not very uncommon to find kidneys otherwise not differing from the healthy standard, about double the usual weight, or between seven and eight ounces each. I have more than once found them to weigh nine ounces each, with very slight marks of disease. When the weight much exceeds this, it is probable it arises from the rare combination of vascular and tubular engorgement.

In kidneys containing oleo-albuminous exudation, the greatest increase of size is attained when the exudation is universal, and unaccompanied by desquamation.

Cystic degeneration of the kidneys, dilatation of the pelvis and ureters (Hydronephrose, Rayer), etc., also give rise to great increase of size and weight.

Diminution of Size and Weight—Atrophy.—This condition sometimes occurs to a certain extent in emaciated subjects, without any disorganization, owing to the diminished activity of secretion. More frequently, however, it is the result of separation of the epithelium, followed by contraction and obliteration of the tubular structure.

Atrophy, from this cause, is liable to supervene in all other varieties of renal lesion, except the waxy degeneration, which appears to lead to a permanently hypertrophied condition of the organ. In kidneys enlarged from exudation, the occurrence of desquamation and its consequences is frequent ; and the diminution of size in such cases, is often not followed by a return to the natural condition but by permanent atrophy.

The course of all disorganizing diseases in the kidney, is to produce first enlargement, and then contraction of the organ. In the extreme stages of the atrophy which results from exudation, exudation is often nearly absent. When exudation therefore, even in very sparing quantity, accompanies a contracted condition of the kidney, there is a probability that it has been abundant at some former period.

Irregularities of Surface—Tuberculated and Granulated Kidneys.—The smoothness of the surface in the kidney is destroyed either by unequal dilatation, or unequal contraction of the tubuli of the cortical substance. The former takes place in the waxy degeneration, the latter in the desquamative processes.

The most frequent irregularities of surface are formed in connection with the granulations of Bright. These are invariably formed when exudation is deposited in kidneys tending to the desquamative lesion ; and, as this runs its usual course, the granulations become prominent from the destruction of the tubes around them. An extreme degree of the irregularities thus produced constitutes the tuberculated kidney.

The puckering and partial atrophy occasionally seen in kidneys otherwise not morbid, or comparatively slightly diseased, are probably in many instances the result of the obliteration of cysts.

The more remarkable changes in colour and consistence are described very fully in many parts of the preceding memoir.

On reviewing the whole of the observations, the result of which I have now laid before the public, I am induced to regard the following conclusions as especially important in relation to the pathology of renal diseases :

1. By far the greater part of the pathological lesions of the kidney arise from, or are connected with, the exudation of oleo-albuminous granules into the interior of the tubes and epithelial cells.

2. The oleo-albuminous exudation is probably often preceded, and certainly occasionally accompanied, by vascular congestion ; but when the quantity of exudation is considerable, more or less complete depletion of the vascular system invariably occurs. This is a secondary result of the obstruction of the *tubuli uriniferi*.

3. The oleo-albuminous exudation occurs in two chief forms, viz. *first*, Universal infiltration of the tubes throughout the organ ; and *second*, Infiltration of particular sets of tubules, the rest remaining free, or nearly so. In the latter mode arise the granulations of Bright.

4. There is no essential anatomical difference between the exudations in the kidney which are the result of chronic processes, and those which have been considered as the result of inflammation.

5. The capillary vessels of the kidney are subject to spontaneous obliteration (unaccompanied in the first instance by any visible lesion of the tubes), giving rise to the peculiar affection which I have called the *waxy degeneration*. This obliteration of the vessels is probably in all cases preceded by a stage of congestion.

6. The consequence of the waxy degeneration is thicken-

ing and varicose dilatation of the tubuli throughout the organ.

7. The tubes of the kidney are subject to contraction and obliteration, in consequence of the desquamation of their epithelium; a condition resulting in atrophy, and complete disorganization of the organ.

8. The desquamation of the epithelium occurs very frequently in all the other diseased conditions of the kidney. When sufficiently long-continued and extensive, it produces contraction, and this indifferently whether exudation be present or not. It is sometimes accompanied by vascular congestion in every stage of its progress.

9. The earlier stages of the exudations can only be discovered by means of the microscope. The progress of the waxy degeneration, on the contrary, is best traced by the unaided eye. The desquamation of the epithelium is only to be discovered with certainty by means of the microscope, and is particularly apt to escape attention, under all circumstances, if the *kidney* only, and not the *urine*, be looked to. It results that careful investigation, both by the microscope and the naked eye, both of the kidney after death and the urine during life, are indispensable to enable the pathologist to determine with exactitude the presence or absence of disease.

ON THE PATHOLOGICAL STATES OF THE LUNG CONNECTED WITH BRONCHITIS AND BRONCHIAL OBSTRUCTION.

From the Monthly Journal of Medical Science, 1850, *vol. xi.* 122 *and* 230; 1851, *vol. xii. p.* 440, *and vol. xiii. pp.* 2 *and* 238.

This contribution was published at a most interesting epoch in the development of modern medicine. The immortal treatise of Laennec[1] *appeared in* 1819, *and it was translated into English by Forbes,*[2] *on the earnest recommendation of Clark, in* 1821. *Shortly afterwards, the work of Scudamore*[3] *further popularised the methods of the great French observer in this country, and the works of Williams,*[4]

[1] *De l'auscultation médiate*, Paris, 1819.

[2] *Treatise on the Diseases of the Chest and on Mediate Ausculation*, London, 1821.

[3] *Observation on Laennec's Method of Forming a Diagnosis of the Diseases of the Chest*, London, 1826.

[4] *Rational Exposition of the Physical Signs of the Diseases of the Lungs and Pleura; illustrating their Pathology and facilitating their Diagnosis*, London, 1828.

The Pathology and Diagnosis of Diseases of the Chest; illustrated especially by a rational Exposition of their physical signs with new Researches on the Sounds of the Heart, London, 1835.

Lectures on the Physiology and Diseases of the Chest, including the Principles of Physical and General Diagnosis and their Application to the Practice, delivered, etc., 1836-37, London, 1838.

Stokes,[1] *and Walshe*[2] *made their appearance thereafter in rapid succession. These were the most important works on diseases of the chest when this paper was published. In the following pages, the subjects which aroused keenest interest, and also sharpest criticism, were those dealing with bronchitic collapse and pulmonary emphysema.*

The author sets himself the task of accounting for certain consequences of bronchitis, beginning with collapse. He describes plugs of inspissated secretion, acting after the fashion of a ball-valve, by falling back and interfering with the passage of air. After giving a brief sketch of the process of condensation taking place in the course of typhoid fever, he deals with collapse. "When a bronchial tube is in any way obstructed," he says, "or much diminished in caliber, at one or more points, the question arises, what is the mechanical effect of the movements of respiration upon the amount of air thus partially imprisoned behind the obstructed part? If the obstruction be complete, of course no change can take place, at least mechanically; but in the case of its being, as it usually is, incomplete, it may be supposed that the inspiratory act tends to draw in more air than the expiration can expel, and consequently that the air tends to accumulate *in the vesicles; or,* vice versa, *that the air behind the obstruction tends constantly to* diminish *in amount, owing to the comparative inefficiency of the inspiratory act; or finally, that the forces equalise each other, and the quantity of the enclosed air* remains unaltered.

"Now, it cannot be denied that from the pathological anatomy of bronchitis, a prima facie *case might be made out for each or all of these theories; for, although only one side of the question has hitherto been brought prominently forward in the preceding part of this paper, the reader will not have failed to remark that in several cases in which bronchitic collapse of the lungs existed, other parts of the same organs were affected with emphysema or dilatation of the air-cells. Indeed, so familiar is this conjunction of emphysema with bronchitis, as to have suggested to Laennec, long ago, the first of the three theories above mentioned as to the cause of dilatation of the air-vesicles in that disease. Again, fatal cases of bronchitis*

[1] *Treatise on the Diagnosis and Treatment of Diseases of the Chest*, Dublin, 1837.

[2] *A Practical Treatise on the Diseases of the Lungs and Heart*, London, 1831.

undoubtedly occur, at least in adults, in which there is no change of the pulmonary texture so marked as to afford support to either of the first two theories.

"Laennec's view of the consequences of obstruction was founded on the idea of the comparative weakness of the expiratory, as compared with the inspiratory, forces."

He proceeds to a criticism of Laennec's views, showing that, although moderate inspiration is more powerful than moderate expiration, yet forced expiration must always be greater than forced inspiration. In this connection, he fully refers to the experiments of Mendelssohn and Traube; and, after pointing out the essential characteristics of the structure of the bronchial tubes, as a series of gradually diminishing cylinders, branching, for the most part, dichotomously, so that, if a plug of any kind—particularly one closely adapted to the form of the tube, and possessed of considerable density—be lodged in any portion of such a tube, it will move with greater difficulty towards the smaller end, and, in this way, close it up more tightly than if it moved in the opposite direction. He therefore concludes that, if such a plug be placed over a bifurcation, it will act in the manner of a ball-valve upon the orifice of a syringe. If such a powerful expiratory effort as a cough should occur, the plug may be entirely dislodged, so as to allow air to return freely into the region obstructed; but, if the expiratory force is only enough to displace the plug slightly, and so allow of the outward passage of air, the inspiration will again bring it back to its old position, so that the repetition of the process must end in absolute collapse of the portion of lung usually supplied with air by the obstructed tube. He sees no reason for accepting the suggestion of Fuchs, that air is absorbed by the blood-vessels. It is hardly necessary to mention in this place that, since the date of Gairdner's contribution, the work of Pearson Irvine[1] and C. H. Fagge[2] on collapse as the result of paralysis of the muscles of inspiration have added considerably to our knowledge of bronchitic collapse.

In the second part of the paper, which deals with secondary or permanent disorganisations, depending on bronchitis, there is an interesting discussion on the modes of clearing the bronchial tubes, and of the actions of the bronchial muscular fibres. It is to be observed that, in passing, he holds asthma to be caused by "some

[1] *Transactions of the Clinical Society of London*, 1876, vol. ix. p. 188.

[2] *Principles and Practice of Medicine*, London, 1886, vol. i. p. 870.

kind of irregular action of the muscular apparatus of the air tubes" —a view which is still the most popular explanation of that affection.

This part of the paper includes the views on emphysema, which have aroused in the past so much controversy. That some degenerative process in the pulmonary tissue preceded the development of emphysema was a view which had been held by certain older authors. The first real attempt to explain the mechanism by which the structural changes are brought about was that of Laennec. He put forward the view that, on inspiration, the air drawn into the lung was detained during expiration, either by some catarrhal swelling of the mucous membrane, or by accumulation of secretion in the tubes, so that the lungs, as a consequence, became over-distended. He expressly points out that the muscles of inspiration are numerous and powerful; while expiration is mostly produced by the elasticity of the tissues, and by a feeble contraction of the intercostal muscles; so that the act of inspiration may overcome the resistance opposed to the entrance of air by the tumid condition of the mucous membrane and the retained secretions; while expiration is unable to overcome these obstacles. This theory is mentioned by most of the writers who immediately followed Laennec, and it is indeed powerfully urged by Williams.[1] *In view of these facts, it is therefore somewhat startling to find that Gairdner, in bringing forward his inspiratory theory, has misapprehended the work of Laennec. Laennec*[2] *states his theory in the following terms:*

"Enfin, lorsque le catarrhe chronique qui accompagne l'emphysème du poumon a évidemment préexisté à son développement, on pourrait en donner une explication en quelque sorte mécanique. On conçoit, en effet, que la mucosité sécrétée dans les bronches par suite du catarrhe pulmonaire, doive opposer, surtout si elle est très-visqueuse, une grande résistance au libre passage de l'air inspiré et expiré; et nous montrerons, en parlant du râle, que cette résistance va souvent jusqu'à produire l'obstruction complète, quoique momentanée, d'une partie des ramifications bronchique. Or, comme les muscles que servent à l'inspiration sont forts et nombreux; que l'expiration, au contraire, n'est produite que par l'élasticité des parties et la faible contraction des muscles intercostaux, il doit souvent arriver que l'air, après avoir forcé, dans

[1] *The Pathology and Diagnosis of Diseases of the Chest.*

[2] *Op. cit.* tome i. p. 221.

inspiration, la résistance que lui opposait la mucosité, ne peut la traverser dans l'expiration, et se trouve en quelque sorte emprisonné par un mécanisme à-peu-près analogue à celui de la crosse d'un fusil à vent. Les inspirations suivantes, ou au moins les plus fortes d'entre elles, amenant dans le même lieu une nouvelle quantité d'air, produisent nécessairement la dilatation des cellules aériennes auxquelles se rend la bronche oblitérée ; et, pour peu que l'accident soit durable, cette dilatation doit devenir un état fixe et permanent."

As will be seen in the following pages, Gairdner attributes an expiratory theory to Laennec, and the only conclusion possible is that, after perusing his work, he had forgotten its exact meaning. The expiratory theory was first suggested by Mendelssohn,[1] *in* 1845. *His belief was that the air, prevented from leaving the lungs by the action of the glottis, was compressed by the expiratory muscles during forced expiratory efforts, in consequence of which the pressure within the lungs became increased, and the air vesicles, therefore, dilated. It seems extremely probable that Gairdner, who was evidently well acquainted with the investigations of Mendelssohn, has attributed to Laennec the theory of the German author, as in discussing the causation of emphysema, he has not mentioned the latter in connection with any theory. He unhesitatingly accepts the opinion of Laennec as regards the connection between bronchitis and emphysema, but points out that we have no direct proof of the power of any expiratory act, such as violent coughing, to produce emphysema, except in connection with some of the results of bronchitis. He holds that in croup, in laryngitis, in aneurysm, there is more violent and distressing cough than in bronchitis, without emphysema ; and he states that the alleged unusual frequency of emphysema among players of wind instruments is destitute of proof. He further shows that the expiratory act is mechanically incapable of producing distension of the lung, as it tends entirely towards emptying the air vesicles by the uniform pressure of the external parietes of the thorax upon the whole pulmonary surface, and that, even when the air vesicles are maintained in a state of fullness by closure of the glottis, any further distension by the expiratory force is as much out of the question as would be the further distension of a bladder, blown up and tied at the neck, by hydrostatic or equalised pressure applied to its entire external surfaces.*

[1] *Der Mechanismus der Respiration und Circulation*, 1845.

These considerations led him to the conclusion that the only tenable theories are those referring emphysema to the act of inspiration. After crediting Williams with having placed the inspiration theory in a more tenable position than it had previously obtained, he fully describes his own views. "It appears to me," he says, "that none of the writers on this subject have clearly apprehended, or at least clearly expressed, the single obvious condition which is necessary to the mechanical completeness of the inspiration-theory of emphysema. Emphysema is, according to this theory, a complementary *lesion, dependent upon the previous existence of some form of occlusion of the vesicles, and invading the remaining sound portions of lung. Thus far it corresponds with all that we have hitherto seen, to an extent certainly not anticipated by Dr. Williams, when, after enunciating his own view, he brings forward Laennec's theory to account for residual unexplained cases. But there is yet another condition necessary, besides mere occlusion of the air-vesicles in a part of the lung: this is* partially diminished bulk;— *in other words, collapse or permanent atrophy of a portion of the lung."*

The position taken up is illustrated by the series of diagrams which will be found in the following pages, rendering the theory quite clear that, when certain of the air spaces are closed, with a resulting diminution of bulk, the expanding force of inspiration will obviously act more powerfully upon the remaining tissue, and will thus attract into the air spaces which they contain, some of the air which has been prevented from entering those which are occluded. The part thus distended would undoubtedly suffer obstruction of the capillary circulation, as in the experiment of M. Poiseuille, which Gairdner fully describes. He therefore states finally his views in the following terms:

"I am prepared, then, to maintain, that emphysema of the lung may, in all cases which I have witnessed, be satisfactorily accounted for by considering it as a secondary mechanical lesion, dependent on some condition of the respiratory apparatus leading to partially diminished bulk of the pulmonary tissue, and consequently disturbing the balance of air in inspiration. *I, therefore, submit this principle to the judgment of the profession, in the confident anticipation, that it will prove no less constant and satisfactory in the hands of other observers, and will establish itself as the exclusive law of the production of this most important lesion."*

Mendelssohn,[1] *as already mentioned, brought forward his expiratory theory in* 1845. *Jenner,*[2] *in* 1857, *in unaccountable ignorance of Mendelssohn's theory—seeing that he had read Gairdner's paper—brought forward an expiratory explanation of emphysema, which was afterwards restated in his article in Reynold's 'System of Medicine.'*[3] *Jenner pointed out that some parts of the thoracic walls yield more than the rest, so that the expiratory force is not so capable of maintaining resistance in those regions. The parts which he more especially indicated are those about the apices of the lungs, and the upper cartilages of the ribs, and he further showed that the base of the right lung is more firmly supported than that of the left, on account of the liver being less yielding than the stomach.*

Such is the modern expiratory theory of emphysema, as the result of expiratory pressure, with more or less closed glottis. The muscles of expiration compress the air within the chest, and, if every part of the thoracic parietes were equally resistant, no harm would result. As certain parts, however, do yield to the pressure, some of the compressed air is driven into the alveoli of the lungs corresponding to the regions of less resistance, so that dilatation takes place in them.

Undoubtedly, the general consensus of opinion is in favour of the expiratory theory; but, in many cases in which shrinkage has occurred, either from collapse, excavation, or fibrosis, the inspiratory theory is regarded as applicable; there can, further, be no manner of doubt that the interesting investigations of Brodie and Dixon[4] *have gone a long way to show that the inspiratory theory cannot be so easily dismissed as has often occurred in modern times.*

Another most suggestive part of the following pages deals with dilatation of the bronchial tubes, which is attributed by Gairdner to the presence of ulceration, and the force of inspiration. He refers to the explanations of Laennec, ascribing bronchiectasis to distension by accumulated secretion, and to the effects of violent coughing, which he cannot accept as adequate causes.

Stokes,[5] *as may be mentioned in passing, believed that bronchitis, leading to loss of elasticity in the longitudinal fibres, and paralysis*

[1] *Loc. cit.* [2] *Med. Chir. Transactions*, 1857, vol. xxii. p. 25.

[3] *A System of Medicine*, edited by J. Russell Reynolds, 1871, vol. iii. p. 475.

[4] *Transactions of the Pathological Society*, London, 1903, vol. liv. p. 17.

[5] *Op. cit.* p. 148.

of the circular fibres, brought about the condition; he further suggested that the action of the cilia was interfered with, and thus allowed the accumulation of secretion. Williams[1] *suggested that an inflammatory weakening of the tubes and forced expiratory efforts were responsible for bronchial dilatation. Not long after the appearance of the paper which follows, Corrigan*[2] *gave his explanation, based upon contractile processes occurring in the tissues of the lung, and the expansive action of the chest, in the act of inspiration. In more recent times, as will no doubt be remembered, Lebert*[3] *believed that there was a weakness of the bronchial walls, dependent upon disturbance of innervation rather than upon any inflammatory process, and this was in part adopted by Grainger Stewart,*[4] *who suggested that the essential element was to be sought in atrophy of the bronchial wall, which might perhaps be connected with constitutional peculiarities.*

The following pages conclude with a most interesting discussion of pulmonary concretion and cicatrices, in which are embodied some far-reaching suggestions as regards the healing of tuberculous processes.

NOTWITHSTANDING the great frequency of bronchitis in European latitudes, and the attention which has always been given to this complaint by physicians, especially since stethoscopic examination came into use, it has appeared to me that what has been written as to its ultimate effects upon the pulmonary texture is not commensurate with the importance of the subject, and in many points not consistent with the truth of nature. Having become deeply impressed with the truth of the views to be advocated in the following papers, and having taken much pains to satisfy myself as to their correctness, I venture to lay them before the profession, together with some of the observations on which they are founded, in the hope that, if corroborated by the further researches of others, they may

[1] *Op. cit.* 1840, 4th edition, p. 96.

[2] *Dublin Hospital Gazette*, 1857, p. 270.

[3] *Traité d'Anatomie Pathologique Générale et Spéciale*, 1857-61, tome i. p. 620.

[4] *Edin. Medical Journal*, 1866.

lead to improvements of some importance, both in pathology and practice.

It is necessary to state that while I have drawn my materials chiefly from personal observation, I have endeavoured to combine and harmonise the conclusions thus arrived at with those of prior observers, so far as known to me; nor have I willingly omitted any species of evidence which seemed to bear upon the subject. It would have been, indeed, an unsatisfactory task to have brought forward the results of personal experience on this subject, had they not been found to agree in many, even in most points, with those of some former authors in respect to matters of detail. In placing these pages in the hands of the public, therefore, I profess myself anxious for judgment as to the *truth*, more than as to the *novelty*, of their conclusions; and shall be satisfied if enough of originality be found in them as a whole, to rescue them from the charge of being a superfluous or burdensome addition to our literature.

Part I.—Primary Results of Bronchitis.

In the present part, the direct and primary results of bronchitis will be discussed; comprising under this head those effects which follow, almost constantly, the accumulation of mucus and inflammatory products in the bronchial tubes, when these are sufficient in quantity to cause serious obstruction. A second part will include the secondary and more permanent disorganizations of the pulmonary texture, which result from the former under peculiar circumstances; being induced either by long-continued intensity of the original disease, or by constitutional states unfavourable to the removal of its results.

Obstruction of the Bronchi.

The ordinary effects of bronchitis upon the mucous and other contents of the bronchial tubes, are, for the most part, well understood and clearly described by authors. The vascular engorgement of the mucous membrane, which

at a later stage becomes much softened and thickened, and the loading of the tubes with an altered mucous secretion, intermixed to a greater or less degree with purulent matter, are familiar to every one. The connection of these sources of obstruction with the auscultatory and other signs is also so well known as to require but few observations.

The varieties in the character of the secretion from the diseased mucous membrane are very great. In the earlier stages of the affection, the mucus is profuse, thin, watery, and frothy, being mixed with air bubbles of all sizes. At this period it contains but few microscopic elements, a few altered epithelium cells being only visible, and these in much greater number in the mucus procured from the dead body than in that expectorated by the living patient. At a later period the mucus has become yellowish, more tenacious and viscid, containing numerous pus corpuscles, and more of the altered epithelial elements. Occasionally, but more rarely, the expectoration, or the fluid in the bronchi after death, is composed of pus nearly pure, but always viscid and thick, like that which flows from old abscesses. In these cases the pulmonary tissue is rarely unaffected.

When the disease has been of considerable duration, and has nevertheless been the result of an acute attack, there are usually found, on incising the bronchi, numerous yellowish pellets, or irregular flocculent masses of somewhat curdy consistence, which float in the more recent and thinner mucus, without in any degree becoming amalgamated with it. These masses are found of all sizes, and constitute a well-known form of the expectoration of bronchitis in its advanced stages. They are no doubt formed by the evaporation of the more fluid parts of the mucus, in consequence of the constant passage over it of dry air; the inspissated material clinging to the walls of the air passages, until detached and washed away by renewed exudation from the mucous membrane. Such a mass is not unfrequently seen plugging the whole calibre of one of the larger or smaller bronchi, in such a position as to leave no doubt that in the act of inspiration it must have

acted the part of a ball-valve, completely preventing the access of air to the part of the lung involved, by falling back upon the orifices of the smaller bronchi, into which its size would prevent it from entering.

When bronchitis, or bronchial catarrh, has existed for some time in a comparatively slight form, and with the expectoration only of an increased quantity of mucus unmingled with pus, portions of this secretion become inspissated in the form of a thick, glairy, tenacious, semi-transparent material, sometimes resembling raw white of egg. In one or two cases I have pressed out of the smaller bronchi plugs of altered mucus of a still more tenacious character, like cold glue, and nearly, if not quite, transparent. But this amount of inspissation is not common.

The effects of these morbid accumulations in producing obstruction of the bronchial tubes are well known to the auscultator. So long as the mucus is thin and watery, or even more or less purulent, there is no serious or complete impediment to the passage of air; which, as the fine and coarse mucous râles, accompanied by vesicular respiration, indicate, finds its way through the fluid to the ultimate bronchial and pulmonary vesicles. The fluids, too, at this stage, move freely throughout the bronchial tree even to its minutest branches, and when in excessive quantity, are readily expectorated in the act of coughing. This stage of bronchitis, therefore, is comparatively little apt to be accompanied by urgent dyspnœa, or by changes in the condition of the pulmonary texture.

It is otherwise, however, when after a time the secretions within the bronchial tubes have become inspissated; the mucus having either become purulent and formed itself into the tenacious pellets above mentioned, or assumed the stringy, tenacious consistence proper to the more chronic forms of the disease. When, under these circumstances, expectoration is hindered, either by the tenacity of the mucus itself, the weakness of the patient, or any other cause, the tubes become really obstructed, the sound of

vesicular respiration is at some points of the lung much diminished, or it may be altogether lost, and the bronchial râles are correspondingly modified, indicating the passage of a smaller quantity of air with a much greater amount of resistance ; the ordinary mucous rattles being supplanted in part by the sharp "clicking" and valve-like sounds so well described by Dr. Williams.[1]

It is quite evident to the observer of a patient in this condition, or, indeed, in any stage of bronchitis attended with laborious breathing, that the chief difficulty is invariably in *inspiration*. This act is accomplished only with the aid of all the accessory muscles ; and, even then, the contracted state of the chest, especially in children, and the imperfect descent of the diaphragm, show that in proportion to the power employed, the amount of air entering is small. The expiratory act, on the contrary, is always accomplished with comparative ease ; and any serious obstruction to the performance of this act is at once got rid of, or dislodged by the additional impulse given to the expired air in coughing or hawking. These phenomena of laborious breathing, particularly the long-drawn, exhausting, inadequate inspiration, are probably quite peculiar to obstructive bronchitis. They occur, it is true, in diseases of the heart ; but in them, I believe, only secondarily, from the accumulation of mucus in the respiratory passages. The dyspnœa of *pure* pneumonia, on the other hand, is something quite different, being a mere *acceleration* of the respiration, without any of the heaving or straining inspiration observed in bronchitis, or in cases where the two diseases are combined. So much is this the case, that I have repeatedly observed patients affected with a great extent of pneumonia in both lungs, and in whom the extreme lividity, and the respirations numbering 50 or 60 in the minute, showed infallibly the amount to which the function of the lung was interfered with : and who nevertheless lay quietly in bed, breathing without any of the violent effort, or the disposition to assume the erect posture, so constantly accompanying the

[1] *Diseases of the Chest*, 4th edition, p. 80.

more dangerous forms of bronchitis. If this freedom from orthopnœa and laborious breathing be not uniformly characteristic of true pneumonia, it is because that disease comparatively seldom exists, as is well known, uncomplicated by some degree of bronchial affection.

The cause of the inadequate and laborious inspiration in bronchitis while the expiration is comparatively easy, is to be found, I believe, not merely in the smaller power of the inspiratory muscles to dilate the chest, nor in the advantage which the expiratory forces derive in the dislodgement of obstructive mucus from the sudden impulse of coughing, but in the mechanical relations of the tenacious inspissated mucus to the calibre of the bronchial tubes. On this subject I have more to say in the next division of my subject.

One fact of considerable importance, in connection with the total obstruction of bronchi by muco-purulent matter, has been adverted to by Andral, and is illustrated by two cases in the *Clinique Médicale*. This is, that the accidental position of such an obstructive plug may be the cause of a very rapid and unexpected fatal issue, in a case by no means threatening from the violence of inflammatory action. In each of the two instances given by Andral, the respiratory murmur became completely suppressed in the upper part of one lung, the patients having been previously affected with moderate bronchitis, and dyspnœa having become suddenly increased after a fit of coughing. The post-mortem examination showed the signs to be due to an obstructive accumulation in the bronchi leading to the upper lobe of the lung ; but the absence of respiratory murmur, combined with clearness on percussion, had led during life to the diagnosis of emphysema. There can be, I think, little doubt that many of the paroxysmal accessions of dyspnœa in persons affected with bronchitis are due to accidental change of position of the pellets and ropy masses of inspissated mucus, which accumulate in the tubes. At least stethoscopic examination frequently reveals the signs of obstruction in particular parts of the lung, supervening

rapidly, and disappearing again with equal suddenness, in consequence of the accession of cough.

I have now to advert to a condition of the pulmonary texture which appears to me, from the results of my own dissections, to spring more directly than any other from obstruction of the bronchi by mucus of a certain degree of tenacity. As this condition, the collapse of the air-cells, has been but little noticed by authors in this connection, at least in the adult lung, the subject will perhaps be best introduced by a narrative of my own observations with regard to it, which I shall endeavour afterwards to connect with the facts furnished by others.

Collapse of the Lung as connected with Bronchial Obstruction.

During the epidemic of continued fever which prevailed in Edinburgh in the greater part of the year 1847, it was frequently observed that the lungs, in persons of all ages, were the seat of a form of condensation, characterised by the absence of the friability and granular appearance on section of pneumonic consolidation, and also by the peculiarity of its microscopic elements, the large granular cells which form so common an ingredient in ordinary red hepatization being either very sparingly or not at all present. The condensed portions were usually scattered over both lungs, and often very limited in extent, being accurately circumscribed by the margins of the lobules ; in most other respects they corresponded with the descriptions to be hereafter given. These appearances occurred very commonly in typhus fever, whether of the ordinary form or the abdominal typhus (now commonly called *typhoid fever*), accompanied by intestinal ulceration ; which latter form had at that time a prevalence quite unusual in Edinburgh. Similar lesions were occasionally, though less frequently, seen in the relapsing fever, which was also epidemic during that year. This state of the pulmonary tissue was the subject of frequent conversation, and repeated microscopic examination, among those chiefly engaged in the pathological theatre of

the Infirmary during this period; although my own notes of fever cases at this time are but few, I am fortunately able to compare my recollections with several sufficiently characteristic descriptions of the lesion in question by Dr. Waters, whose examinations were recorded with the utmost care and fidelity, with a view to the preparation of a history of the epidemic unfortunately not yet published.[1]

The nature of this pulmonary affection appeared to me, at the time, very doubtful. By some it was looked upon as corresponding with the "typhoid deposits" in the lung described by German authorities; and the imperfect cell-forms often seen under the microscope seemed to favour this view. On the other hand, careful examination showed that there was frequently little or no real *deposit*; the cell-forms differing very little from those which might be procured from the normal lung, or still better, from one compressed by pleuritic effusion, or any other cause. At this time, my experience in pathological studies was not such as to enable me to form a decided opinion; but towards the close of the epidemic, the result of observations inclined me to believe that the lesion so frequently witnessed in fever

[1] From one of Dr. Waters' reports I quote the following: "Inferior lobe of left lung felt condensed, and contained less air than ordinary; its section was smooth, not granular, and its consistence somewhat tough, not breaking down under the finger." On another occasion "the lower lobe of the left lung was much gorged with blood, and of a dark colour, and smooth section, not granular, void of air, and sinking entirely in water." The reader may compare these with the characters which follow of the bronchitic collapse. The following description from the case of a child dying of fever, corresponds to the characters of lobular collapse, combined with the bronchial abscesses to be hereafter described. There were condensed nodules in the lung which "presented at the surface of the organ, where they were recognised by their bluish-black colour and resistent feel; they were not elevated above the surrounding surface, but rather very slightly depressed." . . . "On section, the great majority of these masses presented a reddish-blue colour and smooth surface. They were not friable but of firm consistence. A few of them had degenerated into a reddish-grey somewhat friable substance, and two or three were excavated by central cavities, which were empty and collapsed."

was not connected with any specific form of morbid deposit, but was, in its purest form, a condition of imperfect expansion or collapse of the pulmonary tissue, similar to that described in the lungs of infants under the name of *atelectasis*, and differing only in its distribution in patches, and in other accessory circumstances, from the condition of *carnification* described by Laennec as the result of compression of the organ.

That the fever of 1847 was not, in any degree, *specially* characterised by these lesions is fully proved by the researches of other authors, as well as by my own subsequent experience. In fact, in the course of the two succeeding years, I had various opportunities of becoming familiar with collapse, or carnification, of the pulmonary tissue, as a disease distinct from pneumonic consolidation, occurring in various forms, and under circumstances where no external compression of the lung could be conceived to account for it. Many of these lesions occurred in the lungs of children, but those of adults and old persons were scarcely less frequently affected. The disease presented itself, also, frequently in combination with other affections, which gave rise to no small difficulty in forming an opinion on the true nature and mode of origin. Neither was it always very easy to distinguish it from pneumonia, more especially in some of the combined conditions to which I shall hereafter allude ; and I am satisfied that the terms *lobular pneumonia*, *red hepatization*, or the more indefinite expression, *condensation*, have often sufficed in my own case, as well as that of others, to cover ignorance or imperfect knowledge of the condition in question.[1]

[1] Had opportunities of examining the lungs of very young children frequently occurred to me, I could scarcely have failed to have soon become familiar with all the phases of this lesion ; and to have recognised much sooner than I did, the identity of the congenital atelectasis, the "lobular pneumonia," and the carnification or collapse of the adult lung. But owing to the exclusion of all children under five years of age from the Infirmary, my observations were confined almost entirely to the lungs of persons above the age of infancy ; and it was only at a comparatively recent period that the observation of a few infantile lungs, and the

Notwithstanding these difficulties, however, observations made in the years 1848-9 left me no longer in doubt, that the lungs of adults not unfrequently display portions more or less limited or diffused, which are so perfectly condensed as to sink rapidly in water, and yet differ widely, in appearance and microscopic character, from truly inflamed lung, as well as from all the atrophic conditions of the organ which can be clearly traced to inflammation or structural disease as their cause. In some instances, such portions presented exactly the appearance of fœtal lung; and only the manifest absurdity of the proposition could have prevented the observer from ascribing their state to the same cause as the congenital non-expansion of the lung, or atelectasis of Jörg. In the absence, therefore, of any manifest explanation of the compression or collapse of the tissue observed in these cases, I was obliged, provisionally, to rest satisfied with the knowledge of the fact that most of the lobular and many of the more diffused forms of condensation usually ascribed to pneumonia, in the adult as well as the child, were really the result of some other and unknown condition.

Bronchitic collapse of the lung occurs under two distinct aspects: the *diffused* form, and the limited or *lobular* form. Of these, the latter variety is the more striking and characteristic, and has been, especially in the lungs of children, the subject of more discussion than the former; but the diffused form is by far the more common, and is in fact of very frequent occurrence, at least in its slighter degrees. Both forms present the same fundamental changes of the pulmonary tissue, which is usually of a dark violet colour externally, as seen beneath the pleura; and internally of a more or less deep brownish red, or mahogany tint. The colour, however, is by no means an invariable criterion,

perusal of the work of M. Legendre (*Recherches Anatomo-pathologiques et Cliniques sur quelques Maladies de l'Enfance*, Paris, 1846), revealed to me clearly the immense importance and frequency, though not, I believe, the true significance, of this state of the pulmonary texture in the early periods of life.

depending almost entirely on the amount of blood in the collapsed tissue. The affected parts are always more or less condensed; this condensation may amount to a mere diminution of the crepitation, or to a total absence of it, in which case portions are usually found to sink readily in water. These latter portions are both more flaccid and much less friable than the pulmonary tissue when in a state of red hepatization; and they differ greatly from this lesion in the aspect of their section and the nature of the fluid it yields to the knife. In every variety of true pneumonic consolidation, in which the lung is completely void of air, the air cells are occupied by a deposit, presenting to the naked eye (and still more distinctly to a power of 20 to 30 diameters) the well-known granular aspect of the hepatized lung. If the deposit is fluid, or semi-fluid, it is capable of being pressed out, or scraped off, in the form of a thick opaque emulsion-like matter, of yellowish, orange, or grey colour; and in all cases it shows, under the microscope, abundant granular elements and cell-structures, of the kinds usually found in inflammatory exudations in parenchymatous organs. In the collapsed lung, on the contrary, the section is comparatively smooth, having somewhat the appearance, as described by Laennec, of muscular flesh; it presents no trace of granulations, and yields, on pressure, or to the knife, only a semi-transparent bloody serosity, which, under the microscope, is seen to contain little or nothing besides blood-corpuscles, epithelium, and other portions of normal tissue, and possibly a small amount of pus from the interior of the bronchi.

In the *diffused* variety, the collapsed condition may be found affecting a more or less considerable portion of either or both lungs, usually at their posterior part, and passing quite gradually into normal tissue; the supple, dense, tough feeling being exchanged for the normal spongy, elastic crepitation; and the violet, or deep purple colour, shading off into the usual hue of the surface. Even in this form of the lesion, however, a tendency of it at some points to be circumscribed by the interlobular divisions may often

be observed ; this tendency being, so far as I have observed, quite characteristic of the bronchitic, as opposed to the pneumonic, consolidations.

In the *lobular* forms of bronchitic collapse, which often occupy the anterior edges, as well as all other parts of the lung, the affected portions are everywhere accurately and abruptly marked off by the interlobular septa, the portions so limited being various in size and form, but always manifestly shrivelled, and sunk in below the level of the surrounding parts. This is peculiarly manifest when they occupy the anterior edges. When they are scattered through the lung, they communicate to the fingers, in feeling the organ externally, much the same sensation as clustered tubercles in the midst of crepitant tissue. This is the form which was so often described as "lobular pneumonia" in young children, till the experiments of MM. Legendre and Bailly clearly showed it to be nothing but a collapse of the air cells.[1] These observers were in fact the first to apply to this condition of the lung the same test as Jörg had long before used in respect to the congenital atelectasis, and to show that the lung affected with "lobular pneumonia" could be generally restored nearly to its natural condition by forcible inflation, which occasionally requires, however, to be continued for some time.

I have employed inflation in both the diffused and lobular form of collapse, as observed in adults, and as above described, with precisely the same results as those of Bailly and Legendre. I may state, however, that though this test is very useful in demonstrating the nature of the lesion, in a favourable case, to one not familiar with its character, I do not believe it to be applicable to the determination of the presence or absence of pneumonia in those mixed cases in which alone there is any difficulty. For I have observed that on the one hand, the partially pneumonic lung may be inflated when the affection is recent, and combined, as it frequently is, with bronchitic collapse ; and, on the other,

[1] *Archives Générales de Médecine*, 1844 ; and Legendre's work before cited.

that in the latter lesion in its purest forms, complete inflation is often very difficult or impossible after the collapsed state has been of some duration. In fact, the lung then begins to undergo a modification in its nutrition and structure, which ultimately leads to permanent atrophy.

With regard to the combined forms of lesion above alluded to, they are, I believe, by no means of rare occurrence. The collapsed lung, especially in the incomplete and diffused form in which it most frequently presents itself in adults, may become the seat of a true inflammatory exudation into the air-cells, giving to the section an obscurely granular aspect. The exudation has in such cases the usual microscopic characters, but the shrivelled state of the lung, and the minuteness of the granulations, together with the state of the bronchial tubes, demonstrate the participation of these in the inflamed condition. Occasionally, also, the collapsed lobules are subject to an œdematous infiltration of fluid, when this state prevails in the other parts.

I think, however, that I have also observed the collapsed parts, particularly in the well-marked lobular forms of this lesion, to escape, to a certain extent, the inflammatory or œdematous condition prevailing in the parts around them. At least, I have observed cases where these remained comparatively dry, containing only blood, when other parts were bathed with frothy serum or pus. This subject, however, requires further investigation.

In maintaining (as I have no hesitation in doing) that some degree of collapse of the lung is an almost invariable concomitant of bronchitis of a certain degree of intensity, it must not by any means be supposed that *complete* loss of crepitation is to be looked for in any part of the tissue in the majority of cases. The usual fact is, that the collapse is in the incomplete and diffused form; but I believe, nevertheless, that dulness of percussion during life from this cause, and complete lobular or diffused collapse after death, especially in the posterior parts of the organ, will be found to be much more common in the bronchitis, as well of adults as of children, than is commonly supposed. Of

the truth of these opinions, both recorded observations, and unrecorded recollections, appear to me to furnish no inconsiderable amount of evidence.

I have now to advert to the observations of the authors who have described collapse of the lung as a state distinct from pneumonic consolidation, or pleuritic carnification, with the view of showing in how far their researches have tended to throw light on the question of its origin. Laennec described the diffused form of collapse only in connection with pleuritic effusion and compression; and there can be little doubt, from some expressions in his descriptions of the first and second stages of pneumonia, that he must, to a considerable extent, have confounded the two conditions, especially in their more mingled and less characteristic forms. That he has not altogether overlooked the peculiarities of the lobular collapse, however, is evident from a passage in which he speaks of meeting with carnified portions of the size of a filbert or an almond in the midst of very crepitant pulmonary tissue. The occurrence of these he ascribes to "a slight inflammation in the first stage, the resolution of which, hastened perhaps by compression of the lung, has taken place in an irregular and imperfect manner." [1] This expression, while it shows that this great pathological anatomist had been puzzled to account for the lesion in question, will not be accepted as anything more than a hypothetical explanation of it. Very many writers, following Laennec for the most part in their pathology, have thrown this passage out of view altogether.

In 1829, M. Louis described the condition of the lung in a variety of cases of typhoid fever. These descriptions were repeated in his work on fever in 1841, in which the state of the lung in fevers was compared with that found in other diseases. The result of these inquiries was a most accurate description of the collapse of the lung as a state altogether different from pneumonia, and which was found not only in typhoid fever, but in a variety of other diseases; chiefly at the posterior part of the lung, but sometimes

[1] *De l'Auscultation Médiate*, vol. i. ch. v. art. 1, sect. 366.

disseminated (lobular). M. Louis offers no speculation or remark as to the origin of this affection.[1]

In 1830, Dr. Alderson, in a paper on the "Pathology of Hooping-Cough,"[2] pointed out most clearly the distinction between the lobular condensation observed in that disease, and true hepatization of the lung. "In hooping-cough the lung is always dense and contracted, as if the air had been expelled, and from the throwing out of adhesive matter the sides of the air cells had been agglutinated together; while in hepatization the lung is less dense than in hooping-cough, and is rendered more voluminous than in its natural state."[3] The inflation of the lungs would probably, in some cases at least, have clearly disproved this supposed agglutination of the air cells. The state of the bronchi is carefully described by Dr. Alderson. In one case, for instance, "most of the tubes were filled with a light yellow secretion, which, in the greater number, had assumed a concrete form, having very much the character of fibrine; in others, it was in the form of a thick puriform mucus; where it occurred in the concrete form, it adhered, though slightly, to the lining membrane of the tubes."[4] It will not be questioned that these descriptions concur entirely with the views already laid before the reader in this paper, as to the origin of the lesions in question.

The researches of Jörg, in 1832,[5] gave a new impulse to infantile pathology, by showing the frequent occurrence in new-born children of a state of deficient expansion of the

[1] *Recherches sur la Gastro-entérite*, i. 361; and *Recherches, etc., sur la Fièvre Typhoide*, i. pp. 328-334. It is worthy of remark that, from the observations in the preceding pages on the Edinburgh epidemics of 1847, as well as from Dr. Jenner's recent careful descriptions of cases observed in London (*Monthly Journal*, Feb. 1850, p. 115, *et seq.*), this form of pulmonary lesion does not appear to be peculiar to any type or form of continued fever, at least in this country.

[2] *Medico-Chirurgical Transactions*, vol. xvi. p. 78.

[3] *Loc. cit.* p. 91. [4] *Loc. cit.* p. 85.

[5] *De Pulm. Vitio Organico*, Leipz. 1832; and *Die Fötus-Lunge im Gebornen Kinde*, Grimma, 1835.

pulmonary lobules, having all the appearance of being congenital, but often persisting for some time after birth. This state, which Jörg called *atelectasis*, was identical in its characters with the collapse described above, and, like it, was often lobular in its distribution. It was figured as a disease of the new-born child by Cruveilhier,[1] who, however, understood nothing as to its real nature. Jörg was the first who pointed out the effects of inflation in this form of pulmonary affection as contra-distinguished from pneumonia. He ascribed the collapse to various causes, but particularly to weakness on the part of the child, and to the circumstances of a too precipitate birth, which he conceived, somewhat fancifully, to militate strongly against the establishment of respiration. In this last view he has had few supporters.

In 1838, MM. Barthez and Rilliet published a monograph on infantile pneumonia, which afterwards became the basis of the extended treatises in their great work on the diseases of children.[2] While these authors describe with great care and general accuracy the appearances of the carnified lung, whether lobular or diffused, they fall into the error, at that time universal in France, of considering it as a form of pneumonia; and their reference to the researches of Jörg is so slight as to render it probable that they were not aware of the important facts established by him. An interesting remark, in relation to the present subject, is, however, made by MM. Barthez and Rilliet, viz. that bronchitis, especially of the smaller bronchi, is a frequent concomitant of the pneumonia of children, *especially the lobular form* (collapse); the connection of bronchitis with the lobar pneumonia being more rare.[3] Similar observations had been made by M. Fauvel and other authors.

Rokitansky, writing in 1842, does not allude to any of the researches hitherto mentioned; nor does he appear to be sufficiently aware of the distinctive marks and real nature

[1] *Anat. Pathologique*, livraison 15, Plate II. Fig. 1.

[2] *Traité Clinique et Pratique des Maladies des Enfans.* 1843.

[3] *Op. cit.* vol. i. p. 75.

of collapse of the lung. In his description of *lobular, typhoid,* and *catarrhal pneumonia,*[1] however, the reader will trace many of the characters of this condition. In regard to the last of these affections, he says, "It is always lobular, concurring with catarrhal affection of the bronchial ramifications leading to the diseased lobules, and occurring frequently in catarrhal attacks in children, especially hooping-cough and catarrhus suffocativus." . . . "The lobuli affected are blueish-red, dense, and rather tough; the walls of the air vesicles are swollen, so as to obliterate their cavities, and contain, when less swollen, a sero-mucous, slightly frothy secretion; there is no trace of granulations." Rokitansky also notices the sinking in of the affected lobules, which he ascribes to an emphysematous condition of the surrounding parts.[2]

In 1844 were published the important researches of MM. Bailly and Legendre, before alluded to, which demonstrated the identity of the "lobular pneumonia" of children with the congenital collapse, or *atelectasis,* of Jörg. These authors also describe the catarrhal affections of infants as often attended with this change. But it is singular that, notwithstanding their own application of inflation of the lung to show the nature of this lesion, they consider it as produced, in some instances, by distension of the blood-vessels causing closure of the air cells. It is clear that under such circumstances inflation could not effect any considerable change.

The greater number of these authors, in so far as concerns the diseases of children, are passed in review by Dr. West in his late carefully elaborated, and, at the same time, original lectures.[3] The descriptions of Dr. West well deserve to be read, on account of their clearness and accuracy of detail; and his views as to the frequent connexion of the collapse of the lung with bronchitis, and its dependence, in many cases, on that affection, harmonize so closely with my own, that I can scarcely regret the late period at which

[1] *Path. Anat.* vol. iii. [2] *Path. Anat.* vol. iii. p. 106.

[3] *Lectures on the Diseases of Infancy and Childhood*, Lond. 1848.

I became acquainted with them,[1] as it enables me to point out more clearly the identity of the conditions in the adult with those in the child. "In the child," says Dr. West, "nothing more is needed than a copious secretion of mucus into the bronchi, or a feeble condition of the vital powers, to prevent the air from freely entering the pulmonary vesicles, and thus to induce the collapse of a large portion of the lung."[2] I have already endeavoured, in part, to show that in the adult the same causes are capable of producing the same effects; and I shall hereafter explain more at length what I conceive to be the exact mechanism of this change, both in the one and in the other. Dr. West adduces three cases, examined by Dr. Baly, of lobular collapse of the lung in the adult, in persons who died in a state of great exhaustion from fever and dysentery. In two of these cases there was much dyspnœa and distinct signs of bronchitis. The third is said to have presented no complication, and is considered by Dr. West to be the "result of simple debility." I shall refer to this point in the sequel.

On reviewing the whole of the facts here presented to the reader, I think that the frequency of collapse of the pulmonary tissue, both in the adult and the child, must be considered as established, and its connexion with bronchial obstruction rendered at least extremely probable.

I have yet to discuss the exact mechanism by which the collapse is, under these circumstances, produced, and the predisposing and other causes which determine its occurrence; but this subject will be treated after the next effect of bronchitis has been considered, as the mutual connexion of all the primary results of bronchial obstruction can then be more advantageously discussed.

I have endeavoured to show in the preceding pages that, among the pathological phenomena connected with obstruction of the bronchi by tough mucus, or other

[1] Dr. West's researches have come to my knowledge only since this paper was read, in its original form, to the Medico-Chirurgical Society.

[2] *Op. cit.* p. 183.

inflammatory products, one of the most important and frequent is collapse, to a greater or less extent, of the air-vesicles to which the obstructed bronchi lead. I adduced evidence that the condensation of the pulmonary tissue thus produced, which in its slighter degrees is often overlooked by anatomists, had in its more marked forms been described by many careful and exact observers under different names, and with various ideas of its pathological significance,—and that, especially in the case of children, it had been accurately distinguished from ordinary pneumonic condensation. I described the forms in which it had occurred under my own observation, and maintained that, in all essential characters, these lesions are the same in children and adults; that in both a certain degree of pulmonary collapse may be almost invariably found as a concomitant of fatal bronchitis; and that, in some cases, this state of the lung bears so obvious and undeniable a relation to obstruction of the tubes, as to lead to the almost unavoidable inference of the dependence of the former upon the latter.

That this conclusion from anatomical data is, in all respects, consistent, both with clinical experience and with correct views of the mechanism of respiration, I shall presently endeavour to prove; and, in the second part of this memoir, I propose to show the probable dependence of many important chronic alterations of the pulmonary texture on the condition of collapse from bronchial obstruction. The pathological history of this lesion, however, in relation to acute bronchitis, would not be complete without some notice of a condition which occurs so frequently in connexion with it, that I cannot consider it otherwise than as a secondary consequence of bronchitis, accompanied by pulmonary collapse.

Bronchial Abscess.

It not unfrequently happens that, in the centre of the collapsed lobules of a lung affected with acute bronchitis, there are found small collections of pus, varying in size from

that of a hemp-seed upwards to double or treble that volume. These small abscesses present, on section, an appearance so like that of softening tubercles, as to be very readily mistaken by many persons for these bodies; and the resemblance is all the greater on account of the peculiar limited form of the condensation by which they are generally surrounded, which, when felt by the touch from the exterior of the lung, is exceedingly deceptive. In their interior, however, these little abscesses contain, in the recent state, a very fluid pus; moreover, they are often met with as acute lesions produced by a few days of illness, and without a trace of tubercle in any other organ. This is peculiarly characteristic in the young child, in which tubercle of the lung so very rarely occurs without extensive deposits in the bronchial glands, whereas the present lesion is accompanied in its pure form by nothing more than slight enlargement. When the pus contained in these abscesses, in their recent form, is pressed or scraped out, they are seen to be lined with a fine villous false membrane, very different from the thick curdy mass which generally surrounds softened tubercles; in others they are not abruptly limited at all, the pus appearing to lie in contact with the surrounding pulmonary tissue. When the bronchi leading to the lung so affected are carefully incised, they are found much inflamed; the mucous membrane vascular, thickened, and covered with pus; and some of the bronchi thus affected can be observed to communicate with these purulent collections, the mucous membrane having evidently been, at the point of communication, destroyed by ulceration, and either stopping short abruptly, or becoming gradually incorporated with the false membrane lining the abscess. Sometimes these abscesses are found to break into one another, and form more considerable excavations; in one instance I found them connected with a gangrenous condition; more commonly, however, they remain of limited size, preserving perfectly the direction and relations of the bronchial tubes.

These abscesses occur in the diffused as well as in the lobular form of condensation from collapse; and both

forms may sometimes be seen in the same lung. A similar lesion may accompany true pneumonia, but always in those cases where it is combined with intense bronchial inflammation. In this case, also, it tends to the formation of more considerable and more irregular excavations.

Such cavities have been pretty accurately described by Barthez and Rilliet (as well as by other French writers on the bronchitis and pneumonia of children), under the name of "*vacuoles.*" They are, however, far from uncommon in adults, though, perhaps, more frequent in the so-called "lobular pneumonia" of children. As these "vacuoles" unquestionably arise from the accumulation of pus primarily in the extreme bronchial tubes of the collapsed lobules, the name of bronchial abscesses may serve to distinguish them from other forms of excavation of the lung.

That Laennec should have overlooked these small excavations is singular enough. Probably he may have considered them a form of tubercle, a term which has been used by him and others with sufficient vagueness to cover a multitude of anomalous lesions. His assertion that in many hundreds of pneumonic lungs, he had only met with collections of pus five or six times, can only be explained upon this principle. It is, indeed, quite true that in simple pneumonia there is little tendency to the formation of abscess, the pus escaping rapidly by the pervious bronchi; but as we have seen that Laennec has probably confounded the pneumonic with the bronchitic condensation, I think it also probable that he has not distinguished the bronchial from the tubercular abscess, especially as these lesions often resemble each other so much as to have led me habitually, for some time, to call the former *tuberculoid disease* of the lung, which name is inscribed over several dissections in the pathological register from which the following cases are taken.

In three of the four cases of fatal hooping-cough recorded by Dr. Alderson, in the paper formerly alluded to, these lesions are described shortly as dilatations of the smaller bronchi by thick mucus or muco-purulent secretion. Most

of the French writers on the pneumonia and bronchitis of children since Barthez and Rilliet, refer to them more or less distinctly ; and in Dr. West's lectures they are described as a *true* lobular pneumonia—the result, however, of bronchitis. As the work of Dr. West is probably accessible to most readers, I do not think it necessary to transcribe the passage, which contains, however, a very accurate description of the lesion in question (see p. 174, *op. cit.*). In regard to the characters of these abscesses, as distinguished from tubercle, my observations, as detailed above, almost exactly concur with those of Dr. West. In one point alone I am disposed to differ from him. He seems to regard the abscesses as not formed in truly *carnified* lung, because the condensed portions are not always *exactly* circumscribed by the interlobular tissue. I believe this appearance will be fully accounted for by the fact that when bronchial abscesses occur, the bronchitis is usually general and intense, and the collapse correspondingly diffused. Besides, I have found bronchial abscesses, and their results, in exactly circumscribed lobules, in repeated instances ; and that the tissue in which they occur is mostly collapsed there can be no doubt, from the whole of its ordinary and microscopic characters.

The rapid supervention, and sometimes equally rapid evanescence, of dulness on percussion, limited or diffused, in the midst of attacks of general bronchitis of greater or less intensity, and when the respiratory sounds are muffled or supplanted by large and small mucous râles in every part of the chest, is an occurrence which has impressed itself on my recollection in numerous instances,—of most of which, however, I have no records, or only notes too meagre to be placed before the reader. Such an event comparatively seldom occurs in the acute catarrh of healthy adults ; but, as a sequela of measles and hooping-cough in children, it must be familiar to every one ; and I have seen it again and again in adults, in the bronchitis of fever,—in that which often terminates in Bright's disease, or disease of the heart,—and still more frequently in those obscure and

treacherous chest affections, which supervene in the last stage of exhaustion from hectic or malignant disease, and which are the accompaniments, rather than the active causes, of breaking-up of the enfeebled frame. Such affections have commonly been called latent pneumonia, and they undoubtedly may be so in some instances; but much more commonly they are nothing more than bronchitic collapse, determined by obstruction so slight as would in an otherwise healthy individual be easily overcome, and which, even in these emaciated and bloodless subjects, is only *not* overcome because the exhaustion is great and the need of respiration small,—the circulation being at a low ebb, and the attenuated and sluggish blood requiring very much less air, and consequently less lung, for its renovation than under ordinary conditions. Again and again has it occurred to me, under such circumstances, to open the bodies of persons in whom no suspicion of a respiratory affection existed during life, and in whom, nevertheless, condensation, abruptly lobular or diffused, having often the characters of the "peripneumonie des agonisans," or the "peripneumonie hypostatique" of M. Piorry (both of them varieties of the bronchitic collapse), has been found after death affecting large portions of one or both lungs. On the other hand, I can remember instances in which a superfluous zeal, or *nimia diligentia*, in stethoscopic studies, has detected the signs of these lesions, when not a single rational symptom existed to call for such examination; and very many cases where the extent of the lesion was altogether out of proportion to the gravity of the symptoms that attended its accession.

Apart, however, from such latent, or very obscure cases, it has often been remarked of late years, that the pneumonic affections springing from or accompanying bronchitis, are apt to present a peculiarly asthenic or typhoid character. So much has this been the case in France, that within the last few years "pneumonie catarrhale" and "pneumonie typhoide" have become almost synonymous terms with some authors. Seeing that a great number of cases of so-

called broncho-pneumonia which have come under my notice have been instances, not of true pneumonia, but of bronchitic collapse, sometimes combined with engorgement or œdema of the lung, and sometimes with bronchial abscesses, I am inclined to believe that the solution of the asthenic or typhoid character of this disease will be found in a modified view of its pathology. The fact is, that in adults the tendency in bronchitis to complete collapse of the lung is comparatively small, excepting in exhausted or debilitated constitutions, or under the influence of typhoid affections of the system. In these the asthenia is not a result of the disease, but one of its most essential predisposing causes.

In the child, again, owing to causes which will be presently mentioned, collapse, in its most complete form, very readily occurs under all circumstances, as a consequence of acute bronchitic attacks. In commenting upon this peculiarity, Dr. West, whose opinion on this subject I have before referred to, remarks that it is by the collapse of the lung "that we must explain many of the instances in which urgent dyspnœa, and all the symptoms of serious pulmonary disease, have developed themselves in the course of a few hours out of what had seemed to be a severe cold, or a bronchitis of moderate intensity. This, too, accounts for the occasional supervention of dulness on percussion, and of bronchial respiration in the child; so that you may discover them in the morning in a situation where, overnight, the percussion was good, and no sound of graver import than large crepitation."[1] To this remark I would only add, that dulness on percussion supervening under such circumstances is not necessarily attended with any increase of fever; and that careful attention to the state of the patient in this respect may be in some cases the only mode of forming an opinion with any degree of accuracy as to the presence or absence of true pneumonia in a case of bronchitis. Of this fact a striking instance occurred to me lately in the case of a little boy, three years old, whom I attended during

[1] *Op. cit.* p. 183.

an attack of acute bronchitis. He was a delicate child, and had suffered several times from bronchitic attacks. His chest presented a good deal of deformity, particularly on the left side, of the kind called "pigeon-breast." On the fourth day of the attack, which had all the usual acute characters during the first three days, decided dulness on percussion, with obscurity of respiration, appeared in the right back; the dyspnœa continuing considerable, but *the fever rather abating than increasing*. Under the influence of an emetic, and a forced decubitus on the opposite side to that on which the dulness existed, almost every trace of it had disappeared in thirty-six hours, and the dyspnœa was entirely relieved.

If these views be correct, they cannot be without practical importance. The rules, both of diagnosis and treatment, in regard to bronchitis and its complications, must probably undergo some modifications, in order to be safely followed. Such an axiom as the following, at least, which is one very generally conceived to be unquestionable, will not, I believe, stand the test of renewed observation, if the fact of collapse of the lung from bronchial obstruction be kept in view. "The stroke-sound of the chest," says Dr. Williams, "is not materially impaired by catarrh; and, accordingly, the partial suspension of the breath-sound in a part of the chest in this disease *cannot be mistaken for that caused by hepatization*." [1] The reader will at once perceive that the clearness of the stroke-sound here alluded to is not always, perhaps not even generally, to be expected under such circumstances; nor can the opposite condition, even when combined with diminished or suppressed respiratory murmur, or with bronchial respiration, be allowed necessarily to indicate hepatization. The judicious physician has doubtless often been saved from error in the application of his remedies in this, as in many other cases where an unsound pathology has prevailed, by adhering to the great

[1] *Williams on Diseases of the Chest*, 4th edition, p. 80. Similar rules of diagnosis are given by Laennec, Skoda, Watson, and most other writers.

principles of our art, and the teaching of personal experience, rather than to received formulas and rules on matters of detail. But it is difficult to escape from the conviction, that the influence of a name, or the vanity of an *exact* diagnosis, may have misled many into such errors; and that in cases of supposed broncho-pneumonia or typhoid pneumonia, the practice may frequently have embraced blood-letting, heroic antimonials, or calomel and opium, when emetics and expectorants, with suitable stimulation, would have been better adapted to the circumstances of the case. This will be still more evident on considering the mechanism of these affections.

Since the preceding observations were in type, I have received the recent work of Dr. Fuchs, on the bronchitis of children,[1] which in its elaborate descriptions, both of symptoms and post-mortem appearances, furnishes additional evidence of the connexion of collapsed lung with bronchitis. The state of the lung, called "lobular pneumonia" by most authors, and "état fœtal" by Legendre, is regarded by Fuchs as a direct consequence of bronchitis; and its relation to bronchial obstruction on the one hand, and to peculiarities in the infantile system on the other, is certainly more clearly stated than by any previous author. To distinguish this lesion from the congenital form of collapse, as well as from other varieties of condensation, Fuchs proposes the term *apneumatosis*;—but, although evidently anxious to point out a sufficient anatomical ground of distinction between the *unexpanded* and the secondarily *collapsed* lung, he admits that the *diagnosis* must rest chiefly on a consideration of the cause—the one being congenital and the other acquired. Under these circumstances it will probably, I think, appear to English readers unnecessary to burden the science of pathology with another scholastic term; and I am convinced that careful

[1] *Die Bronchitis der Kinder*, etc., von Dr. Caspar Friedrich Fuchs. Leipzig, 1849. The reader will find a notice of this work in the last No. of the *Brit. and Foreign Med. Chir. Review.*

examination of both lesions will convince most observers that Legendre and Bailly are correct, and that there is no real ground for distinguishing them, excepting what may be inferred as to their mode of origin.

The simplicity and clearness of the pathological views entertained by Fuchs, as compared with most other writers on this subject, and the highly original character of his work, render it a most important contribution to the history of bronchitis; and as such, I shall have occasion to refer to it in the sequel. In the meantime, it is only necessary to say that he enumerates three stages of *apneumatosis*, each of which is described at great length. Into the anatomical description of these I do not mean to enter, as it refers exclusively to children below the age of five years. The reader of the original work will see many points of similarity between the characters there given and those which I have indicated as distinguishing bronchitic collapse in the adult as well as the child. In the case of children under five years, Dr. Fuchs, indeed, denies having seen a true pneumonic condensation; and he appears also to have passed over, very lightly, the lesion above described as bronchial abscess; the only allusion to it being in p. 114, where he remarks, cursorily, on "the accumulation of yellow mucus in the bronchi and air-cells, *and their dilatations*;" this being, as he declares (in opposition to Friedleben, as well as to other authors), the only form of suppuration in the lung observed in early infancy. These assertions are entitled to due consideration, but can scarcely be accepted without further, and even more careful, examination.

It appears from the work of Dr. Fuchs, that, in 1837, a year before the publication of the monograph of Rilliet and Barthez, Seifert[1] had given an excellent description of infantile bronchitis, and of the peculiar pulmonary lesions in which it commonly terminates, which he considered to be a peculiar form of pneumonia. This work seems to have excited some attention in Germany, and it is not a little

[1] *Die Bronchio-pneumonie der Neugebornen.* Philipp Seifert. Berlin, 1837.

remarkable, under these circumstances, that the close relation of these lesions to the congenital affection described by Jörg, should have been overlooked until the observations of Legendre and Bailly, in France, seven years afterwards. Seifert also noticed the resemblance of the "broncho-pneumonia" of children, to the "peripneumonie des agonisans" of adults; and seems to have described the bronchial abscesses as a stage of suppuration.

Mechanism, Causes, etc., of Bronchitic Collapse.—The object of the remarks hitherto made, in reference to this subject, has been chiefly to establish the frequent coincidence and probable relation of cause and effect between the obstruction of the bronchi and the collapse of the air-vesicles. The nature and rationale of that connection now fall to be more particularly considered.

When a bronchial tube is in any way obstructed, or much diminished in caliber, at one or more points, the question arises, what is the mechanical effect of the movements of respiration upon the amount of air thus partially imprisoned behind the obstructed part? If the obstruction be complete, of course no change can take place, at least mechanically; but in the case of its being, as it usually is, incomplete, it may be supposed that the inspiratory act tends to draw in more air than the expiration can expel, and consequently that the air tends to *accumulate* in the vesicles; or, *vice-versa*, that the air behind the obstruction tends constantly to *diminish* in amount, owing to the comparative inefficiency of the inspiratory act; or finally, that the forces equalise each other, and the quantity of the enclosed air *remains unaltered*.

Now, it cannot be denied that from the pathological anatomy of bronchitis, a *prima facie* case might be made out for each or all of these theories; for, although only one side of the question has hitherto been brought prominently forward in the preceding part of this paper, the reader will not have failed to remark that in several cases in which bronchitic collapse of the lungs existed, other

parts of the same organs were affected with emphysema or dilatation of the air-cells. Indeed, so familiar is this conjunction of emphysema with bronchitis, as to have suggested to Laennec, long ago, the first of the three theories above mentioned as to the cause of dilatation of the air-vesicles in that disease. Again, fatal cases of bronchitis undoubtedly occur, at least in adults, in which there is no change of the pulmonary texture so marked as to afford support to either of the first two theories.

Laennec's view of the consequences of obstruction was founded on the idea of the comparative weakness of the expiratory, as compared with the inspiratory, forces. "The mucus secreted into the bronchi, in consequence of pulmonary catarrh, must, especially if it is very viscous, present a great resistance to the free passage of air in inspiration and expiration; and we shall show, in speaking of the râle, that this resistance often goes the length of producing complete, though momentary, obstruction of a part of the bronchial ramifications. Now, as the muscles which subserve inspiration are strong and numerous, while expiration is produced only by the elasticity of the parts, and the weak contractions of the intercostal muscles, it must necessarily happen that the air, which has been forcibly driven beyond the obstruction in inspiration, will not be able to overcome it in expiration, and will be in a manner imprisoned, by a mechanism not unlike that in the butt-end (condenser) of an air-gun." I have given this passage from Laennec entire, because, notwithstanding the palpable fallacy it contains, it has been referred to, and its conclusion adopted without comment, by almost every systematic writer in this country as well as in France. The fact is, however, that though *ordinary* inspiration is more of a muscular act than *ordinary* expiration (merely because in the latter there is little or no resistance to be overcome, to which the elastic subsidence of the parietes is not adequate), yet the residual effective force for overcoming adventitious obstruction is very considerably greater in expiration. The *forced* or *muscular* expiratory act is, in fact, about one-third more

powerful, as measured by its effect upon a pressure-gauge, than the extreme force of inspiration ;[1] and it is this force which is thrown into action when obstruction in the tubes is to be overcome. In the act of coughing, moreover, we find a beautiful mechanism, by which the air within the vesicles is discharged outwards at a *maximum amount* of pressure, and brought to bear with all the additional mechanical advantage of a sudden impulse, on every obstructing substance within the bronchial tree,—a cumulative provision which does not exist in the case of the inspiratory force. There can be no great difficulty, therefore, on these grounds, in coming to the conclusion, that the data of Laennec's hypothesis are quite erroneous, and that the practical efficiency of the expiration in forcing air through obstructions must be, *cæteris paribus*, far greater than that of the inspiration. I have already alluded to the fact, that this is consistent with general experience ; for while the inspiratory act is always, in bronchitis of considerable intensity, attended with extreme difficulty, the expiration is never so.

The question of the origin of emphysema of the lung will be considered in the second part of this memoir, in which I shall endeavour to account for its connexion with bronchitis, by referring its production to a totally different mechanism from that just mentioned. In the meantime, I would remark that, in order to establish a direct relation between this lesion and bronchial obstruction, it is necessary to show not merely that emphysema occurs in connexion with bronchitis, but that it occurs especially or exclusively *in those parts of the bronchitic lung where obstruction can be shown to exist.* This is the proposition which I conceive the preceding pages have tended to establish as regards bronchitic collapse, and on the ground of which I have argued for the relation of cause and effect between this lesion and obstruction. In how far does emphysema fulfil these conditions ?

[1] See the numerous experiments of Hutchinson and Mendelsohn, quoted in Dr. J. Reid's article on "Respiration": *Cyclop. Anat. and Physiol.* Part xxxii. p. 336.

It is well known to every one who has studied the anatomy of this pathological state, that the emphysematous portions of a lung can generally be inflated from the bronchi with the greatest ease. Indeed, so far as my own experience in this matter is concerned, I cannot recall any instance in which the pressure of the air was not found to reach the emphysematous parts with as great rapidity as the rest of the lung. In the collapsed lung, on the contrary, as I have already shown, very considerable resistance is often opposed to its inflation from the bronchi—a resistance only to be overcome by pressure many times greater than can ever occur in the vital act of inspiration. If this observation be correct, it is plain that the emphysematous parts of the lung are usually free, the collapsed parts obstructed.

Further, I cannot find that any unequivocal instances have been adduced, to prove that an obstruction, confined to a part of a lung, or to one lung only, is commonly accompanied by a corresponding distribution of emphysematous portions—a proposition which, if true, might surely be easily verified from the records of pathology. So far from this being the case, the habitual seat of emphysema leads to an inference of a directly opposite kind—a point which appears to have been overlooked in the discussion of this question. Let the reader reflect that, in the vast majority of instances, the seat of election of emphysema is the *anterior* border of the lungs, while the stethoscope, as well as the results of *post-mortem* examination, show that accumulations of mucus in bronchitis occupy in an equally numerous proportion of cases, the *posterior* and *lower* parts, which are also, especially in the adult, the principal seat of the bronchitic collapse.

Cases of the impaction of foreign bodies, and other palpable obstructions of the bronchi, are generally recorded with too little attention to the condition of the lungs to be available for the present discussion. Carswell has, however, figured the case of a monkey, in which the left bronchus was much compressed, or rather obliterated, by a mass of tuberculous glands; in this case the corresponding lung had diminished to less than a third of its normal bulk, while the *opposite*

lung, of which the bronchus was free, presented emphysema in several places.[1] Andral has adduced, as before mentioned, two cases of obstruction of the upper lobe of one lung, where, from the stethoscopic phenomena, he supposed emphysema to be present, but where the examination after death proved that this was not the case.[2] He has also recorded a case[3] in which the bronchi of the right lung were compressed by a melanotic mass, and the respiratory murmur greatly enfeebled. In the short note of the appearances after death, no notice is taken of any abnormal condition of the lung in this case. Andral, indeed, states (p. 196) that emphysema is one of the consequences of stricture of the bronchi; but adduces nothing whatever in proof of this assertion, which evidently rests on the ground of Laennec's theory.

All doubt, however, as to the real effect of a solid obstruction in the bronchi on the air in the lung is removed by the direct experiments of Mendelsohn and Traube on animals.[4]

The former inserted a leaden shot into the trachea of a dog, pushing it down as far as possible into the bronchus with a probe. In another instance he inserted a ball of paper. In both cases, the parts to which the obstructed bronchi led were red and void of air. In the former there were emphysematous portions in the other parts,[5] and in the opposite lung.[6] Traube's experiments were similar, but

[1] *Illustrations of the Elementary Forms of Disease—Atrophy.* Plate IV., Fig. 3.

[2] *Clinique Médicale*, v. 2, pp. 187-190. [3] *Ibid.* p. 193.

[4] For an account of these experiments I am indebted to the work of Fuchs, not having access to the original sources.

[5] The expression of this passage is not quite clear, but this is certainly the meaning, and corresponds with the author's inference.

[6] *Der Mechanismus der Respiration und Circulation*, p. 37. Berlin, 1845. Mendelsohn also threw a solution of gum into the air passages of an animal, with the result of collapse of some portions of the lung. In one instance I tried this experiment upon a rabbit, with a similar result; but the difficulty of limiting the fluid to particular parts of the lung makes these experiments less valuable.

more numerous. The general result was, that the artificial obstruction of a bronchus always produced expulsion of the air from the corresponding part of the lung, which had a dark-red colour, and presented the characters of collapse.[1]

It is clear, therefore, from experiment, as well as from pathological observation, that the most usual and most direct effect of obstruction, or of diminished caliber of the bronchi, however caused, is not accumulation but diminution in quantity of the air beyond the obstructed point. It is probable that this is due in part to the comparative weakness of the inspiratory power, and that the proposition of Laennec may, therefore, correctly enough be inverted. There is also, however, another mechanical condition which comes into play in producing collapse from obstruction, especially in the case of a viscid plug of mucus, which is most commonly, in bronchitis, the source of this affection. This condition is to be found in the form of the tubes.

The bronchi are a series of gradually diminishing cylinders, dividing, for the most part, dichotomously. If a plug of any kind, but especially one closely adapted to the form of the tube, and possessing considerable tenacity, be lodged in any portion of such a cylinder, it will move with much more difficulty towards the smaller end, and in doing so will close up the tapering tube much more tightly against the passage of air, than when moved in the opposite direction into a wider space. If such a plug be placed over a bifurcation, it will, even if freely moving in the larger space in which it lies, be of sufficient bulk to fall back upon one or other of the sub-divisions during inspiration, in the manner of a ball-valve upon the orifice of a syringe, and thus completely to occlude it. (See fig. 26.) The consequence of this mechanical arrangement must inevitably be, that at every expira-

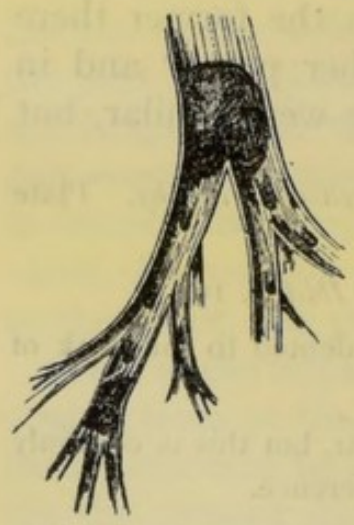

Fig. 26.

[1] *Beiträge zur experimentellen Pathologie und Physiologie*, 1 Heft.

tion a portion of air will be expelled, which, in inspiration, is not restored, partly owing to the comparative weakness of the inspiratory force, and in part to the valvular action of the plug. If cough supervene, the plug may be entirely dislodged from its position, or expectorated, the air of course returning freely into the obstructed part; but if the expiratory force is only sufficient slightly to displace the plug, so as to allow of the outward passage of air, the inspiration will again bring it back to its former position, and the repetition of this process must, after a time, end in perfect collapse of the portion of lung usually fed with air by the obstructed bronchus.

It is not a little surprising that this simple and clear mechanical mode of explaining the collapse should not have occurred to Dr. Fuchs, who, in accounting for the disappearance of the air in the experiments of Mendelsohn and Traube, finds himself reduced to the theory of its absorption into the blood-vessels. (*Op. cit.* p. 63.) Surely nothing can be more superfluous than such an explanation.

In considering, as a whole, the causes which tend to produce bronchitic collapse (as revealed in the preceding investigation), they seem to resolve themselves into the following: *Firstly*, the existence of mucus in the bronchi, which is more liable to produce obstruction according as it is tenacious and viscid; *secondly*, weakness, or inefficiency of the inspiratory power, however caused; *thirdly*, inability to cough and expectorate, and thus to remove the obstructing mucus. Of these conditions the first must be considered as the immediate exciting cause, the others as predisposing causes, co-operating with the first, but incapable without it of producing collapse. Of the exciting cause enough has already been said. The inability to expectorate is obviously enough a formidable condition, and may be owing either to simple debility, or to a laryngeal affection. But I have still a few remarks to make on the circumstances producing inefficiency of the inspiratory act, and their bearing on the present subject.

The inspiratory act is apt to be rendered inadequate from

several causes. Of these the most obvious is weakness of the muscles of inspiration, usually concurring with general debility. I have already pointed out the great predisposition to bronchitic collapse which arises from an exhausted frame; so much so, that a barely appreciable amount of bronchitis, nay sometimes, I believe, the mere accumulation of the natural mucus in a debilitated subject (as in an individual near death), will give rise to a considerable extent of the pulmonary lesion. I cannot, however, see reason to believe with Dr. West, that mere debility, apart from any obstruction in the tubes, is a sufficient cause for collapse in the child. The very fact of the lesion being usually more or less lobular, or partial in its distribution, appears to indicate special circumstances of a local kind, as having a marked influence on the production of this affection; and on this ground, as well as that of theory, I am disposed to think that, in the cases ascribed to debility alone the evidences of more or less obstruction might have been detected during life or after death. That this, however, is often of a very slight character, leading to scarcely any symptoms, and probably in some cases undiscoverable, except by physical examination, I have already indicated.

A second circumstance tending to render the inspiratory act ineffective is distension of the abdomen, impeding the descent of the diaphragm. The influence of this condition in the production of collapse I have repeatedly witnessed; having had occasion to observe that when ascites, or any other cause of similar distension, is present, a very slight amount of bronchitis will determine extensive pulmonary collapse. One of the first cases that awakened my attention to the subject of this memoir, was that of a boy of 17 years of age, who died of an enormous medullary tumour of the abdomen, and in whom a most marked form of lobular collapse was found distributed over both lungs, with a good deal of thick mucus in the tubes.

A third cause of inefficiency of the inspiratory act, and one of the greatest importance in relation to this subject, is the want of due resistance on the part of the thoracic

parietes. The full dilatation of the lungs is only effected when the depression of the diaphragm is accompanied by the elevation of the ribs and widening of the thorax; and if the bones of the latter be very yielding, the external muscles of inspiration cannot, of course, act effectively under an obstruction. This is obviously the reason of the greater tendency in children to collapse of the lung as a consequence of bronchitis. The respiration of the child is at all times, even in health, more diaphragmatic than that of the adult; and the observations of Rilliet and Barthez [1] afford satisfactory evidence of the comparatively small dilatation of the thorax in children, particularly of its lower part. When any obstruction exists to the entrance of air into the chest, even this small dilatation ceases, and collapse of the lung very readily takes place. Under these circumstances, Dr. Rees [2] has pointed out that in very young children the motions of the chest are absolutely reversed, and instead of the descent of the diaphragm being accompanied by expansion of the chest, the ribs give way beneath the exhaustion caused by it within the thoracic cavity, and bend inwards to accommodate themselves to the collapsed lung in inspiration. The altered movement of the chest in infants is regarded by Dr. Rees as pathognomic of *atelectasis*. It is also a prolific source of that permanent deformity of the chest which, in the early years of life, is often ascribed, with too little discrimination, to rickets.[3] Of this deformity I shall have something to say in the second part of this paper.

As to the so-called *atelectasis*, I have had but few opportunities of observing it accurately during life. I may, however, remark that in respect to its causation, it probably differs but little from the acquired collapse, and close

[1] *Op. cit.* vol. iii. pp. 643-4. [2] *Atelectasis Pulmonum.* Lond. 1850.

[3] Rilliet and Barthez describe a reversed movement of the ribs in inspiration as taking place in rickets. There can be little doubt that rickets, combined with chest affections, form a frequent source of the deformity; but the presence of the latter is probably essential. Vol. iii. p. 646.

examination would probably show that mere debility, without some obstruction in the bronchi, is as inadequate to prevent the expansion of the lung as to cause its collapse. The cases published by Jörg himself, although very imperfectly observed as regards physical signs, will, I think, on perusal, convince a careful reader that there is abundant ground for this opinion; but undoubtedly the subject requires renewed investigation.

Origin of Bronchial Abscess.—The mechanism of this lesion it is not difficult to explain satisfactorily. When pus accumulates in the central bronchi of a collapsed lobule, the evacuation of that pus is prevented from occurring, firstly, in consequence of the absence of the expiratory *vis a tergo*; and secondly, from the resistance opposed by the thickened mucous membrane and its secretion, closing up the bronchus in front. The coats of the ultimate bronchi, therefore, softened and injured by disease, gradually give way to ulceration; and the pus, which thus accumulates in still larger quantity, may at first scarcely be circumscribed, but soon begins to be surrounded by a false membrane exactly similar to that of an abscess in any other part of the body. The continuity of this membrane with that of the original bronchus, may be either maintained from its first formation, or it may be secondarily established. I believe, however, that the first of these views is the correct one; and that the bronchus acts the part of an obstructed fistulous opening, not sufficiently pervious to prevent accumulation entirely, but not permitting of its increase beyond a certain amount.

When the bronchial abscess has been of some standing, and the patency of the tube leading to it has become re-established in time to prevent its obliteration, a process of repair takes place, analogous to the cicatrization of a wound, and perfectly similar to that which is observed in all healing excavations in the lung, however formed. The false membrane which lines the cavity becomes intimately blended with the bronchial mucous membrane, and indeed comes to resemble it so closely that it is almost impossible to tell

where the true mucous membrane ceases and the new structure begins.

This reparation, however, is rarely, if ever, accompanied by restoration of the perfect function and structure of the lung; and on this account it will be considered under the permanent effects of bronchitis.

The length to which these remarks on collapse of the lung have extended, can only be excused by the immense importance of this lesion in relation to the pathology of bronchitis; the whole of the organic affections following from which seem to me more or less dependent on that which has formed the principal subject of these observations. These secondary effects of bronchitis and bronchitic collapse will form the subject of a second part of this memoir, as indicated at the commencement of it.

Part II.—Secondary or Permanent Disorganisations depending on Bronchitis.

In the foregoing pages on the affections of the pulmonary texture resulting from bronchitis I have treated, at considerable length, of the collapse of the air-vesicles connected with obstructed tubes,—a lesion of which, as I have endeavoured to show, the true pathological significance has been much neglected or misapprehended, even by authors who have correctly enough described some of the morbid appearances. The more chronic and permanent lesions connected with long-continued bronchitis have now to come under consideration; and although these affections cannot be said, for the most part, to have engaged less than a due share of the attention of pathological writers since they were made familiar by the descriptions of Laennec, yet it will appear from the sequel, that the links which bind them together in a series cannot be understood without reference to those primary results of bronchitis to which I have already adverted. In describing these affections, therefore, I shall avoid as much as possible dwelling on descriptions already familiar to well-informed medical readers, confining myself,

for the most part, to the pathological considerations which flow more or less directly from what has already been advanced in the first part of this memoir.

Results of Bronchitic Collapse of the Lung—Curability of Pulmonary Collapse.—There can be little doubt that the condition of collapse of the air-vesicles, from obstruction of the bronchi, may, when recent, be completely removed, and give place to the normal condition of the pulmonary texture. The imitation of this result, by forcibly inflating the lung affected after its removal from the body, not only proves the absence of any organic change, but shows conclusively that it is in many instances only a sufficiently strong inspiratory force which is required to disperse the obstructing mucus, and make a free passage for the entrance of air into the lung. The collapsed lung, however, is placed under a most serious disadvantage, as compared with that which contains air, in freeing the bronchi from causes of obstruction. The latter can render available the expiratory force, and this, when aided by the impulsive effort of coughing, is by far the most efficient agent in displacing and removing the bronchial plug, which it carries outwards, and expels altogether from the system. The completely collapsed lung, on the other hand, can only bring into play the inspiratory force, a weaker power acting against a greater resistance, and able, at the best, only to disperse inwards, never directly to remove, the cause of obstruction. Under these circumstances, it might seem probable, that a lung when once brought into a state of complete collapse, should be mechanically incapable of perfect recovery ; and even in minor degrees of the affection, it would appear that the tendency *inwards*, or towards the lung, of mucus, and all other obstructions, must greatly increase in proportion as the residual quantity of air in the vesicles diminishes. Again, where the obstruction is much localised, as in limited and lobular collapse, the mechanical forces tending to remove it, whether inspiratory or expiratory, will at all times be apt to diffuse themselves over the surrounding normal or

comparatively unobstructed lobules, so that the removal of the bronchial plug under such circumstances is not easily understood, if we take into account only the forces we have hitherto been considering.

Does collapse of the lung, then, necessarily, or in the majority of instances, lead to organic and permanent change of structure ? This doctrine I should be very slow to admit. A consideration of the cases in which bronchitis occurs, and is even repeated frequently in the same individual, without appreciable permanent change, while we know, from post-mortem appearances, and understand on mechanical grounds, that accumulation even to a very moderate extent in the bronchi is often sufficient to cause a certain amount of the lesion, will, I think, even in the absence of more detailed clinical experience, constitute a strong case for believing in the existence of some more active remedial and conservative mechanism in such cases than that of the inspiratory and expiratory forces. Such a view is altogether borne out by the observations of writers on the bronchitis and lobular pneumonia of children, which, though often a grave, and even a fatal affection, is never regarded as being, in favourable cases, less capable of perfect resolution than any other form of pulmonary condensation. The remarks of most other practical writers are so much governed by pathological views, differing from those we have been considering, that it is quite impossible to eliminate the information they may contain as to the results of pulmonary collapse. I have already remarked that a great number of the varieties of so-called catarrhal and typhoid pneumonia are undoubtedly affections of this kind, sometimes combined with genuine pneumonia, and sometimes uncomplicated ; and in particular, that the hypostatic pneumonia of M. Piorry, and the " peripneumonie des agonisans " of Laennec, are generally instances of the diffused form of pulmonary collapse. The former observer has devoted so much attention to the observation of this particular form of disease, as to render his remarks valuable, even though probably modified by an erroneous pathology. He says : " The first stage of

hypostatic pneumonia, while the blood is still contained within the vessels (*i.e.* while no exudation has occurred into the air cells) is *very susceptible of cure*; indeed, it may be said that this state exists *in a large number of invalids* (chez beaucoup de malades), and *is dissipated during convalescence.*" [1] The other stages, especially the third and fourth, in which solid or purulent deposits exist in the air-cells, are, according to M. Piorry, more grave, and even generally incurable; but these again are obviously not uncomplicated instances of pulmonary collapse. I shall not at present enter more fully into the discussion of M. Piorry's views on this subject, than to remark, that the passage above quoted is in harmony with all that has been already submitted to the reader, more particularly with the observations alluded to in the last volume of this *Journal*, pp. 234–6. Indeed, I cannot entertain a doubt, judging from the facts there mentioned, that a more extensive and exact clinical experience bearing on this subject will demonstrate the extreme frequency, and in many cases the easy and rapid removal, of a certain degree of pulmonary collapse, which may or may not have led to serious symptoms during life.

De-obstruent Function of the Bronchial Tubes.—Supposing these views correct, the mechanism by which the viscid mucus is expelled to such an extent as to permit the return of air into the occluded vesicles, demands further consideration. We have seen that the expiratory forces are, under such circumstances, thrown out of action; while those of inspiration, even if strong enough to displace the obstructing plug, can never permanently remove it. Under these circumstances, it seems to be reasonable to ascribe to the bronchi themselves an active part in the expulsion of obstructive mucus, by means of the slow contraction of those circular fibres, the muscular character of which was demonstrated by Reisseisen, and whose physiological properties have been fully illustrated by the experiments of Dr. Williams and others. It is now well established, that these fibres have no such vital endowments as would enable

[1] Piorry, *Pathologie Iatrique*, vol. iv. p. 411.

them to co-operate with the movements of respiration, influenced as these are by the will. "The contractility," says Dr. Williams (of the bronchi), "resembles that of the intestines or of the arteries more than that of voluntary muscles or of the œsophagus, the contractions and relaxations being gradual and not sudden. They are, however, much less tardy than those of the arteries." [1] This kind of contractility is precisely that which empties the arteries of their blood after death, and which, in all probability, contributes to the passage of calculi along the ureters or gall-ducts. It is also more or less analogous to the peristaltic contraction of the intestines, or of the elongated tubular uterus of many of the lower animals, by which the solid or fluid contents of these viscera are gradually expelled towards their outlet. The experiments referred to appear to prove that the contractility of the air-tubes is readily excited, not only by galvanism applied externally, but by mechanical and chemical stimuli in contact with their mucous membrane. It is easy, therefore, to understand, that the bronchi (or at least those which have not cartilaginous walls) may have a most important power of dislodging obstructions altogether independent of the forces of respiration. When these forces are in active operation indeed, the tonic or slow contraction will be in abeyance, or very slightly manifested, as the air-tubes will then be dilated to their full extent at each inspiration and expiration. But, according as the admission of air to any part of the lung becomes less from obstruction, the detrusive action of the bronchial muscles will increase, being thus called into effective action precisely at the period when most required. Perhaps, also, the slighter contractions of these muscles may be in almost constant operation in the normal condition, to aid, by a kind of peristaltic movement, the outward passage of the physiological secretion. This secretion, comparatively small in quantity as it is, would almost necessarily tend to accumulate in the air-tubes (seeing that no efforts of coughing or forced expiration are

[1] Williams, *Diseases of the Chest*, 4th edition, p. 330.

made for its removal) ; and this would take place, particularly in the smaller bronchi, which we know to be especially subject to mechanical obstruction, and in which the ciliated epithelium, so abundant in the cartilaginous bronchi and trachea, gradually gives way to transition forms, not constantly furnished with cilia.

It may not be easy to adduce direct proof of the theory here proposed, as to the function of the bronchial muscles in health and disease ; but as *no* theory upon this subject has yet been found consistent with our present physiological knowledge, and as the above speculation appears in all essential points to correspond with what is already known of the action of these muscles, it may be worth while to give it consideration, were it merely to rescue us from the unphilosophical predicament of supposing the circular fibres of the bronchi to be endowed with contractility solely for the purpose of producing the asthmatic paroxysm. That these fibres are probably perfectly passive, as regards the respiratory act, is now generally admitted (contrary to the ancient opinion) by physiologists ; and under these circumstances the theory of their de-obstruent action, even in health, but more especially in the diseased states of the pulmonary texture above described, appears to supply a gap in the chain both of physiological and of pathological phenomena.

The ordinary form of the paroxysm of spasmodic asthma, of the humoral kind, is full of instruction, when considered by the light of the preceding views. Notwithstanding the extremely doubtful and difficult pathology of this disease, it seems impossible to avoid referring its most obvious symptoms to some kind of irregular action of the muscular apparatus of the air-tubes. The copious expectoration, again, with which the attack concludes, and by which it is immediately relieved, appears to indicate that undue accumulation of mucus has been taking place ; while the absence, in some instances, of all considerable catarrhal symptoms appears to demonstrate that this accumulation is directly connected with the spasmodic derangement which produces the paroxysm. The connexion of these

two phenomena it is by no means difficult to understand, according to the principles already laid down; in fact, if the removal outwards of the pulmonary mucus depends, in the normal state, upon the regular peristaltic contraction of the bronchial muscular fibres, it is obvious that accumulation must accompany the derangement of that action, just as constipation is the invariable concomitant of the analogous derangement of colic or ileus. In both cases the paroxysm ceases when the normal action is restored; and in general there is in both a copious discharge of the previously retained excretions.

Asthmatic persons are often subject to a slight habitual wheezing in some part of the chest, and also to an occasional cough, with or without slight expectoration, but with no other symptom of catarrh. These symptoms have been described to me as occurring on exertion in the open air after prolonged rest; they are accompanied with slight dyspnœa, and this, together with the rest of the symptoms, ceases when the exertion is continued long enough to produce some degree of re-action. These phenomena are unquestionably the minor degree of the paroxysm; they are probably caused by the same irregular action of the bronchial muscles as causes the latter, but do not reach the climax, because the nervous centres are awake to the first approaches of disorder, and the excitement and quickened respiration consequent on exertion produce the cure. The aggravated asthmatic paroxysm always occurs during sleep, when the energy of the nervous system is at the lowest, and the comparatively quiescent condition of the respiratory function favours the accumulation of mucus. It seems probable that the asthmatic paroxysm is attended with more or less of pulmonary collapse, the consequence of the accumulation in the bronchi; but I have not had an opportunity of direct observation on this point. It is certain, however, that this accumulation must seriously contribute to the production of the most distressing symptoms of the paroxysm. The spontaneous cure in the real paroxysm, as in the minor attack, or threatening of asthma, above referred

to, usually takes place when the nervous centres have been thoroughly roused, and the whole system brought into a state of re-action by the exertion consequent on the dyspnœa.

An interesting fact, in connection with asthma and other spasmodic respiratory diseases, is the frequent occurrence of vomiting during the paroxysms,—a fact which points to the probable dependence of all these affections on some morbid condition in the communication of which the pneumogastric nerve and the medulla oblongata are the principal parts concerned. A phenomenon exactly the converse of that just alluded to, is the profuse and immediate expectoration in cases of obstructive bronchitis after the administration of an emetic. Now it is interesting to observe, in relation to both these facts, and their bearing on the subject we have been considering, that Volkmann has apparently succeeded in demonstrating the influence of stimuli applied to the trunk of the vagus nerve upon the muscular contraction of the bronchi,—a point left open to doubt, both by the experiments of Williams and by the subsequent ones of Longet.[1] The expeditious and complete relief afforded by an emetic in cases in which there has been extreme difficulty of expectoration, is one of the most striking phenomena connected with bronchitis; and one of which, I believe, no sufficient explanation has yet been afforded. It appears, however, to be completely in harmony with the theory I have advanced in the preceding pages.

Another fact tending still further to illustrate this view, is found in the experiments of Reid, Longet, Schiff, and others,[2] on the effects of section of the pneumogastric trunk,

[1] Volkmann introduces into the trachea of a decapitated animal a tube having its outer end tapering and perforated by a rather small opening. This being placed opposite a flame, he isolates and galvanises the vagus nerve, when, at every application of the stimulus, the flame is observed to be blown aside. Wagner's *Handwörterbuch der Physiologie*, vol. ii. p. 586.

[2] *Edin. Med. and Surg. Journal*, April, 1839; or Reid's *Anatomical and Physiological Commentaries. Monthly Retrospect*, 1849, p. 3. Longet, *Système Nerveux.*

or of its visceral branches, on the lungs and bronchi. All experiments concur in proving that these operations are followed by a very large accumulation of frothy mucus in the bronchi. Changes in the lungs have also been observed, which seem to be of the nature of congestion and collapse, but are imperfectly described. M. Longet has also found emphysema of the lungs, the relations of which to pulmonary collapse will in these cases hereafter be considered; and there can be little doubt that we have all the phenomena of bronchial obstruction and collapse following the division of the nerve which, according to the views above proposed, is the chief regulator or excitor of the bronchial de-obstruent function.

To sum up the results of this discussion, as respects bronchitis, I would recapitulate the following points, which, if not established, seem to be at least rendered highly probable. *Firstly*, That pulmonary collapse from bronchitis, when recent and uncomplicated, appears to be susceptible of cure, on removal of the bronchial obstructions. *Secondly*, That this is usually effected, not so much by the agency of respiration, as by the muscular contractions of the obstructed bronchi themselves. *Thirdly*, That the derangement or paralysis of this de-obstruent function becomes a cause of bronchial accumulation even in the normal state of the mucous membrane, and, *a fortiori*, in cases of bronchitis. *Fourthly*, That the de-obstruent function of the bronchial tubes may be impaired by various causes acting on the pneumogastric nerve, either directly or through the nervous centres. And *Fifthly*, that it may be stimulated by remedies or other agents acting in a similar manner.

The application of these principles to pathology might be almost indefinitely expanded, if it were desirable at the present stage of the inquiry to indulge in much farther speculation. But enough has probably been brought before the reader to show that the symptoms, causes, and cure of bronchitis and other allied affections, even when not resulting in demonstrable or organic disease, are illus-

trated by a clear conception of the phenomena of pulmonary collapse and its attendant conditions.

I now proceed to the consideration of some permanent disorganisations, for the most part well known to anatomists.

Permanent Lesions of the Air-Vesicles and Bronchi depending on Bronchitis.—Those which are enumerated by authors, and which I have myself seen, may be described as being principally of two kinds, viz. permanent obliteration or constriction of these parts, and permanent abnormal dilatation of them.

The obliteration of the air-vesicles, leading to *atrophy of the lung*, has been viewed as a consequence of bronchitis by Dr. Stokes, and in this respect he stands almost alone among the authors who have treated of this disease. In his work on *Diseases of the Chest*, he says: "Atrophy of the lung has been recognised in a variety of diseases, such as tubercle, pneumonia, cancer, and pleurisy; but its direct connexion with bronchitis has not been sufficiently examined" (p. 203). Dr. Stokes argues for the possibility of this connexion, on the ground, that "it is easy to see that when the air-tubes are obstructed, the cells to which they lead will diminish in volume." He gives, however, no experience of his own upon the subject; and in speaking of emphysema, he adopts Laennec's opinion of its origin, which ascribes an entirely opposite result to obstruction of the air-tubes. The views of Dr. Stokes upon this subject cannot, therefore, be considered as very clear or consistent, though I believe the merit of suggesting that atrophy of the lung may depend on bronchitis, belongs, in the first instance, to him.

Dilatation of the air-cells, or *emphysema of the lung*, a condition mentioned by Sir J. Floyer, Morgagni, Baillie, and others, was first accurately described by Laennec, and was by him considered as a consequence of bronchitis. Almost all pathological writers since Laennec's time have concurred with him in this opinion, with the exception of M. Louis, who, on grounds which will be hereafter noticed,

has cast a doubt over the alleged production of emphysema by catarrh, while admitting their frequent co-existence. I have already alluded to some points in which Laennec's view of the connexion of these two affections is open to attack; but the correctness of his observation, as to the dependence of emphysema in many cases on bronchitis, is, I think, quite unquestionable, notwithstanding the arguments of M. Louis. This will, I hope, appear clear from the sequel.

Along with this affection may be placed the "interlobular emphysema" of Laennec, which is produced under nearly the same circumstances, and often simultaneously with the dilatation of the air-vesicles. It is, however, an essentially different affection, not only in its site, anatomical character, and mode of production, but also in its result,—inasmuch as it is never, I believe, a truly permanent lesion, and would, therefore, have been more correctly, though not so conveniently, noticed in the former part of this memoir.

Obliteration of the Bronchi is somewhat slightly and confusedly alluded to by Andral as a consequence of bronchitis, having been altogether passed over by Laennec. It is treated anatomically as an independent affection by Reynaud at great length;[1] and the conclusions of Reynaud's memoir have been elaborately adapted to the subject of bronchitis by Dr. Stokes. With these exceptions, the obliteration of the bronchi has received, like the atrophy of the lung, but little attention, except as a sequel of tuberculous and other primary diseases of the lung itself.

It is very different with the opposed condition of the *dilatation of the bronchi*, which, since its elaborate description by Laennec, has been almost universally admitted by pathological writers to be connected with, and probably caused by, bronchitis, although the nature and origin of the affection have led to much difference of opinion, and are still among the obscurest points of pulmonary pathology.

In addition to these lesions, I have likewise to include, as a consequence of bronchitis, the pulmonary concretions

[1] *Mémoires de l'Académie de Médecine*, tome iv.

which are frequently found in the midst of atrophied and indurated portions of pulmonary tissue, and which have been described by Bonetus, Morgagni, and almost all the earlier pathological anatomists, as often connected with asthma. It is well known that the tendency of modern opinion has been to follow Laennec in considering these bodies as the result of cured tubercle,—a view of their origin which, though undoubtedly correct in many cases, I am induced to regard as too exclusive, and as therefore requiring modification.

Upon the anatomy of these affections I shall have so much to say in my next paper, as may serve to illustrate their relation to one another, and to the primary results of bronchitis already described. I propose, however, to treat chiefly of the causes and mechanism of their production,—a pathological difficulty to which the knowledge of pulmonary collapse, as a frequent result of bronchitis, furnishes, I believe, the sole and indispensable key. Those who are acquainted with the confusing and apparently almost hopeless discordance of opinion on most of these subjects, among both practical and systematic writers, will not look upon any attempt at their reconciliation as a superfluous task. . . .

Part III.—Permanent Lesions of the Air-Vesicles and Bronchi depending on Bronchitis.

Relation of Bronchitic Collapse to Pulmonary Emphysema.—In the last paper I discussed the mode in which the collapsed lung, under favourable circumstances, reverts to its natural condition. The mechanism by which this is effected, and particularly that portion of it which I have theoretically suggested under the name of the de-obstruent function of the bronchial tubes, must be considered as of vast physiological interest, if we reflect that there is scarcely a case of fever, or any other debilitating disease, in which the signs of mucous accumulation and of partial pulmonary collapse may not be discovered at one period or other at the lower and back part of the lungs ; and that the same forces which

under these circumstances restore the lung to its normal state, by throwing off the load in the bronchi, are probably perpetually in action to prevent a similar accumulation in the state of health. A little careful reflection on the mode in which the free and unembarrassed play of the lungs is maintained in health, notwithstanding the constant presence of a viscid secretion from the bronchial mucous membrane, will probably satisfy every one that a special function for the removal of this secretion must form an element of the highest importance in normal as well as morbid respiration ; and the ciliary apparatus, as I have already mentioned, is not calculated fully to perform this office, being less abundantly distributed to the smaller bronchial tubes, where its presence is apparently most required.

In many persons the removal of the bronchial mucus is habitually ill-performed. The quantity of mucus is not materially greater than natural, but it is not discharged as rapidly as it is secreted, owing to some defective condition of the de-obstruent apparatus, or perhaps a defective innervation of the bronchial muscles. In this condition, whatever be its cause, the tranquil and insensible processes of the normal economy are exchanged, sometimes for fits of coughing, which bring up pellets of tenacious pearly mucus, the result of undue accumulation ; and sometimes for paroxysms of dyspnœa, which end in more copious evacuations of bronchial mucus, and a return to comparative health. These persons are the subjects of the *dry* or *humid asthma* of English authors, and of the *catarrh sec* of Laennec. Their disease, though simple and free from danger in its outset, is, according to the judgment of all physicians, apt to lay the foundation of organic disorder, which usually assumes the form of pulmonary emphysema. This consequence is peculiarly apt to occur, if care be not taken to guard against the supervention of bronchitis, which in these individuals generally assumes characters of great intensity, and is uncertain and protracted in its cure.

There are other persons who, with a habitually normal state of the respiratory functions, are subject to repeated

acute bronchitic attacks of great severity. They are careless in their mode of life, frequently exposed to cold, or endowed with a peculiar sensitiveness of the pulmonary mucous membrane, while they are at the same time free from the tuberculous constitution, with its organic sequelæ. In such persons, also, pulmonary emphysema is known to be a frequent disease; being left behind as the legacy of the bronchitic attacks, and aggravated after each successive invasion.

Not unfrequently a considerable, or even an extreme, amount of pulmonary emphysema is observed to follow a single attack of acute disease in the chest. Thus emphysema frequently arises in the earliest years of infancy and childhood, as the consequence of some form of severe infantile bronchitis; and all practitioners can bear witness to many cases in which shortness of breath and incapacity for exertion can be traced distinctly back to the date of an attack of hooping-cough or measles. Some of the most marked instances of emphysematous lungs in young subjects that have fallen under my notice in dissection, have had a similar history; and all authors on the diseases of children, who have carefully investigated the morbid anatomy and history of these affections, concur on this point. Again, in adults otherwise healthy, the severer forms of epidemic influenza are peculiarly apt to be attended with, or followed by, the development of emphysematous lesions; a fact which has been well observed and carefully recorded by Dr. Peacock, in his excellent history of the last London epidemic of that disease.[1]

[1] *The Influenza or Epidemic Catarrhal Fever of* 1847-8. By Thomas Bevill Peacock, M.D., etc.; London, 1848. See pp. 31-32, 134-135, 143-144, for graphic descriptions of the morbid appearances after death from influenza. Dr. Peacock has favoured me with a letter on this subject since the publication of the first part of this memoir, and I am happy to be able to state, that this experienced pathologist—my predecessor in my present office—is convinced of the correctness of my explanations of the bronchitic collapse of the lung, and satisfied of the identity of that affection with many of those indicated by him in the pages to which I have referred.

Finally, a certain amount of emphysema of the lungs is of so frequent occurrence in the aged, as to be scarcely entitled to the name of a disease, distinct from the other evidences of corporeal decay. This fact was first pointed out by Magendie, and the form of emphysema here alluded to has since been described by many pathologists as a peculiar one, constituting a kind of senile atrophy of the pulmonary tissue. But there can be little doubt, that here also the pulmonary lesion is the concomitant of a bronchial affection,—the chronic bronchitis or bronchorrhœa,—which is the almost constant companion of the more advanced periods of human life. In cases where this has been absent, I have repeatedly found the lungs of very aged individuals quite free from all trace of emphysematous lesion.

Considerations like these have, ever since the accurate descriptions of emphysema by Laennec became generally known, given rise to a general belief among practitioners that emphysema is related to bronchitis as effect to cause; and that it is indeed the organic lesion of the lung of all others most closely and invariably connected with long-continued or severe bronchial affections. In taking it, therefore, as the starting-point of the following researches on the permanent lesions of the lungs connected with bronchitis, I shall have the advantage, not only of beginning with a disorganisation so palpable and well-known as to be rarely overlooked at the present day by any one acquainted with pulmonary pathology, but one, the relation of which in some way or other to bronchitis, is almost universally admitted, notwithstanding the numerous differences of opinion as to its mechanism and causation. The observation of it somewhat more rigorously will serve, therefore, as a criterion of the correctness of the observations in the first part of this memoir, and at the same time will lead naturally to the consideration of other subjects. . . .

I am aware it will be said, that tubercle stands in a wholly peculiar relation to this subject, as several pathologists of great eminence have maintained the doctrine of

the incompatibility of emphysema and tubercle; supposing, on the ground of their comparatively rare co-existence, that the former confers on those attacked an immunity from the latter affection. Rokitansky has, indeed, given the sanction of his high authority and immense experience to a doctrine which may be considered inclusive of this assertion of the French pathologists, viz. that all affections producing *venosity*, or imperfect oxygenation of the blood, such as cyanosis, curvature of the dorsal spine, emphysema, etc., confer an immunity from tubercle. Without entering here on the discussion of this doctrine, in its more general relations, it may be confidently stated, that the portion of it relating to emphysema gains no support from the numbers just quoted. According to this doctrine, it might reasonably be expected, that among persons dying with emphysematous lungs, tubercle would bear a decidedly lower proportion to the whole numbers than in a mixed hospital mortality; whereas, by a remarkable enough accident, it happens that the proportion is, in the above numbers, precisely the same, viz. 20 per cent. in both classes of cases.[1] And although it would be too much to argue from this coincidence, that emphysema and tubercle exert no influence upon each other, yet I think it may justly make us pause before accepting a doctrine which has not, *a priori*, much argument in its favour, and the evidence of which has never been presented to the public under a form approaching to exactness. I shall have occasion hereafter to state my own views on this subject.

The following table exhibits, in one view, the percentage of most of the lesions referred to above, in emphysematous and in mixed cases of disease,—the numbers from which it is calculated being derived from the same hospital returns,

[1] Rokitansky admits the conjunction of obsolete or cretaceous tubercle with emphysema. But, in the cases above referred to, all the instances of obsolete tubercle have been excluded from both lists. In the cases conjoined with emphysema, it will be seen that there existed cavities in four cases; the others were miliary or yellow tubercle without excavations.

so as to assimilate the conditions of observation as nearly as possible.

	In mixed Cases.	In Emphysematous Cases.
Hepatization,	9.8 per cent.	10.0 per cent.
Tubercle,	20.0 „	20.0 „
Condensation (collapse),	11.8 „	67.6 „
Bronchial abcesses,	5.5 „	17.5 „
Induration and atrophy,	7.5 „	25.0 „
Concretions,	4.1 „	20.0 „

It will be seen that while the first two lesions in the preceding table appear to have no special numerical relation whatever to emphysema, their percentage being nearly the same in this affection as in the general returns, the remaining four are found to be greatly more frequent in connexion with emphysema than under other circumstances. But this is not all; for, as tubercle is almost invariably connected with some form of condensation, and was so connected in many of the cases here referred to, and as all the cases of hepatization are also to be found under the head of bronchitic condensation, it becomes nearly certain that, of the whole forty cases of emphysema, not one had any direct connexion with either hepatization or tubercle, as such, but only through the medium of the other lesions mentioned. Tubercle and hepatization, therefore, are in all probability merely the accidents, and not either the causes or effects, of emphysema of the lungs.

If now we consider the all but invariable connexion of emphysema with one or other of the remaining lesions of the lungs, and the frequency with which all of them occur in emphysematous as compared with mixed cases, we shall be driven almost inevitably to the conclusion, that some circumstance, common to them all, and not necessarily present in hepatization and tubercle, is closely connected with the production of emphysema, if not, indeed, its real pathological cause. What that circumstance is, we may now endeavour to discover.

Mechanism of Emphysema.—Emphysema of the lungs was said by Laennec, in one of the most original and accurate

of his descriptions, to have two varieties: the one being a dilatation of the air-cells, and finally a rupture of them one into another by removal of their septa; the other, a rupture of the air-passages directly into the interlobular areolar tissue. It is needless to repeat these descriptions, the distinction of vesicular and interlobular emphysema being well known to everyone, or at least accessible to all, in words which cannot be improved. It is only necessary to add, that the microscope and other modern means of investigation, which have done so much for morbid anatomy, have scarcely availed here to augment our knowledge; having only succeeded in demonstrating more clearly the fact, known to Laennec, of the gradual breaking up of the vesicular septa, and the obliteration of their capillary network.[1]

Emphysema, therefore, is an abnormal distension of the pulmonary tissue with air. In its earliest stages, whether interlobular or vesicular, or, as frequently happens, both combined, nothing can be more certain than that it is essentially a mechanical lesion: in fact, the distension of the air-cells, giving the peculiar cushion-like and pale appearance to the lung, can be exactly imitated by inflating it with undue force artificially. Moreover, the whole of the subsequent structural changes implied in the gradual removal of the septa and obliteration of the capillaries, are readily explained by the mechanical effects of distension. Upon this subject M. Poiseuille, to whom we owe so many interesting facts in mechanical physiology, has a very beautiful experiment.

An instrument being adapted to the pulmonary artery of an animal, by which a given quantity of liquid was

[1] After frequent personal observation on this subject, I am compelled to regard the late theory of Mr. Rainey, in regard to the dependence of emphysema on fatty degeneration of the lung, as fallacious. The granules described by him certainly do not always occur in emphysematous parts; and when they do so, they are so few, and so little characteristic of this particular lesion, that it is plain Mr. Rainey's views have been founded on an imperfect appreciation of the relations of the so-called "fatty granules" to morbid tissues.

propelled with a given force through the capillaries of the lung, he found that this was effected, in the normal condition, in 29 seconds. M. Poiseuille now inflated the lungs so as exactly to fill the cavity of the chest; the time was still 29 seconds. On distending the lungs, however, farther, so as to produce the appearance of a partial emphysema, the time required for the passage of the fluid became lengthened to 62 seconds; when the emphysematous appearance was increased, 95 seconds; when it pervaded the whole lung in consequence of excessive distension, 129 seconds were required, and the fluid returned from the pulmonary veins mixed with some bubbles of air.[1] From these results, it is evident that whenever the air-cells are distended beyond the amount required or possible in the healthy condition, the flow of blood through the ultimate capillaries of the lung must be retarded or obstructed;—a condition not only corresponding with the appearances observed in emphysema, but readily accounting for the structural changes, the absorption of the walls of the air-cells, and the tension and obliteration of vessels observed in the later stages of the disease.

It is, therefore, nearly certain that the source of emphysema is to be sought in a derangement of the mechanism of respiration, and not in any previously morbid condition of the affected part. Every thing denotes that the emphysematous parts of a lung are usually free from all diseased changes, with the exception of those which are the result of inordinate distension. The freedom from œdema and from morbid deposits, when other parts of the lung are so affected; the absence of accumulation in the bronchi, or at least its comparatively slight character, allowing of the perfect and easy inflation of the emphysematous parts when others are collapsed; finally, the habitual seat of emphysema in those parts of the lung which are usually most exempt from other disease,—all tend to prove what I have now stated. The diminished elasticity, the dryness, the anæmia, which have all of them been supposed to be the predisposing

[1] *Bulletin de l'Académie Royale de Médecine*, vol. viii. p. 705.

cause of this lesion, are manifestly nothing more than the effects of the distension with air upon the circulation and nutrition of the compressed walls of the delicate pulmonary air-cells. Even the small accumulations of granular deposit found by Mr. Rainey may be accounted for by these secondary nutritive changes.

But emphysema is not merely a lesion resulting from inordinate distension of previously sound portions of lung; it is, as we have already seen, the product of mechanical derangement in the *sound parts* of lungs *otherwise diseased*. The existence of bronchitic condensation, of induration, of concretions, etc., if not a necessary cause of the production of emphysema in the sound air-vesicles, is at least in some way related to it. The theory of emphysema by Laennec, besides the objections offered to it in the former part of this memoir, in no way accords with the facts now adduced. Mucous obstruction of the bronchi, even if proved to exist, cannot determine, *directly*, both condensation and rarefaction of the lung; and we have already learned, from unquestionable and multiplied evidence, which of these two is its real result. The opinion of Louis, derived, apparently, chiefly from a consideration of the seat of election of emphysema as compared with that of bronchitis,[1] is opposed to the idea of any precise relation between these two affections; but this negative opinion would appear to be sufficiently answered by the numerical facts above adduced.

Some writers, conceiving, like Laennec, that emphysema is produced in the act of expiration, believe it to be the result of violent efforts of coughing, or other forcible expiratory acts. But have we really any direct proof whatever that cough, however violent, or any similar act, can produce emphysema, apart from the other accidents of bronchitis?

[1] "Si l'on se rappelle que le *maximum* de l'emphysème ordinairement a son siège au bord tranchant des poumons et dans leur voisinage, tandis que le catarrhe pulmonaire aigu intense a le sien en arrière et en bas, on sera forcé de conclure que si ce catarrhe a une influence quelconque sur la développement de l'emphysème, cet influence est peu considérable et ne s'exerce sans doute que bien rarement." *Mémoires de la Société Médicale d'Observation*, tome premier, p. 253.

In croup, in laryngitis, in aneurism of the aorta, we have cough even more violent and distressing than that of bronchitis; yet these affections are not known usually to cause emphysema, and I have repeatedly seen cases opposed to the idea of their having any such influence. The alleged unusual frequency of emphysema among players of wind-instruments is likewise totally devoid of proof, and rests upon one unsupported assertion by Laennec; whereas, if the real cause of emphysema were such as above described, no singer or wind-instrument player could in all probability remain long exempt from this disease. But it would require further to be known whether an increased liability to emphysema in this class is not accompanied with a similar proclivity to other pulmonary affections, before the question could be decided on such grounds.

But the most serious objection to the expiration-theory of this disease is, that the expiratory act is *mechanically* incapable of producing distension of the lung, or of any part of it. The act of expiration tends entirely towards emptying the air-vesicles by the uniform pressure of the external parietes of the thorax upon the whole pulmonary surface; and even when the air-vesicles are maintained at their maximum or normal state of fulness by a closed glottis, any further distension of them by the expiratory force is as much out of the question as would be the further distension of a bladder blown up and tied at the neck, by hydrostatic or equalised pressure applied to its entire external surface. The air-vesicles can sustain no distending pressure from the column of air *within* the tubes, as that air only becomes compressed in virtue of a force acting on the *exterior* of the lung, which opposes exactly as much resistance without as it creates pressure within. It is singular that a theory so radically unsound, and so devoid of direct proof, as this of the production of emphysema by expiration, should have been allowed to maintain a place in medical literature.

The only theories of emphysema which remain, are those which refer it to the act of inspiration. The most usual

form assumed by these theories, is the supposition that emphysema of the lung is a physiological *compensation* for the occlusion of a diseased portion of lung ;—a view not only giving no real explanation, but totally inconsistent with the fact, that in truly morbid emphysema there is always a diminished respiratory surface and consequent dyspnœa. Dr. Williams, however, and some others, have placed the inspiration-theory in a more tenable position,—supposing that, when certain portions of the lung are occluded, the air is brought by inspiration to penetrate with greater force, and in greater volume, into the remaining parts.

This view is certainly near the truth, and is quite consistent with clinical stethoscopic experience. But it is clogged in Dr. Williams's work with a reference to the incompetent expiration-theory of Laennec, as if the author did not see his way clearly to the explanation of all cases of emphysema by his own. Moreover, it is not the whole truth ; because certain obstructive lesions have, as we have seen, no appreciable influence in causing emphysema ; and also because it is evident that the inspiratory or expansive power of the chest is exactly limited by its capacity, and that even when a portion of lung is impervious to air, as in hepatization, the inspiratory force can no more distend the air-cells to the degree observed in emphysema than it can do so in the normal state. This fact will appear more clear from the following observations.

It appears to me that none of the writers on this subject have clearly apprehended, or at least clearly expressed, the single obvious condition which is necessary to the mechanical completeness of the inspiration-theory of emphysema. Emphysema is, according to this theory, a *complementary* lesion, dependent upon the previous existence of some form of occlusion of the vesicles, and invading the remaining sound portions of lung. Thus far it corresponds with all that we have hitherto seen, to an extent certainly not anticipated by Dr. Williams, when, after enunciating his own view, he brings forward Laennec's theory to account

for residual unexplained cases. But there is yet another condition necessary, besides mere occlusion of the air-vesicles in a part of the lung: this is *partially diminished bulk*;—in other words, collapse or permanent atrophy of a portion of the lung.

The operation and importance of this condition will be at once seen by the aid of a diagram. Suppose that in the accompanying Fig. 27 the three equal partitions represent the *maximum* air-space, in the normal condition of full

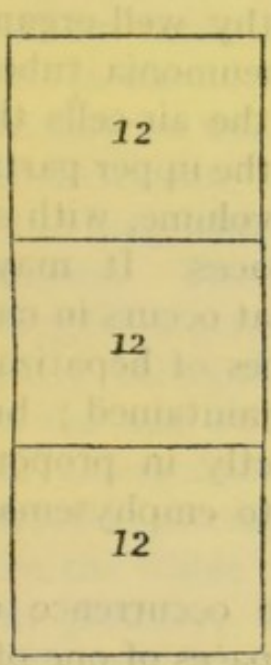

Fig. 27.

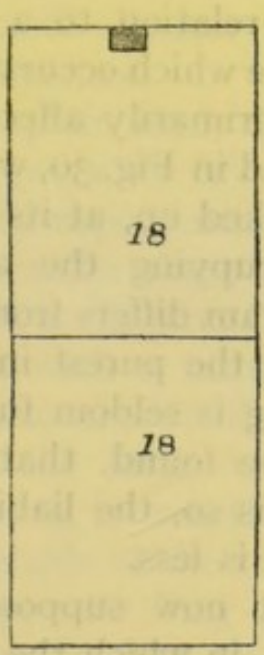

Fig. 28.

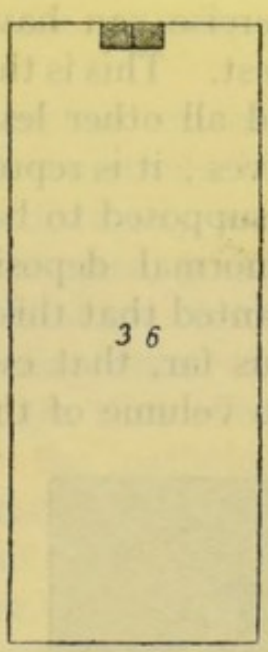

Fig. 29.

inspiration of three lobes or portions of a lung (represented equal for the sake of simplicity). Each lobe holds, on a full inspiration, say 12 cubic inches or other measures of air; and it is adapted normally to hold this quantity, without pressure on the capillary circulation, or risk of violence to the texture of the organ. It is at once obvious that no amount of lesion, which leaves the upper partition or lobe of its normal volume, can at all affect the maximum expansion of the other two. They will continue, under all circumstances, to be capable of receiving their normal 12 measures of air; they will be prevented from receiving more, not by the tendency of the pulmonary texture to

resist further expansion, but by the inadequacy of the mechanical apparatus *for producing* further expansion. No strain can in this case be thrown upon the walls of the air-cells; these still preserve their normal relation to the capacity of the chest which contains, and, by its dilatation, expands them. The inspirations will indeed be multiplied, —they will also be increased in fulness and force beyond the ordinary condition; but this can have no more effect in producing emphysema in the free air-cells of a diseased lung (under the above conditions) than running or violent exercise can have in relation to a healthy well-organised chest. This is the state which occurs in pneumonia, tubercle, and all other lesions primarily affecting the air-cells themselves; it is represented in Fig. 30, where the upper partition is supposed to be blocked up, at its full volume, with some abnormal deposit occupying the air-spaces. It may be granted that this diagram differs from what occurs in nature thus far, that even in the purest instances of hepatization the volume of the lung is seldom fully maintained; but it will be found, that exactly in proportion as it is so, the liability to emphysematous lesion is less.

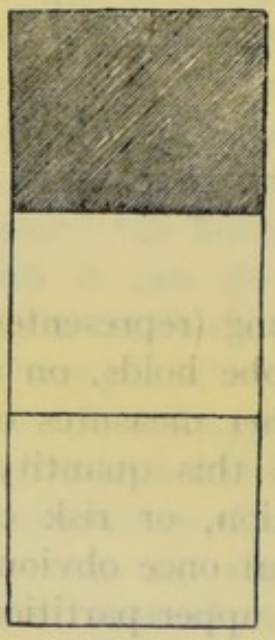

Fig. 30.

But now suppose the occurrence of a lesion, in which the air-spaces of one of the partitions are closed by the collapse of its parietes, with diminution of bulk of the lung in this lobe. In this case, it is obvious that the expanding forces of inspiration will act inordinately upon the remaining lobes, and tend to attract into them the air which is prevented from entering the occluded one. If these forces were sufficiently powerful to overcome the resistance offered by the tissue of the sound lung under these circumstances, and if the sound portions of lung yielded equally in all directions, it is obvious that the condition established would be that in Fig. 28, in which the lung is expanded to the normal maximum; but the air is differently distributed, being

excluded from one lobe, and present in the others to the extent of 18 measures in each, instead of 12 as formerly. In like manner, the occlusion of two lobes, if accompanied with collapse of the tissue, would necessarily lead, in the event of the lung being fully dilated, to the accumulation of the whole 36 measures of air in the remaining lobe, as in Fig. 29. A lobe thus distended would certainly suffer obstruction of the capillary circulation, as in the experiment before mentioned of M. Poiseuille; and the original purely mechanical condition would pass into one complicated by those structural changes which are actually produced in chronic emphysema.

It may be well to explain here, that a certain amount of over-distension, *when gradually effected*, is sometimes borne by the lung without the supervention of a distinctly morbid condition. The lung, under those circumstances, probably undergoes a genuine hypertrophy, the air-vesicles becoming slightly enlarged, but with a nutritive adaptation of the vascular and other structures to the changes thus effected. This enlargement of the lung, without the pathological characters of emphysema, is sometimes observed in disease, when the whole of one side of the chest has been contracted from pleurisy, the opposite lung passing, as the stethoscopist well knows, for an inch or two across the median plane in front, and having all its parts seemingly adapted to its increased size and function. A large power of adaptation of the lung to external circumstances is also shown (as has been pointed out by an acute critic of my original communication on this subject to the Medico-Chirurgical Society of Edinburgh) "among the inhabitants of such lofty situations as the high table-land of South America," in whom "the chest becomes of a size considerably beyond its ordinary dimensions,"[1] owing to the permanent and constant necessity for inspiring a greater volume of air than in less elevated situations.

True hypertrophy of the lung is most readily produced when, as in the above cases, the distending force acts equally

[1] *Med. Times*, July 20, 1850, p. 72.

on the whole or a large part of the pulmonary tissue, and when it is so gradual as to give time for corresponding changes in the nutrition and circulation of the parts. For this reason, emphysema seldom arises to a marked extent when one lung replaces the function of another destroyed by pleurisy; the expansion of the sound lung remaining limited by the normal conditions until all the structures have gradually accommodated themselves, under the influence of exercise and habit, to the altered circumstances of the system. That emphysema may be produced, however, to a certain extent in such instances, is shown by cases which I have witnessed. The forms of pulmonary disease in which emphysema is most readily produced, on the other hand, are those where the primary lesions have been much disseminated, so that every part of the chest, in its expansion, acts at once directly upon corresponding portions of lung partially collapsed or atrophied, and yet containing many comparatively unobstructed lobules, which yield readily to the distending force. Hence the most frequent of all combinations with recent emphysema, as may be seen by reference to the table, is a certain extent of collapse of the posterior portions of the lungs, with a number of disseminated lobular condensations between the emphysematous parts. As these lesions are also very rapidly produced, and give rise to dyspnœa extremely urgent, they are apt to induce accelerated and laborious efforts at inspiration, in the midst of which emphysema, either of the interlobular or vesicular kind, or both combined, very readily arises.[1]

The theory here proposed has already been advanced by various writers, and with different degrees of precision of statement, to account for those cases of emphysema which are connected with the cicatrisation of tubercular cavities and other kinds of pulmonary atrophy. It is obvious, however, that its true significance, and the extent of its application, cannot be understood, till it is clearly appre-

[1] The relation of emphysema to the violence of the inspiratory efforts, rather than to the apparent importance of the pulmonary lesion, is noticed by Rilliet and Barthez, *Maladies des Enfants*, vol. i. p. 139.

hended that all cases of considerable obstruction in bronchitis bring with them, *as a necessary consequence*, a certain amount of diminished volume in the obstructed parts of the lung; and, therefore, that the connexion of emphysema with bronchitis need present no difficulty to the pathologist, even when the latter has not been so violent or long-continued as to lead to any considerable amount of permanent and evident occlusion. That emphysema prevails in the opposite parts of the organ to those in which the direct effects of bronchitis are observed, becomes, in this point of view, one of the strongest evidences of its connexion with that affection. That in the great majority of cases it is found in company with bronchitic collapse, or some lesion implying diminished size of the organ, amounts, I think, almost to demonstrative proof of the correctness of the theory here advanced.

I am prepared, then, to maintain, that emphysema of the lung may, in all cases which I have witnessed, be satisfactorily accounted for by considering it as *a secondary mechanical lesion, dependent on some condition of the respiratory apparatus leading to partially diminished bulk of the pulmonary tissue, and consequently disturbing the balance of air in inspiration.* I, therefore, submit this principle to the judgment of the profession, in the confident anticipation, that it will prove no less constant and satisfactory in the hands of other observers, and will establish itself as the exclusive law of the production of this most important lesion.

A very few facts, in addition to the evidence already adduced, appear to be so striking as to deserve to be placed in an isolated form before the reader. A child, in whom the right lung was normal, excepting imperfect bronchitic collapse, had in the left lung a mass of tubercular bronchial glands pressing on the bronchi passing to the anterior prolongation of the lower lobe, which was accordingly perfectly collapsed, void of air, and flaccid. The corresponding prolongation of the upper lobe, which in the acts of inspiration glides into the same angle of the pleural cavity, and the

bronchi of which in this case were free, presented very marked interlobular emphysema in its early and perfectly recent condition, and the other parts of the lung were normal. Nothing can be more clear in this case than the relation of the collapse to the emphysema, both being recent. The following case, which occurred to me lately, is an equally striking illustration of this point. An aneurism of the aorta produced sudden death, by bursting into the air-passages. There was reason to think, however, from the symptoms, as well as the post-mortem appearances, that bleeding to a less extent had taken place internally some time before death, without being rejected by expectoration. The bronchi on both sides contained frothy blood, but the *lower* bronchial branches of the left lung were completely stopped up with coagula of blood. The mucous membrane throughout the air-passages was quite healthy, though stained purple. The right lung appeared externally uniformly emphysematous, or at least distended with air throughout the upper and middle lobe, and less so in some parts of the lower lobe. The surface was marked with purplish irregular mottlings, which could be seen to be quite beneath the pleura, and shining through from the substance of the lung (blood-stains, without condensation). Considerable portions of the lower lobe presented distinctly-marked lobular collapse. The left lung was also generally emphysematous in the upper two-thirds of the superior lobe. The inferior third was partially condensed and flaccid. The whole of the lower lobe was violet-coloured, completely condensed, and flaccid, having all the external characters of carnified lung. The whole sequence of the phenomena is here again most evident: the coagulated blood in the lower air-passages, especially of the left lung, producing obstruction and collapse, while in the upper part of both it had merely produced staining or mottling of the tissue, the bronchi being free, and the tissue generally emphysematous. In like manner, in emphysematous lungs having, as is usually the case, distinctly marked collapsed lobules or portions in the anterior edge, I have frequently been able

to demonstrate the excess in the bronchi of the latter of muco-purulent matter; and in all cases the greater amount of obstruction may be demonstrated by the attempt to inflate the lungs, when the emphysematous portions will be found to yield at once, while the others follow slowly and often imperfectly.

Relation of Emphysema to Hepatization and Tubercle of the Lung.—It has already been shown, that no apparent numerical relation exists between emphysema and hepatization or tubercular deposit in the lung; the percentage of cases of emphysema accompanied by these affections being nearly the same as in the general hospital dissections. These facts agree in all respects with the theory just stated, which shows that morbid deposits, affecting the ultimate tissue of the lung, can have no direct connexion with the production of emphysema, unless they lead, in the first place, to diminution of bulk, or atrophy of the parts involved. This is not the case either with tubercle or hepatization in their recent condition, except when connected with bronchitis, in which case they may lead to the condition represented in the diagram, Fig. 31. If a lobe of lung be, in the first place, completely hepatized, a subsequent attack of bronchitis may produce the collateral lesions of collapse and emphysema in the remaining lobes, the hepatized part remaining indifferent both to the one and the other tendency. On the other hand, bronchitis and its attendant phenomena may be succeeded by hepatization or tubercle. In either case they exert no direct influence upon the mechanical conditions under which respiration is accomplished.

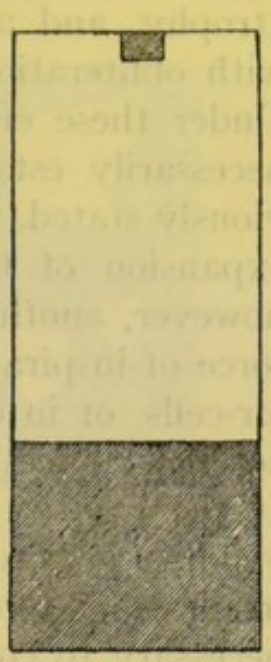

Fig. 31.

Relation of Emphysema to Excavations, and to Tubercular or Bronchial Abscesses.—The above remarks apply to tubercle in the state simply of morbid deposit, before it has proceeded to excavation. The formation of cavities, however, in the lung, from whatever cause, is calculated to modify

so considerably the mechanical conditions for the production of emphysema, that it is necessary to devote some consideration to this subject. As cavities are usually surrounded by condensed tissue, it becomes important to know the type of condensation found under these circumstances. It is not always easy to assign a distinct specific character to the anomalous forms of condensation found around tubercles in the stage of excavation; but one circumstance is, I think, usually evident enough, that they assume more or less decidedly the appearances of pulmonary atrophy, and are, especially in the later stages, attended with obliteration of all trace of the true pulmonary tissue. Under these circumstances, the mechanical conditions are necessarily established, which, according to the law previously stated, might be expected to lead to a compensating expansion of the sound tissue. In the case of cavities, however, another element comes into play; the expanding force of inspiration may expend itself either in dilating the air-cells, or in dilating the cavities; and it will, of course, act most effectively where it finds the weakest resistance. If the cavities be progressive, ill-defined, with weak and yielding parietes, they will fall under the influence of the inspiratory force, and be expanded to the full amount necessary to compensate the atrophy; and this result will follow the more easily, in proportion as they occupy, collectively, a larger proportion of the lung. On the other hand, if the cavities be healing, and surrounded, as they usually are in such circumstances, by very dense fibrous tissue; and if there be much sound pulmonary tissue, the cavities being few in number, and small, the expansion during inspiration will principally take effect upon the air-vesicles, and emphysema will be produced. These conclusions harmonise in all respects with the results of experience. Most pathological writers have noticed the frequent connection of emphysema with cicatrised cavities and healed tubercle; and even Rokitansky, whose opinions on the incompatibility of the two affections I have already noticed, admits that, with healing or healed (obsolete) tubercle,

emphysema is frequently found. Is it too much to suppose that this whole doctrine of alleged incompatibility has been founded on the fact, that rapidly ulcerating and extending cavities, form a condition unfavourable to the development of emphysema; and that consequently, *in this stage*, they rarely co-exist? Such is, in truth, the whole extent to which, I believe, experience will be found to corroborate this hypothesis.

The relation of cavities to emphysema, then, may be shortly stated as follows: 1st, Large or numerous cavities,

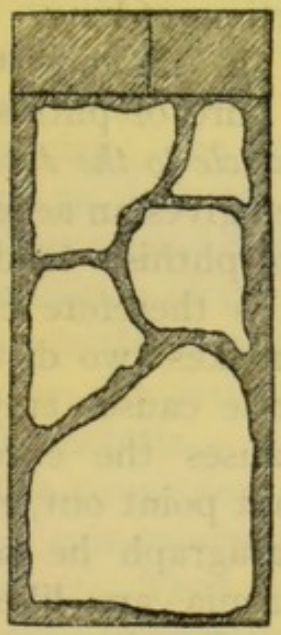

Fig. 32.

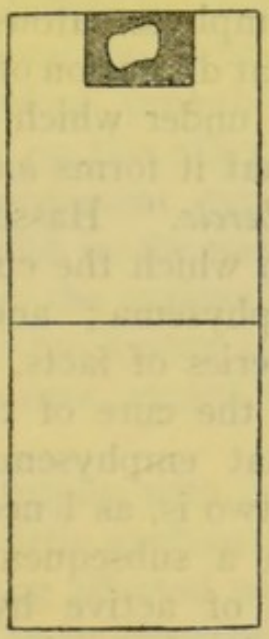

Fig. 33.

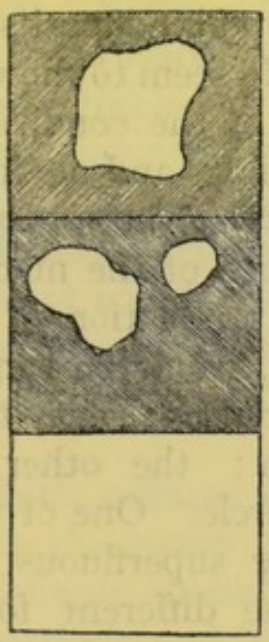

Fig. 34.

with flaccid walls, are, even when accompanied by atrophy (see Fig. 32) unfavourable to the development of emphysema; 2nd, Cavities in process of cicatrization, if few or small in extent, and surrounded by firm atrophied walls (see Fig. 33) are extremely favourable to the production of this lesion; 3rd, Cavities, not surrounded by atrophied walls, whether large or small, exercise no particular influence in relation to pulmonary emphysema. (Fig. 34.)

The great confusion which has hitherto existed on this subject, may be shown by reference to the best pathological works of the present day. The theory of Rokitansky, which derives one of its chief proofs from the relation of emphysema

to tubercle, has been already noticed. Not to mention the absurd propositions made at various times to cure tubercular lungs by establishing emphysema, the following passage, from the careful and truthful work of Hasse, will show conclusively the correct apprehension he had of the facts, and the difficulty of finding a sufficiently general solution for them. "Those much in the habit of examining the dead body, cannot but be struck with two circumstances,—First, The almost invariable existence of emphysema in lungs, which bear the characteristic marks of recovery from phthisis;—and, Secondly, The *proportionate* rareness of tubercular deposits in emphysematous portions of lung. This would seem to show, that dilatation of the air-cells constitutes one of the conditions, under which the cure of phthisis is possible, and again, that it forms *an obstacle to the development and progress of tubercle.*" Hasse then gives an accurate picture of the mode in which the cure of phthisis tends to the production of emphysema; and it is therefore clear, that, from the same series of facts, he makes two distinct inferences: one, that the cure of tubercle causes emphysema; the other, that emphysema causes the cure of tubercle. One of the two is, as I need not point out, obviously superfluous. In a subsequent paragraph he says: "The different forms of active hyperæmia are likewise subject *to the control of emphysema.* Emphysematous portions of lung seldom become affected with œdema. Pneumonia, for the most part, *leaves exempt such lobules as happen to be emphysematous.*"[1] There is here the same inverted inference as in the other case; but no hint of the correct solution of the phenomena. The principle that emphysema is always a secondary lesion, and always complementary of other states involving diminished bulk of the lung, at once solves all such difficulties.

There remains for consideration the relation of emphysema to the chronic forms of pulmonary atrophy and induration, including concretions, and of these lesions to affections of the bronchi. This will form the subject of the concluding remarks.

[1] Hasse's *Path. Anat.* (Sydenham Society), pp. 313, 314.

Permanent Atrophy of the Lung in relation to Bronchitis.—In the preceding investigation of the causes and mechanism of pulmonary emphysema, the connexion of that lesion with all causes leading to partially dimished bulk of the lung has been fully displayed. Among these causes of emphysema, bronchitic collapse of the lung and its sequelæ unquestionably hold the first place in importance and frequency ; about 67.5 per cent. of the cases of emphysema in the table, having been manifestly connected with these affections. The forms of condensation here referred to may be divided into the more recent and the more chronic ; the former being pure instances of bronchitic collapse, usually capable of easy removal by artificial insufflation of the dead lung, and therefore probably remediable in their character ; the latter, on the other hand, having acquired more or less of a permanent type. The latter kind of condensation I distinguish by the name of *pulmonary atrophy*, a condition of the lung, the various forms of which are very imperfectly described by systematic writers, and by no one, so far as I am aware, except by Dr. Stokes, ascribed to bronchitis.

The connexion of atrophy with bronchitic collapse of the lung, can require but little explanation to the reader of this memoir. That the lung affected with collapse should after a time become altered in its structural relations, and become the subject of a permanent contraction or even obliteration of the air-vesicles, is no more than might have been apprehended from the knowledge of what takes place in the pulmonary tissue when subjected to long-continued pressure from pleuritic effusion. In such cases, it becomes, after a time, more or less impermeable to air, and incapable of its former expansion, even when the fluid has disappeared from the pleura, and all the mechanical conditions are favourable for its return to the normal state. Observation and experiment on the dead body also show, that in those cases the proper tissue of the lung has in part disappeared, and that the air-vesicles which remain are incapable of assuming their original volume by any amount of expanding

force. In some instances, indeed, where compression has not been too long continued, we have good evidence that time will do much towards restoring the lung to its former condition; but in the majority of cases of chronic pleurisy, a permanently contracted side, with some degree of hypertrophy in the opposite lung, form the nearest approach to a perfect cure.

A precisely similar series of changes may be observed in the collapsed bronchitic lung, and has, indeed, been already alluded to in the first part of this memoir. In the dead body, the following gradations may be traced: 1. In the quite recent forms of the affection, the collapsed lobules yield before a force somewhat greater than that which inflates the sound portions of the lung; when inflated to the utmost, they are pale, emphysematous in appearance, and of volume equal to the surrounding parts; and when allowed again to subside to the ordinary condition of the dead lung, they are undistinguishable from the originally sound portions. This is the condition of simple collapse without atrophy. 2. The collapsed lobules cannot be inflated without the application of considerably greater force than in the former case; they then yield, however, and though perhaps not gaining altogether the full volume of sound lobules, are, on subsidence, not very easily distinguishable from them. 3. On insufflation of the lung, the collapsed lobules yield after very considerable resistance, but evidently not to the full extent; on allowing the lung to subside, they return more or less completely to the collapsed and non-crepitant condition. These phenomena are of course best observed in the well-defined lobular collapse which affects the anterior and lower edges of the lungs already alluded to. 4. The collapsed lobules cannot be inflated with air, except perhaps by a force sufficient to rupture the tissue; and then the air passes more readily into the interlobular spaces than into the obliterated air-vesicles. This is the condition of complete simple atrophy.

In *simple atrophy of the lung*, the result of uncomplicated bronchitic collapse, the affected parts usually present some-

what different characters from other forms of pulmonary atrophy. They are, in fact, reduced to a lax fibrous or areolar texture, inclosing the remains of bronchi and vessels ; perfectly flaccid, free from all induration or abnormal exudation, and very frequently, in the purest form of the lesion, free even from that excessive deposit of carbonaceous pigment, which is so apt to accompany all chronic affections of the lung. Such atrophied lobules will almost invariably be found, on examining the free anterior or lower margins of old emphysematous lungs ; and, in more recent specimens of emphysema, the anatomist will generally be able to trace several of the stages which I have indicated above, as intervening between collapse and atrophy. The atrophied lobules at the edge of the lung, correspond to the indentations and grooves between the emphysematous parts. On examining them closely, there will often be found a thin lamina, spread out between two emphysematous prominences, like the web of a frog's foot, and composed of the two pleural layers, enclosing the attenuated remains of the pulmonary tissue. Such portions are generally clearly and definitely marked off by the interlobular septa from the emphysematous lobules in their neighbourhood. In other instances, scarcely even this amount of tissue can be traced, and the two pleural layers may appear to be almost in contact over a small space, with little or no intervening substance. To any one who attentively studies a variety of such specimens, it will be apparent that simple atrophy of the lung, in its most complete form, is a lesion only to be distinguished by negative characters. The proper and special elements of the pulmonary tissue have disappeared ; but they are not replaced (as in atrophy from other causes) by any adventitious structure, or even by the thickening or induration of the fibrous basis. For this reason, simple atrophy is sure to be overlooked, unless its traces be sought for in the manner I have described. In the centre of a lung, very many lobules may be entirely atrophied, and leave no visible or tangible evidence of their previous existence.

Simple atrophy, like the lesion which gives rise to it, occurs in the lobular and the diffused form. The latter is chiefly found in the posterior portions of the lungs, near their root, among the great bronchi and the bronchial glands, which are often, in these cases, dark-coloured from infiltrated carbon, even when the lungs are by no means remarkably so. In diffused simple atrophy, the lung is rarely entirely condensed, generally retaining a certain degree of crepitation, but being dense, tough, and fibrous ; sometimes dark slate-coloured, at other times not so ; and in the most marked and exaggerated examples, crossed in every direction by fibrous processes, or septa of considerable thickness and density, corresponding to numerous depressions and irregularities on the surface of the lung, which is usually in these cases very emphysematous in front, and over the surface generally. Such lungs will always be found, when a fresh section is inspected with or without a lens, to present the most remarkable varieties in the size of the air-vesicles ; some of which are entirely obliterated, or very small, and others greatly expanded beyond the normal volume ; the latter condition prevailing, of course, towards the anterior margins in the most emphysematous parts.

The changes impressed upon the form and movements of the chest, by the chronic sequelæ of bronchitis, form so marked an illustration of the doctrines above recorded concerning the supervention of atrophy on bronchitic collapse, that some reference to them here is quite necessary to the complete treatment of this subject. I have already alluded in the first part of this memoir to the modifications of respiratory movement which take place in acute bronchitis in children, while the bones and cartilages are, as yet, inadequate to the task imposed on them of expanding the chest under conditions of increased resistance. Under such circumstances, it is not very unusual to find the movements of the lateral regions of the chest actually reversed, the parietes being, as it were, sucked inwards at each descent of the diaphragm, owing to the external atmospheric pressure overcoming their power of expansion. This yielding

of the ribs I have indicated as probably one of the causes of the extremely frequent occurrence and great extension of bronchitic collapse in very young subjects. In rickety individuals, it is not only more marked, but apt to become permanent, especially when such subjects are affected with any considerable or persistent bronchitic affection. In such cases, the reversed movement of the ribs is stereotyped, as it were, in the form of chest called *pigeon-breast*, in which the sternum is protruded, particularly below, and the whole lateral region, including also the lower costal cartilages in front, flattened, or even at some points rendered irregularly concave.

Many slighter and more partial permanent irregularities in the form of the chest are no doubt owing to infantile bronchitic attacks, either modifying the original expansion of the lung, or producing subsequent partial collapse of its tissue. The immense frequency of such diseases in childhood, and the unquestionable tendency which they are now shown to have towards structural changes, will probably go far to account for many of those disorganizations in the lung, revealed by morbid anatomy in a large proportion of cases, and which often seem to have no connexion with anything in the history of the individual. To a similar source may, in all probability, be justly traced most of the so-called "physiological heteromorphisms" of the chest, described and investigated with such elaborate minuteness by M. Woillez.[1] According to this writer, these slight and trivial deviations of the apparently healthy chest, occupy, for the most part, the same situations as those which are known as the results of disease; and, indeed, it would appear that the "physiological" and "pathological" irregularities are by no means separated by a very distinct line of demarcation. It is quite true that, in many of the individuals presenting these changes, no history of chest-disease can be procured; but every one accustomed to the task knows that the elimination of information in regard to diseases of

[1] *Recherches Pratiques sur l'Inspection et la Mensuration de la Poitrine*, Paris, 1838.

early life, is, in the cases of most hospital patients, nearly impossible, even where the disease has been of considerable importance. In so far, therefore, as these "physiological" irregularities are worthy of consideration at all,[1] I cannot but think that infantile bronchitis may probably have a large share in their production.

In adults, the motions of the chest are altered to a considerable extent in bronchitic affections, though not nearly to the same degree as in children. The greater solidity and firmness of the bones and cartilages opposes an effective resistance to that abrupt and well-marked retraction of portions of the thoracic wall which has been noticed as occurring in infantile bronchitis; the chest expands more uniformly and forcibly over its whole surface, and the phenomenon which gives rise to the pigeon-breast, is not observed when the bones are healthy. That the respiratory motion may be seriously limited, and sometimes even reversed, at certain points in the adult chest during bronchitis, is demonstrated by the observations of Dr. Sibson, made with the aid of his ingenious and useful instrument, the chest measurer.[2] These observations I regret that I have not yet been able to repeat, but of their general accuracy

[1] This qualification is not unimportant, seeing that M. Woillez, by dint of rather ponderous statistical machinery, has arrived at the singular conclusion, that only 1 in 3 of the *healthy* chests, and about 1 in 5 of *all* the chests examined by him, present a strictly *regular* conformation. It is obvious that, with a few more refinements such as those to which this observer has devoted so much labour, the ideal of regularity would require to be sought altogether beyond the pale of humanity. It appears very doubtful whether even the Apollo or the Antinous could withstand the search for "physiological heteromorphisms" by M. Woillez. At all events, artists and anatomists are well aware that, among the poor sons of Adam, strict symmetry and regularity in every point of form, is an occurrence of almost fabulous rarity. The very general lateral curvature of the dorsal spine, and the all but invariable lateral deviation of the nose, are glaring instances known to every one. How often do the phrenologists find a *regular* head? or would any two of them agree upon the subject?

[2] *Medico-Chirurgical Transactions*, vol. xxxi.

I can entertain no doubt. It is evident, indeed, to the eye (which, when employed with the requisite care, is in this case a far less deceptive, and more instructive instrument, than the ordinary measuring tape) that, while even in the severer forms of bronchitis, the chest on the whole expands both in its upper and lower zones, the movement of the latter is much more restricted than that of the former ; and that while the *lateral* expansion of the thorax is circumscribed, the *anterior* movement of projection of the sternum and costal cartilages is usually even exaggerated. The result of this curious modification of respiration is, that in cases of long-continued chronic bronchitis, even during the intermissions of accumulation in the air-tubes, an altered habit of breathing is acquired and permanently retained ; and the stethoscope, as well as the inspection of the chest, can often determine in such cases that respiration is effected chiefly by the upper and anterior portions of the lung, and by the movements of elevation and projection of the sternum ; while the parts of the lung corresponding to the lateral and posterior regions of the chest, remain comparatively little affected by the respiratory act. The modification in the permanent form of the chest which supervenes upon this condition, is tolerably well-known as the " cylindrical " or " emphysematous " chest ; it is marked by increased fulness and prominence of the whole anterior thoracic vault ; often also, but not invariably, by increased arching of the sternum from above downwards ; and perhaps yet more characteristically by a diminution in the lateral, and a relative increase of the antero-posterior, diameter of the thorax.

The true relation of these changes to the existence of collapse posteriorly, and emphysema anteriorly, in the lungs, is not altogether so clear as it may at first sight appear. That the permanent modification of form is the consequence of the peculiarly altered movement of the chest which I have described above, will admit of little doubt to those who have witnessed this movement in characteristic cases of acute and chronic bronchitis. It may

also be freely admitted that the diminished lateral motion is the direct effect of the diminished expansion of the lung in consequence of bronchitic accumulation, with partial collapse, and perhaps subsequent atrophy of its tissue. But to ascribe the increased movement and consequent deformity of the anterior part of the chest, to the production of emphysema, appears to me an error both of logic and of observation. I believe, on the contrary, that whatever be the relation of emphysema of the lung to the " emphysematous chest," *it is not directly or indirectly the cause of that deformity.* And this conclusion appears to be borne out by the following considerations :

In the first place, the increased respiratory movement in the anterior part of the chest, which appears in all cases to be connected with the generation of emphysema, as well as of the deformity above-mentioned, exists in a large number of instances of bronchitis, before either the one or the other condition has yet arisen ; indeed, in its slighter degrees, I believe the increased anterior thoracic movement to be an almost constant concomitant of that affection. In the second place, the existence of very well-marked emphysema, though unquestionably concurring with the highest degrees of the deformity, has always appeared to me to tend to diminution of the abnormal excess of motion ; this excess being always detected most characteristically in company with simply puerile, not emphysematous, respiration. It appears, therefore, impossible that the generation of emphysema can be the cause of that exaggerated motion. Lastly, according to arguments and observations already laid before the reader, it appears that emphysema is a lesion directly due to the forcible expansion of the chest under peculiar circumstances, which seems fairly to exclude the opposite proposition, that undue permanent expansion of the chest can ever be owing to the existence of emphysema.

From observations on this subject, it appears to me susceptible of demonstration,—that the abnormal motion of the chest, in the cases above alluded to, always precedes both the deformity and the emphysema ; that the emphy-

sema frequently precedes the deformity, but in its more chronic and exaggerated forms generally follows in its wake; that a certain amount of emphysema may exist without deformity, and a certain amount of deformity without marked emphysema; and that, in any given case, when emphysema supervenes on exaggerated anterior movement, with or without deformity, its natural effect is to diminish that excessive movement. This last proposition is strongly confirmed by the state of the lung in extreme emphysema, in which the emptying of the air-vesicles is effected with great difficulty, or even may be absolutely impossible, owing to the existence of an apparently valvular obstruction to the egress of air; a condition which suggested to Laennec his theory of emphysema, but which I believe to be a secondary effect, and not a cause of that structural alteration.

Were I to hazard a speculation as to the mutual connexion of this complicated series of phenomena, it would be that indicated in the following propositions, which I submit to the reader, not as ascertained truth, but simply as being the most probable conclusions at present attainable in relation to this subject: 1. The direct tendency of bronchitis is to produce bronchial accumulation, and thereby to restrain the expansion, or even to produce retraction, of the whole lung, and consequently of the chest. 2. To overcome this tendency, forced respiration is at once thrown into action, and the breathing, from being, as in the normal state, mostly diaphragmatic, becomes in a high degree costal and thoracic. 3. In overcoming resistance, by means of costal superadded to diaphragmatic respiration, those parts of the chest whose movements are performed by the most powerful muscles, acting at the greatest mechanical advantage, tend to assume the principal function, while the remaining portions fall into abeyance, or yield in part to the opposing resistance. 4. On this principle the elevation of the sternum, and of the anterior ends of the true ribs, which is effected by the powerful aid of the cervical muscles in addition to the intercostals, becomes the

predominating movement along with the descent of the diaphragm ; while the motions of the posterior and lateral parietes of the chest, which are maintained, in the normal state, by a much weaker force, tend to fall into abeyance. 5. The respiratory forces, instead of acting equally on all parts of the pulmonary surface, and tending to expand it from all points at once, are thus spent in greater measure upon the anterior edge and upper part of the lung, which are in contact with the most mobile parts of the thorax, as well as upon the lower edges and diaphragmatic surface ; and these parts, therefore, become the principal seats of respiratory movement, while the root of the lung and its lateral and posterior surfaces only receive the inspiratory impulse secondarily, or in greatly diminished ratio. 6. The consequence of the inferior power of movement in the posterior and lateral parts of the lung, is accumulation and stagnation of mucus in the tubes ; thence a greater liability to pulmonary collapse and atrophy as the consequence of bronchitis. 7. The consequences of the superior power and greater extent of movement at the edges and upper parts of the lung, and on the diaphragmatic surface, are comparative freedom from mucous accumulation, and consequently from pulmonary collapse and its consequences, and on the other hand, much greater tendency to the development of emphysema from violent and repeated forced inspiration, when partial collapse or atrophy is present elsewhere. 8. The irregularities of movement of the thorax tend ultimately to affect its form, producing in the child the *pigeon-breast*, by lateral flattening of the yet flexible and soft ribs, with depression of the lower costal cartilages, and protrusion of the sternum ; in the adult or older child, slighter lateral flattening, with expansion or bulging of the cartilages, and arched protrusion of the sternum ; and in both the child and the adult, increase of the antero-posterior diameter relatively to the lateral, and of the upper zone relatively to the lower. 9. The deformity of the chest usually accompanying emphysema of the lungs is neither a cause nor an effect of that lesion, but both emphysema

and the "emphysematous chest" depend on the altered respiratory movements in bronchitis, and the exaggerated respiration necessary to overcome the tendency to bronchitic collapse of the lung.

It may appear to some readers that the above explanation of the seat of election of pulmonary collapse and emphysema is superfluous, and that the gravitation of the mucous obstructions in bronchitis to the posterior portions of the organ, is a sufficient reason for the occurrence of collapse in that situation, and of emphysema in the opposite region. To this opinion, however, some facts stand in direct opposition. The most important is that in the horse, in which emphysema and the other diseases of the lung are common, and in which the position of the lung as respects the effect of gravitation is precisely the reverse of what occurs in man, the seats of election of emphysema and of pulmonary condensation are nevertheless nearly as in the human subject. In various experiments on the rabbit, also, I have noticed the same tendency of emphysema to the borders of the lung, and of collapse to its root, although the animals were allowed to maintain the natural position, in which the force of gravitation ought to have had an opposite tendency. For these reasons, I have been induced to ascribe very much less effect than most observers to the simple statical condition of the fluids in pulmonary diseases, and to look for some dynamical cause which would explain the position of the lesions found in bronchitis, pneumonia, and emphysema, in a more satisfactory manner than hitherto. To what extent the preceding paragraph is a successful attempt at such an explanation, must be left to the judgment of the reader, and to the future observation of facts bearing on the subject.

Pathological alterations of the Bronchi in Pulmonary Atrophy and in Emphysema. The memoir of M. Reynaud [1] on obliteration of the bronchi, has been referred to by most subsequent writers as having enumerated and described with great completeness all the more ordinary varieties of

[1] *Mémoires de l'Académie de Médecine*, tome iv.

permanent contraction or dilatation of the air-passages. Indeed it is difficult to conceive anything more completely exhaustive than this memoir, when considered purely from an anatomical point of view, and solely with reference to the air-passages; and having frequently had opportunities of verifying nearly all of his observations, I find it, like most others who have referred to them, not easy to state anything novel upon this subject. But M. Reynaud's researches, though full of anatomical truths, are strangely barren, at least in his own hands, of real pathological interest; which arises chiefly from his having too exclusively pursued the inquiry relative to the bronchi themselves, and not having sought to connect their alterations with those of the pulmonary tissue, with which they are, according to my experience as well as that of others, constantly and indissolubly associated. Somewhat of the same objection applies to Laennec's observations on dilatation of the bronchi, which first gave to this disease a place in pathological anatomy. Accordingly it has been reserved for future observers to discover, that both the dilatation and the contraction of the bronchi are almost always secondary lesions, or at least invariably connected with some kind of disorganization of the pulmonary air-cells. Several of the later pathologists have adopted these views, with more or less decision; among whom may be mentioned Hasse, Rokitansky, Stokes, etc.; but the ideas of Laennec and of Reynaud have still been adopted by many writers on the diseases of the chest, with perhaps too little discrimination, and very little real advance has been made in the pathology of these affections.

The forms of obliteration and contraction described by Reynaud are numerous. It is scarcely necessary to enter into the consideration of the anatomical varieties. The different kinds of dilatation, as described by Laennec and Reynaud, and their relations to obliteration of other parts of the same bronchial divisions, are of more interest. The most frequent are the following: A small bronchus of normal caliber suddenly opens out into a sacculated dilata-

tion, lined by smooth thin membrane, and of more or less rounded form. This dilatation sometimes terminates all trace of the bronchus; in other specimens, the contracted and obliterated remains of bronchi pass from its opposite end towards the circumference of the lung. Again, a bronchus may show a succession of marked irregular dilatations through its whole length, at some parts having the sacculated character, at others being irregularly cylindrical, with partial annular projections, and transverse septa arising from the walls of the dilated tube. Finally, the whole of a lung or of a lobe may be broken up into a series of cavities, having free communication with each other, and with the main bronchi of which they are presumed to be dilatations.

On all the changes here described, there is one important remark to be made—they are invariably found in close connexion with atrophied lung, either of that kind which results, as we have seen, from bronchitic collapse, or some of the more complex varieties which proceed from other lesions, such as tubercle or chronic hepatization. With regard to obliteration or contraction of a bronchus, it is indeed self-evident that this must be the case; and the reader of the foregoing parts of this memoir will see without difficulty by what steps the obstruction of the air-tubes leads to their contraction, along with the collapse and gradual atrophy of the vesicles to which they lead. In the case of dilated tubes, it is an observation of all the later pathologists, in which I fully concur, that the pulmonary tissue around these dilatations is usually impermeable, and in a condition of fibrous atrophy, most commonly without marked induration. It was this circumstance which gave rise to the theory of bronchial dilatation by Dr. Corrigan, which led him to call this disease "cirrhosis of the lung," [1] conceiving that the formation of a peculiar contractile fibrous tissue in the interstices of the bronchi, and the obliteration of the air-cells, led to the expansion of the tubes, by the gradual operation of the inspiratory forces,

[1] *Dublin Journal*, May, 1838.

and constituted the true pathological condition of the disease. Although I have little doubt that Dr. Corrigan's theory is fallacious, in so far as regards the existence of any *new* or *peculiar* fibrous element in this affection, yet there is no doubt that his observations were in other points correct, and that in particular he has the merit of being the first to draw attention in a decided manner to the morbid alteration and obliteration of the air-cells, a fact singularly enough nearly overlooked by Laennec.

What, then, is the origin of bronchial dilatation? The explanations of Laennec and others, which ascribe it to violent coughing, to distension by accumulated mucus, etc., are clearly unsatisfactory, on precisely the same grounds as have been already indicated in the case of emphysema, and which there is therefore no occasion again to repeat here. As in the case of emphysema of the vesicles, it seems more consonant with reason to ascribe these dilatations, as is done by Dr. Corrigan, to the expansive forces of inspiration acting upon the bronchi of atrophied lung. But it is difficult to understand, on this principle, the occasional partial character of the lesion—the expansion of one portion of a bronchial tube into a sacculated globular enlargement, while adjoining tubes and adjoining portions of the one affected, retain their natural size. In such cases it becomes necessary to suppose the existence of some more local affection, rendering the bronchial tube dilatable at the point in question.

To those who have studied this subject only in the light of Laennec's description, the following remarks will probably appear too bold and sweeping a generalization. They are, nevertheless, the result of much consideration, both of the descriptions of authors, and the facts observed in very numerous dissections, as well as in most of the public pathological collections of this country. The conclusion to which I have been led by this survey is, that almost all the so-called bronchial dilatations, and all of those presenting the abrupt sacculated character here alluded to, are in fact the result of *ulcerative excavations* of the lung communicating

with the bronchi. That such ulcerations are not uncommon in bronchitis, especially in the case of children, has been already sufficiently indicated in the first part of this memoir, under the head of bronchial abscess. I have, in fact, incised the bronchi with great care in cases of recent bronchitis with bronchial abscess, and have found the small cavities so described to occupy precisely the same relations to the caliber of the tubes as the larger dilatations which are found in connexion with chronic atrophy. The expansion of these small cavities, either by increase of ulceration, or by the act of inspiration, would clearly in these cases have led to an appearance closely resembling the so-called dilatations of the bronchi in everything except the fine, smooth, and consistent lining. On the other hand, the examination of a very great number of unquestionable instances of chronic pulmonary excavations from tubercle and other causes, has satisfied me that cavities originally formed by ulceration, may become lined by membranes exactly resembling those found in the " dilated bronchi " of Laennec. In no instance that I have seen, has this membrane exactly resembled the mucous membrane of a bronchus; even in the cases of so-called true bronchial dilatation, it is thin, dense, very smooth and glistening, and with comparatively few vessels; in fact, more resembling a serous than a mucous tissue. This description altogether concurs with those of Hasse[1] and others as applied to bronchial dilatation. It is, I think, almost conclusive upon this question, that in chronic cavities, evidently of tubercular origin, I have been able to trace quite satisfactorily the gradual assimilation of their lining to this type; and in several instances to observe cavities, which, but for the existence of others in the same lung in a different condition, would scarcely have been distinguishable from the so-called bronchial dilatations; they being lined by membrane perfectly smooth and glistening, and gradually passing into that of the undilated portion of the tubes leading into them. I have also observed that in such cases there is even the formation of an incipient

[1] *Pathological Anatomy*, article " Bronchiectasis."

epithelium upon this new membrane ; or, at least, of numerous cells which, under the microscope, sufficiently resemble the columnar epithelium of various parts of the air-passages, differing, however, from that of others, and especially of the trachea and larger bronchi, in being of inferior size, and never, so far as I have observed, furnished with cilia. The opportunities of making such observations under the requisite conditions to ensure accuracy, are too rare to permit of my entering into this subject at greater length.

If these observations are admitted as bearing on this question, it will, I think, become probable that the usual origin of bronchial dilatations is in cavities formed in atrophied lung, in consequence of bronchitis or tubercle, and afterwards expanded beyond their original dimensions by the inspiratory force. The conditions that conduce to such expansion have been already considered under the head of emphysema, and its relation to excavations. It will be at once evident that the tendency to expansion of a cavity must be great in proportion to the flaccidity of its walls, and the absence of crepitant lung in its neighbourhood. It is owing to this circumstance, that bronchial dilatation and emphysema of the lungs have been found, to a certain extent, mutually exclusive. The cases most fitted for the development of such permanent excavations, are those in which the whole of one lung has been converted into a series of cavities, with no intervening crepitant tissue. Of this I have seen several examples. One such instance, figured by Cruveilhier,[1] forms an admirable illustration of Corrigan's cirrhosis, and of the real mode of its origin, viz. by ulceration. The whole upper lobe of the lung is converted into the condition of chronic cavities, lined by smooth membrane, and communicating freely with each other ; while in the lower lobe are found recent excavations ; and every intermediate condition between the two

[1] *Anatomie Pathologique*, Livraison, 32, Pl. V. Fig. 3. The case was considered to be of tuberculous origin, yet "the cicatrization (of the excavations) was perfect ; the parietes presented the appearance of accidental mucous membranes."

varieties can be readily traced. Of this case I have on one occasion seen an almost exact counterpart, in a boy, aged about 12 years, affected for a long time with hepatic and pulmonary disease, under the care of Dr. Renton.

Pulmonary Concretions and Cicatrices.—The existence of cicatrices and puckerings in the pulmonary tissue, sometimes accompanied by distinct induration, with much thickening of the pleura in the neighbourhood, and sometimes by rounded whitish masses of atheromatous, chalky, or stony consistence, imbedded in the tissue of the lung, and surrounded by a fibrous cyst, has been long known to morbid anatomists, although more attentively studied of late years. Morgagni, summing up his own experience with that of his predecessors, signalised their existence in connexion with asthma and other symptoms of disease of the respiratory organs.[1] Portal considered calculous concretions of so much importance that he indicated, by means of them, a particular species of phthisis, the "phthisie calculeuse." He maintained the entire dissimilarity between the calculi of gouty and those of scrofulous origin, showing their frequent connexion with phthisical symptoms, cough, hemoptysis, dyspnœa, etc.,[2] which, however, he did not consider as exclusive of the gouty diathesis, inasmuch as he has a "phthisie arthritique et rhumatismale." Portal also considered these calculi as formed in some instances by inspissation of the bronchial humours, as well as by dust inhaled from without,—an opinion which has received some recent confirmation in the special case of the stone-hewer's phthisis, but which, as a general deduction, is contradicted by the chemical nature of these bodies, the pulmonary calculi being now known to consist in a great part of phosphate of lime, and other calcareous and magnesian salts, evidently of organic origin. Laennec was the first distinctly to

[1] *De Sed. et Causis Morb.* Epist. xv. 17, *ad finem.*

[2] *Phthisie Pulmonaire.* Edition of 1809. Vol. i. 478 *et seq.*; vol. ii. 321, 349. The connection of calculi with phthisis was also maintained by Bayle, as well as other still older authors. See Sauvages' *Nosologia Methodica*, article "Phthisis."

protest against the opinion that these pulmonary concretions were necessarily attended by symptoms; having, as he says, frequently found them "in subjects who had presented no sign of oppression or embarrassment in the respiratory organs." The description by Laennec of these bodies bears the marks of a very attentive observation of all their pathological relations. He notices their frequent occurrence with or without the accompaniment of other lesions, and remarks that they are occasionally found in the centre of tubercles, and very frequently along with pulmonary cicatrices, like those which are found in tuberculous individuals; from which circumstance he concludes, that, "in the greater number of instances, they are the result of a cured tubercular affection." He does not, however, deny that osseous and cretaceous concretions may be developed independently of tubercle, but regards this as occurring very rarely.[1]

Since the enunciation of the above opinion by Laennec, as to the source of these pulmonary lesions, it has been for the most part acquiesced in by pathological authorities; and, as usually happens when a doctrine gains general support, even the prudent reservations of its author have been in some danger of being consigned to oblivion. The accuracy of Laennec's observation as to the frequency with which these lesions occur, is more than justified by the later researches of MM. Rogée and Boudet of Paris, and Professor Bennett,[2] who have found that pulmonary contractions and puckerings, with or without concretions and thickenings of the pleura, occur in a very large proportion of the bodies subjected to examination in hospitals. In the cases of Dr. Bennett, the proportion is about 40 per cent. of the whole; while M. Rogée and M. Boudet give respectively 51 and 86 per cent. in their different spheres of observation. It thus becomes a matter of considerable importance to determine whether the connexion of these lesions with

[1] *Auscultation Médiate*, tome ii. chap. iv.

[2] See the paper of Dr. Bennett, in *Ed. Med. and Surg. Journal*, vol. lxiii. p. 406.

obsolete tubercle be subject to no exception; more especially as none of the observers above noticed have, in drawing inferences from the facts adduced by them, indicated any doubt upon this subject. I have thought it right, therefore, to make the following remarks, tending to limit the application of the doctrine of Laennec, which in being made to include all, or nearly all, pulmonary cicatrices and concretions, under the designation of healed or obsolete tubercle, appears to me to have been scarcely warranted by the facts of the case. That I may not be suspected, however, of an equally exclusive bias upon the other side, I may state that the healing of tubercles in this particular manner admits, in my opinion, not of the smallest doubt; and that to anyone who has seen, on a sufficiently large scale, the progress of these lesions, as exemplified in the lungs of those dying of unquestionable tubercle, the conclusions of Laennec as to the frequent cure of tubercular lesions, especially in their early stages, must appear perfectly irrefragable. Nor is there anything in the doctrines of Carswell, Cruveilhier, or other subsequent writers, from which I feel called on to dissent, in so far as they illustrate the different modes in which this cure is accomplished. But, as regards the precise frequency with which the early stages of tubercle become obsolete, I believe that we are not yet in possession of accurate statistical results; and that neither the 40 per cent. of Dr. Bennett, nor the 51 and 86 per cent. of the French observers, represent correctly the proportion of such cases in our hospital bills of mortality.

That simple bronchitis must be responsible for a certain number of pulmonary contractions and puckerings, will be at once evident to the reader of the preceding pages. Every instance of pulmonary atrophy, from whatever cause, which is abruptly defined and surrounded by normal or emphysematous tissue, will necessarily present the appearance of a cicatrix,—more especially if the pleura over it be, as often happens, thickened. It might be supposed that such cicatrices would in simple bronchitis occur chiefly or exclusively at the back part of the lungs; but this is by no

means the case; for while partial atrophy occurs with extreme frequency at the back part of the lungs, it is rarely complete, and almost always in the diffused form; while the obvious pulmonary cicatrices arise from lobular atrophy, which occurs chiefly at the edges and upper parts of the lung. I know of no means by which a simple cicatrix, formed by bronchitis or broncho-pneumonia, surrounded (as such cicatrices often are) by a certain amount of induration and carbonaceous infiltration, could be distinguished from a tubercular lesion, unless the absence of tubercular traces in other organs, and presence of lobular atrophy along the whole edges of the lung with diffused incomplete atrophy behind, and the comparative exemption of the summit of the lung, be considered to indicate such a distinction. Even to one aware of all the characteristics of both forms of disease, I believe cases will constantly occur in which no distinct opinion can be formed; at least such is the result of my experience since my attention has been particularly drawn to this subject. Nor can I for a moment doubt, considering the frequency of bronchitis at all periods of life, that a proportion of pulmonary cicatrices, quite large enough to modify considerably the statistical results alluded to, has been included among the tubercular lesions without having a just claim to this designation. This is particularly the case with M. Boudet, whose enormously high proportion of 86 per cent. could only have been attained, as it appears to me, even among old subjects (and it is not asserted that his were exclusively such) by diligently seeking out every trace of pulmonary contraction, by whatever cause produced. By such a method the traces of extinct pulmonary disease may indeed be discovered in a very large proportion of cases; but certainly not of extinct tuberculous disease.

With regard to concretions, which were found by Dr. Bennett in about 22 per cent. of the bodies opened by him, I have little doubt that they have a tuberculous origin in a large proportion of cases. But something in the way of reservation requires even here to be kept in view. The

occurrence of such obsolete masses of exudation in connexion with old-standing bronchitis is far from uncommon; and as Laennec, and after him many others, have clearly traced tuberculous matter through all its stages into that of complete calculous induration, so has it occurred to me repeatedly to see old bronchial abscesses, having evidently the characters and the usual distribution of bronchitic lesions, in every stage of conversion into these bodies. Even when the conversion was complete, it has appeared to me that a certain proportion of those I have met with might be reasonably referred to this source, from their prevalence at all the borders of the lung, or from their being surrounded rather by what I have called *simple lobular atrophy*, than by any considerable induration. But in this, as in the former case, the observer who looks to all the possibilities of the case is apt to find instances of a sufficiently equivocal and doubtful character in the present state of our knowledge.

Without being prepared to defend the following conclusions as absolute, and without at present entering at length into the grounds for some of them, I may here endeavour to state briefly the inferences to which I have been led by the preceding and other researches in regard to such lesions of the lung as may be suspected to be connected with the extinction of tuberculous disease. 1. There can be no reasonable doubt that open excavations, one or many, completely cicatrised on their internal wall, and lined by a membrane possessing the appearance, and in some degree the minute structure, of an epithelial membrane, may be of tuberculous origin; [1] but such excavations (the *dilated bronchi* of Laennec) may also originate in simple bronchitis, broncho-pneumonia, simple isolated abscess (a very rare disease), multiple or metastatic abscess, syphilitic ulceration, gangrene of the lung,—which last lesion also may have several causes. Of these forms of healed excavation, the tuberculous is probably the most frequent; but there are as yet no good characters to distinguish it from

[1] See the text of *Cruveilhier*, *loc. cit.*

the others, unless collateral lesions, sufficiently characteristic, be found in other organs, or unless the original lesion of the lung be, in other parts of the same or opposite organ, in an earlier stage of its development. 2. Concretions of inspissated pus, atheromatous matter, cretaceous or calculous matter, surrounded by atrophy, with or without induration, are in the great majority of cases the remains of obsolete tubercles, softened or miliary ; but they may also arise from any other form of pulmonary ulceration or abscess, as above enumerated. 3. Pulmonary atrophy, simple or with induration, and carbonaceous deposit, accompanied or not by adhesions and thickening of the pleura, may arise from obsolete tubercles, from bronchitis, from broncho-pneumonia, and probably also, though more rarely, from simple pleuro-pneumonia ; and pulmonary cicatrices arise frequently from all these sources except the last. The *relative* frequency of these lesions as leading to cicatrices is as yet undetermined. 4. The lesions above mentioned (1, 2, 3) *are probably tubercular*, if they occur exclusively or chiefly at the apices and back parts of the upper lobe of both lungs at once ; or in the apex of one lung only, without trace of a lesion elsewhere ; or generally diffused throughout both lungs, but chiefly in their upper lobes, and especially at their back part and apex ; or in any case in company with characteristic traces of tubercular lesions in other organs. In reference to this last clause, it is doubtful whether cretaceous or other deposits in the bronchial glands can, in a case of such pulmonary disease, be considered as a characteristic indication of the tuberculous taint ; but such deposits in other parts of the lymphatic system, especially in the mesenteric and cervical glands, or the traces of old deposit or ulceration in the mucous membrane of the small intestine, would necessarily determine the tuberculous nature of the affection, except in some cases, rare enough in this locality, but of which I have seen examples, in which typhoid or cancerous disease might throw doubt upon the diagnosis. 5. All these lesions *are probably non-tubercular* if they occur in one lung in a generally diffused form, without

traces of tubercle, even obsolete, in the other lung; or in the lower lobes to the exclusion of the upper; or at the edges of the lung in both lobes and not at its apex; or at the root of the lungs only; being in all these cases unaccompanied by tubercles, or the traces of tubercles, elsewhere. 6. After employing all these characters of distinction, a certain number of cases of all the lesions in question will remain of indefinite or unknown origin.

In concluding this series of papers on bronchitis, I am well aware that many important practical relations of the various subjects discussed have been necessarily left almost untouched; but if I have succeeded in throwing any light on these relations, or in giving the practitioner a key to their apprehension in any degree simpler and of more extensive application than has hitherto been accessible, I am confident that the numerous minds at work in this country and elsewhere in the furtherance of practical medicine, will not allow these researches to remain destitute of the assistance which they are calculated to derive from more extensive opportunities of clinical research than I have been able to command. With this conviction, I leave the foregoing papers in the hands of the profession.

NOTE.

Another careful revision of the foregoing paper has revealed the fact—which had escaped my notice—that, on page 266, Gairdner actually shows how the consideration of the connection between bronchitis and emphysema had suggested to Laennec an inspiratory theory. As the paper was composed at different times, spread over a period of about two years, it is probable that some lapse of memory, or absence of mind, on the part of the author must be held responsible for the discrepancy between this statement and the contradictory remark attributing an expiratory theory to Laennec.

CONSIDERATIONS ON THE CAUSES OF DILATATION OF THE HEART, WITH AN ANALYSIS OF EVIDENCE BEARING ON THE CONNEXION OF THAT AFFECTION WITH DISEASE OF THE LUNG.

British and Foreign Medico-Chirurgical Review, 1853, *vol. xii. p.* 209.

The following paper is one which has, ever since its appearance, exercised much influence upon the study of circulatory affections. Even in regard to facts already recognised at the date of the paper, as to dilatation in connection with valvular disease, Gairdner expressed some new ideas and emphasised the capriciousness displayed by dilatation and hypertrophy—a capriciousness difficult to account for. The paper is chiefly devoted to the elucidation of dilatation and hypertrophy, consecutive to pulmonary changes, and shows that both pulmonary emphysema and cardiac dilatation, when associated together (as is so often the case) have one common factor, in partial atrophy or collapse of the lung tissue.

DILATATION of the heart, accompanied, as it usually is by hypertrophy, and by all the symptoms of cardiac disease in their most aggravated form, is a condition which has at all times attracted the attention of pathological anatomists; and by Morgagni, Senac, Corvisart, and almost all subsequent writers, the causes of dilatation, and of the concomitant hypertrophy, have been discussed with a fulness which may appear at first sight to have exhausted the subject. Yet it will not be denied by anyone much in the

habit of observing diseases of the heart, that there occur many cases of dilatation and hypertrophy which cannot be adequately explained upon any clearly ascertained principles; and which even the best-informed and most careful theorists are obliged to dismiss into the region of pure speculation. Thus Rokitansky, after enumerating at great length the individual causes which lead to these diseased conditions,—first, by mechanical obstruction of the circulation, and secondly, by disease of the heart's structure,—concludes, that "in all those cases in which cardiac disease cannot be referred to any of the above enumerated causes, it may originate in *excessive innervation* of the heart." In like manner Hope, after exhausting the known and clearly intelligible causes of hypertrophy of the heart, and adding a few others not so generally known or so clearly intelligible (*e.g.* the influence of "pedestrian tours among the Swiss and Scotch mountains" in producing disease of the heart), takes refuge at last in the theory of "inflammation" to explain (*obscurum per obscurius*) the residual cases. It is unnecessary to refer to the numerous fanciful theories of cardiac hypertrophy and dilatation advanced by the older authors. Most of them repose on too slender a basis of induction to entitle them to the smallest consideration in modern science.

The causes of dilatation, so far as they can be distinctly traced, and are generally acknowledged, may be referred to two heads. In the first place, everything that leads to obstruction of the circulation is conceived to exercise a mechanical effect in overloading the cavities of the heart; and from this over-distension, when permanent, springs organic dilatation. In the second place, all circumstances which diminish the power of the heart relatively to the amount of blood which it has to propel, tend to cause accumulation of blood in the organ, and overloading of its cavities; dilatation being produced in this case as in the former, but not so decidedly, or so frequently in combination with hypertrophy.

These alleged causes of dilatation of the heart are little

open to objection. Of the first, the valvular lesions afford satisfactory examples, and the great constancy with which dilatation and hypertrophy attend on these affections will not be disputed. The fatty degeneration of the heart is an example of the latter; and I believe with Rokitansky and others, that observation will bear out, to a certain extent at least, the idea that this and other causes of diminution of the heart's activity and power may be found concerned in the production of some cases of dilatation. The coincidence of degenerated muscular fibre with dilatation of the heart, especially in those forms in which dilatation is partial and irregular, and associated with a comparatively small degree of hypertrophy, is in truth extremely frequent; though, on the other hand, it must be confessed that the most exaggerated forms of cardiac dilatation and hypertrophy not unfrequently occur apart from any such combination. I think I might even say with truth, that it is quite common, among hypertrophied hearts, to find what in the ordinary forms of chronic fatal disease is rather an unusual occurrence—an organ presenting throughout a normal condition of the muscular fibre.

The most indisputable of all the causes of dilatation of the heart is valvular disease; and the relation of the different valvular lesions to hypertrophy with dilatation of particular cavities, constitutes perhaps the only really unquestionable evidence of the influence of obstruction to the circulation in producing these conditions. Thus, it is well known that extreme contraction of the mitral orifice usually gives rise to great hypertrophy and dilatation of the right side of the heart, and of the left auricle, while the left ventricle often remains small and contracted. Mitral regurgitation, on the other hand, without considerable contraction, leads to hypertrophy of the right side, and enlargement of the corresponding cavities and left auricle, with a minor degree of hypertrophy and dilatation of the left ventricle; and aortic valvular lesions almost invariably determine great hypertrophy and dilatation of the left ventricle, followed by a lesser degree of affection of the right side.

But even in the case of valvular disease, there is often an amount of *capriciousness* (so to speak) displayed as to the occurrence of dilatation and hypertrophy for which it is not easy to account. Nothing is more common than to find the extent to which the heart is altered in size and weight out of all proportion to the amount of valvular obstruction or regurgitation. The most remarkably dilated heart which I ever saw, either with or without valvular disease, was one in which very slight rigidity and possibly slight incompetency of the aortic and mitral orifices existed without contraction of either. The heart lay quite across the thorax, and could not have been less than ten or eleven inches in length, with corresponding increase of breadth. The cavities were enormous. In the private house at which the dissection was performed, I had no means of appreciating accurately the weight of the organ. Similar instances are recorded by Morgagni and others. I sent to the Edinburgh University Museum, some time ago, a heart in which, the ventricles being normal, and the mitral valve alone slightly rigid, the auricles had undergone a dilatation quite enormous. The left auricle was capable of containing a moderate-sized cocoa-nut; the right would have held a billiard-ball; the walls of both were slightly hypertrophied. Unfortunately, the condition of the lungs and other organs was not very accurately noted in either of these cases. As a counterpart to them, I have not unfrequently seen extreme valvular disease with but little hypertrophy and dilatation. In the case of mitral contraction, I have even seen the size of the heart small, and the relation of its cavities all but normal.

These facts show that some important truths are still wanting to complete our knowledge of the causes of cardiac dilatation. But this fact becomes still more evident in considering the cases of dilatation and hypertrophy without valve-disease. For here we constantly find, that without any appreciable mechanical cause of dilatation—at least without any cause of obstruction to the circulation at all commensurate with the effect produced—dilatation arises

to a degree almost unexampled in cases of valvular disease, often accompanied by corresponding hypertrophy; and in these cases, as I have already observed, often without the concomitant of diseased and degenerated muscular fibre. Nothing can be more puzzling than many of these cases of simple hypertrophy and dilatation, or more calculated to seduce the mind into vague and ill-founded speculation; yet it is undoubtedly to the careful observation and analysis of such cases of disease of the heart that we must look for information as to their obscure or unknown causes. I trust, therefore, it may prove neither uninteresting nor unprofitable to give an account of the results deducible by analysis from a considerable recorded experience of these affections, and bearing particularly upon the cases unconnected with valvular disease.

The organic causes adduced by authors as most commonly engaged in the production of dilatation and hypertrophy, apart from valvular disease and congenital malformation, are diseases of the lungs, liver, and kidneys, together with atheroma and aneurism of the great arterial trunks, and especially of the aorta.

Disease of the lungs is commonly supposed to act by producing obstruction to the circulation in the capillaries of these organs, and hence occur distension, and subsequently permanent dilatation and hypertrophy of the right side of the heart.[1] This explanation, though plausible enough, and probably correct to some extent, is not, as I shall presently show, sufficient to account for the peculiar influence of the state of the lung over the volume of the heart—an influence so generally observed by all authors, and so

[1] This opinion is as old as Senac, who states it in most distinct terms, and says, "I have observed that in pleurisies the right auricle and its ventricle are very often dilated: Willis confirms this observation. . . . Some have ascribed the same consequences to phthisis pulmonalis, and apparently with reason." He observes, however, that many phthisical hearts are not dilated, and are even flaccid and small. In asthma, dilatation is more frequent, but it is often difficult to determine whether the asthma or the heart-disease is the primary lesion.

consonant with daily experience, that it can scarcely be doubted.

Chronic diseases of the liver and kidney have been supposed to exercise an influence on the volume of the heart, and to tend to its dilatation. But, further than the undoubtedly correct observation, that these diseases are frequently connected with diseased heart of all kinds, I know of no good evidence on this subject, nor of any rational theoretical explanation of this supposed influence. It seems much more probable, and more consonant with experience as well as theory, to suppose that the affections of the liver and kidney are in general secondary diseases, resulting, like the well-known secondary diseases of the lungs and serous membranes, from disorder induced in the circulation by heart-disease.

With regard to disease of the vessels, and particularly aneurisms of the aorta, there seems to be good reason to accord to these affections some degree of influence over the size of the heart and its cavities—an influence probably due to their interference with the normal reaction of the arterial system upon the circulating blood : as it is a tolerably well-established fact in physiology, that the elastic, and probably also the vital or tonic, contraction of the arteries, holds an important relation to the due propulsion of the blood into the capillaries. So far as my own limited experience goes, however, I should incline to ascribe to disease of the arterial system a much smaller influence in causing hypertrophy and dilatation of the heart than many authors have contended for ; because I have repeatedly seen extreme disease of the aorta in its whole length, and even large aneurisms of its descending portions, without the slightest appreciable hypertrophy of the heart. The only aneurisms which have appeared to me to be invariably, or even generally, productive of disease of the heart, are those of the ascending portion of the arch of the aorta or of the transverse portion when very large, and producing great dyspnœa by interference with the lungs. The facts in my possession are not sufficiently numerous, however, to be

reduced to a statistical form upon this subject, or to warrant any very precise theoretical conclusions.

Passing from these vague statements of opinion, which I have merely thought it right to indicate, because all previous statements have been almost equally vague, I pass to the more precise examination of the facts bearing on the most frequent class of dilatations without valvular disease; those, namely, dependent on affections of the lungs. I have already observed that the influence of pulmonary disease on the heart has been recognised in pathology from the middle of the last century; and probably there is no doctrine in relation to this subject which has remained so unvaried, or has been so universally admitted, as that which ascribes the connexion of hypertrophy of the heart with pulmonary disease to the obstruction of the capillary circulation in the lungs, occurring in all diseases accompanied by dyspnœa, and especially in emphysema and bronchitis. If I call attention again to the connexion of pulmonary and cardiac disease, it is not for the purpose either of re-asserting this doctrine, or of directly opposing it. But theoretical considerations having some months ago led me to reflect upon another view that might be taken of the connexion between primary disease of the lung and cardiac dilatation, I lately undertook a review, and a tolerably searching analysis of the documents which I possessed on this subject, and which were collected before any other theory than the one generally received was present to my mind. The result was, a conviction that the main link in the chain of cause and effect in this case has been entirely overlooked by pathological writers, and that obstruction of the pulmonic capillaries plays a very subordinate part in the dilatation of the heart which results from disease of the lung. I shall, in the first place, endeavour to make good this point, and then state the theory to which I have been led by observation and analysis, as well as by theoretical considerations.

The first circumstance to which I think it necessary to call attention, as demonstrating the insufficiency of the

current theory, is, that in a considerable proportion of cases, and in all the extreme cases of cardiac dilatation and hypertrophy, combined with pulmonary disease, and not accounted for by any other organic cause, the left cavities are dilated as well as the right. This is clearly demonstrated by the first table of observations. No doubt the received opinion as to the connexion of pulmonary disease with hypertrophy and dilatation of the *right* ventricle, is borne out by numerous and well-attested facts, which are every day observed and reported; but the existence of a considerable minority of cases in which marked dilatation of the left ventricle may be observed to accompany the hypertrophy of the right with pulmonic disease, has been to a great extent overlooked; or perhaps such cases have been permitted to swell the lists of cases of what has been called "simple hypertrophy," in the absence of any sufficient theoretical explanation. It is nevertheless true that I do not remember to have seen a single case of *the more considerable* degrees of dilatation of the right side, presumably connected with pulmonary disease, in which the left auricle, and even the left ventricle, did not participate in the affection.

Almost all the numerous instances of pure hypertrophy of the right side are attended by very slight dilatation, and even in these it is scarcely ever to be remarked that the left ventricle and auricle are either diminished in volume or in capacity, as might naturally be expected in dilatation from obstruction of the pulmonic capillaries. On the contrary, the dilatation of the left side has always appeared to me to follow, not indeed always *pari passu*, but at no long interval, that of the right; and undoubtedly, whenever the latter has assumed such a degree of prominence as to affect seriously the weight of the heart, the left ventricle and auricle may be observed to participate in the abnormal condition.

Now why this absence of contraction of the left cavities? Still more, why should they ever dilate under the influence of obstruction of the pulmonic capillaries? The appearances

in hypertrophy and dilatation from disease of the lung differ remarkably in this respect from those in extreme contraction of the mitral orifice, in which it may often be observed, that the left ventricle is almost as remarkably diminished in volume and power as the corresponding auricle, and the right cavities are increased in these respects. M. Cruveilhier, the only author who, so far as I know, has fairly met this difficulty,—and who boldly and correctly states that the "Influence of bronchitis upon the heart exercises itself at once upon the left and upon the right cavities," [1]—ascribes this fact to the strictly harmonious action (*l'étroite solidarité*) which exists between all the cavities. But this explanation, to be valid, *ought* to extend to the facts I have just alluded to of mitral contraction, while, in reality, these are entirely opposed to this view.

The next fact to which I have to direct the attention of the reader is, that it is only a limited number of affections of the lung which exercise the influence I have been alluding to on the cavities of the heart. Of these, emphysema and bronchitis have been long recognised as having the first place; a circumstance which has been naturally enough ascribed to the very great dyspnœa and great amount of obstruction to the pulmonary circulation which occurs in these cases. It is equally a recognised fact in pathology, that phthisis pulmonalis does not, as a rule, lead to hypertrophy of the heart, and, in fact, that this disease is usually attended by a positive diminution in the bulk of all the cavities of the organ. I would add, that while a very considerable proportion of cases of phthisis in a state of progressive advancement show the atrophy I have mentioned, an equally considerable proportion of cases of tubercle in a retrograde condition are accompanied by hypertrophy of the heart, and dilatation, especially of its right cavities. These facts are not difficult of explanation on the obstruction theory; for it is commonly and correctly held that phthisis is a disease in which the constitutional vice, the destruction and waste of the entire system, and

[1] *Anatomie Pathologique Générale*, vol. ii. p. 717.

especially of the blood itself, predominate so much over the local disease, that in the majority of cases death occurs, not from want of breathing-power, but from emaciation and deficient assimilation. Still I might urge it as remarkable enough, that in a disease where the obstruction of the pulmonary capillary circulation is perhaps greater than in any other without exception, and where pathological anatomists have even attempted to show, with some appearance of truth, that the obliterated pulmonary circulation is replaced, in the diseased parts of the lung, by an increased flow through the bronchial arteries and veins (furnished by the systemic vessels and left side of the heart), there should not in the majority of cases be the slightest departure from the normal relations of the two sides of the heart to one another.

I leave, however, this argument, and the more precise statistical detail of the observations I have recorded, as to the connexion of cardiac dilatation with several other pulmonary diseases. It is sufficient to state that, according to the analysis of Sections I. *b*, II. *b*, of the appendix, neither phthisis nor pneumonia appear to exercise any material influence upon the heart, in the way of producing either hypertrophy or dilatation. I cannot, however, pass over so lightly another class of cases, which have been often supposed, theoretically, to determine cardiac hypertrophy by obstruction ; and which certainly fulfil, in a remarkable degree, all the requirements of the obstruction-theory.

When one or both pleuræ are full of fluid, and lung carnified on one or both sides, the pathological conditions of obstruction to the pulmonic capillaries arise in a remarkable degree. This is usually clearly manifested also in the symptoms. If ascites or some cause of abdominal distension be added, perhaps no cases could be named which are in so high a degree attended with cyanosis, distension of the veins of the neck, and all the symptoms of obstructed circulation on the right side of the heart. Yet it is a result of the most careful analysis of my experience of these affections, that they do not in the slightest appreciable degree tend to produce cardiac hypertrophy and dilatation.

I am now in a position to state the theory which has all along guided me in these researches, but in support of which, it will be observed, I have appealed to evidence collected without the slightest reference to any such theory, or indeed any theory on the subject. Until the last few months, it had never occurred to me to doubt or modify the prevailing opinion, and I trust that the reader will accord me the credit of not having allowed preconceptions to prevail with me in the statement of the above facts.

Led by fact and observation, as well as by theory, I am now well assured that it is chiefly, if not exclusively, those diseases of the lung that lead to diminution of its volume, which have any appreciable influence in dilating the heart. Nothing can be more certain than that in emphysema and the collateral affections, dilatation of the heart is a secondary result of extreme frequency. It would probably not be too much to state, that nine-tenths of the cases of cardiac hypertrophy not referrible to valvular disease, are found to be connected either with emphysema of the lungs or with some of the atrophic affections which I have already, in another department of this journal, shown to be its congeners.[1] Now in these cases I have endeavoured to show that emphysema is not the primary, but in all cases the secondary lesion ; and that it depends upon the dilatation of the chest in inspiration, acting upon lungs some portions of which are diminished in bulk by disease. Not to retrace these arguments, I shall content myself with stating, as a corollary of the doctrine I have maintained with regard to emphysema, that the same organic causes which tend to the abnormal dilatation of the air-vesicles of the lung, tend also to the abnormal dilatation of the cavities of the heart : that this affection, therefore, accompanies diseases of the lung, not so much in virtue of the existence of obstruction of the capillary system, *as in consequence of partial atrophy or collapse of the air-cells* ; the special forms of disease with which cardiac hypertrophy is connected being bronchitis,

[1] *British and Foreign Med.-Chir. Review* for April, 1853, p. 452 : article on " Bronchitis, Pulmonary Collapse, and Emphysema."

bronchial obstruction, some forms of chronic pneumonia, retrograde and healing tubercles, and chronic pleurisy or empyema, *when accompanied by contraction of the chest.* If this view be correct, it will follow that emphysema (*per se*) is in no material degree *the cause* of cardiac dilatation ; but rather that cardiac dilatation and emphysema have one common cause—viz. partial atrophy of the lung ; and that emphysema owes its acknowledged frequent coincidence with dilatation of the heart to their arising in many cases out of the same conditions. Further, if the justice of my views in general be admitted, it will follow, that a certain amount of atrophic disease of the lung being given, it will react with even greater power upon the heart, when, from the extended character or peculiar disposition of the lesion, the lung is enabled to resist the dilating power of the chest ; and, therefore, that we may look for a certain number of cases where emphysema is insignificant in amount, and where collapse or atrophy of the lung and the concomitant dilatation of the heart exist in the most extreme form. Such cases, I believe, are not unfrequent, but they have hitherto been generally misinterpreted, the disease of the lung being supposed to be the result, and not the cause, of disease of the heart. The occurrence of dropsy of the pleura and of hæmorrhage of the lung in many cases of heart-disease, has a peculiar tendency to mask the original affection ; and I believe that I have myself observed many cases of cardiac disease so accompanied, and have erroneously supposed the entire pulmonary disease to be a complication, when I might, with at least equal probability, have looked to it as the real source of the more prominent cardiac affection. I am therefore led to hope, that by more careful observation in future, the law I have now attempted to elucidate may be more extensively confirmed, and may even be brought to explain a large class of cardiac diseases which are at present inexplicable, or have fallen to be accounted for by extremely vague pathological theories.

ON THE MECHANISM AND SOUNDS OF THE DILATED HEART.

Edinburgh Medical Journal, 1856, *vol. ii. p.* 55.

This paper may be regarded as in a sense complementary to that which immediately precedes it, and, like it, has had a powerful effect upon subsequent medical opinion. It embodies a most careful investigation into many conditions attended by cardiac dilatation, but the part which is of greatest interest is that in which Gairdner espouses the view, only expressed by one previous observer —M'Dowel[1]*—that regurgitation at the auriculo-ventricular orifices may be produced entirely by dilatation, without any valvular affection. The explanation of what, therefore, was a mystery to Laennec, a puzzle to Graves, and a problem to Stokes, became current coin after the papers of M'Dowel and Gairdner.*

THE doctrine of Laennec in regard to the modification of the heart's action in hypertrophy and in dilatation of the organ, has had, as might be expected, a very great influence on that of subsequent systematic writers. His opinions have been adopted in substance, not only by his own countrymen, but by Hope; they have passed into most of our current text-books; and even the observers in the present day, who have written most guardedly, have not submitted them to nearly so searching a criticism as they appear to me to require. As I have long been of opinion that the whole of this subject demanded careful reconsideration; and as the results of my inquiries (part of which have been

[1] *Dublin Journal of the Medical Sciences*, 1853, vol. xvi. p. 76.

already published in abstract), have tended to somewhat different conclusions from those currently adopted, I think it right to attempt, in this paper, a more extended exposition of these conclusions.

It is convenient to start from the statements of Hope on the alteration of the sounds in hypertrophy and dilatation of the heart, because these statements are substantially the same as those of Laennec ; and, in particular, they contain, in short compass, some of the questionable doctrines on which I mean to remark.

" The first sound " (in *simple hypertrophy*), says Hope, " is duller and more prolonged than natural, in proportion as the hypertrophy is more considerable ; and when this exists in an extreme degree, the sound becomes nearly extinct, but never, according to my observation, wholly so, as stated by Laennec. The second sound is weaker than natural. Laennec says, that in extreme cases it is scarcely perceptible, but I have always found it distinct when the stethoscope was placed about an inch or two higher up than the sigmoid valves." [1]

" The first sound in *dilatation* becomes loud, brief, and clear, like the second. This arises from the muscle, in consequence of its thinness, contracting with increased facility and velocity ; whence the extension of the auricular valves with their chordæ tendineæ, and of the muscular walls themselves, is more sudden and smart. The sound is not prolonged by *bruit musculaire*, apparently in consequence of the feebleness of the contraction. In dilatation with attenuation, the first sound is often so brief and feeble a click, that I believe it to be produced by valvular extension alone.

" The second sound is more or less increased, because the thin ventricle, from having greater facility of movement, performs its diastole, as well as its systole, with greater velocity ; whence the recoil of the sigmoid valves is more sudden. In dilatation, with extreme debility of the organ,

[1] *Diseases of the Heart*, 4th edition, p. 60.

however, I have often found both sounds weaker than natural, from the excessive feebleness of the organ." [1]

It would be easy to show how largely the teaching of our schools and text-books has been influenced by these propositions; but as this is of less consequence than to inquire wherein they are erroneous and uncontradicted, I shall limit myself to the latter investigation.

The statement of the signs of dilatation here given, is, I believe, completely at variance with fact and with observation. It is difficult, indeed, to imagine whence the materials could have been drawn for a description of mere dilatation, apart altogether from other morbid changes; [2] but, in so far as cases of predominating dilatation are concerned, I have sought in vain for any increase in the clearness of either sound of the heart as resulting from it. The whole phenomena of dilatation have appeared to me to consist, not in an increased, but a diminished clearness of the sounds, as compared with those of the heart, or of the hypertrophied organ. In short, they have been the phenomena of an enfeebled heart, modified by certain circumstances, which will be afterwards described. The phenomena of dilatation can rarely, if ever, be observed in such a way as to permit us absolutely to separate them from those of hypertrophy. But the latter condition is often found to terminate in predominating dilatation; and in such cases, when watched over a lengthened period of time, it is not difficult to observe the gradual modification of the sounds which indicated the yielding of the cavities of the heart to the pressure of the blood; a change usually accompanied by a marked increase in the severity of the secondary symptoms. In such cases, it has invariably occurred to me to observe precisely the reverse of what has been stated by Laennec and his fol-

[1] *Diseases of the Heart*, 4th edition, p. 61.

[2] "This disease" (simple dilatation), says Dr. Stokes, "is one of extreme rarity; and as I cannot produce any original observations of such a condition, it appears better to state generally, that the diagnosis is to be drawn more from theoretical considerations, than from observed facts."—*Diseases of Heart and Aorta*, p. 269.

lowers, viz. that the clearness of the sounds (and especially of the first sound), has been maintained up to the point when the great increase in the dimensions of the heart, the feebleness of its action, and the supervention of dropsy and other complications, gave reason to suppose that dilatation had become considerable; and that from this period till death, the clearness both of the first sound, and of the second sound corresponding to the dilated ventricle, has constantly diminished, until, in extreme cases, both sounds have become nearly extinct.

In almost all the works in the hands of our students, in the present day, I find the error of Laennec (if error it be), more or less distinctly reproduced. To this statement there are, however, two remarkable exceptions. By Skoda, whose work might almost be regarded as a professed criticism on Laennec, his statements on this subject are passed over in silence; but the opinions of the German reformer of auscultation very clearly do not correspond, in this matter, with the doctrine of the "Traité de l'Auscultation Médiate." It is, however, only in the work of Dr. Stokes that I find the doctrine of Laennec distinctly disclaimed. "Laennec has stated," says Dr. Stokes, "that a certain clearness or sharpness of the sounds attends the dilated state. This can hardly be admitted, unless we suppose a case, in which there are the combined conditions of thinning of the parietes, with an increased vivacity or force of the muscular contraction. Whether such a state of the heart ever exists, is very doubtful; and it is not improbable that, in the mind of Laennec, the connection between clearness of sound and thinning of the parietes of the ventricles was but a corollary to his doctrine—that the second, or clear sound, was produced by auricular contraction."[1] This remark, taken in connection with the passage above quoted from Dr. Stokes, in a foot-note, may be taken as amounting almost to a denial, on his part, that the doctrine of Laennec has any foundation in nature.

If a statement be incorrect in fact, it is of little consequence on what theoretical view it has been founded. The

[1] *Diseases of the Heart and Aorta*, p. 270.

disproof of Laennec's assertions must, in the first instance, therefore, be the result of exact observation; and, unless my remarks on this subject are consistent with what has been observed by others, Laennec's doctrine will probably continue to be held and taught as correct. But, it is impossible to refrain from observing, that the suggestion of Dr. Stokes, as to its theoretical origin, is, to say the least, extremely probable. We know, as a fact, that Laennec, careful and exact as he undoubtedly was, was misled into many erroneous opinions, by his idea that the second sound of the heart was caused by the contraction of the auricles. It was a clear sound—the auricles were large cavities with thin walls; therefore it was inferred that the ventricles, when brought by disease into a somewhat similar condition to the auricles, would give a somewhat similar sound. Moreover, the same conclusion appeared to follow, from the examination of the first sound over the right, as compared with that over the left, ventricle. The latter appeared to be more dull. The thinner wall, therefore, in this case too, gave the clearer sound: a fact, however, which is, in reality sufficiently explained by its greater proximity to the surface.

It is worthy, also, of remark, as bearing on the evidence by which Laennec's opinion on this point is supported, that even Hope's clear and precise statement of it is guarded by a most significant qualification. "In dilatation, with extreme debility of the organ, I have often found *both sounds weaker than natural.*" [1] A similar qualifying clause occurs in Dr. Walshe's book. "The systolic sound, short and abrupt, may be unnaturally clear, both at the apex and base, if the heart's tissue be firm. . . . If the dilated ventricle be soft, flabby, or fatty, the first sound may be very weak, faint, and toneless; and the second so feeble as to be inaudible at the apex, especially if the heart's rhythm be markedly irregular." [2] Now, I by no means deny that a healthy heart, in excited play, and struggling

[1] *Loc. cit.*

[2] *Diseases of the Lungs and Heart*, 2nd edition, p. 656.

with an increased quantity of blood, will give rise to an increase of one or of both sounds ; or that an hypertrophied heart, in like circumstances, may do so to an even greater extent. In strictly morbid dilatation, however—much more in perfectly uncomplicated dilatation (if such a disease be admitted to exist)—feebleness of the heart's action must always be expected to be commensurate with the degree to which the cavities are abnormally distended. Such feebleness is, in effect, always found in permanent dilatation ; and therefore, to say that clearness of the sound is characteristic of dilatation *only in those cases in which the ventricles are not enfeebled*, is, in other words, to say that this sign is not characteristic of dilatation at all, but of some other condition with which dilatation may be allied.

The next subject to which I wish to advert, is the connection of endocardial murmurs with dilatation of the cavities of the ventricles, in cases in which that condition (almost always accompanied by hypertrophy) occurs without primary valvular disease. It is the doctrine of most authors on heart disease (rather, it is true, inculcated by omission than by express assertion), that a murmur heard under these circumstances is an exceptional occurrence, not due to the dilatation itself, but to some coincident condition. It is my conviction, on the contrary, founded on observations to which I shall presently allude, that a murmur caused by auriculo-ventricular regurgitation (mitral or tricuspid) is at least as frequent an indication of considerable dilatation of the corresponding ventricle as any other of its physical signs ; and, further, that even in cases where such a murmur does not occur, or is not observed, auriculo-ventricular regurgitation is frequently present, and plays a most important part among the sequelæ of dilatation.

It is now many years since I first became convinced that the murmurs heard in the heart during life, and ascribed, on good and sufficient grounds, to the auriculo-ventricular orifices, and especially to the mitral orifice, were not always explained in a satisfactory manner (according to the

state of science at the time) at the *post-mortem* examination. While acting as pathologist to the Royal Infirmary, I was frequently called on to give an opinion in cases in which signs of mitral regurgitant disease had been considered to exist during life, as to whether the condition of the heart, observed *post-mortem*, was such as to account for the murmur. The state of the heart, in most of these cases, has been nearly similar; there have been dilated ventricles, somewhat dilated orifices, stretched and enlarged valves, and not unfrequently a more or less thickened state of their edges, or of the tendinous cords. The degree and kind of the thickening has usually been such as to constitute, in my view, a wide distinction between these cases and those of primary valvular deformity; although I have often seen physicians of considerable experience assume the valvular thickening to be the cause of the hypertrophy, and thus invert, in my opinion, the whole pathology of the case. Further observation did not fail to convince me that the slight degrees of deformity of the valve that existed in some of these cases, were quite accidental, and had no real influence either on the production of the murmur, or on the progress of the disease.

The following are two cases, selected from many others among my records, illustrative of the preceding remarks:

Case I.—G—— R———, æt. 45, was admitted into Ward I., December 5, 1851, with severe bronchitis of six months' standing, which was relieved by treatment, and he was dismissed much better on the 31st of January, 1852. The examination of the heart at this time proved that the precordial dulness was moderate, and the apex beat in the usual position. There is no note of any abnormal sound, although the chest was repeatedly examined. On March 21st, he was readmitted, with a renewed attack of his complaint. The lungs presented signs of emphysema, in addition to those of bronchitis; the impulse of the heart was increased and diffused, and there was a loud rasping systolic murmur heard most distinctly at the apex. On April 20th, signs of pneumonia were developed, and the systolic murmur remained still audible. Two days afterwards, the dyspnœa continuing urgent, he died (under the care of Dr. Christison).

On dissection (P. R. xiv. 109), the heart was found enlarged, particularly on the right side. It was unfortunately not weighed, but the following measurements were taken :

Average thickness of walls of right ventricles, $\frac{1}{4}$ inch.
" " " left " $\frac{5}{8}$ "
Circumference of pulmonic opening, $3\frac{1}{2}$ inches.
" aortic " 3 "
" mitral " 4 "
" tricuspid " 5 "

The valves were normal.

Case II.—J—— Y——, æt. 55. Admitted into Ward I., under Dr. Alison, 14th June, 1852, with symptoms of disease of the chest, of four years' standing. On examination of the heart on admission, there were signs of hypertrophy, together with "a harsh sawing murmur heard distinctly over the apex, and also over the lower part of the chest, indistinctly audible at the base. The second sound was altered in character." This patient died within a fortnight after admission, with symptoms of a complex character, dependent on disease of the heart, brain, lungs, liver, and kidneys. On dissection (P. R. xiv. 150), the heart appeared much dilated and hypertrophied in all its cavities ; the tricuspid orifice was 6 inches in circumference ; the mitral orifice $4\frac{3}{4}$ inches in circumference ; all the valves normal except some slight and doubtful thickening at one or two points of the mitral. Among the columnæ carneæ of the left ventricle there were some shreds of attached fibrine, which appeared to have contracted an organic union with the endocardium.

The well-marked murmur which existed in this case, may possibly be ascribed by some to the great enlargement of the tricuspid orifice, or to the vegetations among the columnæ carneæ. These conditions certainly render the analysis of the phenomena more difficult ; but it will not be denied by any one familiar with the physical signs of heart disease, that according to the recognised principles of physical diagnosis, the murmur scarcely found an adequate explanation. Most authorities affirm, that tricuspid regurgitation is usually unaccompanied by murmur ; and that vegetations on the endocardium, unless placed where the column of blood passes with considerable velocity, *i.e.* in the neighbourhood of the orifices, are not an adequate cause of the *bruit de soufflet*, much less of so harsh and prolonged a murmur as this appears to have been.

In addition to these cases, I could extract from my records a good many others, where the existence of mitral regurgitation was rendered probable or certain by the physical signs observed during life, although the morbid appearances scarcely appeared at the time to explain the occurrence of the murmur. I proceed, however, to notice a case which was observed by myself with the greatest care throughout, and which served to confirm my previous impressions, as well as to excite renewed reflection on this subject.

Case III.—A groom, æt. 33, a very stout and robust man, enjoying general good health, began to be subject to swelling of the scrotum about twelve months before admission to the hospital, on November 28, 1853. He ascribed the swelling originally to exposure to cold and wet; it was painless, and was aggravated by his occupation, which obliged him to ride horses (barebacked) for several hours a day; it ceased shortly afterwards, on his taking care of himself and wearing a suspensory bandage. Six months before admission he became affected with cough; no marked dyspnœa, expectoration scanty. A week before admission, swelling of the scrotum recurred, followed by swelling of the lower limbs, commencing in the thighs; this continued on admission, when the scrotum presented a large mass of solid œdema, about the size of a large melon; the penis being also much swollen and affected with phymosis. He was extremely cyanotic, inclined to drowsiness, but disturbed and restless when asleep, when the breathing was usually attended by stertor. When spoken to, he was intelligent, but the articulation was thick and peculiar. With all this there was no positive dyspnœa, no pain, no paralysis, and no complaint, except of the state of the scrotum. The urine was non-albuminous. The precordial dulness was slightly increased, the impulse strong and diffused, *the sounds generally muffled and low-toned, unaccompanied by murmur; second sound over pulmonary artery very loud.* Pulse small and regular. In left back, slight comparative dulness on percussion, with crepitating râle.

He was bled to ℥xvi, and diuretics were administered. There was some relief, after the bleeding, to the drowsiness; but, notwithstanding the remedies, and an attempt to drain off the scrotal fluid by acupuncture, it was found necessary, on the 10th December, to incise freely the thickened skin over the scrotal tumour. The incisions were followed by a large drain of fluid, and in a few days the scrotum had diminished to a moderate

size, the incisions correspondingly contracting, without any untoward accident.

Towards the end of December, he began to complain of increased uneasiness; the expectoration became more abundant, and the cyanosis more considerable. On the 29th of December, a *distinct bellows murmur was for the first time heard, accompanying the first sound, and exclusively audible at the apex.*

From this time the anasarca and cyanosis continued to grow worse, notwithstanding the employment of elaterium, and of various diuretics and purgatives. The drowsiness, thickness of articulation, and difficulty of breathing likewise increased, and on January 8th, it was considered proper to bleed him again to ℥xviii.

The murmur had, in this interval, greatly increased in intensity, and was now audible all over the chest, still preserving, however, its original character.

On the 13th January, anasarca, ascites, and scrotal œdema had all increased; the dropsy of the limbs to a very threatening degree, and attended with erythema over the dorsum of the left foot. Both feet were ordered to be punctured, and mercury was prescribed, along with the squill and digitalis pill. In the course of the night, a large quantity of serum drained away from his legs, with corresponding relief. The punctures, however, soon healed, and the legs became as swollen as before. Being apprehensive of the results of continued over-distension of the skin (the tendency to erythema having already shown itself, as noted above), I desired an incision to be made into the skin on the back of each foot, and the serum to be drained off as rapidly as possible. It happened that a small artery was divided in the right foot, and as this was not observed at the moment, a great deal of blood was lost in the cloths placed to collect the serum before the accident was noticed and the vessel tied. The immediate effect of this, however, was very salutary; next day he was in all respects better, less oppressed, and much less cyanotic. Two or three days afterwards, a slough presented itself in the incision of the left foot (the one which had been formerly erythematous) and this was followed by phlegmonous erysipelas, requiring farther incisions. Erythema extended up the limb; diarrhœa occurred; ascites and hydrothorax became prominent symptoms, and under this complication of disasters he expired rather suddenly on the 4th February.

During the last month of life, *the cardiac murmur had always been present in an intense degree, diffused extensively over the chest,*

and attended by a distinct thrill, to be felt by the finger at the apex of the organ.

The *post-mortem* examination showed a considerable quantity of serum in the abdomen and in both pleuræ. The lungs were œdematous and collapsed; the other organs mostly healthy, with the exception of the heart.

The heart weighed 22½ oz. The length of the cavity of the left ventricle, after being emptied of blood, was 4 inches; its breadth between the columnæ carneæ of the opposite sides, was 2.8 inches. The thickness of the ventricular wall was, on the left side, five-eighths, on the right three-eighths, of an inch. Speaking generally, and without geometrical exactness, the left side appeared greatly dilated, and the ventricle had exchanged its pointed for a more globular form: the right ventricle was more hypertrophied in proportion than the left, and had kept more of its natural form. The orifices, as measured by graduated balls, had the following diameters:—

Tricuspid . . .	1.8 inches.
Mitral	1.6 „
Pulmonary . . .	1.35 „
Aortic . . .	1.0 „

The aortic and pulmonary valves were competent; the mitral and tricuspid valves were perfectly free from abnormal thickening, as was the whole endocardial surface.

On comparing the measurements of the orifice here given, with those in Dr. Reid's well-known paper on the measurements of the heart, I find that (after the necessary calculations to convert diameter into circumference measurements) the tricuspid and pulmonary orifices correspond almost exactly with the *maximum* of his sixteen observations on the normal male heart, while the mitral is somewhat above, and the aortic very slightly below, the *mean*. It is, therefore, possible that slight, but only slight, relative dilatation of the orifices of the right side may have existed, which would be the more remarkable, as it was the left ventricle which was chiefly dilated. It is also possible that, all the orifices having been originally small, they may all have undergone a degree of dilatation;[1] but, it can scarcely, I

[1] The weights and measurements of the heart, furnished by Bizot and Bouillaud, are much less than those of Reid; and this applies, in a

think, be maintained as probable, that the dilatation of the auriculo-ventricular orifices was such as, *per se*, would be expected to render their valves incompetent. Certain it is, that the valves *looked* quite competent to close the orifices, and in the case of the pulmonary orifice, which was as much dilated as any, they were so. The difficulty, or rather impossibility, of applying any strict mechanical test as to the competency of the auriculo-ventricular valves, is one of the great difficulties in the present inquiry.

In this case, I shall venture to assume that incompetency of the mitral orifice was proved by the physical signs observed during life. The patient, admitted with signs of simple hypertrophy, especially of the right side, acquired, while under observation, a systolic apex-murmur, which shortly afterwards became not only almost musical in character, but was accompanied by very distinct purring tremor at the apex. The development of this murmur was coincident with an increase of the dropsy and cyanosis which existed at his admission. What then was the cause of this murmur in the left auriculo-ventricular orifice? It is probably not to be found in mere dilatation of the orifice itself; it is still more certainly not to be explained by anything in the state of the valve. It appeared to me, therefore, that the condition of the ventricle itself, apart from that of the orifice and of the valve, was probably sufficient to account for regurgitation through the auriculo-ventricular orifice.

Before submitting, however, the further considerations which have induced me to adopt the opinion alluded to, I shall examine the evidence on this subject which results from the clinical observations of others. I may remark, in the first place, that my own observations, since the period of the case above recorded, have furnished me with many cases to which I am disposed to apply a similar interpretation;

marked degree, to the orifices. But, apart from the smaller average size of the Frenchman, as compared with the native of Scotland, I believe that Dr. Reid's subjects were more carefully selected as illustrative of the strictly normal state of the heart, which, in disease, very readily undergoes a degree of atrophy.

but from the very general coincidence of enlargement of the orifices with dilatation of the ventricle, I have not yet succeeded in obtaining another fact so well fitted to carry conviction to the mind of those who may require (as indeed they have a right to require) the most exact kind of evidence. The observation above recorded was made with peculiar care as to every detail, in consequence of my attention having been particularly drawn to the subject almost immediately before, by the occurrence of an almost exactly similar case in the same ward. In both cases, the report was drawn up in the most elaborate and careful manner by Dr. Cowan, then my clinical assistant (now assistant-surgeon, 55th Regiment). But, the latter case considered as a crucial instance, was vitiated by a degree of uncertainty about the site of maximum murmur, and by the fact of a considerably dilated tricuspid orifice; and I have preferred, therefore, not to advance it here.

The occasional existence of a *bruit de soufflet*, with simple hypertrophy and dilatation, was recognised by Laennec. "The *bruit de soufflet*," he says, "exists almost constantly in the heart, in subjects affected with contraction of the orifices of this organ. *It is met with often enough in those affected with hypertrophy or with dilatation*; but it is found still more frequently in persons who have not any lesion of these organs, and who are attacked with very different affections." [1] From the remarks which follow these sentences, as well as from those on the *frémissement cataire*, in the next article,[2] it is plain that Laennec, while recognising the various pathological conditions under which these phenomena occur, did not trust himself to enter minutely into the mechanical causes of their production; but rather hastily merged the difficulties of the subject in the general idea, that a "more or less marked nervous agitation," or a "peculiar modification of the innervation," was their most constant accompaniment, if not their proximate cause.

These statements of Laennec have been much modified, and, in part discredited, by later authors, who have possibly

[1] *Auscultation Médiate*, vol. ii. p. 441. [2] *Idem*, p. 453.

considered them to be biassed by a doubtful theory; that which ascribed the *bruit de soufflet*, as well as the *frémissement cataire*, to a modification of the nervous influences acting on the heart. Most of Laennec's contemporaries, and especially Bertin and Bouillaud, appear to have rejected the idea of murmurs occurring in cases of uncomplicated hypertrophy and dilatation—at least, the presence of a murmur is not alluded to in relation to the diagnosis of these affections. Hope, on the other hand, did more justice to the statements of Laennec. He asserts that murmurs do, indeed, accompany simple hypertrophy and dilatation, but that they occur only in the aortic orifice, and with the first sound, and that they are "restricted to those cases almost exclusively in which there is anæmia."[1] Accordingly, he remarks, in another place, that an inorganic murmur "may easily be distinguished from that of valvular disease, by the following criteria: 1. It is confined to the aortic orifice (so far as I have yet discovered), and to the first sound," etc., etc.[2] These very precise statements seem to have produced an all but universal impression—firstly, that the anæmic, or functional murmurs, are heard only at the base of the heart; and, secondly, that a systolic apex-murmur is pathognomonic of valvular deformity. It is true, that many of the best writers on heart disease (as, for instance, Latham, Walshe, and others), forbear to repeat Hope's assertion in an unqualified form, and thus mark a certain want of confidence in it. Dr. Stokes, moreover, goes a step further, in commenting on a case adduced by Dr. Graves, and presently to be referred to,[3] as well as in other parts of his excellent work. The general views of Dr. Stokes, on the signs of dilatation, with respect to valvular murmur, are of the highest importance. "Although, in theory," he

[1] *Diseases of the Heart*, chap. iv. section iii. 4th ed. p. 88.

[2] *Ibid.* chap. ix. section iv. He adds: "Here is one of the *great advantages of particular valvular diagnosis*, as the auscultation can *at once exclude* the other seven murmurs to which the heart is liable *from organic causes only*."

[3] *Diseases of the Heart and Aorta*, p. 183.

remarks, " we do not admit true valvular murmur as a sign of dilatation of the heart, yet, on the other hand, when the dilatation of the cavities is carried beyond a certain point, valvular insufficiency may result, and then a murmur is produced, which, though having its origin in the valvular orifices, does not proceed from valvular disease." [1] And in another place, he says, " We are not justified in declaring, that simple dilatation is never attended by murmur ; nor again, that where murmur does exist, it is to be attributed, as Dr. Walshe believes, to an enlargement of the orifices consequent on dilatation of the cavities." [2]

Since the greater part of this paper was written, I have been favoured with a communication from Dr. Stokes, on this subject, which places his opinion, if possible, in a still stronger light. He informs me, that in one case, which occurred to him many years ago, he witnessed a systolic apex-murmur, in what he was inclined to consider, and what actually proved to be, a dilated heart without valvular disease. The murmur became afterwards less distinct, and then disappeared altogether ; nor did it return, although the symptoms and signs of dilatation underwent a progressive increase. This patient, Dr. Stokes writes, " consulted a number of the London physicians, and was told by them, that he had valvular disease. His confidence in me was so much shaken, that, on his return to Dublin, he gave me up for a long time. I had recommended a tonic treatment, and that he should travel ; but the diagnosis given to him in London, made him believe that I had been in error, and had done him a mischief. The case is an example of the cessation of a bellows murmur, while the symptoms of dilated heart went on increasing, and is so far interesting. It illustrates also the danger of the diagnosis adopted by Hope's followers."

From more than one careful and experienced observer in London, I have obtained information as to the occurrence of cases of dilated heart attended by mitral regurgitant murmur. Indeed, I apprehend it would be found, on

[1] *Diseases of the Heart and Aorta*, p. 271. [2] *Idem*, p. 261.

investigation, that in no hospital possessing a large series of carefully recorded *post-mortem* examinations, have such cases remained wholly unnoticed, although the silence, or, at least, the hesitating and equivocal admissions of many systematic writers upon this subject, show, in the strongest manner, the necessity of its reconsideration, with a view to bring the doctrines of the schools, and the opinion of the profession at large, more into harmony with individual experience. In the meantime, the following incidentally recorded cases and remarks of authors may not be without their value, as showing the present position of the question.

Dr. Graves [1] records a case which is very much to the point. In a man, subject to chest disease for eight years, there was a systolic bellows murmur " audible all over the cardiac region, but remarkably intense to the left of the nipple. This did not ascend along the course of the aorta, nor was it accompanied by any fremitus. There was no evidence, during life, of tricuspid regurgitation, and, after death, all the valves of the heart were found perfectly healthy. The heart was hypertrophied, and the pericardium universally adherent, by firm cellular tissue." Dr. Graves intimates his opinion, that such a case cannot be distinguished from one of mitral valve disease ; and Dr. Stokes, in referring to it (*Diseases of the Heart and Aorta*, p. 182), says that it is " a good illustration of the accidents, so to speak, which may be in store for those who are over-confident in special diagnosis."

In an excellent series of papers, illustrative of the signs and symptoms of dilatation of the heart,[2] Dr. M'Dowel has adduced a number of instances in which that condition, apart from valvular deformity, was accompanied by a systolic apex-murmur. In all of these, however, the murmur is ascribed to regurgitation, consequent upon dilatation of one or other of the auriculo-ventricular orifices. In opposition to the usual doctrine, with respect to tricuspid regurgitation, Dr. M'Dowel contends that this condition

[1] *Clinical Medicine*, vol. ii. p. 167.

[2] *Dublin Quarterly Journal*, vols. xiv. xvi. xviii.

frequently gives rise to a murmur; although he admits that, in a softened state of the heart, it may fail to do so. This paper is a most valuable contribution to the clinical history of cardiac disease.

In two cases of great interest, detailed by Dr. Halliday Douglas,[1] a systolic apex-murmur, "not to be distinguished from the murmur of mitral incompetence," accompanied a sacculated aneurism of the left ventricle, situated at the apex, and causing divergence of the *columnæ carneæ*. In one of these cases, Dr. Douglas considers it probable that the valve was actually incompetent from this cause—in the other, he regards the aneurism itself as the probable seat of the murmur.

On the 4th of February, 1851, Dr. Hare, of University College Hospital, presented to the Pathological Society of London, the heart of a child (age not stated), who had, during life, the symptoms and signs of hypertrophy of the heart. A murmur accompanied the systole, "loudest at the angle between the ensiform and left costal cartilages. Here it was a loud rough sound, entirely superseding the first natural sound, and of longer duration than a natural first sound. In the middle of it was superadded a more acute sound of about a third of the duration of the rough murmur, and like the sharp switch of a whip, or a short sharp mew. The murmur was distinctly audible in the left interscapular region, especially at its lower part, and rather less distinctly in the right interscapular region. There was exceedingly strong pulsation in the external jugulars, both in the recumbent and sitting posture; each was distended to more than the diameter of the child's thumb—the right one rather more than the left." In this case "*no malformation nor alteration of the valves or openings could be found.*" The murmur was, therefore, attributed, by Dr. Hare, "to regurgitation through the tricuspid orifice, caused by excessive distension of the right side of the heart, the anterior and right curtains of the tricuspid valves attached to the yielding wall of the right ventricle

[1] *Monthly Journal*, vol. ix. pp. 1033-5.

being, from this cause, so far drawn aside as to prevent the complete closure of the auriculo-ventricular orifice at each systole of the heart."[1] Dr. Hare's explanation of the murmur in this case, is very similar, in some points, to the one I am about to propose; but I think that it admits of a more general statement, and an application to the case of both auriculo-ventricular orifices. This case is, nevertheless, most instructive; for, whether the source of the murmur was on the right or on the left side of the heart, the fact of tricuspid regurgitation, accompanied by a systolic apex-murmur, and without any marked dilatation of the orifice, is important. From expressions in the report quoted above, I gather that the size of the orifice was considered by Dr. Hare not disproportionate to that of the valve, although it is difficult to form a precise conclusion from the measurements given, inasmuch as the age of the child is not stated; and, if it were, there are not sufficient data from which to form a judgment as to the natural size of the orifices in early life. It appears, however, that the circumference of the aortic orifice was $1\frac{3}{4}$ inch, that of the pulmonic $1\frac{1}{2}$ inch, of the mitral 2 inches, and of the tricuspid $2\frac{1}{2}$ inches; and, as the aortic and pulmonic valves were sufficient to close their respective orifices, I think it will be admitted that no such disproportion of size existed, as, when compared with the healthy adult heart, to favour the idea of regurgitation through either auriculo-ventricular opening through *mere* dilatation of the orifices. The case is, in this point of view, important, though perhaps not conclusive.

[1] Report of the Proceedings of the Pathological Society of London. Fifth Session, 1850-51, p. 72.

A SHORT ACCOUNT OF CARDIAC MURMURS

Edinburgh Medical Journal, 1861-62, *vol. vii. p.* 438.

The paper which has now to be considered marked the commencement of a new epoch in physical diagnosis. It made its appearance while the early enthusiasm over auscultation had not quite exhausted itself, but when the study of physical signs was entering upon a more critical phase. It will be found in these pages that the author points out how very harmless many murmurs are. This is a matter to which Gairdner paid the greatest attention all his life, and he led the way in recognising the real importance of a thorough appreciation of cardiac murmurs in prognosis. The largest part of the following paper is devoted to a careful analysis of the rhythm and localisation of murmurs. In connection with the former, Gairdner, for the first time, made use of diagrams, in order to represent the relation of the audible and tangible phenomena to the physiological movements and sounds of the heart. This was quite a new departure, and it is one which has been followed by every teacher of medicine since his introduction of this graphic method. Along with this new departure, he also brought into existence a very exact terminology of murmurs—auricular-systolic (A.S.) when the murmur is caused by the expulsion of the blood from an auricle; ventricular-systolic (V.S.) when the murmur is caused by the blood flowing outwards from the ventricle, either in its natural course, through the aortic and pulmonary orifices, or backwards, by regurgitation, through the auriculo-ventricular orifices; ventricular-diastolic (V.D.) when produced by the stream of blood entering the ventricle, either from the auricle, or from an artery.

The rhythm having been determined, he next passes on to consider

the place of origin—at which of the four valvular orifices the murmur is produced, and in this he lays down, more definitely than had been done up to the date of the paper, clear regulations for the localisation of cardiac murmurs.

The only matter of contention which is still left, as regards the subjects of this paper, is concerned with the murmurs of mitral and tricuspid obstruction. The auricular-systolic murmur was heard, although misunderstood, by Bertin,[1] *but it was clearly described, and correctly explained, by Fauvel.*[2] *Stokes*[3] *followed in his footsteps, and the teaching of Gairdner may be said to have placed the current theory upon its present basis. It is to be remembered, however, that Ormerod,*[4] *not long after the appearance of the following paper, assailed the views of Fauvel, Stokes and Gairdner. Barclay,*[5] *at a later period, again revived the objections to Fauvel's views, which were once more attacked by Dickinson*[6]*; at the present day, Brockbank*[7] *has, in season and out of season, kept the flag of revolt flying. The recent work of several observers, especially of Mackenzie,*[8] *shows the presence of an auricular form of venous pulse, when the auricular-systolic murmur is present, and its absence when the murmur disappears. Lately, on the strength of tracings obtained with the string galvanometer, doubts have been thrown by Stuart Hart*[9] *upon the auricular origin of this murmur. From a careful study of several admirable electro-cardiograms obtained in my own wards, it is clear that the auricular wave may be present without a presystolic murmur; but no presystolic murmur has ever been found in the absence of the characteristic auricular wave of the electro-cardiogram. On general principles, it appears to me impossible that the presystolic murmur can ever appear*

[1] *Traité des Maladies du Cœur*, Paris, 1824, pp. 176 and 186.

[2] *Archives Générales de Médecine*, Paris, 1843, série 4, tome i. p. 1.

[3] *Diseases of the Heart and the Aorta*, Dublin, 1854, p. 128.

[4] *Medical Times and Gazette*, London, 1864, vol. ii. p. 154.

[5] *Lancet*, 1872, vol. i. pp. 283, 353 and 394.

[6] *Lancet*, 1887, vol. ii. pp. 650 and 695.

[7] *Medical Chronicle*, Manchester, 1897, vol. vii. p. 161; *Edin. Med. Jour.* 1899, N.S. vol. v. pp. 236 and 341; *The Murmurs of Mitral Disease*, Edin. and London, 1899.

[8] *Diseases of the Heart*, London, 1908, p. 297.

[9] *Medical Record*, 1911, vol. lxxx. p. 2.

without the P. wave (as it is generally called), i.e. *that produced by the movements of the auricles. It is to be remembered that, if the auricular-systolic murmur occurs in the absence of any wave, it means that it takes place independently of the generation of any electro-motive force—which, to my mind, is absurd.*

In estimating the importance of a cardiac murmur, we must take care not to be misled by its mere *acoustic quality*, so to speak. I need hardly tell you that it is not the loudest or the roughest murmurs in all cases that are the most really significant, or the most fraught with danger to life. Often, indeed, quite the opposite of this is true; it not unfrequently happens that as the prognosis of a murmur gets worse, the murmur itself becomes less and less, or even vanishes altogether. And the *same* murmur may at different times appear to be blowing, sawing, grating, rubbing, or even musical in character, while its real value to the auscultator, as respects diagnosis and prognosis, may remain unchanged through all these changes of quality. On the other hand, the quality of the murmur may remain the same; it may be constantly very rough and loud, and the prognosis may be good, or constantly very soft and low, and the prognosis bad. In short, *the mere fact that a murmur exists, and has a certain acoustic quality*, tells very little indeed as regards the character of the case.

For, in the *first* place, the existence of a murmur does not tell that there is organic disease, or even any disease at all.

In the *second* place, it does not tell, in many cases, whether the disease, supposing organic disease to exist, is external or internal to the heart.

In the *third* place, the mere existence of a murmur, apart from a careful and detailed scrutiny of the collateral facts, does not tell if the disease is old or recent, increasing or diminishing in intensity, requiring or not requiring treatment, likely or not likely to affect greatly the duration of life.

Generally speaking, I would say that the tendency of half-instructed auscultators is to overestimate the importance

of the murmur *as a fact*, and to underestimate its importance *as a means of investigation*,—to pay too much attention to the mere existence of the sound, and too little to the circumstances in which it occurs. And from this springs another tendency, which is, to take too grave and sombre a view of cardiac murmurs generally, and especially of such as are loud and obtrusive. I could give you numerous instances of this tendency, but the following may probably be sufficient.

I was made acquainted some years ago with the case of a medical practitioner of great eminence, and of special reputation as an auscultator, who, at a period at least a quarter of a century before his death, and when he was quite a young man, discovered a cardiac murmur in himself, referable to the aortic valves. He at first made himself very anxious and unhappy about it, and used flexible stethoscopes to follow its variations ; but finding the murmur persist, while he felt himself none the worse, he gave up thinking about the matter, and in the end died of a form of tubercular disease, without any distinct symptoms connected with the heart. There was found in his body a lesion of the aortic valves showing the following characters : two of the valves were united by their edges over about a third of an inch in length ; the current of blood was therefore obstructed, or at least broken, and the obstruction produced murmur ; but it was not sufficiently obstructed, as the result showed, to interfere with the circulation, or precipitate a fatal result. Nay, it did not interfere even with the size or form of the heart, which was in every other respect normal, or even rather small.

I know another case at this moment in which a mitral murmur has persisted in a medical brother for nearly half a lifetime, without seriously interfering with the functions, or showing any obvious tendency to shorten life.

Again, a young and active man once applied to me on account of a cardiac murmur which had been detected by a medical friend some time before, and had caused him considerable uneasiness. I examined and found a quite

decided aortic murmur with the first sound; and from its permanence and constancy of character, after several examinations, I was convinced that it was valvular. But, keeping in view the insignificant character of the symptoms, and the absence of evidence of hypertrophy, I thought myself justified in giving merely a cautious, but not a very discouraging, opinion. After losing sight of him for some months, I met my patient accidentally on the banks of Loch Tummel, in Perthshire. He was in the midst of a walking tour in the Highlands, and had been pacing it over the hill from Blair in Athole, along with a stalwart and kilted companion, whom I knew well as a man in the full vigour of an extremely robust frame and excellent constitution. He told me that he had suffered nothing, and was, in fact, quite well. To this case, too, I know almost a parallel one in another young man, who has had a very well-marked aortic murmur for several years, but is able to perform without uneasiness all the ordinary duties of an active life.

Once more: The young son of a merchant in this city was brought to me by his father on account of a very singular and loud murmur which had been observed almost over the pulmonary artery, and with the first sound. There had also been certain symptoms, but not at all aggravated symptoms, of cardiac disturbance. I do not profess, even now, to be quite sure as to what is the character of this murmur; but I certainly was far from entertaining so favourable an opinion of the case as in the others mentioned before. For, not to dwell on the fact that there was a certain amount of cardiac uneasiness, it is consistent with my experience that valvular diseases formed during the period of growth of the body are commonly much sooner fatal than those established for the first time at a later period of life. I mention this case to you, not as a parallel one to the others, but simply to note as a remarkable fact that this boy has grown up to manhood not only without increase, but with a positive diminution of his symptoms; while the murmur remains, or remained when I last examined him, at least as loud as ever.

I will give you only one other instance of life preserved, and a certain degree of bodily activity maintained, long after the occurrence of a cardiac murmur, and one of a kind usually more formidable than any of the foregoing. There is a man in this city, whom I meet frequently in the street, and whom I know to have been for *at least* the last five or six years affected with aortic obstruction and regurgitation, indicated by a double murmur over the base of the heart, and in the great vessels at the root of the neck. This man discovered the murmur for himself, as, indeed, there was no difficulty in doing ; for the murmur with the second sound was at one time of highly musical quality, and could be heard at almost any distance. He is a highly irritable and nervous person, dyspeptic, thin, and disposed to be fidgety about himself ; but so far from having got worse since I knew him, I am confident he has been much better ; and he now works as a light porter in an office with apparently quite as good a tenure of life as he has had at any period since the murmur began.

Of course I do not adduce these facts to infer from them that the prognosis of a cardiac murmur is not usually grave. I only wish to show by instances that there are good grounds for qualifying the gravity of the prognosis in certain cases, and that hopes may be held out, where the symptoms are not such as to forbid them, of considerable comfort and extension of life even in persons affected with incurable organic lesions of the valves. And hence it follows that the determination of the mere fact of a cardiac murmur, and even of its character to the ear, leads a very small way indeed in the thorough appreciation of a case of cardiac disease. It is necessary to follow up the inquiry by a great number of considerations of detail, some of which, but only a very few, I shall try to overtake at present.

The first question that follows on ascertaining the fact of a murmur is,—What are its characters and seat ? in other words,—How is it pathologically related to the structures which produce it ? Generally speaking, I would say that of every murmur, unless its acoustic character and history

are very well defined, you must assume, in the first instance, that it *may* be exocardial or endocardial, old or recent, from disease, malformation, or mere functional disturbance; it may be caused by the smallest fragment of obstruction to the passage of the blood, or by the most minute leakage of a valve, or by the slightest possible bit of old rough deposit on the surface of the heart; or it may, on the other hand, be the result of a ruinous injury of structure, which will certainly bring the circulation into an unmanageable condition in a very short time. You have to work out these questions as well as you can from the evidence, taking care not to lose sight of any part of it; and the first part (only the first and smallest, but still an *essential* part of the inquiry) is—What is the pathological character of the murmur?

To determine this it is necessary to observe particularly two points, which form the finger-posts (as it were) of the whole inquiry, and in many cases serve to direct it to a safe conclusion. We have to study carefully—1st, The Rhythm; 2nd, The Place of the Murmur. And as the great majority of murmurs are valvular in origin, it is on the whole a good rule to try all murmurs which require detailed investigation by the tests of the valvular murmurs; to exhaust the hypothesis of a valvular origin before proceeding to any other.

I. The first point, then, is *Rhythm*. What has to be ascertained under this head, is the relation of a murmur to the different physiological acts which constitute a complete cardiac pulsation, viz. the contraction, dilation, and rest of each of the cavities. You have to define the murmur as occurring during this or that portion of the heart's action, or during the pause which intervenes between two periods of activity. To do this, you have to watch carefully its relation to the normal sounds, to the impulse, and to all the other externally appreciable phenomena which attend upon the action of the heart.

Now, these phenomena are partly audible, and partly conveyed by the sense of touch. We may set aside, in the meantime, the visible phenomena as of minor importance.

I have found it desirable in teaching to use a diagram (Fig. 35) by which you will be able to see at a glance the whole audible and tangible phenomena of the heart's action, and their relation to the physiological movements which cause them. If I venture to dwell on such elementary facts for a little, it is because I have often observed that great confusion results, not so much from ignorance of them,

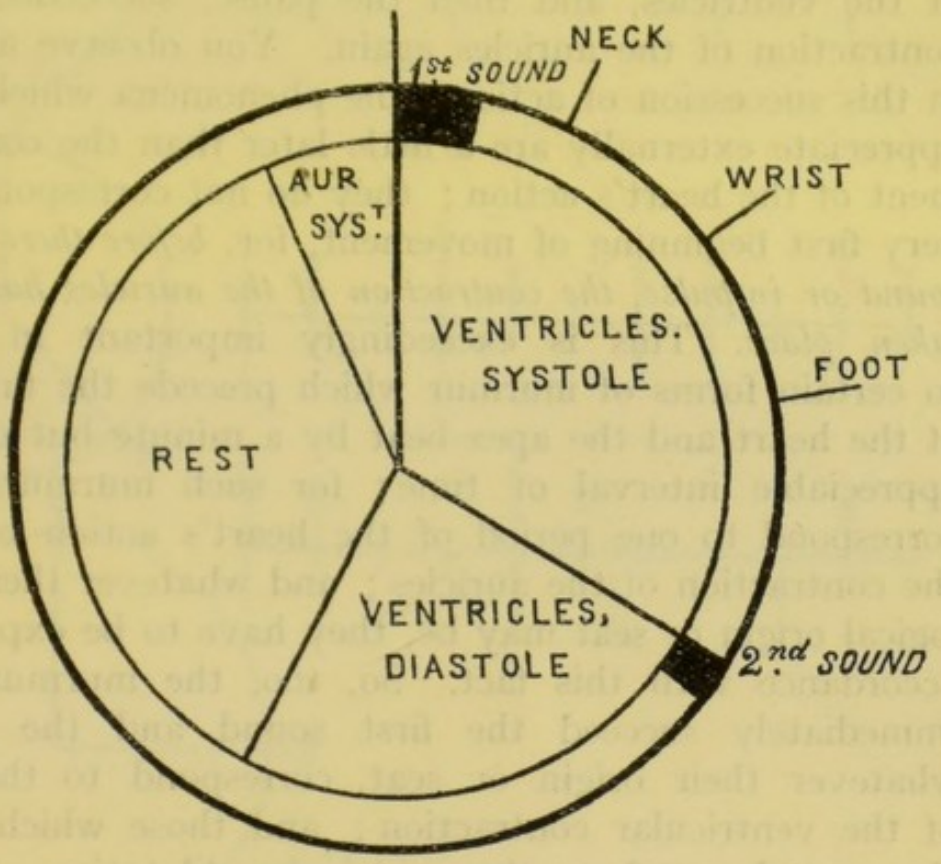

Fig. 35.

as from the want of such an accurate and instantaneous appreciation of them as is required for purposes of diagnosis. It is necessary not only to know the rhythm of the heart as a matter of theory, but to have such a vivid conception of it as calls up immediately, in connexion with any single phenomenon, the whole of the others with which it is in relation. And it is to communicate such useful and practical conceptions that I have been in the habit of employing the diagrams that you see here.

In Fig. 35, the physiological action of the heart, apart from its external manifestations, is indicated by the inner

circle and its divisions; the external rim is occupied by marks corresponding to the sounds; and the different pulses or impulses are portrayed by lines projecting from the circumference of the outer circle. You observe the succession of actions, or physiological facts, which constitute a cardiac pulsation, beginning with the contraction of the auricles, then that of the ventricles, then the rapid dilatation of the ventricles, and then the pause, succeeded by the contraction of the auricles again. You observe also, that in this succession of actions, the phenomena which we can appreciate externally are a little later than the commencement of the heart's action; they do not correspond to the very first beginning of movement, for, *before there is either sound or impulse, the contraction of the auricles has already taken place.* This is exceedingly important in relation to certain forms of murmur which precede the first sound of the heart and the apex-beat by a minute but distinctly appreciable interval of time; for such murmurs clearly correspond to one period of the heart's action only, viz. the contraction of the auricles; and whatever their pathological origin or seat may be, they have to be explained in accordance with this fact. So, too, the murmurs which immediately succeed the first sound and the impulse, whatever their origin or seat, correspond to the period of the ventricular contraction; and those which succeed the second sound, to the ventricular dilatation. Of these facts I shall make use presently in the further definition of murmurs.

Let me, however, in the first place, advert to certain difficulties that are apt to occur in estimating the normal rhythm of the heart, and therefore of its murmurs in disease. The entire period of the heart's action may be divided into a period of motion and one of rest; the former being again subdivided into three distinct stages or periods indicated in the diagram (Fig. 35). Now, it is important to observe that when the heart's pulsations follow one another with great rapidity, the period of rest is reduced to a minimum; and when, on the contrary, the heart's action is slow, the period

of rest is much lengthened, in proportion to the period of movement. The consequence of this is, that the normal

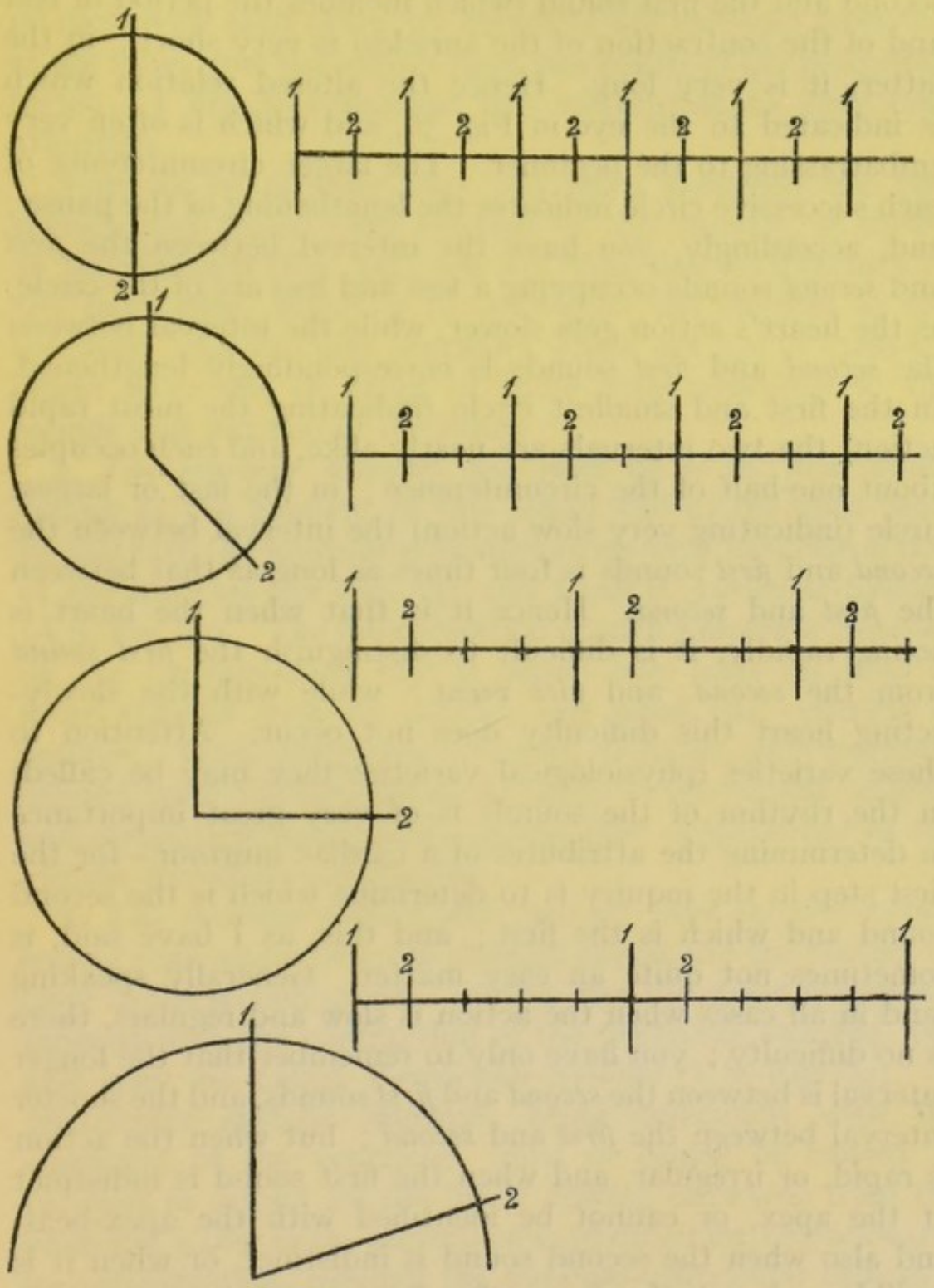

Fig. 36.

sounds, which occur during the contraction and dilatation of the ventricles, change their relation to one another,

according as the pulsations are in rapid succession, or the contrary. In the former case, the interval between the second and the first sound (which includes the period of rest and of the contraction of the auricles) is very short ; in the latter, it is very long. Hence the altered relation which is indicated to the eye in Fig. 36, and which is often very embarrassing to the beginner. The larger circumference of each successive circle indicates the lengthening of the pause ; and, accordingly, you have the interval between the *first* and *second* sounds occupying a less and less arc of the circle, as the heart's action gets slower, while the interval between the *second* and *first* sounds is correspondingly lengthened. In the first and smallest circle (indicating the most rapid action) the two intervals are nearly alike, and each occupies about one-half of the circumference ; in the last or largest circle (indicating very slow action) the interval between the *second* and *first* sounds is four times as long as that between the *first* and *second*. Hence it is that when the heart is acting rapidly, it is difficult to distinguish the *first sound* from the *second*, and *vice versa ;* while with the slowly-acting heart this difficulty does not occur. Attention to these varieties (physiological varieties they may be called) in the rhythm of the sounds is of very great importance in determining the attributes of a cardiac murmur—for the first step in the inquiry is to determine which is the second sound and which is the first ; and this, as I have said, is sometimes not quite an easy matter. Generally speaking (and in all cases when the action is slow and regular), there is no difficulty ; you have only to remember that the longer interval is between the *second* and *first* sounds, and the shorter interval between the *first* and *second* ; but when the action is rapid, or irregular, and when the first sound is indistinct at the apex, or cannot be identified with the apex-beat, and also when the second sound is indistinct, or when it is audible only at the base, the first sound being audible only at the apex (as sometimes happens), the difficulty of recognition of the two sounds is very considerable. It is in such circumstances that I have found Dr. Scott Alison's

double or bin-aural stethoscope useful in cardiac diagnosis, for it enables you to identify the sounds at the point at which you can hear them best, and to bring them into accurate relation with each other, by means of the two ears receiving at the same time the sounds from two points of the precordial region. I beg to commend this instrument to your best attention.

Now, let us suppose that you have identified the two sounds, and traced their relation to the impulse, as shown in the diagram (Fig. 35), you will then find no difficulty in the next step, which is to determine the rhythm of the murmur. All valvular murmurs which are not of complex origin have one or other of these three relations to the sounds and impulse of the heart.

1. *The murmur precedes and runs up to the first sound,* ending at the moment of this sound and of the apex-beat. In this case the murmur is simultaneous with the contraction of the auricles (see Fig. 35), and I call it *Auricular-Systolic* (A.S. Fig. 37). The interpretation of such a murmur depends on the consideration that it occurs *only when the blood is being expelled from an auricle,* and when the ventricle is passive. With very rare exceptions, therefore, such murmurs depend upon contraction of the auriculo-ventricular orifices, and consequent interruption to the flow of blood out of the auricle during its contraction.

2. *The murmur follows and runs off from the first sound,* ending somewhere between the first sound and the second, or close to the second sound. In this case the murmur is simultaneous with the contraction of the ventricles (see Fig. 35), and may be called *Ventricular-Systolic* (V.S. Fig. 37). A ventricular systolic murmur, being coincident with the emptying of the ventricles, must of course be caused (if valvular in origin) by the blood flowing outwards from the ventricle, either in the natural onward direction, or backward, by regurgitation, through the auriculo-ventricular orifices.

3. *The murmur follows and runs off from the second sound,* ending somewhere during the interval between the second and first sounds (in very rare cases prolonged through the

period of rest). In this case the murmur is simultaneous with the dilatation of the ventricles (see Fig. 35), and may be

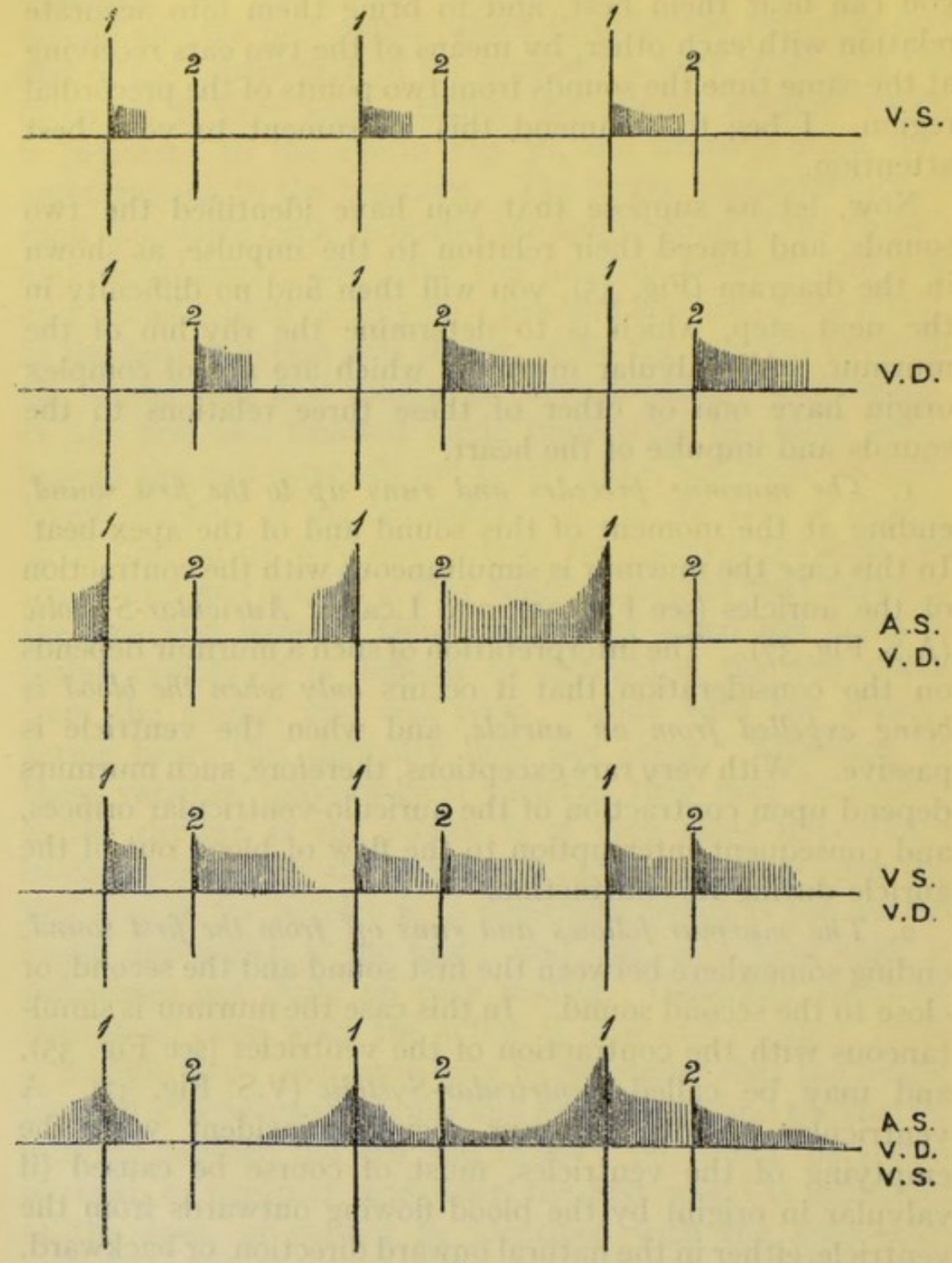

Fig. 37.

called ventricular-diastolic (V.D. Fig. 37). A ventricular-diastolic murmur is coincident with the filling of the ventricles by their rapid expansive movement. It is therefore

always due (if of valvular origin) to blood entering a ventricle, either from the auricle or from the artery; and in this last case of course the semilunar valves must be deficient, so as to admit of regurgitation.

Not unfrequently we find in practice various combinations of these different murmurs in the same case; and this, I need not say, renders the diagnosis much more perplexing. But even then it is usually possible to arrive at a conclusion, either by studying carefully the whole murmur in its relations to the rhythm of the heart, or by finding that it undergoes certain changes in tone or quality which can only be explained on the supposition of its being complex. For instance, it is not unusual to have A.S. and V.S. combined, as in Fig. 37; and they may even appear to be so combined as to constitute but one murmur; you will commonly, however, be able to detect the first sound in the middle of this murmur, splitting it, as it were, into two; and you know (see Fig. 35) that the part preceding and running up to the first sound must be A.S., and the part succeeding the first sound must be V.S. In like manner V.S. and V.D. are very frequently combined in one case, but here the second sound intervenes, and makes the rhythm quite plain. The greatest degree of difficulty is when the normal sound is merged in the murmur, as it often is when A.S. and V.S. are combined; but even in this case you will often find that the first part of the murmur is very rough, and the second part more of a bellows character; that the murmur, in fact, abruptly and suddenly changes character about the moment of the apex-beat; or you may find that one element of the complex murmur is heard more purely at the apex, and another at the base, or over the right ventricle; and in this way you are led to distinguish the one from the other. As a matter of fact and observation, I can tell you that I have been able to distinguish accurately three, and to suspect even four, sources of murmur in a case in which no period of the heart's action, or even of the pause, was free from abnormal sound; and in which, in fact, all the valves of the heart were more or less diseased. You will obtain

some idea of the complication I mean, by consulting the lowest diagram in Fig. 37.

Let me add, before leaving this part of the subject, that I believe it is impossible to make much progress in the exact diagnosis of cardiac murmurs, without a system of notation founded on their rhythm; and in all cases of interest, I have been in the habit for some years of using diagrams similar to those before you (in Fig. 37) at the bedside. They are exceedingly simple and effective; they save a great deal of tedious description; and they bring the facts at once before the mind in a way that no description can do, at least in the case of those who have any difficulty in understanding cardiac diagnosis. They are therefore very favourable to accuracy in the recording of facts.[1]

II. Having determined the rhythm of a murmur, the next step in the investigation is to fix, within as narrow limits as possible, the *place of origin.*

Now, the point at which a murmur is produced being, in the majority of cases, one of the four valvular orifices, it is commonly desirable to test all doubtful murmurs, in the first instance, on the supposition that they are valvular; and, in general, only on the failure of this hypothesis, or on its being rendered improbable by collateral circumstances, to admit some other to consideration. The first branch of the inquiry as to the seat of origin of a murmur, is therefore commonly this: *At which of the four valvular orifices is it produced?*

To this question a very satisfactory answer can commonly be obtained, if the murmur is not too complex, and if there be no remarkable deformity or displacement of the heart or great vessels to mask its true character. To avoid this last source of fallacy every means should be taken at the

[1] A convenient mode of indicating the character of cardiac murmurs is by imitative combinations of consonants, chiefly labials and linguals. But this, though used to a considerable extent habitually in the lecture-room, I have found to be rather cumbrous and subject to misapprehension on paper. I have therefore omitted the illustrations of this kind actually employed during the lecture here recorded.

outset to determine, in the particular case under examination, the actual size and position of the heart, together with its relations to the thoracic wall, and to the surrounding organs. In particular, percussion should be accurately performed, so as to define, with as much exactness as possible, the limits of that portion of the heart which is in contact with the thoracic wall, and uncovered by lung. In making this observation, the presence of any abnormal state of the lungs or great vessels, affecting the percussion, should be observed; and, in particular, the presence or absence of substernal tumour. The exact point of the apex-beat is next to be determined, if possible, and the characters of the impulse, both of the right and left ventricle, should be at the same time carefully studied. Lastly, the sounds are to be used as an aid to the other phenomena. The object of this preliminary investigation (and it is a most important one) is to avoid mistakes in the rest of the inquiry, for it is plainly very necessary to have as clear an idea as possible of the exact position, size, and relations of the heart with respect to the surrounding parts, before proceeding to speculate on the relation of the murmurs to the superficial areas over which they are heard.

Having proceeded thus far, we next endeavour to determine, by careful stethoscopic observations, the exact seat, and the limits of diffusion of the murmur actually under observation. If the murmur is very loud, or very generally heard over the cardiac region, there may be some difficulty; and still more, if there are several murmurs interfering with, or crossing each other in, the field of observation; but, in ordinary circumstances, the stethoscope will enable the observer to fix on a few points, or a few restricted spaces, over which each murmur is audible with special distinctness; areas within which murmur is heard, there being no murmur elsewhere; or, if not so, then areas within which murmur is heard decidedly more loud, more articulate, and apparently nearer to the ear, than elsewhere. Modern diagnosis localizes the murmur (and therefore the disease) chiefly from the observation of these areas of transmitted sound.

Now, as there are four valvular orifices, so there are four distinctive areas to which murmurs arising at these orifices may be propagated. This is a fact thoroughly established by modern inquiry, although the exact definition of these areas is a matter of some difficulty, owing to the great difference in the diffusion of murmurs in different cases, and the numerous exceptional circumstances interfering with the diffusion in many instances. I have been accustomed to use a diagram like this (Fig. 38),[1] as indicating these areas in a general way; and when due allowance is made for causes

[1] *Explanation of Fig.* 38.—In this figure the drawing of the heart and great vessels, in their relation to the front of the thorax, has been accurately copied on a reduced scale, from the large folio work of Professor Luschka of Tübingen. (*Die Brust-Organe des Menschen in ihrer Lage.* Tübingen, 1857.) The outlines of organs, which are partially invisible in the dissection, are indicated by very fine dotted lines; while the areas of propagation of valvular bruits, as described in the text, have been roughly marked out by additional, much coarser, and more visible dotted lines: the character of the dots being different in each of the four areas. A capital letter marks each area, viz.: A, The circle of mitral murmur, corresponding with the left apex; B, the irregular space indicating the ordinary limits of diffusion of aortic murmurs, corresponding mainly with the whole sternum (keeping in view the qualifications stated in the text), and extending into the neck along the course of the arteries; C, the broad and somewhat diffused area (roughly triangular in most cases) occupied by tricuspid murmurs, and corresponding generally with the right ventricle, where it is least covered by lung; D, the circumscribed circular area over which pulmonic murmurs are commonly heard loudest, when not interfered with by overlapping lung. In many cases it is an inch, and even more, lower down, corresponding not so much with the actual seat of the valves, as with the conus arteriosus of the right ventricle, where it touches the thoracic wall. The seat of greatest distinctness of pulmonic murmurs is very much influenced by inspiration and expiration, and should be examined chiefly in the latter state. The internal organs and parts of organs are indicated in the figure by italic letters, as follows: *r.au.*, right auricle (partially traced in fine dotting); *a.o.*, arch of the aorta, just seen in the first intercostal space, and traced in fine dotting on the sternum; *v.i.*, the two innominate veins; *v.c.*, vena cava descendens; *p.*, pulmonary artery, where it lies near the thoracic wall, but often overlapped by a thin edge of lung; *l.au.*, left auricle, always covered by lung; *l.v.*, left ventricle, deeply overlapped by lung,

of variation, I think it will be found more in accordance with the facts than most of the descriptions. The following rules will also be found useful in recognising these areas in actual practice :

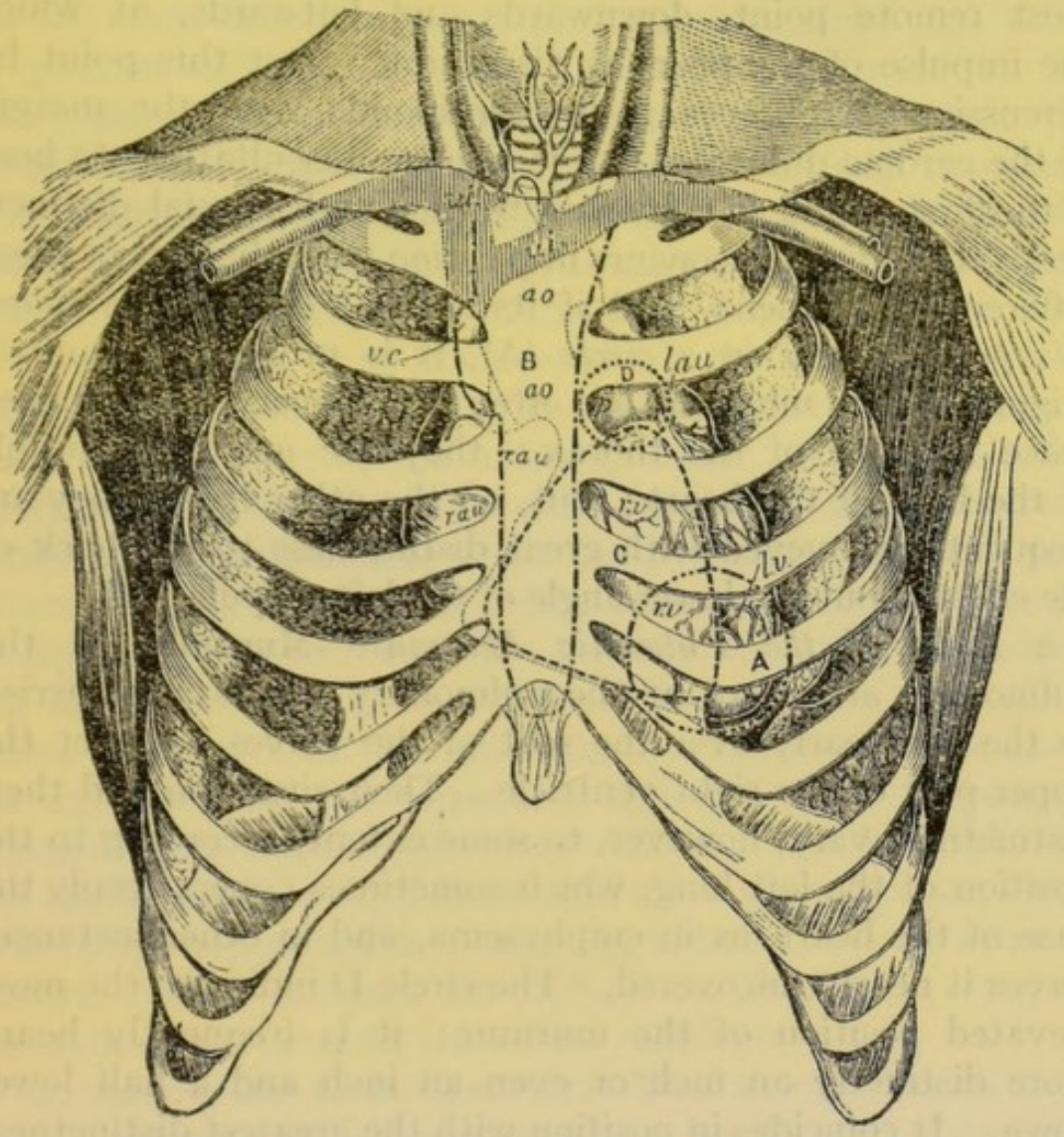

Fig. 38.

1. *Area of the Mitral Murmur.*—The mitral murmur corresponds generally with the apex of the left ventricle, to which point this murmur is readily conducted, as being the only part of the left ventricle which is in close contact

except at the apex, within circle A ; *r.v.*, right ventricle, to a considerable, but variable, extent in contact with the thoracic wall ; in disease, often extending downwards nearly to the xiphoid cartilage, and propagating its murmurs in this direction beyond the limits of the area C, as shown in the figure.

with the wall of the thorax. To find this area with precision, it is requisite to determine the exact seat of the apex-beat; the patient lying a little towards the left side, or even on the face. If there is no distinct apex-beat, find the most remote point, downwards and leftwards, at which the impulse of the heart is discernible; test this point by percussion to observe if it corresponds with the margin of the cardiac dulness; test it also by auscultation, to hear if the first sound is conveyed thither with special distinctness. If a murmur concurs in position with the seat of these different phenomena, and if its seat of diffusion is *round this point merely in a circle* (A), it is probably of mitral origin. Mitral murmurs are often heard over a very limited space in front of the thorax; they are mostly inaudible at the base of the heart; but, on the other hand, they are frequently conveyed with great distinctness to the back of the chest about the lower angle of the left scapula.

2. *Area of the Pulmonic Murmur.*—Murmurs in the pulmonary artery, or at the pulmonary valves, are carried to the ear nearly over the seat of the valves, or over the upper part of the right ventricle. Their situation and their distinctness vary, however, to some extent, according to the position of the left lung, which sometimes covers deeply the base of the heart, as in emphysema, and in other instances leaves it nearly uncovered. The circle D indicates the most elevated position of the murmur; it is frequently heard more distinctly an inch or even an inch and a half lower down. It coincides in position with the greatest distinctness of the pulmonic second sound, as contra-distinguished from that heard over the aorta, a little higher up, and to the right of the sternum; frequently also it coincides in position with a certain tactile vibration, difficult to describe, but easily recognised as being perfectly characteristic, and accompanying the second sound of which it conveys, as it were, an exact impression to the finger.[1] Pulmonic murmurs are

[1] Like all the other cardiac murmurs, the pulmonic murmur may likewise be accompanied by a more prolonged tactile vibration, the *frémissement cataire* of Laennec, or purring tremor. This, however, accompanies

usually very superficial, and therefore often very distinct, and apparently near the ear; they are nevertheless limited in their power of diffusion, being usually inaudible at the apex, and also along the sternum; they are never heard in the neck, or in the course of the great vessels.

3. *Area of the Tricuspid Murmur.*—Murmurs at the tricuspid orifice are usually represented as very rare; this is not in accordance with my experience, at least as regards the murmur of regurgitation, which is often confounded with (indeed not unfrequently is associated with) the mitral murmur. The tricuspid murmur is heard over the right ventricle, where it is uncovered by the lung; *i.e.* at the lower part of the sternum, and over the whole space between this and the seat of the mitral murmur. It is usually but little audible, however, above the level of the third rib, and is thus distinguished both from the pulmonic, and still more from the aortic murmur. The communication of the tricuspid murmur to the surface directly through the substance of the right ventricle is so obvious as to require no explanation. The area of this murmur, in ordinary circumstances, is indicated by the triangular space C; but in cases of considerable hypertrophy and dilatation of the right side of the heart, especially in connexion with emphysema (when the ventricle pulsates in the epigastrium) the murmur is heard loudest towards the xiphoid cartilage, and along the margin of the seventh left costal cartilage.

4. *Area of the Aortic Murmur.*—The law of diffusion of the aortic murmur is rather mysterious, for it is not only found in great intensity over the base of the heart and the *manubrium sterni,* which are in the immediate neighbourhood of the seat of its production, but it is frequently not less audible along the whole length of the sternum; and, rather oftener than not, it is absolutely louder close to the xiphoid

the first sound fully more often than the second; it is, in fact, simply the impression of the acoustic vibrations of the murmur on a less sensitive organ. The A.S. mitral murmur is very often easily recognisable in this way, and may be perfectly well distinguished even without the aid of the ear.

cartilage than at many points nearer to its origin. This fact, though not ordinarily stated in the text-books, is one of some importance, and rests (with me) upon very numerous observations. The aortic murmur is distinguished from all the other valvular murmurs by being propagated (though sometimes very faintly in the case of diastolic murmurs) into the arteries of the neck. Over the *manubrium sterni*, also, it is generally more distinct towards the right than towards the left of the bone, and has not unfrequently (though not always) a quite special distinctness over the sternal end of the second right costal cartilage. It is the most widely-diffused of all the cardiac murmurs, and can sometimes be traced to great distances along the spine, and even along the bones of the extremities. I have heard it at the occiput, at the sacrum, and even at the elbow. It is in many cases undistinguishable from the murmur of aneurism of the arch, or of the innominate artery.

By due attention to these peculiarities of localization, a large amount of knowledge may be acquired of the special characters of valvular murmurs. I have not alluded hitherto (for I was anxious to avoid too wide a range in my observations) to the collateral phenomena by which some of these diseases are further distinguished, to the characters of the pulse, to the swelling and pulsation of the veins in the neck, to the alterations of the sounds apart from murmur, in certain cases. These would form ample materials for another lecture, and even for more than one, as also would the functional and symptomatic phenomena proper to the different forms of cardiac disorder attended with murmur. In the meantime, I must be contented with a very brief *résumé* of what I have already stated, with a few important additions.

Murmurs (and especially valvular murmurs) are judged mainly by their *rhythm*, and by their *limits of diffusion* or *area*.

1. An *auricular-systolic* murmur (A.S. Fig. 37), *i.e.* one preceding and running up to the first sound of the heart, is in all probability produced in one or other of the auriculo-

ventricular orifices; inasmuch as it coincides with the forcible emptying of the auricles into the ventricles through these orifices. Its reasonable interpretation therefore is—*obstruction to the current of the blood entering a ventricle.* If the left auriculo-ventricular orifice is affected, the murmur will be found to have the character of a mitral murmur, and to be localized at A (Fig. 38); if, on the contrary, the tricuspid orifice be obstructed, the murmur will occupy the area C.

2. A *ventricular-systolic* murmur (V.S. Fig. 37), *i.e.* one succeeding and running off from the first sound, may be produced either in the auriculo-ventricular, or in the arterial orifices. In either case, it coincides with the *emptying* of the ventricles; and therefore,

a. If *auriculo-ventricular* as to its origin, it is necessarily a murmur of *emptying backwards into the auricles,* or of *regurgitation*:

b. If *arterial* as to its origin, it is necessarily a murmur of *emptying forwards into the arteries,* or of *obstruction.*

A ventricular-systolic murmur may thus have four distinct solutions among the organic valvular diseases; and here the consideration of the area of diffusion of the murmur comes to solve the difficulty. For if the area be (according to Fig. 38):

A. Mitral—it is *mitral regurgitation*:
B. Aortic—it is *aortic obstruction*:
C. Tricuspid—it is *tricuspid regurgitation*:
D. Pulmonic—it is *pulmonic obstruction.*

3. A *ventricular-diastolic* murmur (V.D., Fig. 37), *i.e.* one succeeding and running off from the second sound, may be produced either in the auriculo-ventricular, or in the arterial orifices. In either case it coincides with the *filling* of the ventricles; and therefore,

a. If *auriculo-ventricular* as to it origin, it is necessarily a murmur of *filling forward from the auricles,* or of *obstruction*:

b. If *arterial* as to its origin, it is necessarily a murmur of *filling backwards from the arteries,* or of *regurgitation*:

A ventricular-diastolic murmur may thus have four distinct solutions among the organic valvular diseases; and here the consideration of the area of diffusion of the sound comes to solve the difficulty. For if the area be (according to Fig. 38):

A. Mitral—it is *mitral obstruction*:
B. Aortic—it is *aortic regurgitation*:
C. Tricuspid—it is *tricuspid obstruction*:
D. Pulmonic—it is *pulmonic regurgitation.*

4. One, two, or even three of the murmurs above-mentioned may be found in combination in the same case. The most frequent combinations are,—

a. Aortic obstruction and regurgitation, indicated by V.S. and V.D. murmurs (Fig. 37), heard over area B (Fig. 38);

b. Mitral obstruction and regurgitation, indicated by A.S. (sometimes also V.D.) and V.S. murmurs, heard over area A;

c. Various combinations of the two preceding forms, the aortic and mitral valves being both diseased;

d. Mitral obstruction, with dilated right ventricle, and consequently tricuspid regurgitation, indicated by A.S. murmur, heard over area A, and V.S. murmur, heard over area C.

The rarest of all murmurs are the pulmonic, and the murmur of tricuspid obstruction; and these murmurs are still more rarely observed singly, being usually in combination with diseases causing murmur on the left side of the heart.

5. Pericardial murmurs are generally more or less present with both sounds of the heart, and often occur over the right ventricle and the mid-sternum, but are not carried into the arteries of the neck, or in the direction of the xiphoid cartilage. They differ, therefore, in their localization, for the most part, both from aortic and mitral murmurs, the former of which they sometimes resemble in tone and rhythm.

6. Aneurisms of the arch, especially of its ascending part, give rise to murmurs which are with difficulty distinguished from those of aortic valvular disease.

7. Anæmic and functional murmurs, when heard over the heart and great arteries, are always V.S. in rhythm, and they almost always simulate aortic or pulmonic murmurs as regards their area (B and D). They are to be distinguished chiefly by the circumstances in which they occur, and by the absence of symptoms of valvular disease.

8. Mitral and tricuspid murmurs of regurgitation frequently occur without primary valvular deformity, either from dilatation of the orifice, or from dilatation of the cavity of the ventricle, and want of adaptation of the muscular walls of the ventricle to the columnæ carneæ and chordæ tendineæ.

CASE OF OBSTRUCTION OF THE RIGHT AURICULO-VENTRICULAR ORIFICE

Caused by a Tumour in the Auricle acting as a Ball-Valve during the direct current from the Auricle to the Ventricle, and without apparent Disease of the Valve, or of the Heart otherwise. Clinical Remarks on Diagnosis and Prognosis.

Edinburgh Hospital Reports, 1893, *vol. i. p.* 221.

Little is required by way of introduction to this contribution. It refers to the case of a patient who was under the care of Gairdner during his last year of office as Physician to the Royal Infirmary of Edinburgh, and it is the earliest instance on record of a positive diagnosis of tricuspid obstruction, made during life, and verified after death. The whole dramatic circumstances are so vividly described by the author that there remains but little to be added. The case is perfectly unique, inasmuch as the obstruction was the result of a tumour, growing by a narrow pedicle from the posterior wall of the right auricle, and hanging down, so as to reach the tricuspid orifice. The condition, therefore, was not due to any valvular lesion; none the less, however, did it give rise to all the physical signs of obstruction. It has always seemed to me that this classical instance is, in itself, absolutely destructive of the regurgitant theory advanced by Ormerod and his followers, referred to in the prefatory note to the last paper; but Brockbank, in his criticism of the case, claims that, with the tumour obstructing the orifice, a murmur of regurgitation would result, owing to blood escaping through the gaps in the imperfectly closed curtains, or between the curtains and the tumour. He holds that, as the obstruction was being gradually

forced out from between the valve curtains, the extent of their incompetency would diminish, until complete competency were regained. It is impossible for me, in view of the whole mechanism of the auriculo-ventricular cusps, to accept this view.

THE extremely rare, if not absolutely unique, case here reported came under my notice during the life of the patient, in the Edinburgh Royal Infirmary in 1861. It was very carefully observed at that time, not only by myself, but by colleagues and others who were associated with me in the Infirmary, and also by not a few students and accidental visitors to the wards, who had heard of the case, being more or less specially interested in cardiac murmurs. It would not be a very unfair view of the matter to say that the patient was detained in the hospital for a considerable period as a " show case," the very unusual character of the phenomena being fully recognised, although the man all along declared himself quite able to work, and was not at all apparently suffering much from his disease. Perhaps it was owing to this notoriety of the case in 1861, and to the great importance attached to it in my *Clinical Medicine*, published in 1862,[1] that the late Dr. Greig of Dundee (where the patient resided and worked as a common labourer) was led to interest himself in the matter ; and on the death of the patient from what is described as acute pneumonia, in April 1872, my late friend, though not otherwise connected with the case, was able to do for me, and for the whole art and science of medicine, the inestimable service of obtaining a post-mortem examination, which, but for his care and circumspection, would certainly have been neglected, and (as it was) only enabled him to remove the heart, and send it to me in Glasgow. Owing to various circumstances the case, although known to a not inconsiderable number of *experts*, and to many of my own students, has never been recorded in such a way as to be accessible, with proper illustrations, to the profession

[1] This volume having been for more than a quarter of a century out of print, the quotations from it in this paper may be justified on the ground of its not being readily accessible to many of the younger generation.

at large ; and on this fact being represented to me by the promoters of the present undertaking, it seemed to them, as to me, a very fitting occasion for repairing past omissions in this respect. I have only to add now, by way of preface, that the case as now presented, with the excellent drawings kindly furnished by my colleague Professor Cleland, and Dr. T. W. Dewar, will tell its own story so well as to require but little commentary. I shall not even attempt, in this place, to deal, however slightly, with the literature of the subject, or to discuss at length the questions arising out of it. On one occasion, at a very crowded meeting of the Section of Medicine at the International Medical Congress in 1881, I exhibited the preparation here illustrated, and made some remarks on the clinical aspects of the case as already recorded ; but, as I spoke in the belief that reporters were present (which proved not to be the case), the whole of the discussion which followed was practically lost, and I was so much discouraged by this accident, that the case remained unreported, and does not appear at all in the Transactions of the Congress. Notwithstanding this, the facts as then stated excited much interest among many of the leading pathologists and clinical teachers both of London and the Continent, who happened to be present ; and some knowledge of these facts gained a wider circulation in this way. Even before this, Dr. Walshe had particularly referred to the case, on the basis of facts partly recorded, and partly supplied by me in correspondence.[1] More recently a brief account of it was given, in a series of cases of cardiac disease of long duration, to the meeting of the British Medical Association at Brighton in 1886 ; and the report of this[2] has formed the basis of a reference to the facts in the recent work of Dr. Sansom.[3] The preparation, which has for a long time formed a part of my private museum for class purposes, and has been demonstrated, since 1872, to many hundreds of students, with full verbal explanations of the clinical facts, has more recently

[1] *Diseases of the Heart*, 4th ed. 1873, p. 375.

[2] *British Medical Journal*, 5th Feb. 1887, p. 262.

[3] *The Diagnosis of the Diseases of the Heart and Aorta*, etc. 1892, p. 315.

been transferred to the Museum of the Western Infirmary, Glasgow, where it is still available for class demonstrations, but is perhaps more secure against accidental loss or injury, and more accessible to pathological inquirers generally. The present narrative, will, I trust, afford the necessary data for future reference, both as to clinical and pathological aspects of the case, in one continuous article; and although the precedence is of course due, in the order of time, to the clinical history, yet I think it will conduce to clear apprehension, and also to the strict impartiality of the narrative, if I give first the description furnished by Dr. Joseph Coats, as Curator of the Western Infirmary Museum, to the interleaved Catalogue of the Pathological Collection, Series II., 37A :—

"37A. OBSTRUCTION OF TRICUSPID ORIFICE BY TUMOUR IN RIGHT AURICLE.

"A tumour of a globular shape, and about 1½ in. in diameter, is attached by a narrow base to the posterior wall of the right auricle, immediately to the right of the opening of the inferior cava. From its attachment the tumour hangs down, so that its lower extremity is at the tricuspid orifice. The surface of the tumour is somewhat lobulated, and it is of a dense almost cartilaginous consistence. Under the microscope no organised structure is visible, merely an indefinite fibrous condition, with almost no cells or nuclei, and no proper connective tissue corpuscles. On the surface of the tumour there was found a more recent coagulum, which surmounted it and sent a process into the appendage. This is hung alongside the preparation. The heart is not appreciably enlarged or altered in any other way."

The following is Professor Cleland's short description to accompany Fig. 39 :—

"The specimen shown me by Professor Gairdner from the Western Infirmary Collection (Series II., No. 37A), is a pedunculated vegetation, attached to the anterior or right end of the Eustachian valve. Other small vegetations

occur on the auricular wall above the valve. The left half of the valve presents nodulated thickening. The annulus ovalis has disappeared, and the inter-auricular septum is thick, and the foramen ovale quite imperforate. There seems to be no evidence in the preparation to indicate whether the disease of the Eustachian valve dated from birth or prior to it, or from a later period."

The reference to this case in my *Clinical Medicine*, p. 602, as mentioned above, is part of a discussion of the whole subject of cardiac murmurs, occupying Chapter 18 of that volume, followed by a report of a lecture, dated 28th February, 1862, in which three months' entire experience of the different murmurs in hospital is summed up clinically, so as to show their relative frequency, and their most usual combinations, with a synopsis of all the cases referred to. At this time, it is to be remembered, the characteristic murmur of mitral obstruction, or stenosis, was by no means so well understood, or so generally admitted to be distinctive, as is now the case. It is, therefore, not without importance as regards the present narrative, that I was able to single out seven cases regarded as of mitral obstruction, with the auricular-systolic murmur, during the three months' hospital experience above referred to.[1] The opinion expressed as to these seven cases (some of which, however, had the murmur of regurgitation as well) was as follows :—" I feel as sure as I can well be of anything in medicine, that we have to do (in these seven cases) not only with mitral *disease*, but with mitral *obstruction* " (p. 599). But it is added : " In addition to these seven cases of mitral obstruction (two of which were of regurgitation also), there have been five other cases of mitral regurgitation, having the V.S. murmur only. We cannot be sure that there is *not* obstruction in these cases ; we can only be sure that there *is* regurgitation ; there is no *evidence* of obstruction, but of course, there

[1] Of course it has to be admitted, and is hereby admitted, that I may have had rather more than an average number of these cases, owing to the interest I was well known to have taken in them.

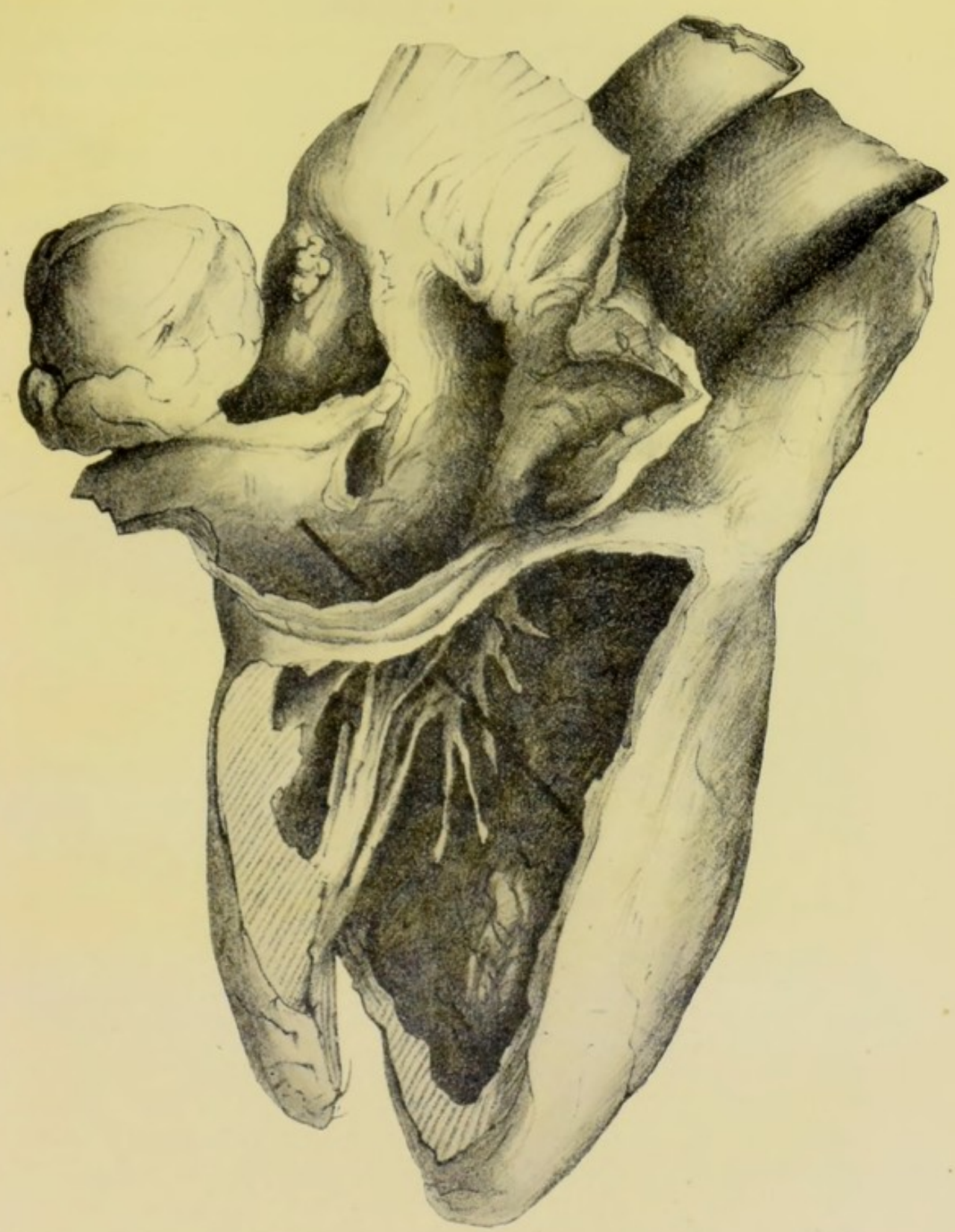

Fig. 39.

See page 710.

View of the tumour, as seen when the right auricle has been everted artificially (and therefore altogether out of its natural position in reference to the auriculo-ventricular orifice), displaying fully the pedicle, and the attachment to the auricular endocardium, as described by Dr. Cleland, page 709. A much smaller concretion (probably of more recent formation) is seen attached just below the upper part of the auricular endocardium where it adjoins the appendix. The auriculo-ventricular opening is indicated by a piece of whalebone passed through it.

may be obstruction without the characteristic murmur. . . . The regurgitation may, as I said before, be in some of these cases quite independent of any of the deformities of the valve ; it may even occur without dilatation of the orifice. It may, in these circumstances, depend on a dilated ventricle, causing derangement of the mechanism by which the valve is closed in the normal condition " (p. 600).

In contrast or comparison with these statements as regards the mitral orifice, it is held (p. 601) that tricuspid murmurs are by no means rare (as was supposed by some), but " very apt to be confounded both with mitral and with exocardial murmurs ; indeed, they are very frequently associated with mitral murmurs." The passage immediately following this (p. 602) affirms, however, that tricuspid murmurs (unlike mitral) *are almost always regurgitant, i.e.,* V.S. in rhythm ; and that they " very rarely proceed from deformity of the valve, but only from dilatation of the orifice or of the right ventricle, with secondary regurgitation. . . . The tricuspid murmur of obstruction (A.S.)[1] is among the rarest of clinical facts in my experience." It is at this point that reference is incidentally made to the case which is the subject of the present communication, not as one then under observation, but as constituting the rare exception which (according to the proverb) " proves the rule " ; the absolute value of which, however, as a clinical observation, is enormously enhanced by the post-mortem examination made more than ten years later, and the results of which, so far as the heart is concerned, are set forth in this paper.

I propose, accordingly, to place before the reader all the essential clinical facts noted in this remarkable case in 1861 exactly in the words in which they were briefly noticed in my *Clinical Medicine* (pp. 602-604) ; and I may add, that although complicated cases, like those still more briefly

[1] It can scarcely be necessary at this late date, perhaps, to indicate the nomenclature adopted ever since 1861 by me, as characterising the rhythm of cardiac murmurs, viz. A.S., *auricular-systolic* ; V.S., *ventricular-systolic* ; V.D., *ventricular-diastolic.* See Finlayson's *Clinical Manual,* 3rd ed. 1891, p. 619, *note.*

indicated in the same paragraph, have occurred to me since, the case of Patrick M. is still the only one in my experience which could be cited in the terms used at the commencement of the following extract. The diagnosis moreover, as made in 1861, would have been *absolutely* correct, had the word *obstruction* been used (as in all the other passages referring to murmurs of A.S. rhythm) instead of *contraction*, employed in this particular instance. This one little *lapsus* may be, perhaps, condoned without much difficulty, if it is remembered that not one clinical or pathological expert among the crowded audience in London in 1881 professed to have seen or heard of a similar case; and rare as *contraction* of the tricuspid orifice occurring alone undoubtedly is, it is probably immensely less rare than the lesion displayed in the drawings of this case, and described so carefully and impartially by Dr. Coats and Professor Cleland.

"I have indeed heard, but once only, an A.S. murmur over the tricuspid orifice, absolutely uncomplicated, and free from the suspicion of mistake. The patient is an Irish labourer, Patrick M., æt. about 20, known to Dr. Greig of Dundee, where he is still living, and happily likely to live for some time. He suffers no very great amount of inconvenience from his disease, except from a very remarkable undulating movement in his neck, for which he came over to Edinburgh about two years ago, to consult Mr. Syme, supposing that it was something that might be cured by surgery. He afterwards came under my care, and remained a good while in my ward on two occasions, but more, I must confess, with a view to my scientific curiosity than to his own advantage, as there is little excuse for keeping him as an hospital patient.

"He is rather pallid, and perhaps not very strong, but of firmly-built frame, tolerably active, and neither livid nor dropsical. The undulation is beyond all question in the jugular veins on both sides of the neck; and it is quite evident that these veins are much dilated or enlarged permanently, without being much distended with their contents. The cardiac murmur begins immediately after the second

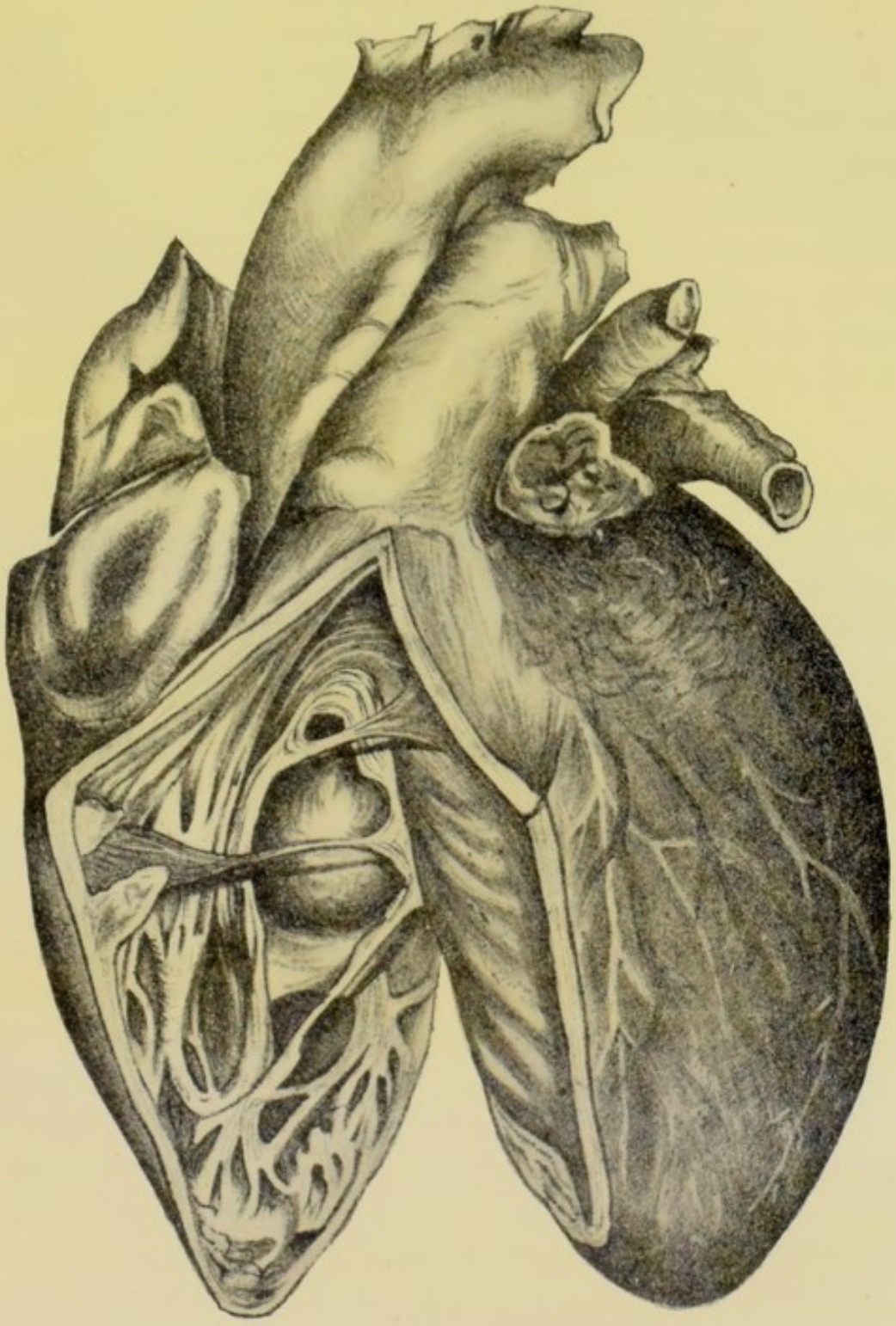

Fig. 40.

See page 712.

The tumour as seen from the ventricle, in its position of maximum descent into the auriculo-ventricular orifice, or as it may be supposed to have been placed during life by the action of the direct current during the auricular systole, when it gave rise to the A. S. murmur described.

sound; continues, *diminuendo*, throughout the pause, and then goes on, *crescendo*, up to the first sound, at which it stops abruptly. I think tricuspid contraction may in this case be predicted with all but mathematical certainty; the fact, however, of having witnessed this typical instance, only serves to make me more entirely confident that I cannot have overlooked the fact in many other cases. In one other instance I must, and in yet another I may, have heard this murmur. In both these cases death occurred, and the hearts, now in my possession, and shown by me at the time to the Medico-Chirurgical Society, have a contracted tricuspid orifice, as part of a complex morbid condition of the valves. In one of them—Ann D., æt. 25, Register of Dissections, 9th August 1859,—the murmur covered nearly every part, at some examinations quite every part, of the heart's sounds and their interval; and I thought I could distinguish the right side of the organ as being affected. In the other the murmur was quite unequivocally on the right side, but appeared to be a murmur of tricuspid regurgitation, which condition, no doubt, existed in addition to obstruction. This last was the case of the young girl, Mary P., æt. 11, mentioned at p. 97,[1] and I think there is good evidence that the disease began in the mitral orifice, and extended at a much later date to the tricuspid."

I now come to the circumstances attending my own receipt and first examination of the heart in this case, which were such as to give to this terminal scene of the narrative almost a dramatic interest in connection with what has been already set forth. Patrick M. lived for more than ten years after I first saw him, and died, as already stated, in April, 1872. I had occasionally heard of him through Dr. Greig, but only in a very perfunctory fashion, as he was working,

[1] The clinical estimate of this case, three years before, was as follows: "She has, I think, a contracted mitral orifice; and with this there is associated, at present, a great deal of lividity, with feverishness, and marked prostration; the consequences, no doubt, of influenza (which was current at this time) acting upon organs predisposed to disease." She made a good recovery from these critical symptoms, but died later on.

professing to be well in his health, and never presenting himself willingly, either for advice or treatment. Beyond this, I have no authentic information as to his later years, and none as to the details of his fatal illness, which was said to have been acute pneumonia. Indeed, both the beginning and the ending of the life-drama of this case may be said to be involved in mystery; but they are not less mysterious than the question which emerged and still emerges from it altogether, viz. How the active life of a common labourer could be tolerated for all these years under the conditions shown forth in the preparation? Of course, it must be admitted or assumed that the obstruction was incomplete, and that notwithstanding all the noise made in the passage of the blood from the right auricle into the ventricle, a nearly normal amount of blood must, in some way or another, have got past the obstacle at each diastolic and presystolic period. Had this not been so, the man must have got livid and dropsical, or even died by sheer cardiac suffocation, as in cases of embolism or thrombosis of the pulmonary artery. The remarkable thing, from the pathological as well as the clinical point of view, is that although in the dilated and leaping veins in the neck we have indubitable evidence of a back-wave, probably, I think, from the auricle (though no very exact observations were made on this point),[1] the auricle itself was not found after death to be very materially dilated or hypertrophied. In conformity with the general plan of this communication, however, I prefer to leave these facts without further commentary in the meantime.

To return to the incidents connected with the heart. The parcel containing it arrived from Dundee, without previous

[1] I need scarcely say that at this time the graphic methods of Marey and others were but little known, and certainly had not been practised in Edinburgh. My own impressions, however (*quantum valeant*), founding upon previous observations made on the well-known case of congenital sternal fissure in M. Eugène Groux, were that the auricular pulse could be differentiated under favourable circumstances from the ventricular; and in this instance (according to the best of my recollection) I always held the venous pulse to be auricular. Dr. Sansom, I observe, has also adopted this view of it.

notice, and with a simple statement by Dr. Greig that it contained the heart of Patrick M., which he had been fortunate enough to be able to secure for me, the patient having died of acute pneumonia. I was engaged at the Royal Infirmary at the time, and surrounded by students, some of whom had probably heard of the case before. We adjourned, accordingly to the Pathological Department, and there the parcel was opened after a short verbal statement as to the importance of the clinical facts, and the diagnosis to be verified on the lines given above. The heart, to my astonishment, looked externally very much like a healthy heart of moderate size, without any marked disproportion between its right and left sides, or between its auricular and ventricular cavities. On incising first the right ventricle, I felt with my finger for a ring of calcified stenosis, such as is so often found in the mitral orifice in the so-called " button-hole " form of contraction, or, alternately for a " funnel-shaped " stricture with the shortened and thickened *chordæ tendineæ* drawn to a point, as it were, midway in the ventricle. Nothing of the kind (I need not say) could be felt, and I had almost been led into raising the question, " What if there should be *no* obstruction after all ? " " If by any chance this should be so, gentlemen," I said quite frankly to the bystanders, " we shall have to rewrite our whole cardiac diagnosis and pathology of murmurs ; for it is impossible to find a stronger case than this for the *absolute* diagnosis of tricuspid obstruction." These words, or words to the like effect, had hardly been uttered, when, on passing my finger *beyond* the perfectly normal valve into the auricle, I at once encountered the bullet-shaped dense tumour so well depicted in Dr. Cleland's drawing (Fig. 39), but rather more nearly in the position shown in Fig. 40, though perhaps a little more receding from the lips of the valve. A very little more search with the finger, and afterwards a carefully devised incision of the auricle, now disclosed the real nature of the obstruction, and the astonishing fact that it must have been present in that situation *only* when the current was *towards* the ventricle, and must have been mechanically withdrawn from the

orifice as soon as that current ceased to flow. In other words, we have to deal with *a case of tricuspid obstruction, which could only be obstructive during the diastolic and A.S. periods, and could by no possibility become the physical cause of a murmur of regurgitation.*

Now, observe what follows from this. A great deal of argumentative discussion has taken place within the last few years as to whether the ordinary murmur of mitral stenosis (admitted to be distinctively such, even by those who differ from me as to its mode of production) is really *direct* or *regurgitant.* The discussion has been raised by eminent and excellent men, and in my small personal contribution to it I have fully recognised this fact. But, none the less, I may be permitted to say that to me, personally, the arguments for the latter view have appeared all along inconclusive, not to say wearisome and unprofitable, as being founded on a complete misapprehension of essential facts. One fact, of itself sufficient, has always seemed to me absolutely inconsistent with the theory of regurgitation as presented in the arguments alluded to. Regurgitation, as a cause of the presystolic, or (as I call it) A.S. murmur, must of necessity be limited to the mere instant of time when the valves are *in the act of closing or of tending to close.* This is not a mere academic distinction for argument's sake, but arises from the fact that regurgitation—to be such at all—must be closely tied to the ventricular systole and the outgoing current from the ventricles. But, as I carefully pointed out in 1861, the A.S.[1] murmur—or what I preferred to call such, for distinction—although markedly present, as a rule, during the period assigned to the normal auricular contraction, is not by any means limited to this period, but may extend backwards more or less into the pause, or even

[1] This name was applied to it not, as several critics have supposed, because it was *produced by* the auricle in systole, but simply because the most characteristic element of the murmur—the so-called presystolic rhythm—was, in fact, *coincident in time* with the auricular systole. The name, therefore, involved no theory, but was simply the statement of a fact.

into the diastolic interval. Here is the passage referred to in my original sketch in 1861,[1] which almost innumerable observations since that time have amply confirmed as regards mitral stenosis, and which, as a mere statement of fact, has never been disputed.

"The auricular-systolic murmur may merely precede the first sound, *i.e.* it may follow the pause of the heart's action, or it may appear to be prolonged out of, or even quite through the period of rest, being in this last case necessarily associated with a degree of the ventricular-diastolic murmur, presently to be described. Its essential character, however, is preserved in every case as above defined, and as represented in the diagram."[2]

In the present case of tricuspid obstruction, we have a murmur precisely of the character above described, but occurring under circumstances which render it impossible to suppose that there should have been any difficulty or delay in the closure of the valve, any more than in a normal heart. And even had there been regurgitation, the obstacle, not being fixed, but movable, would not have been in the way any more than the ball-valve of a syringe is in the way of the current generated by suction. Moreover, the murmur covering as it did the entire diastolic, post-diastolic, and pre-systolic intervals, was quite unquestionably one and the same murmur throughout, only varying in its intensity without any sudden break or change of essential character. How, then, could any portion of it be reasonably held to be regurgitant? Now, in the average rumbling murmur of mitral stenosis, so familiar to all of us nowadays, precisely the same

[1] Published first in the *Edinburgh Medical Journal* of that year, and reprinted in my *Clinical Medicine*, 1862, p. 575.

[2] This diagram, illustrating three different states of the murmur as described, will be found at p. 575 of my *Clinical Medicine*, and has been copied into Dr. Finlayson's *Clinical Manual*, 3rd ed. p. 620. It is one of a series long used in my class teaching, and employed in a lecture on "Modern Cardiac Pathology and Diagnosis," delivered in Edinburgh at the request of the Council of the Royal College of Physicians, in July, 1861. As the facts are now so well known, and have never been controverted, it seems unnecessary to reproduce the diagram here.

phenomena of rhythm may often be observed. It is more difficult, of course, to prove that there is *no* regurgitation[1] than in the case now reported, because the fact that the valves are diseased and the orifice permanently contracted, lends itself more easily to theories such as those which have been alleged, viz. that the murmur is not *pre*-systolic (as regards the ventricles), but occurs from reflux at the very beginning of the systole, due to the imperfect closure of the diseased valves. But the character of the murmur, its prolongation, and its covering, more or less completely, the diastolic period, are no less manifest in many cases of mitral stenosis than in the present case. And, accordingly, I have never had the slightest difficulty in my own mind in dismissing the *regurgitant* hypothesis of the A.S. murmur as wholly unsupported, and even plainly inconsistent with facts of quite ordinary occurrence, which every one may ascertain for himself.

One other consideration of practical importance and significance appears to arise out of the present case; and although I cannot hope here to develop fully what I have long thought, and taught, with respect to it, a few words in conclusion on the *prognosis of obstructive lesions in the A.V. orifices* may not be out of place. Many of the best writers on cardiac diseases have assumed, without attempting to prove, that mitral stenosis is of far graver prognosis than mitral regurgitation, and indeed that the former has the worst prognosis of all the commoner valvular lesions. This has always seemed to me an hypothetical or *à priori* conclusion, in no way justified by the facts of clinical experience. It is rather difficult, of course, to prove the opposite; and no

[1] In point of fact there often *is* regurgitation, more or less, and when this is indicated by a murmur, I have pointed out that the murmur (V.S.) is entirely different, *i.e.* differs in its acoustic character, as well as in its rhythm, being quite sharply divided from the A.S. murmur by the first sound. No one who has ever got a firm and intelligent grip of these facts, now so familiar to most of us in Scotland, could possibly make the mistake of supposing that the A.S. murmur, as above described, and the V.S. which follows it, are in these cases *both* of them due to regurgitation.

one would wish, from taking up a merely controversial attitude, to appear to under-estimate the gravity of a stenosed mitral orifice. But it is quite safe to say that in no form of valvular lesion affording a sufficient basis of numbers for a general statement, can a larger proportion of instances be found of comfort maintained for years, and life prolonged almost indefinitely without the graver symptoms of heart disease. Absence of dropsy and cyanosis are so frequent as to be almost the rule in *simple* cases of mitral stenosis not complicated with marked dilatation of the right side of the heart ; and very many of such cases, as they occur in practice, are not so complicated. One is often surprised at finding the murmur of mitral stenosis characteristically present in persons who can only by rigid questioning be brought to admit that they have any cardiac suffering at all ; and in not a few of these the emphasis or reduplication of the second sound over the pulmonary artery shows that undue tension in the pulmonic circulation is already present, though in some way or other so corrected or compensated as not to lead to serious accidents. A slight occasional hæmoptysis may occur at intervals, sometimes repeatedly, and over years, without necessarily leading up to more grave consequences. Perhaps, indeed, such small leakages, when observed apart from a tubercular history, are the most easily attainable facts from which the duration of a case of mitral stenosis can be inferred, in the absence of graver symptoms. I have known several instances of married women with large families, in whom mitral stenosis existed, and in whom it was impossible to infer anything directly as to the origin of the cardiac disorder, but that in a number of successive pregnancies, otherwise quite normal, such leakages of blood had occurred. I was tempted to infer in some of these instances that the cardiac lesion might have existed before marriage, or even during childhood. At all events, the mere occurrence of such cases at all must be taken as a very considerable set-off against the extremely unfavourable prognosis of this form of heart disease during pregnancy, as set forth in the late Dr. Angus Macdonald's

well-known book.[1] I repeat that these statements are not to be taken as explaining away the real risks of mitral stenosis, which as regards secondary lesions in the heart itself, the lungs, kidneys, and brain, are simply incalculable. But they may at least serve to qualify the statements which have been made in the opposite direction ; and to show that compensation, within limits, and under favourable circumstances, is not less possible in this than in aortic valve disease.[2]

The present case of tricuspid obstruction is an isolated one, and of too rare a kind to permit of any inference being founded on it, apart from the much more common case of the left A.V. orifice. But it seems to show that on the right side of the heart also compensation, of a kind not to be expected *à priori*, may take place, to such an extent as to allow of more (probably *much* more) than ten years of the fairly active life of a common labourer being passed under the circumstances stated in this paper. These facts, which I stated verbally in London in 1881, have been adverted to constantly in my clinical teaching, and may probably have already tended somewhat to modify the extremely grave views entertained and expressed elsewhere as to the prognosis of mitral stenosis.

[1] *The Bearing of Chronic Disease of the Heart upon Pregnancy, Parturition, and Childbed.* By Angus Macdonald, M.D. London, 1878.

[2] I feel bound to refer particularly to a very able and interesting paper by my late clinical assistant, Dr. Middleton, now an acting physician in the Royal Infirmary, giving the results of a considerable amount of methodical observation of cases in the dispensary of that institution, and entitled, "A Contribution to the Study of Chronic Valvular Disease of the Heart, etc.," *Lancet*, 26th October and 2nd November, 1889.

CASE OF ANEURISM OF THE AORTA, ARISING FROM THE BACK PART OF THE ARCH SIMULATING LARYNGEAL DISEASE, AND FATAL BY SUFFOCATION; WITH REMARKS.

Monthly Journal of Medical Science, 1851, *vol. xiii. pp.* 137 *and* 184.

The following paper is the first of an interesting series of contributions upon the subject of aneurysm—a series which culminated in the admirable article contained in the first edition of Allbutt's "System of Medicine."[1] *It is concerned with the case of a man who had symptoms of intense dyspnœa, with feeble, muffled voice, and reddish or purplish expectoration, but no physical signs connected with the thorax. The conclusion was forced upon Gairdner that there was interference with the larynx, and he urged the performance of tracheotomy; this, after repeated refusals, was only entertained when the patient was moribund. After death, a small aneurysm, about the size of a walnut, was discovered, at the back of the transverse portion of the arch of the aorta, involving the recurrent laryngeal nerve. The paper enters into a full discussion of the relation of aneurysm in certain situations to the recurrent laryngeal nerve, dealing fully with the observations of Legallois and Reid. The clinical recognition of paralysis of the laryngeal muscles from pressure upon the vagus or recurrent laryngeal nerve, is due to Stokes,*[2] *whose observations were, somewhat later, confirmed by Traube*[3] *and the symptom has been familiar to every physician since*

[1] *A System of Medicine*, London, 1900, vol. vi. p. 345.

[2] *Diseases of the Heart and the Aorta*, Dublin, 1854, p. 569.

[3] *Deutsche Klinik*, Berlin, 1860, band xii. s. 395; and 1861, band xiii. s. 263.

their times. In recent years, we owe a valuable contribution to Semon,[1] *who has shown that in pressure upon the nerves of the larynx, the abductors are the first muscles to suffer. Gairdner further goes into the diagnosis of the condition; and finally discusses the question of tracheotomy, in view of laryngeal distress.*

THE following case of aneurism of the aorta is interesting, 1st, from the absence of physical signs, and the prominence of the symptoms of a laryngeal affection; 2nd, from the free communication of the sac with a mucous canal, without causing serious hemorrhage; 3rd, from the termination of the disease by suffocation, and the remedial measures suggested by this termination. On these grounds I beg to lay it before the Medico-Chirurgical Society, along with a preparation showing the parts involved in the disease.

Thomas O'Brien, æt. 46, a robust labourer, was admitted into the hospital at the hour of visit on the 30th May, 1851. He complained of great dyspnœa, which, in the recumbent posture, was so extreme as to threaten suffocation. The breathing was sonorous, with distinct stridulous character on inspiration. The countenance anxious and flushed; no fever or pain complained of. Expectoration considerable; the chest was examined as well as his state permitted, and revealed only slight bronchitic râles, the harsh laryngeal breathing being heard over all the bronchi. The voice was evidently produced with effort, scarcely husky, but having a somewhat muffled character; there was no tenderness over the larynx; the epiglottis and throat were natural. Shortly after his removal to a ward, the paroxysm subsided to some extent. I then learned that this was only an accidental exacerbation of a state which had existed for some months, and for which he had undergone active treatment. A blister was applied to the nape of the neck; and he was ordered ipecacuan wine ʒss every second hour.

On the 31st he had slept well; but the dyspnœa had returned in the morning; at visit he was better, but not able to lie down. The chest was examined with more care;

[1] *British Medical Journal*, London, 1898, vol. i. p. 1.

no abnormal percussion at any part ; the respiratory murmur everywhere abundantly audible, and natural ; some coarse mucous râles in both backs, and a few dry bronchial râles elsewhere ; the sounds over the region of the heart and of the great vessels strictly normal. The pulse was hurried, but natural in character.

He continued in much the same state till June 4th ; paroxysms of extremely difficult breathing occurring three or four times a day, and lasting generally from twenty to thirty minutes. He expectorated daily from six to eight ounces of frothy mucus, tinged of a distinct rusty colour, with streaks of purple. Repeated examination of the lungs, however, showed that they admitted air abundantly in every part, and were free from every physical sign of disease. The treatment was not altered ; the operation of tracheotomy was proposed, and, though urged as a certain means of relief from the paroxysms, was objected to in the strongest terms by the patient, who said he would rather die than submit to it.

On the morning of the 4th June, between 7 and 8 a.m., he had an attack of laryngeal suffocation of peculiar intensity, accompanied by dull pain in the lower part of the chest. He was seen by the resident clerk, and again refused the operation ; he seemed to obtain relief by being supported in the erect position, and walking up and down the ward, friction being also applied to the front of the chest. Another paroxysm, not so severe, occurred in the afternoon. At half-past 7 p.m. there was a return of the paroxysm. He was seen at 8 o'clock, when he was livid and exhausted ; he expressed a desire to be bled, and again refused to permit tracheotomy. At half-past 8 the dyspnœa was intense, the lividity of lips very great ; the face generally pale ; the skin covered with cold sweat. The operation was performed by the resident surgical clerk in attendance, but his eyes became fixed before the tube was introduced. He continued after the operation to breathe slowly and at long intervals ; the pulse continued perceptible for about ten minutes, but he did not rally, and died about a quarter

of an hour after the tube was introduced. Artificial respiration was employed without effect. A small quantity of blood was lost during the operation, some of which entered the trachea, and was apparently expelled again with considerable force.

Post-mortem examination, 6th July.—The body unusually robust; post-mortem lividity considerable; rigor mortis well marked. No emaciation either of fat or muscle.

Pleuræ containing little fluid; adhesions at apex of right lung, corresponding to a few encysted cretaceous concretions, little larger than a barley-corn. A little emphysema in the anterior parts of both lungs, and slight collapse of the tissue posteriorly, otherwise they were healthy. The greatest bronchi had the mucous membrane slightly congested, and contained a considerable quantity of tough mucus and muco-purulent matter rather deeply tinged with blood; but nowhere any distinct coagula.

The heart weighed 12½ oz.; its muscular tissue much congested. On the aortic valves, which were quite competent, and not at all deformed, there were one or two very minute granulations, and a few similar ones on the inner membrane of the vessel near its origin. The other valves perfectly normal.

The thoracic aorta had its inner membrane throughout uneven and thickened, but with little distinct abnormal deposit. The arch presented no general dilatation; it was, however, slightly dilated upwards at the root of the innominata; and this vessel, as well as the origin of the right subclavian, was uniformly large relatively to the vessels on the opposite side. The two carotids were of equal size; but both of them, as well as the left subclavian, were very slightly expanded at their origin.

At the back part of the arch, half an inch below and between the origin of the innominata and left carotid, was an oval opening, through which a hazel nut might be passed lengthways. Its edges were tolerably smooth and rounded; and it was three-quarters occluded by a mass of firm granular coagulum, which passed from this opening into the aneuris-

mal sac beyond. This was of the size of a walnut, and was situated between the aorta and the trachea, being adherent to the perichondrium of some of the tracheal rings ; the sac was nearly full of laminated, decolorized coagula, with a little fluid blood.

The left recurrent nerve, emerging from below the aorta, passed immediately to the left of the sac, and rather behind it, being bent over it, and at one point almost imbedded in the thickened cellular tissue which surrounded it ; at this point there were also one or two indurated lymphatic glands around the nerve, dark from carbonaceous deposit. The pneumogastric nerve on both sides, and the recurrent on the right, had their normal relations, excepting that the subclavian artery, where it was surrounded by the right recurrent, was, as before mentioned, somewhat dilated.

The tongue rather brown, and dry in front. Its root, and the fauces natural.

The epiglottis normal in size and form ; its mucous membrane faintly rose-coloured on the posterior aspect, and displaying a somewhat granular surface, from prominence of the mucous follicles, especially in the neighbourhood of the arytenoid cartilages. Ventricles of larynx and vocal cords natural.

The cricoid cartilage and three upper tracheal rings divided by a perpendicular incision in the middle line.

The mucous membrane in the larynx and upper fourth of the trachea nearly natural in colour and appearance. Below this the mucous membrane presented rose-coloured vascularity, deepening towards the bifurcation, on the left side, into purple. The mucous membrane slightly granular throughout this injected part from hypertrophy of the follicles.

About an inch and a quarter above the bifurcation on the left side there was a circular opening, admitting readily a crow-quill, and passing into the aneurismal sac before mentioned, which lay in contact with the outside of the costal cartilages.

Nearer the bifurcation there were three or four small

points slightly elevated, and of an opaque yellowish colour, as if the mucous membrane were stretched over some abnormal deposit. The cartilages of the two tracheal rings immediately behind the opening were entirely separated from their perichondrium at the part opposite the aneurismal sac.

The abdominal viscera were congested as usual in asphyxiated persons, but had no other morbid appearance. The abdominal aorta was not so uneven internally as the thoracic, but presented more distinctly atheromatous opaque deposit in its inner membrane.

The first question which suggests itself in connexion with this case is, *What was the cause of death?* On this point, I think, a consideration of the whole circumstances will leave no doubt that the patient died chiefly from laryngeal suffocation, induced by pressure of the sac on the recurrent nerve of the left side. The occurrence of suffocation from this cause is too well attested by numerous cases of aneurism and tumours of the chest now on record, to admit of reasonable doubt. The evidence adduced by Dr. Hugh Ley upon this subject in his work on laryngismus stridulus, although certainly insufficient to establish his exclusive theory of that disease, is strongly confirmatory of the correctness of the views entertained nearly two centuries ago by Willis as to this source of death in some intra-thoracic tumours. The experiments of Legallois, and the far more elaborate and satisfactory ones of Dr. John Reid, have demonstrated, in the most unquestionable manner, the production of laryngeal suffocation by various kinds of interference with the recurrent nerve on one or both sides of the neck. "From the experiments we have detailed," says Dr. Reid, "it is apparent that severe dyspnœa, amounting to suffocation, may arise both from irritation and compression of the inferior laryngeal nerves, or the trunks of the pneumogastrics. For when both, or *even one recurrent nerve*, was irritated, the arytenoid cartilages were approximated, so as in some cases to shut completely the superior aperture of

the glottis." [1] Section of the vagi, also, according to Dr. J. Reid, produced "sudden and violent attacks of dyspnœa which generally went off in the course of a very few minutes, when they did not terminate in suffocation;" leaving, however, the animals liable to renewed paroxysms on the occasion of a violent struggle, or any exertion tending to hurry the respiration. It is unnecessary to enter into the physiological details and principles connected with these curious results; it is sufficient for the present purpose to observe, that they fully explain the numerous cases recorded in pathological and practical works from the time of Bonetus, in which tumours involving these nerves (in the great majority of cases aneurismal) have been shown to produce death by sudden orthopnœa, often independently of any pressure directly on the air-tubes. Indeed it is worthy of remark, that spasmodic dyspnœa is a cause of death in a very considerable proportion of cases of aneurism of the aorta.[2] It is sufficient to refer, in illustration of this point, to the cases by Drs. Graham and Alison, communicated to the Edinburgh Medico-Chirurgical Society in 1835,[3] in which aneurisms of the aorta were accompanied by marked laryngeal dyspnœa from this cause, in Dr. Graham's case altogether simulating a primary laryngeal affection; to a similar case under the care of Dr. Todd,[4] in which the recurrent nerve of the left side, and all the muscles to which it was distributed, had undergone atrophy from the pressure to the tumour; to the case of aneurism of the innominata, detailed by Mr. Lawrence,[5] in which death took place from

[1] *Physiological, Anatomical and Pathological Researches*, p. 120. See also pp. 167 and 272.

[2] It may even be said that this symptom is rarely absent in those aneurisms which spring from the back part of the arch of the aorta. See Dr. Greene's collection of cases of this kind in the *Dublin Quarterly Journal*, No. 3, new series; and Mr. Crisp's table of aneurisms, *Treatise on the Blood-vessels*, p. 235; for numerous instances bearing on this point.

[3] *Edin. Med. and Surg. Journal*, vol. 43, p. 292, *et seq.*

[4] *Lancet*, June 1841, p. 400. [5] *Medico-Chirurgical Transactions*, vol. vi.

suffocation, tracheotomy being proposed but not performed; and to several examples of this form of dyspnœa detailed in a paper by Dr. Henderson,[1] and in the work of Dr. Ley before referred to.[2] I shall only say farther, that the violent paroxysms of dyspnœa experienced by my patient on many occasions before they were actually fatal, the highly stridulous respiration, the difficulty he evidently had in producing vocal sounds, and their altered character, pointed unquestionably to the glottis as the source of his danger; and the absence of any physical signs or morbid appearances indicating serious pressure of the aneurism on the trachea render it not probable that death can be ascribed to this cause. There remains only one other possible cause of suffocation, and to this I am willing to allow its due influence. The exhaustion consequent upon the numerous attacks of dyspnœa during the last days of life evidently told severely upon his strength; and the considerable and increasing quantity of mucus in the bronchi must have been expectorated far less freely in the last hours of his existence. The accumulation of this mucus, which was found in the larger tubes after death, fully explains why the operation of tracheotomy performed *in articulo mortis*, was followed by so imperfect a result. It is worthy, however, of remark, that up to the last visit which I paid him, the evidence of obstruction in the bronchi continued to be not greater than in cases of very slight bronchitis, and fully warranted the idea, that the greater part of the mucus expectorated came from the upper part of the air-passages. This idea corresponded also with the morbid appearances in the dead body.

The muco-purulent matter found in the larger bronchi after death was considerably more tinged with blood than that expectorated at any period during his fatal illness. But it is very doubtful whether this increased hæmorrhage was from the sac; as I am told that a good deal of blood was drawn into the trachea during the operation. At all

[1] *Monthly Journal of Med. Science*, 1841, p. 10.
[2] On *Laryngismus Stridulus*, etc., p. 453, *et seq.*

events, it is clear that hæmorrhage was not connected with the fatal event, nor did it ever form a serious complication, —never amounting to more than was sufficient to give a purplish, and often only a rusty tinge to the expectoration.

With this absence of material hæmorrhage, it is important to ask, *how long the communication of the sac with the trachea had existed before death?* The lungs having presented no symptom of disease, and being found after death free of all serious lesion, it is difficult to suppose that even the small quantity of blood in the sputa had any other source than the aneurism; and yet we have evidence that, if this be so, the opening must have continued for months, yielding only these small quantities, as the patient distinctly stated that he had at no time coughed up clots of blood. When we consider the nearly complete occlusion of the sac by coagula, this phenomenon will appear less difficult to understand; at all events, it is far from rare in the histories of aneurisms opening on mucous surfaces, and especially into the air-passages, to find, even after one serious hæmorrhage has given evidence of a rupture of the sac, that the hæmorrhage reduces itself to a very trifling amount, and sometimes is altogether suspended. The case of Mr. Liston at once suggests itself as an illustration of this fact. In the only record published of the fatal illness of this distinguished member of our profession, it appears that the first hæmorrhage was followed by a period of exemption from symptoms, and that when these recurred, it was in the form of a cough attended with expectoration, which was "difficult, small in quantity, and of a rusty colour;" no further material hæmorrhage occurring till his death, which was from orthopnœa.[1] In the remarkable case of abdominal aneurism which I read to the Society last year,[2] it is more difficult, owing to the situation of the first opening into the duodenum, to judge of the amount of blood that

[1] *Lancet*, December 11th, 1847.

[2] Aneurism of the superior mesenteric artery opening into the duodenum, twenty-two months before death.—*Monthly Journal*, vol. x. 1850, p. 83.

may at different times have been ejected, and it is highly probable that some bleedings may have taken place unobserved; but it seems in every way probable that no considerable hæmorrhage occurred during twenty-two months, from an aperture which had evacuated gallons of blood in a few days, and which was found very nearly, if not entirely, sealed up after death.

Could the aneurism have been discovered during life? On this point we have the following data; no dull percussion, abnormal pulsation, or tremor at the upper sternum; no abnormal sound over the heart and great vessels in front or behind; normal and symmetrical percussion over the lungs in every part; no abnormal respiratory sound over the trachea in front or at the root of the lung behind; abundant and symmetrical respiratory murmur in both lungs and over every part of them, mixed with slight mucous râles behind, and a very little sonorous râle in front. These physical signs, in regard to which full and careful examination may be relied on, form the elements of a tolerably complete negative diagnosis of aneurism, the suspicion of which was certainly entertained at the second examination of the chest, but soon dismissed, the case being treated as one of laryngeal affection. Perhaps it is still doubtful if more than a bare guess could have been formed under the circumstances; but the event showed that the suspicion thus negatived by physical diagnosis was allowed too hastily to be driven from the mind by the apparently greater probability of an ulcerative lesion of the larynx and trachea; and during the short period he was under my care no time was given for considering the case in all possible points of view, especially as the indications of practice appeared sufficiently distinct. After a careful consideration of the diseased parts, however, I am led to think it probable that while no sure sign of aneurism of the aorta could have been made out, a very close and accurate examination at the root of the neck, and in the course of the arteries, *might possibly* have discovered the dilatation of the innominata and subclavian vessels. Circumstances which did

not come to my knowledge till after the patient's death were also calculated to arouse suspicion. The long persistence even of a trifling amount of blood in the expectoration would certainly have justified the belief in something more than an ordinary ulceration in the larynx ; and a pain which he is said to have suffered at one time at the upper part of the sternum, but which was not complained of during the last part of his illness, would have confirmed the diagnosis of some fault in the thoracic region. The absence of any marked tenderness on pressure over the larynx, and of swelling of the epiglottis was calculated to attract, and did attract, attention from the first ; but this negative circumstance was considered as outweighed by the rest of the evidence.

It is worth while to remark, although it is difficult to obtain any accurate data on the subject, that the combination of symptoms presented by this case may probably be expected not unfrequently to occur in chronic or acute ulceration of the laryngeal mucous membrane. Local pain and tenderness are by no means of constant occurrence in these cases ; neither can alterations of the epiglottis and upper vocal cords be recognized in all cases, though some kind of local symptom will doubtless be accessible in the great majority. On the other hand, the presence of blood in the sputa, though of course a suspicious circumstance when long continued, is neither universally present in aneurism, nor always absent in laryngeal ulceration. I have lately seen a case almost precisely parallel to this one in every important feature, in which paroxysmal laryngeal dyspnœa, apparently very little under the influence of remedies, and accompanied for a considerable period by blood in the expectoration, is probably due to a primary laryngeal affection, of which the local symptoms have lately become more distinct, while physical signs of aneurism remain, after repeated and careful examination, undiscoverable. If this man remains under observation for a sufficient length of time, it is probable that a more secure diagnosis may be formed ; but at first it would have been

impossible to act on an assured conviction either of thoracic or laryngeal disease, while the state of the patient has been, and continues such, as may render a recourse to tracheotomy an extremely necessary expedient for his security, or even his rescue from impending death.

In reference to diagnosis, the practical conclusions which follow from the above remarks are no less evident than important. The mistake of an intra-thoracic tumour for a laryngeal affection is one of those accidents which has probably occurred in practice far more frequently than it has been accurately recorded ; although a sufficient number of instances have been published to show that it may readily occur in the most careful hands, in the absence of stethoscopic examination. It cannot, therefore, be too strongly insisted on, that a physical examination of the chest should take place in all cases of supposed laryngeal disease. This is indeed an invariable rule with all careful practitioners, on many grounds ; although it may be doubted whether the lungs and air-passages do not often too exclusively absorb attention in such examinations. But the present case, while it proves still more strongly that no amount of caution in the examination of the chest, and especially of the great vessels, is superfluous, also shows, I think, conclusively, that the absence of the physical signs of aneurism or tumour should not suffice to remove completely the suspicion that they may be concerned in the affection of the larynx. It is obvious that the part of the aorta most apt to be affected in these cases is the middle or transverse portion of the arch, and particularly its posterior or inferior surface, where it is most removed from the possibility of physical diagnosis. It is also evident that a very small tumour in these situations is enough to give rise to all the symptoms of laryngeal obstruction. Dr. Todd correctly remarked, in his clinical lecture upon the case above alluded to as having occurred under his care, that " most observers had attributed these symptoms (those of chronic laryngeal affection) to compression of the trachea and bronchi, and had overlooked the condition of the recurrent nerve." In the present case,

as in that of Dr. Todd, " there was the most ample evidence that the pressure upon that nerve occasioned the laryngeal distress."

With regard to the treatment of such cases, the present narrative seems also not devoid of instruction. Had an aneurism been discovered or strongly suspected in this case, it seems probable that general blood-letting, together with such remedies as would have contributed to control the heart's action, might have been pursued farther with advantage to the patient; whereas the chronic nature of the supposed laryngeal affection, and the active treatment to which he had already been subjected, were accepted as sufficient reasons for foregoing these remedies and trusting to blistering, ipecacuan, and the performance of tracheotomy. This operation was absolutely refused by the patient while he had sense and vigour, and the case accordingly adds one more to those in which the operation was performed too late to be of any service; but I think it is impossible not to admit that it would probably have prolonged life had it been performed at any period before the final agony, and that the patient's sufferings throughout his illness would have been greatly less severe had advantage been taken of one of the earliest threatening paroxysms of dyspnœa to place a tube in the trachea. In a clearly ascertained case of aortic aneurism, such a proceeding could of course only be proposed as a temporary relief from immediate and pressing danger; and even in this point of view it could only be prudently recommended after careful examination had ascertained the freedom of the lung and of the air-tubes from any considerable pressure; but under these circumstances, I should certainly not hesitate in offering to the patient the benefit, even though temporary, which this little operation is calculated to afford. Much less should I feel justified in withholding it, on the ground of the uncertainty of diagnosis, in cases like the present, where an obvious laryngeal spasm exists, the source of which cannot be discovered, but which is unconnected with any other ascertainable affection of the respiratory passages.

ILLUSTRATIONS OF THORACIC ANEURISM, WITH REMARKS

Monthly Journal of Medical Science, 1853, *vol. xvi. p.* 114.

In this paper the author resumes the discussion of the laryngeal symptoms connected with aneurysm, and details the facts of two cases of pressure upon the right and left recurrent laryngeal nerves respectively. He enters into a full description of the symptoms connected with instances of this kind, and, once more, takes up the question of tracheotomy. The third case, in which there were paroxysms of breathlessness, of an asthmatic character, was found to depend on the presence of an aneurysm of the descending aorta, which compressed the left bronchus, and led to condensation of the left lung. A matter of the greatest moment is taken up in dealing with the nervous influence over the musculature of the bronchial tubes; Gairdner is of opinion that cases of this kind must be due to pressure on the pulmonary plexus. The fourth case contained in the following paper is that of an aneurysm of the ascending portion of the arch of the aorta, with well-marked anginous symptoms, which are attributed to irritation of the nervous mechanism. This is quite a new departure in the study of cardiac pain. All that requires to be said in regard to this contribution is that the accumulated experience of the six decades which have passed since its appearance amply confirms the accuracy in observation and clearness in reasoning exhibited by Gairdner.

At a meeting of the Medico-Chirurgical Society, in June 1851, I brought forward a case of aneurism of the aorta, in which, as in some other published cases, the symptoms were almost exclusively referred to the larynx;

spasmodic dyspnœa with stridulous respiration forming a complication so serious as apparently to warrant the operation of tracheotomy. Although, owing to the objections of the patient, the operation was delayed till too late to be of any practical service in this case, I took the opportunity of justifying the recommendation and the practice, by appealing both to the theoretical explanation of the laryngeal symptoms which attend upon aneurism, and to the experience of other cases, which show,—1st, that dyspnœa is the direct cause of death in a large proportion of aneurisms of the arch of the aorta, especially those arising from its back part; 2nd, that the dyspnœa produced under these circumstances has generally a well-marked spasmodic character, and often distinctly the laryngeal type; 3rd, that the aneurismal dyspnœa is, therefore, not necessarily connected with direct mechanical obstruction of the air-passages, but (as was long ago hinted by Willis) is caused in many instances exclusively by interference with the nervous trunks, and especially the pneumogastric and recurrent nerves; 4th, that the nature of the laryngeal affection is proved in some cases of aneurism (as first pointed out by Dr. Todd) by the atrophy of the internal muscles of the larynx, on the side on which the recurrent is interfered with; 5th, that the operation of tracheotomy is known, in at least one remarkable instance, to have prolonged life (for thirteen days) under the peculiarly unfavourable circumstances of a *very large* aneurismal tumour not only *compressing* the trachea, but having opened into it, and caused considerable hæmorrhage. From these data I argued, that if tracheotomy, performed *in articulo mortis*, under the circumstances to which I have just alluded, could produce even the most temporary relief, there must undoubtedly be cases of aneurism in which life may be materially prolonged, and very much suffering avoided, by means of this small but important operation; and that the current rule, sanctioned by many of the best authorities in medicine, which regards the performance of tracheotomy in aneurism of the aorta as a practical error, must receive some

considerable modification. The case which I laid before the Society appeared to me to be one in which the operation was strongly called for, all the more so that the existence of aneurism was uncertain and almost unsuspected (its physical signs being absent), while the laryngeal spasm was apparent, and the absence of mechanical obstruction was rendered certain by stethoscopic examination.

Since this communication was read to the Society, several other cases have come under my observation bearing upon the subject of the functional diagnosis of thoracic aneurism, and especially its relation to disorders of the respiratory function. The peculiar importance of this inquiry must not only be evident to every practical physician acquainted with the history of the subject, but is more peculiarly illustrated by the case which I have already laid before the Society. In that case it was remarked, that there was "no dull percussion, abnormal pulsation, or tremor at upper sternum; normal and symmetrical percussion over the lungs in every part; no abnormal respiratory sound over the trachea in front or at the root of the lung behind; abundant and symmetrical respiratory murmur in both lungs, and over every part of them," etc. At a period, therefore, in the growth of the aneurism at which no physical sign could be discovered, its diagnosis might have been made, or at least its existence might have been strongly suspected, from the functional disturbance of which it was the cause, conjoined with other circumstances in the history and progress of the affection.

The following case, though not recorded in detail during the life of the patient, will tend further to show that I have not exaggerated the obscurities of physical diagnosis, as applied to small aneurisms within the thorax, and producing serious interference with the respiration:

Case I.—Shortly before my former communication was read to the Society, and while the impressions derived from the case to which it referred were in full force, a robust labourer, æt. about thirty-five, came under my observation in the Royal Infirmary, while acting for Dr. Andrew. He had very much the

same history and symptoms as the former, the symptoms of his case being frequent attacks of dyspnœa, evidently spasmodic, and accompanied by laryngeal stridor, but without fever, tenderness over the larynx, or any ascertainable local laryngeal affection, except slight and doubtful redness about the arches of the palate. The expectoration had been at various periods considerable, muco-purulent, and intermittingly tinged, streaked, or stained with blood; there was no trace or sign of any considerable affection of the lungs or bronchi, and the heart and great vessels appeared in all respects healthy. The examination was in this case made with the express suspicion of aneurism on the mind, and every known physical evidence of thoracic aneurism was sought for with the greatest care. In particular, the vessels at the root of the neck were explored with the fingers, by the stethoscope, and by percussion, without giving a trace of anything abnormal; and the trachea and bronchi, examined both at the front and over the root of the lung behind, showed the air-passages to be free from material obstruction, except at the larynx. Being thus foiled in discovering proof of an aneurism, and yet by no means satisfied of its absence, while, at the same time, the laryngeal symptoms appeared frequently to approach the point of threatening life, I told the patient that the last resource for him would be to open the windpipe, but that all other means should be tried first. This conclusion was steadily kept before him for some days; and though strongly protesting against the extreme measure, he submitted very attentively and quietly to sedative and antispasmodic treatment, which, in a week or two, seemed to have somewhat diminished the violence of the spasms, and reduced the frequency of their occurrence. On one occasion I requested Dr. Robertson to see him with me, with a view to the complete re-investigation of the question of aneurism; and the physical examination of the chest and root of the neck was again made with the utmost care by Dr. R., with a negative result. The sputa were still sanguinolent, though often slightly so, and very slight tenderness could be apparently elicited over the larynx. Tracheotomy was never insisted on with urgency, owing partly to the strong suspicion which still remained, that the laryngeal affection was dependent on an aneurism within the thorax, and partly to the slight abatement in the symptoms, which removed the principal apparent reason for urging the operation on an unwilling patient. On handing over the patient to Dr. Andrew on his return, the same course was pursued, and the chest was

again repeatedly examined without any further discoveries to which importance could be attached as indicating aneurism. Shortly afterwards, I requested a physician, who was at that time on a short visit to Edinburgh, and who has a large special practice in diseases of the throat and larynx, to examine this patient along with Dr. Bennett and myself, telling him at the same time, the doubt which appeared to hang over the case. This gentleman pronounced the epiglottis and the neighbouring parts to be ulcerated and thickened ; and his opinion was given so confidently, as the result of examination by touch, and even by sight, that the lingering doubt of aneurism was for the time dissipated ; and I believe Dr. Andrew was even induced to make a few trials of the solution of nitrate of silver, applied with a sponge to the glottis. No very material change took place in the symptoms ; but the difficulty of breathing underwent no diminution, the sputa remained purulent and sanguinolent, and a few weeks afterwards the patient expired suddenly from profuse hæmoptysis.

Dissection discovered an aneurism, the size of a large date, communicating with nearly the entire length of the trunk of the innominata, and projecting from its orifice backwards towards the lower third of the trachea, into which it opened near the mesial line. The aneurism pressed on the right recurrent nerve, which was flattened and involved in the sac. The internal muscles of the larynx, on the right side, were slightly atrophied. The mucous membrane of the larynx and epiglottis was perfectly pale and normal, unless a very slight and scarcely appreciable development of its mucous follicles be considered a disease. There was certainly no trace of ulceration, even the most superficial. The right carotid and subclavian were of normal caliber ; the arch of the aorta was not dilated ; the heart was nine and a half ounces in weight, and normal ; the aneurismal sac was nearly full of coagulum ; and this circumstance, together with its deep position, may probably account for the expansion being so slight as not to be appreciable at the root of the neck during life.

This case will, I trust, be considered to demonstrate clearly the extreme importance of the functional symptoms of aneurism within the thorax, even where the tumour, from its size and situation, may elude the most careful physical diagnosis. It is, moreover, a case where, as it appears to me, the operation of tracheotomy might with

great propriety have been performed, had the laryngeal symptoms attained the same intensity as in the former instance, or had they even remained as threatening as they were for a short time after the patient's admission. From the anatomical relation of the aneurism in this as well as in the former instance, there is little doubt that the recurrent laryngeal was the only important nervous trunk implicated in either case; and I cannot doubt, therefore, that the frightful and agonising dyspnœa which attended both cases, and especially the former, would have been materially relieved, and the tenure of life rendered for a time less precarious, had the operation been performed.

Since the preceding case was read to the Medico-Chirurgical Society, I have received from Dr. Gibson, of Dundee, the following very interesting account of a case of aortic aneurism which occurred under his care, and of which he has kindly permitted me to publish the details. I have great pleasure in directing attention to it, as an instructive instance of the successful application of what may be called a physiological diagnosis in thoracic aneurism. The performance of tracheotomy, with a certain measure of temporary success, under circumstances apparently the most desperate, was at once a bold and a well-considered application of the principles which I ventured to deduce from my first case.

Case II.—W. R., an athletic seaman, aged thirty-four, on the 31st of October, 1851, fell to the ground from a height of several feet, suffering what appeared, from a careful examination which I made shortly afterwards, to be simply a severe muscular contusion of the right shoulder and side of the chest.

From the effects of this accident he had to all appearance, with the exception of some remaining stiffness and weakness of the limb, quite recovered at the end of three weeks, when he returned to duty; and I accordingly lost sight of him till he called on me again, on the 22nd of January, 1852, to consult me respecting what he thought a bad cold, which had been hanging about him, he said, for ten days or a fortnight; and I confess that, at the time, I thought he was right. He had the muffled husky voice, the sharp ringing cough, the dyspnœa, and stridulous inspirations of a sufferer from acute laryngitis. He confessed to an uneasy,

sensation at the top of the windpipe, which pressure over the larynx increased, though it scarcely amounted to pain; and as a careful examination, by percussion and the stethoscope, failed to detect anything wrong with either the heart or the lungs, and he had no pain of chest, I at once concluded laryngitis to be the disease under which he was labouring.

He told me moreover, that he had, again and again, while at work, been suddenly seized with a feeling of instant suffocation, which he referred to his throat, and under which he had more than once come to the ground in a momentary state of unconsciousness. All this seemed still further to point to the glottis as the chief seat of the mischief; and as his pulse at the same time was hard, frequent, and jerking, my treatment was correspondingly active.

Into any details on this point, however, it is needless to enter. Suffice it to say, that it was only after the successive adoption and failure of every remedial measure suggested by the belief that the case was one simply of laryngeal disease (a view of its nature in which my friends Drs. Munro and Matthew Nimmo entirely concurred), that I began to entertain serious doubts of the accuracy of my diagnosis; and that the suspicion that this was perhaps, after all, an instance of aortic aneurism, simulating, through its pressure on the recurrent nerves, disease of the larynx, now first struck my mind on meeting, in the course of my reading during this my dilemma, with a paper by Dr. W.T. Gairdner, on an analogous case, and the details of another by Dr. W. H. Gooch; the former in the *Monthly Journal of Medical Science* for August, 1851, the latter in the *Provincial Medical and Surgical Journal* for February, 1852. These together threw quite a new light on the case. Nor did the universally acknowledged obscurity in the diagnosis of intra-thoracic aneurism during life in some instances appear to me to render less probable the accuracy of this view of the case. For, though auscultation still failed, at the end of two months of close observation, to detect in the chest of my patient the least signs of aortic disease, it was nevertheless easy, with the evidence of similarly obscure cases before me, both to surmise its existence, though too deep for detection, and to find a likely cause in the accident he had so recently met with.

I ought to add that Dr. Alison, whom my patient now consulted in Edinburgh, took, on the whole, the same view of the case.

The treatment was now for some weeks longer directed accordingly, in the hope that, under the effects of complete bodily and

mental repose, extremely low and spare diet, the occasional abstraction of blood by venesection and cupping, and the administration of such sedatives as digitalis and opium, the disease might be at least kept in check, and time afforded for Nature's attempts at a cure. But his condition, on the contrary, became progressively worse; his dyspnœa, and stridulous breathing, and cough, more distressing and constant, and the paroxysms of laryngeal suffocation in particular, so frequent and alarming upon the slightest exertion, that, with the concurrence of the two professional friends whom I formerly mentioned, I determined on opening the windpipe as the only means left of prolonging his life; though indications which the stethoscope had latterly given of pressure upon, and consequent narrowing of, the left bronchus especially, made this hope somewhat doubtful. I ought also to mention, that a very slight *whizz* was now to be detected, we thought, with each systole of the heart, at the upper part of the sternum.

Such was the patient's state when, at midnight of the 6th of last May, and with the kind assistance of these gentlemen and Dr. James Drummond of Edinburgh, I performed tracheotomy; the suffocative agony, and the whole aspect of the patient indicating that there was no time to be lost.

It saved his life at the moment, and he lived for twelve days thereafter; for although, probably owing to pressure at the root of the lungs, respiration continued still very laboured and panting, yet he managed to breathe through the opening, and while he also got rid of large quantities of ropy mixed purulent matter by coughing, he remained free from those paroxysms of laryngeal suffocation under which, previous to the operation, he had so often nearly expired. He at last sank from sheer exhaustion apparently, scarcely having closed his eyes in sleep for a fortnight or more.

Permission was given us to open the body; but I deeply regret to say that, having carefully removed the whole parts involved with a view to their being examined with more leisure at home than we then could command, and unavoidable delay having occurred in so doing, they unfortunately went so fast into a state of complete putrefaction in the keeping of another professional friend, who was present, and who undertook to preserve them, as to be found quite unfit for the purpose. Nevertheless, we saw enough at the time of our first somewhat hurried inspection to satisfy us, that a tumour of about half the size of a fist, which we found closely attached to the under part of the aortic arch,

was aneurismal ; the left recurrent nerve being, at the same time, seen passing into the condensed areolar tissue, with which it was closely invested.

I think it will be admitted, that the result of the operation in this case, while it illustrates very well the circumstances which may often be expected to interfere with the relief which tracheotomy is calculated to afford, justifies the bold and decided practice of Dr. Gibson, and holds out a strong inducement to the performance of this small operation in cases of aneurism, accompanied by well-marked laryngeal symptoms. Had the aneurism not been so situated as seriously to interfere with the caliber of the left bronchus, it is probable that the relief to suffering by the suspension of the laryngeal symptoms, and even the prolongation of life, would have been more decided ; and, with a fair chance of such a result on one side, and certain agony and rapid death on the other, the duty of the medical practitioner appears to me in no way doubtful.

The following case occurred under my own care, and as it involves numerous important practical considerations, it is given in detail almost in the words in which it was originally recorded :

Case III.—W. G., porter, robust, full-blooded, aged about sixty. Admitted into the Royal Infirmary, August 9th, 1851, at three p.m., suffering under excessive dyspnœa.

Has been ill since Monday last (five days) with cough, attended with little or no difficulty of breathing until three days ago, when he was compelled to give up working. He ascribes his illness to carrying a heavy load, for a great distance, last week. He never had any pain in the chest (he was repeatedly questioned on this point with the above result ; pain in the back was not specially inquired after). His cough was accompanied by expectoration, mixed with blood, ever since it came on. He never brought up blood in any quantity ; and has always up to this illness enjoyed uninterrupted good health. Habits not temperate.

Orthopnœa extreme ; marked lividity of face and lips ; surface of body covered with large drops of perspiration. Pulse tolerably full, regular, hurried. Has an expression of extreme

anxiety. Sputa consist of mucus, rather viscid, deeply and uniformly stained with blood. Chest expands pretty freely on both sides. Vocal thrill and resonance feebler on left side. Percussion on left side, both in front and behind, almost absolutely dull. On right side, respiration is sonorous and wheezing; on left side, comparatively feeble. Inspiration, accompanied by numerous rather coarse mucous râles; and throughout the chest sibilant and sonorous râles are audible. Heart sounds appear natural.

Bled to ℥xx., with the effect of producing an approach to faintness. Blood slightly buffed. During the venesection, perspiration, in excessively large drops, poured from the forehead and the whole body.

Vespere.—After an hour, breathing became easy, lividity of lips diminished; face was slightly flushed; pulse about 90, full. Had still cough, sometimes in paroxysms. Ordered sulphuric ether, in ℥ss. doses, with a little whisky. At half-past six p.m., Ipecac. gr. iij., and Squill gr. ij., in powder, every four hours.

August 10th.—*Vespere.*—Slept well last night, and continued well during this day. Took his meals heartily, and made no complaint. Sputa as before.

Nine p.m.—A new attack of orthopnœa; condition as on admission.

Again bled to ℥xx., without relief, and died between one and two a.m. on the 11th.

Supplementary Note on the 11th.—(From memory.)—A very plethoric man, robust, neither emanciated not corpulent.

Chest large, well developed. Dulness of percussion (as abovementioned) nearly absolute over left side; traces of pulmonary resonance only over upper lobe; respiratory murmur much enfeebled, but still audible at back; vocal resonance and thrill present over greater part, but enfeebled in lower lateral region. On right side percussion good; respiration very loud and wheezing, obscuring both the heart's sounds and those of the left lung. On this account the examination of the heart was not satisfactory; but nothing abnormal could be detected over it, or over the great vessels. No pulsation of veins, nor abnormal pulsation of arteries in neck.

Expansion of the two sides appeared not visibly different, but was not accurately estimated. The right side exceeded the left slightly (quarter of an inch) in circumference below the nipple. Heart's sounds not displaced. Position of apex indistinct.

Skin cold on admission. Afterwards no trace of fever. Countenance livid and congested on admission. Lividity had almost disappeared, though some congestion remained, at second visit.

On the 10th, was observed eating freely and heartily, without apparent dysphagia.

Examination of the Body, August 12th.—On opening the thorax, the right lung appeared voluminous and distended; the left retracted, and comparatively small. In the left pleura there were about twenty-four ounces of serum, mostly clear, with a few flakes of lymph. The right pleura contained only a few ounces of fluid. No adhesions in either pleura, except at the root of the left lung at its upper part. The heart was not displaced.

The whole of the organs in the neck and thorax were removed together, when it became evident that there was a tumour pressing upon the bodies of three or four dorsal vertebræ, and connected with the descending aorta. The pharynx and œsophagus being slit open, it appeared that the œsophagus, about its middle, was in the close neighbourhood of the tumour. The muscular coat of the œsophagus was at this point slightly atrophied, and there was a little sub-mucous ecchymosis, but the mucous membrane was normal. The larynx and trachea were normal, except that the latter contained a quantity of blood-streaked mucus.

The heart weighed fourteen ounces; all the valves normal. The aorta at its commencement was not enlarged, but its inner membrane was rough and uneven, with much opaque deposit, not calcareous. The arch of the aorta was slightly dilated near the origin of the innominata, and this vessel was also slightly enlarged. The carotid and subclavian arteries on the right side were normal in size, the former studded internally with yellow opacities. The left carotid and subclavian also contained slight atheromatous deposit, but were otherwise normal. The internal coat of the whole thoracic aorta abounded in atheromatous deposit, and was extremely uneven, but without any induration or calcareous matter. In the descending aorta, there were two considerable abnormal openings in the wall of the vessel. One was situated about an inch beyond the origin of the left subclavian artery; it was oval, about an inch by three quarters of an inch in diameter; its smooth and rather sharp edge formed by the internal coat of the vessel. This opening led into an aneurismal sac, about the size of a large orange, which, arising from the posterior wall of the artery, projected upwards and backwards, and was adherent

to the periosteum of the third and fourth dorsal vertebræ, to their left transverse processes, to the articulations of the ribs, and, through the medium of the parietal pleura, to the inner and back part of the upper lobe of the left lung. The bodies of the above-mentioned vertebræ were considerably deformed by atrophy of their anterior part. This sac was about half-full of stratified and decolorised coagula.

Another aneurismal sac arose from the second opening above mentioned in the wall of the descending aorta. This opening was round, somewhat less than a sovereign, and quite smooth-edged, like the former, from which it was distant about three-quarters of an inch downward and inward. The sac connected with this opening arose from the right wall of the artery, and passed inwards towards the left bronchus, to which it was firmly adherent, and into which it opened by a very wide communication. This aneurism was likewise half-full of laminated coagula, and was about the size of a large plum. The two sacs above-described were so close together as to form what at first sight appeared as a single tumour. They did not, however, communicate at any point.

The left bronchus had its posterior wall deficient for about an inch; and its caliber was almost completely occupied at this point by a firm, gray coagulum of blood, which projected out of the aneurismal sac.

The left lung was almost completely condensed; crepitation remained only at a few points in the apex, and still fewer in the lower lobe. Its tissue had a nodulated and dense feeling externally, and presented a remarkably variegated appearance, some portions being violet-coloured, and others of an almost sandstone-gray tint. The interlobular spaces were everywhere well marked, depressed, and often dividing sharply the red from the gray parts. On section, the same varieties of colour prevailed throughout the lung; the tissue was dense, opaque, resistant, very obscurely granulated to the eye, and here and there dotted with distinct yellowish points, which presented somewhat the appearance of tubercles, but on examination proved to be the smaller bronchial ramifications, completely plugged with opaque, viscid, muco-purulent matter. The greater bronchi contained a similar stringy mucus, more or less mingled with pus and blood; their mucous membrane was stained, but little congested.

The right bronchus and its principal branches contained a little blood-coloured mucus, but were otherwise natural. The

corresponding lung was distended, and almost emphysematous in appearance ; on section, its tissue stained here and there with blood, but highly crepitant throughout.

The only other morbid appearances of importance were, incipient cirrhosis of the liver, atheroma, with calcareous degeneration of the abdominal aorta, and slight opacity of the cerebral arteries in some points.

The case just related is an excellent example of aneurism giving rise to spasmodic dyspnœa of an *asthmatic* character, without the slightest tendency to laryngeal spasm. In this last particular it is strongly contrasted with Cases I. and II., as well as with the one recorded in the former paper. The situation of the present aneurism, and the freedom from compression of the laryngeal nerves on both sides, sufficiently explain the absence of phenomena connected with the glottis. But the positive facts of the case, and especially the symptoms of disorder of the respiration, demand to be considered in relation to the position of the aneurism and its effect on the left lung.

The tumour pressed on the greater part of the left bronchus, obstructing it almost completely, and opening into it by a considerable aperture, which, however, was greatly diminished as an outlet for blood by coagula, and by the almost complete flattening of the bronchus at the point of opening. Hence the hæmorrhage, though sufficient to attract attention, was never more considerable in amount than might have been accounted for by a more ordinary cause, such as hæmorrhagic condensation of the lung, or even ordinary acute pneumonia. The blood, confined in its passage outwards, appears to have pressed backwards towards the lung with considerable force, and was found in all the bronchial ramifications of the left lung, mingled with a large quantity of glairy, viscid mucus, which completely obstructed the greater number of the air-passages. The condition of the pulmonary texture found in connection with this form of bronchial obstruction, is well worthy of remark. It was a very striking, peculiar, and rare form of condensation, equally distinct from hepatisation and

hæmorrhagic condensation in their more ordinary forms, as well as from that simple collapse of the air-cells which I have elsewhere described[1] as supervening on bronchial obstruction in other circumstances. Yet it had characters in common with all these lesions. That the left lung was diminished in volume was evident, not only from the effusion of a certain amount of serum into its pleural cavity, but from the voluminous and almost emphysematous condition of the opposite lung, which was evidently over-distended, and had crossed the mesial plane of the chest from the collapse of its fellow. Yet this incomplete collapse of the left lung was not accompanied by an empty state of the air-vesicles, and the pulmonary tissue, although perfectly non-crepitant, was by no means flaccid, dry, and smooth on section, as usual in bronchitic collapse. On the contrary, many of the air-cells were filled with blood, and others with a soft grayish exudation, being a mixture of fibrin, in various proportions, with blood and pus, which, however, did not give to the section the distinctly granular appearance usual in pneumonic condensation, although in density and resistance the lung resembled the condition of that organ in hepatisation or hæmorrhagic condensation from disease of the heart. In fact, I believe that this curious and unusual pathological condition of the lung, of which I have preserved a very accurate drawing, can only be explained as follows :—The obstruction of the main bronchus produced in the first instance, a considerable degree of pulmonary collapse ; and after this condition had lasted some time, the bronchial hæmorrhage forced itself into the collapsed air-vesicles, much in the same manner as an artificial injection through the bronchi in a partially atrophied lung ; the lobules thus assuming a distended appearance, the intervening septa remaining depressed. The remarkable distinctness with which the surface of the lung showed the various lobules in their different degrees of distension, and the very firm plugs in most of the bronchi,

[1] Papers on the Pathology of Bronchitis, *Monthly Journal of Medical Science*, 1850-51.

seem to prove the correctness of this solution of the anatomical phenomena.

I have next to consider the *dyspnœa*. That a certain considerable amount of dyspnœa is fully accounted for by the condition of the left lung, it is impossible to deny. But the peculiarly exquisite character of the symptom in this case, its very perfect intermission within an hour or two after the first bleeding, as well as during the whole of the second day of observation, and its equally sudden re-accession on the next evening, are phenomena much more allied in character to spasmodic asthma than to any other disease of the respiration, and certainly not due to the condensation of the lung, or to the permanent obstruction of the air-passages. Bearing in mind the analogy of the cases of compression and irritation of the laryngeal nerves, and the recent experiments of Volkmann[1] and others, which show that the nervous system has a powerful influence over the contraction of the bronchial muscles, it is impossible to avoid the conclusion, that the spasmodic asthma in this case was due to the pressure of the aneurism on some of the branches of the left pulmonary plexus of nerves, which must have lain in great part between the tumour and the bronchus. The main trunk of the pneumogastric was not involved by the tumour, and accordingly there was no dysphagia; nor were any of the other important nervous structures in the thorax at all implicated. The facts of the case, therefore, both positive and negative, seem to be perfectly in harmony with the morbid appearances as thus explained.

In relation to *diagnosis*, this case is not a little instructive. The idea that the patient was the subject of aneurism was strongly presented to my mind at the first examination, and was mentioned at the dissection as the most probable solution of the case. Notwithstanding the absence of the elements of a physical diagnosis, this opinion seemed to be justified by the following circumstances: —The almost complete dulness on percussion over the right lung, and the

[1] *Handwörterbuch der Physiologie*, vol. ii. p. 586.

feebleness of respiration, vocal fremitus, etc., on that side of the chest, could only be produced either by considerable pleural accumulation, or by pulmonary consolidation, accompanied by nearly complete obstruction of the bronchi.[1] The former supposition was excluded by the absence of dilatation of the left side of the thorax; the latter was entirely in harmony with the supposition of an aneurism of the descending aorta. The presence of spasmodic dyspnœa, and the absence of laryngeal spasm, seemed equally to point to this as the probable situation of the aneurism, if present. The presence of blood in the sputa lent strength to the idea of aneurism, as it has been shown in numerous other cases that this disease does not necessarily lead to profuse hæmorrhage by bursting in a mucous surface. Although the amount of blood observed in the expectoration in this case was not greater than is often observed in heart disease with pulmonary hæmorrhage, yet as evidences of valvular disease were wanting, and as the consolidation of the lung in pulmonary hæmorrhage is usually double, I was induced to regard this idea as less probable than that of aneurism; and was thus led, among various circumstances suggestive of doubt, to the correct diagnosis. Admitting the idea of aneurism, there could be little doubt as to the situation of the tumour. The absence of laryngeal symptoms and of dysphagia, the signs of obstruction of the left bronchus, the absence of dull percussion or abnormal sounds on the front of the chest, the normal condition of the vessels in the neck, combined to give a correct idea on this point. If a more accurate history had been procurable, the presence of pain between the shoulders would have probably formed an additional element in the diagnosis.

The present case illustrates very clearly a respiratory symptom dependant on aneurism, probably due, like the laryngeal dyspnœa before mentioned, to nervous irritation,

[1] I have been for several years in the habit of observing that pneumonic consolidation so accompanied will give rise to all the signs of a large pleuritic effusion, except distension of the side. Of this I have now seen many instances besides the present.

but of a kind beyond the reach of surgical relief. It may easily be believed that these two symptoms may be combined in one case, especially if the aneurism be of large size ; and in such instances little or no relief would be procured from tracheotomy. I do not, however, regard the possible existence of this, or any other unascertained complication, as a sufficient reason for withholding the operation ; but only as suggesting the propriety of a very guarded prognosis, great care being taken in all cases to ascertain as nearly as possible the relations of the tumour when tracheotomy is proposed. The cases which hold out most encouragement to the operation are evidently those of small tumours of the innominate artery, or of the lower and back part of the arch ; and every part of the air-passages ought to be accurately explored, with a view to the discovery of evidences of obstruction.

The catalogue of symptoms due to the pressure of intrathoracic aneurisms on the nerves in their neighbourhood, is not exhausted by those to which I have hitherto alluded. Various kinds and degrees of uneasiness or positive pain arise from this cause ; although the most severe and constant painful sensations produced by aneurism are no doubt due to the erosion of the bones and cartilages on which, as in the above case, they so often exercise deleterious pressure. Dysphagia, although frequently due to direct pressure on the œsophagus, is sometimes so distinctly spasmodic in character as to suggest the idea of its having its source in pressure on the pneumogastric nerve ; and the respiration sometimes suffers (as I believe I have seen in one instance) from lesion of the phrenic nerve in large aneurisms. But there is one symptom of by no means unfrequent occurrence, and which I believe will be found to be almost confined to aneurisms of the ascending aorta, especially in the immediate neighbourhood of the heart. This is a greater or less degree of *angina pectoris*, as described by Heberden, a form of nervous irritation, which may well be referred to compression of the great plexuses of nerves ramifying on either side of the ascending aorta, and com-

municating freely with the cardiac ganglia and plexuses of the ventricles. The following case affords a good illustration of this symptom in an almost uncomplicated form :

Case IV.—A man, aged about 45, affected with manifest signs of aneurism of the ascending aorta pressing on the costal cartilages, came under my notice repeatedly at intervals during last summer. He had been ill for many months, when first seen ; had been formerly under medical observation ; had borne his fatal complaint to Ireland and back, and by watching the gradual course of his disease had acquired spontaneously so complete and intelligent a conviction of its nature and certain termination, that his whole manner and habit of life conveyed the impression of a man prepared for instant death. As he was very patient and calm, he often related to me, at considerable length and with great clearness, the narrative of his sufferings. These were very variable in amount ; he had almost constantly a certain degree of gnawing pain, corresponding to the site at which the aneurism had produced a pneumonia at the front of the chest. He had no dysphagia, and little if any feeling of dyspnœa ; the lips were, however, somewhat livid, and the venous return by the superior cava was manifestly impeded. The most severe symptoms arose from a feeling, which seized him from time to time, and the minor degrees of which were present, to a greater or less extent, every day. It was a sensation of pain rarely amounting to agony, but attended by extreme distress, and an indefinable species of uneasiness, which he could only describe as driving him to desperation, and as being quite unrelieved by change of posture or by any kind of repose or exertion. This sensation was not distinctly localised within the chest ; he often compared it to the approach of death ; it usually passed off, at least in its intense form, after a few hours, but a certain degree of it was almost constantly present with him. The pulse was little affected by this condition ; the respiration always free. Percussion revealed a large tumour, occupying the position of the ascending aorta, and projecting to the right side of the sternum ; a double murmur existed over the tumour, and the heart presented distinct signs of hypertrophy. This patient died in the middle of August, during my temporary absence from Edinburgh. Many remedies had been used, but none were of any avail as palliatives, except the exhibition of large doses of opium.

The dissection was performed by my friend Dr. Sanders. A large aneurismal sac was found adhering to the parieties on right

side at the fourth and fifth costal cartilages. The pericardium contained some serum, deeply stained with blood; and it presented reticulated lymph on both its surfaces. The aortic valves were incompetent; the heart much hypertrophied.

The aneurismal dilatation, on being laid open, showed a large pouch, consisting of the entire vessel, dilated from its very origin to the descending portion, where it resumed nearly its normal diameter, but presented much disease from atheromatous and calcareous deposits. From this dilated arch of the aorta there opened laterally on the right side, through a ring-shaped aperture of two inches and a half in diameter, another sac, lined only by thin fibrous walls, and adherent, as above mentioned, to the third and fourth right costal cartilages. The pneumogastric and recurrent nerves were carefully traced, but neither of them appeared to have been involved in the tumour or stretched over it.

In the present and in the former paper I have adduced illustrations of three distinct kinds of spasmodic or paroxysmal affection liable to be connected with aneurism of the thoracic aorta, and of its branches within the chest. *Firstly*, Spasmodic *laryngeal* dyspnœa or *laryngismus*, attended frequently but not invariably, by alterations of voice or cough, and sometimes by habitually harsh stridulous respiration. *Secondly*, Spasmodic *bronchial* dyspnœa or *asthma*, which may be accompanied by disease of the lung, or signs of pressure on a bronchus. *Thirdly*, Paroxysmal suffering referred to the heart, of the character now well known to practical physicians by Heberden's name of *angina pectoris*. The view, that these affections have their origin in the interference of aneurisms with the thoracic nerves, seems to have so much theoretical probability as to claim from future observers more attention than it has yet received in the instance of the last two; and the very decided manner in which observation has corroborated this view in the instance of the laryngeal spasms, suggests the propriety of increased accuracy of anatomical and physiological investigation in relation to this subject. As the instances which have occurred to myself have not presented a sufficient basis for induction, I endeavoured

to obtain a broader ground for generalisation, by consulting the preparations of aneurism in numerous museums here and elsewhere, and the records of older observation on this subject ; but the result was far from satisfactory, and it soon became evident that very little was to be added to our knowledge in this way. The museums to which I have had access abound in preparations of aneurism in which every relation of the slightest physiological interest has been laboriously cleared away by the knife of the dissector ; and the guesses that might have been formed as to these relations are, in the vast majority of instances, rendered valueless by the negligence which has allowed the records of the case to perish. On the other hand, the literature of aortic aneurism has not appeared to me to yield many cases in which the symptoms I have described above admitted of precise analysis in connection with anatomical details of the position of the aneurism. It is sufficiently evident that the subject requires renewed observations, conducted with a view to the elucidation of these symptoms ; and I trust this paper may prove the means of attracting additional attention to the relations of aneurism with the nervous system especially in the thorax, where results of considerable importance to diagnosis and treatment have been shown to depend upon these relations.

Not a few cases are now on record, in which the sufferings caused by aneurism have been of so variable and capricious a character, as to suggest the idea of a purely nervous affection ; and the complete remissions which occurred in such cases, naturally surprised the older anatomists, who could not conceive "but that the effects of a permanent cause, such as an aneurism is, must be permanent."[1] These paroxysmal attacks are in some cases described as resembling suffocation or orthopnœa, the breathing being sometimes accompanied with stertor (stridor ?), or with a sensation of a cord binding the trachea ; in other instances they have evidently none of the character of angina, being accompanied by palpitation of the heart, swooning, etc.

[1] Morgagni, *de Sedibus et Causis Morborum*, Lib. 2, Epist. xvii. 27.

Frequently these symptoms are mixed, and very often details are wanting to distinguish between them. In the 17th and 18th Letters of the second book of Morgagni's work, the reader will find many interesting cases and most able discussions, which reflect the entire information of his age upon the subject.[1] In my former paper on this subject, I have alluded to the later observations of compression of the recurrent nerves by aneurism: on the other departments of the subject I have failed to discover any more precise data. Anatomical knowledge would lead us theoretically to look to compression of the cardiac nerves by aneurisms of the ascending aorta immediately above the valves, as the probable cause of angina; and, on the other hand, to refer the asthmatic paroxysm to aneurisms having the relations of the one in Case III. So far as my reading extends, I have found those views corroborated by the few cases from which any approach to precise deductions can be formed. It is certain, at least, that aneurisms of the ascending aorta are found not to produce dyspnœa in the same large proportion of cases as those of the descending and transverse part of the arch, except where this symptom occurs as a sequela of cardiac hypertrophy and valvular disease; and these are, again, more frequent in connection with aneurisms of the ascending aorta than with those of any other part of the vessel. I desire, however, to avoid laying too much stress on these imperfect observations, and to leave the subject to more mature and careful investigation.

[1] See especially Epist. xvii. Sect. 14 (angina and spasmodic dyspnœa); Sect. 25 (spasmodic dyspnœa); Sect. 26 (do., interesting remarks on treatment by warm water fomentations of arms; in this case there was probably angina); Epist. xviii. Sect. 17 (spasmodic dyspnœa, probably with angina, in a physician of Modena). He was seen by Ramazzini and Malpighi, and, with the exception of the latter, no one who saw him suspected an organic disease. A case of great interest; the dissection, however, very imperfectly recorded. With these examples contrast the case in Sect. 25, in which dyspnœa, non-paroxysmal, accompanied an aneurism pressing directly on the lung.

CASE OF ANEURISM OF THE AORTA PROJECTING INTO THE NECK, AND ACCOMPANIED BY CONTRACTION OF THE PUPIL ON THE AFFECTED SIDE.

Edinburgh Medical Journal, 1855-56, *vol. i. p.* 143.

Changes in the size of the pupil are very common, as the result of pressure upon the sympathetic nerve, anywhere above the lower extremity of the cilio-spinal part of the gangliated cord. We owe the explanation of the phenomenon to the following paper. It is perfectly true that changes in the pupil from pressure upon the sympathetic nerve by other lesions had been mentioned by Reid,[1] *while Walshe*[2] *noticed them in aneurysm, without making any attempt to explain them. It was Gairdner, therefore, who undoubtedly elevated the altered size of the pupil into an important symptom of aneurysm. In this paper, Gairdner points out that the patient described furnished "an interesting example of a pathological condition explicable by physiological laws." He reviews the old work of Petit, Cruickshank and Dupuy, and the modern investigations of Reid, Valentin, Budge and Waller, as well as the clinical observations of Walshe; the case on which the paper is based is then fully narrated and the physiological observations and pathological explanations clearly described. The later observations contributed by Ogle*[3] *and Argyll Robertson*[4] *are of much value;*

[1] *Edinburgh Medical and Surgical Journal*, 1838, vol. xlix. p. 132.

[2] *Diseases of the Lungs and Heart*, 2nd edition, London, 1853, p. 759.

[3] *Medico-Chirurgical Transactions*, London, 1860, 2nd series, vol. xxiii. p. 397.

[4] *Edinburgh Medical Journal*, 1869, vol. xiv. p. 696.

more recently, alterations in the prominence of the eye-ball and in the size of the pupil, from diseases of the thoracic organs, have been very carefully studied by Purves Stewart,[1] *while their relation to angina pectoris has been described by myself.*[2]

THE following case was narrated, and the patient was exhibited to the Medico-Chirurgical Society of Edinburgh, as an interesting example of a pathological condition explicable by physiological laws. The experiments of Petit, considerably upwards of a century ago, demonstrated that the section of the united vagus and sympathetic nerves in the neck of the dog has a marked effect upon the pupil, and on the conjunctiva of the eye; from which he not unnaturally drew the inference, that "the intercostal (sympathetic) nerves furnish branches, which convey the spirits into the eyes."[3] Although this conclusion was a sufficiently startling one, and although Cruickshank, Dupuy, and others performed experiments tending more or less distinctly in the same direction, the first exact investigation of the subject was due to the late Dr. John Reid, whose attention was attracted to it in the course of his researches on the vagus nerve, and who succeeded in proving distinctly the dependence of contraction of the pupil upon section of the sympathetic in the neck, independently of every other source of disturbance.[4] Dr. Reid did not fail to anticipate for this inquiry a pathological as well as a physiological importance, and he refers to "a case described in the *Medical Gazette*,[5] where the right carotid, the vagus, and surrounding parts are described as being enveloped in a large morbid tumour, and where, consequently, the sympathetic could hardly be supposed to escape," in which "the pupil of that side is described as becoming smaller in the course of the disease."

[1] *Diagnosis of Diseases of the Nervous System*, London, 1911, 3rd edition, pp. 136 and 334.

[2] *Brain*, 1905, vol. xxviii. p. 52.

[3] *Histoire de l'Académie Royale des Sciences*, An. 1727.

[4] *Edinburgh Medical and Surgical Journal*, January 1841.

[5] September 29, 1838.

Valentin, from further experiments, and from a consideration of the whole arrangement of the nerves involved, concluded that the pupil derives its nervous supply from two sources; the nerves, which act on the radiating fibres of the iris, from the spinal system, *through the sympathetic*, and those which supply the circular fibres (or those which contract the pupil), from the inferior branch of the motor oculi nerve. The section of the sympathetic trunk in the neck, according to Valentin, paralyses the former nervous filaments, and resigns the pupil to the exclusive influence of the circular fibres, which keep it in a state of permanent contraction. The more recent experiments of Budge and Waller tend to confirm the views of Valentin, and to show that while the circular fibres are supplied from the third (oculo-motor) and also the fifth (trigeminus) cerebral nerves, the radiating fibres receive filaments from the sympathetic, which join the ophthalmic branch of the fifth after it has passed through the Gasserian ganglion. Hence, stimulating the sympathetic in the neck dilates the pupil, and cutting it causes contraction of the pupil, more or less permanent. These experiments further appear to show that the whole of the sympathetic fibres which go to the pupil from the cervical ganglia, are originally derived from the anterior roots of the spinal nerves, and, consequently from the spinal cord in the lower cervical and upper dorsal region. If the spinal cord be destroyed between the fifth cervical and the sixth dorsal vertebræ, contraction of the pupils occurs. If one side only of the spinal cord be destroyed in this region (which MM. Budge and Waller call the *regio cilio-spinalis*), or if the emerging spinal nerves or their anterior roots be cut, a similar effect is produced. Further, a consideration of the different distribution of the nerves in different animals goes far to explain the discrepancies which have been met with in former experiments.

These investigations seem to be very clearly applicable to the explanation of the case to be presently detailed. They are mentioned here, because the subject does not seem to have attracted the attention which it deserves from physi-

cians. At the time I introduced this patient at the Medico-Chirurgical Society, I was not aware that the occurrence of contracted pupil, as a symptom of thoracic or cervical aneurism, had ever been placed on record. I remarked, indeed, that accidental differences in the size of the two pupils were not uncommon; and that in the present, or in any other isolated case, the observation would be very apt to be disregarded, as a mere coincidence, unless the physiological explanation were present to the mind of the observer. I have, however, had my attention directed to a case of aneurism at the root of the neck, noted by Dr. Walshe in the last edition of his work on *Diseases of the Lungs and Heart*, p. 759, in which one pupil was observed to be "very notably smaller than the other, where no cerebral symptom of any kind existed." Unfortunately, the state of the nervous structures in the neck does not appear to have been investigated; and I am informed that no recorded details exist, as to the position of the aneurism, sufficiently precise to allow of an accurate appreciation of its relations. The previous observation of this case, however, together with the one above noticed in the *Medical Gazette*, will, I trust, concur with the facts adduced in this communication, in fixing the attention of physicians on the state of the pupils in similar cases of disease.

Physical Signs, etc.—J. W., æt. 40, quarryman, very robust. Was seen first by me in autumn 1854. He had at that time all the signs of a considerable aneurism at the root of the neck in the left side. The expansion of the sac was chiefly above the clavicle, but there was dulness on percussion below it for about half an inch. There was no bruit—only a strong double vibrating shock; second sound natural over aorta. The circulation not interrupted either in subclavian or carotid, if anything, rather feebler in left subclavian than right. Never any œdema of left arm, but numbness and pain frequently experienced. More pain in arm than at site of tumour; considerable darting pain, too, around the back of shoulder; heat of left side of face and head, but face never seemed to flush. No evidence of hypertrophy or other disease of heart.

He did not at first ascribe his disease to any particular accident;

but afterwards gave the following account of it to Dr. Inglis, resident-physician :

History.—" On one occasion, two or three years ago, he recollects distinctly lifting a smith's anvil 22 stone in weight. He had a bad grip of it, and to hoist it up was obliged to rest the greater part of the weight on his left arm. He felt himself very much strained, and after completing the work he was quite blind for a time ; for long after he had pain in his left side. He applied to a doctor, but got no relief. He continued at work till the day before entering the hospital, although not able to do as much as formerly. The pain of the arm going down to the fingers began about a year after the strain. He has no recollection of any later accident. For twelve months he has had difficulty of breathing on going up a hill. Never noticed that there was a swelling, until it was pointed out to him in the hospital."

Observations on Pupils.—The difference in size of the pupils, as shown to the society last December, was made the subject of particular observation throughout the progress of the case. At the time of my first seeing the patient, the difference was quite as great as it ever was at any subsequent period ; indeed, for a good many weeks before death it had become scarcely recognisable. Both the pupils were of small size, but the left very remarkably so, generally not more than a line in diameter, in the light. Both pupils dilated and contracted under different degrees of light, but the dilatation of the left, even in deep shadow, was very slight. This observation was made repeatedly during six weeks, during which nothing was applied calculated to affect the pupils. Once or twice, it was thought that the left conjunctiva was slightly congested, but this cannot be stated with confidence.

Experiments with Atropine.—In December several experiments were made with atropine and belladonna, of which the following are the results :

1. The affected pupil was susceptible of dilatation under a solution of atropine placed on the conjunctiva. The dilatation was perhaps scarcely complete, but very nearly so. It did not commence, however, till about three-quarters of an hour after the atropine was applied. The dilatation continued nearly two days, and for several days more the original inequality of the pupils did not return.

2. The pupils being in the usual condition of inequality, extract of belladonna was given internally in repeated doses, till both pupils were dilated. In doing so, it was observed that throughout

the experiment the left pupil continued smaller than the right.

Progress.—The tumour did not undergo much enlargement till the end of the year. Considerable relief was derived from the application of freezing mixtures, although the pains were scarcely ever absent. No internal remedies were applied. Various liniments were tried, and given up in favour of the application of cold.

On December 31, in consequence of increased pain and throbbing in the tumour, local depletion by leeches was employed for the first time, and the patient was put on a very restricted scale of diet. He had always been a moderate eater, and found, as he told me, no difficulty of restraining his appetite, as the pain often took away the desire for food. For a time, too, he thought he derived benefit from this system. I allowed him to have leeches repeatedly applied, and he decidedly lost flesh and strength. The tumour, however, extended very decidedly during January and February. Freezing mixtures were continued. On February 27, 10 oz. of blood were taken from the arm, but I was not induced to repeat this experiment. By this time the local applications had lost their effect, and the pains had become greatly aggravated. Repeated doses of opium were required to procure sleep. On March 27, it is noted, that "he takes 80 minims of solution of morphia every night." About this time I observed him seated near the fire with both hands grasping the edge of a shelf almost as high above his head as he could reach. He said, that in that position the pain was somewhat alleviated. He was much weaker at this time from want of sleep and general distress, but never lost his appearance of being a strongly made muscular man.

In the course of the month of March, a number of sedatives were tried, aconite, cannabis, opium, applied by Dr. Wood's method. He always begged, however, to be allowed to return to the internal use of morphia in large doses, which he found to be, on the whole, the most effectual way of procuring rest. The tumour continued to become more and more diffused, occupying a great part of the posterior triangle of the neck, but not tending at any point to become superficial.

New Symptoms.—Two new symptoms were noticed, or at least became much more distinct during this interval; slight cough, with mucous expectoration, and diminished power of motion of the fingers of the left arm, which were usually bent half-way into the palm. A little dysphagia, and slight hoarse-

ness of voice, had been present from the first time he came under observation ; and these, too, increased at this time.

Hæmoptysis.—On April 12, he expectorated a little blood. His general uneasiness, and all the chest symptoms, were likewise increased. He lay mostly in bed.

Death by Hæmorrhage.—On April 22, he brought two or three teacupfuls of arterial blood, and suddenly expired.

Post-Mortem Examination.—On examination after death, an aneurism was found arising, by an opening about an inch in diameter, from the upper part of the aortic arch. The sac involved the origin of the left carotid, which vessel was slightly dilated for about three-eighths of an inch above its origin, but elsewhere normal. The left subclavian artery was normal throughout, and lay in front of the aneurismal sac at its connection with the aorta. The sac, which was irregularly oblong, and appeared to be of the size of a very large lemon, occupied the subclavian space and root of the neck on the left side, and passed deeply backwards to the 5th, 6th, and 7th cervical, and to the 1st and 2nd dorsal vertebræ, with which it was in contact. In front, the deep fascia of the neck, the carotid artery, jugular vein, and pneumogastric nerve were slightly stretched over the sac, which, however, exerted its pressure in the opposite direction. The subclavian artery and vein, and the scaleni muscles were also very slightly displaced forwards and outwards, but were not adherent to the sac. The inner and back part of the tumour adhered closely to the vertebral column, and to the œsophagus ; it may also have exerted slighter pressure on the trachea and the left recurrent nerve, but these did not appear to be much displaced. The lower divisions of the brachial plexus were stretched over the sac at its upper part, where it came in contact with this point of origin from the spinal column. The sympathetic nerve came in contact with the sac at the point where it passes downwards to form the ganglion in front of the vertebral artery. The bodies of the vertebræ above-mentioned were pretty deeply eroded on the left side, and the transverse processes of the 6th and 7th were absorbed to a considerable extent ; the latter transverse process, indeed, had entirely disappeared. The upper surface of the first rib near its tubercle, and the corresponding transverse process on the left side, was slightly eroded. At the level of the 7th cervical vertebra, a large oval aperture of communication, nearly an inch in length, existed between the aneurismal sac and the dura mater of the cord, in consequence of the deficiency of the

transverse process, with a portion of the arch and body of the 7th cervical vertebra. Opposite the diseased bones, the sac was, to a great extent, filled up with moderately firm laminated fibrinous deposit.

On examining the œsophagus, a ragged opening, about three-quarters of an inch in its long diameter, was found opposite the aneurismal sac, and communicating with it. The mucous membrane of the trachea was normal.

The stomach contained about a pint of blood.

The heart, and the greater part of the aorta were normal.

The apex of the left lung was firmly adherent to the aneurismal sac, over more than a square inch. The sac was at that point very thin. The lungs, elsewhere, were normal, as were the other organs.

The pupils were in the usual slightly dilated condition observed after death, and there was no appreciable difference between the two sides in the amount of dilatation.

The occurrence of this examination during the holidays, when my friend Dr. Struthers was absent from Edinburgh, prevented my securing his co-operation in a plan which I had contemplated, of having the whole dissection conducted under his eye, and in his anatomical rooms. The interest which he took in the case during life, led me to hope that the anatomical relations of an aortic aneurism, presenting so remarkable a character as that which I pointed out in a former occasion to the Society, would receive that full and complete investigation which they deserved. As it was, the impossibility of retaining the body when not claimed for the school, and my own want of the necessary skill, have combined, I fear, to render this account of the dissection less satisfactory than was to have been desired. Still, the following facts and inferences may be regarded, I think, as fully established :—

1. The aneurism did not, as I at first supposed, involve the inner portion of the sub-clavian artery, but lay on a plane entirely behind it. This fact explains the extremely slight impediment to the circulation of the limb, whether arterial or venous.

2. The aneurism exerted its chief pressure in a direction backwards and inwards ; more on the emerging roots of the

spinal nerves, and their connection with the sympathetic, than on the carotid, pneumo-gastric, or recurrent. This was fully anticipated from the symptoms, and from the situation of the tumour, as I indicated in December. Since that period, however, the marked hoarseness of voice, and evidences of œsophageal pressure, show that the aneurism had, at a late period, been extending so as to involve structures originally not much affected.

3. The vertebral artery, and the sympathetic ganglion lying upon it; a portion, at least, of the brachial plexus; the anterior roots of several of the lower cervical nerves, with the branches given off by them to the sympathetic; the inferior attachments of the longus colli muscle, must have been either entirely sacrificed, or very much altered in their structure and relations. In addition to these extensive encroachments, the aneurism may have exerted a certain amount of pressure on the spinal column; and especially on that region of it described by Budge and Waller as the cilio-spinal. The localized character of the paralysis, however, and particularly the small amount of paralysis of sensation in the left arm, render it probable that the spinal cord had not been actually disorganized to any appreciable extent, and that the chief pressure of the aneurism was on the anterior roots of the spinal nerves. In these facts the symptom pointed out to the Society, the permanent contraction of the pupil, finds ample explanation.

4. It is uncertain whether the equality of the pupils, in the last few weeks of life, depended upon the assimilation of the condition of the left to that of the right, or of the right to that of the left pupil. In the latter case, it is open to question whether the contraction of both pupils was a result of interference with the cord, or of the opium which the patient took so largely as an anodyne.

Finally, it may be supposed that the equalization of the two pupils, in the latter part of the history of the case, was due to some compensating or collateral nervous influence, which had arisen to supply the deficiency caused by the pressure of the aneurism. It is certain that the dilating

power of the pupil was not in this case, at least, at the time of the experiments with belladonna, destroyed, but only weakened.[1]

5. Death took place by hæmorrhage into the œsophagus, from which the stomach and a portion of the intestinal canal appear to have been filled with blood before any was ejected by vomiting. The comparatively small quantity of blood which came up with the sputum, may possibly have been hawked up from the œsophagus; but it seems more probable that it was the result of direct bleeding of the sac into the apex of the left lung. Three fatal terminations, therefore, were simultaneously impending: 1st, hæmorrhage into the œsophagus; 2nd, hæmorrhage into the lung; 3rd, pressure on the cord and paralysis. Perhaps we may also consider that serious symptoms connected with the respiration, were not far off; as the recurrent nerve and the trachea, would very soon have been involved.

I have thus attempted to indicate a new source of functional disturbance in thoracic aneurism, as connected with the interference of such tumours with the nervous system. In this point of view, the present case may be advantageously considered in connection with those which I have laid before the Society on former occasions.[2] It results from the whole series, that aneurisms of the aorta may, in virtue of their pressure on different portions of the nervous system, produce four different classes of symptoms: 1st, angina pectoris; 2nd, spasmodic laryngeal dyspnœa; 3rd, spasmodic asthma, or bronchial dyspnœa; and 4th, permanent contraction of the pupil on the affected side. On the important considerations connected with diagnosis and treatment involved in these four aspects of aneurismal disease, I need not now detain the Society by any further remarks.

[1] It would appear from one of Dr. Reid's experiments, that the contraction of the pupil in these cases is not absolutely permanent. "In a cat . . . the pupil was nearly natural a month after portions of the sympathetic and par vagum on one side were removed. *Loc cit. in a Note.*"

[2] *Monthly Journal of Medical Science*, Aug., 1851, and Feb., 1853.

CASE OF ANEURISM OF THE AORTA,

OCCUPYING THE ARCH, AND OBSTRUCTING THE LEFT CAROTID AND SUBCLAVIAN ARTERIES; WITH LESSER ANEURISMS, ONE OF WHICH OPENED INTO THE LEFT AURICLE; AND WITH AORTIC AND MITRAL REGURGITATION.

Edinburgh Medical Journal, 1855-56, *vol. i. p.* 429.

The case which forms the subject of the following contribution afforded Gairdner an opportunity of putting the views of Kussmaul, contained in an academic dissertation of 1855, *to the test of clinical application. The short analysis embodied in the following paper will show that an experimental alteration in the supply of blood furnished by the carotid arteries in the physiological quadrupeds effects definite changes in the size of the pupil. The case which is described by Gairdner was one of aneurysm which entirely obliterated the lumen of the left carotid and left subclavian arteries. There was, therefore, a permanent loss of blood supply by the left carotid artery. Throughout the entire period during which the patient was under observation, there was absolutely no alteration in the size of the pupil. This case alone is sufficient to prove that the change in the size of the pupil is not dependent upon blood supply.*

In a previous paper[1] I narrated a case of aneurism at the root of the neck, accompanied by contraction of the pupil on the affected side, and thereby forming an illustration of the results of injury to the important nervous structures in that situation. Taking the case in

[1] *Edinburgh Medical Journal*, 1855-6, vol. i. p. 143.

connection with the physiological data there mentioned, I believe this conclusion to be irresistible ; but as others may be led to suppose that the interference of the aneurism with the circulation of the left carotid had, by its influence on the cerebral circulation, something to do with the contraction of the pupil, I am led to advert again to the subject, in order to show that this was probably not the cause. I take pleasure in referring to the elaborate and interesting inaugural dissertation of Dr. Kussmaul,[1] "On the Influence of the Circulation on the movements of the Iris, and other parts of the Head," as bringing to the test of experiment all that can be said on this view of the subject. This dissertation bears the date 1855, and has been transmitted to the Editor of the *Edinburgh Medical Journal* since the publication of my last paper.

Dr. Kussmaul's researches show, that the result of suddenly cutting off the flow of blood through the carotid arteries is to produce a certain amount of contraction of the pupil, *followed*, however, *after a short interval, by dilatation*. On the other hand, an increase in the flow of blood is usually succeeded by dilatation of the pupil. But, admitting that these results are clearly and unquestionably as stated, it does not appear that a really permanent and considerable contraction is, under any circumstances, the consequence of interference with the circulation alone. I am led, indeed, by the extreme care and delicacy with which the measurements in Dr. Kussmaul's experiments were conducted, and his great and praiseworthy caution in endeavouring to avoid collateral sources of error, to the conclusion, that the amount of effect produced was by no means such as to be easily ascertained, or to carry confidence to his mind by any means short of those which he employed. It could not, therefore, have been in any degree comparable with the contraction of the pupil produced by interference with the sympathetic trunk, or with that observed in the case of aneurism narrated in the August number.

But it is best to let nature answer the questions which

[1] Wurzburg, 1855.

she herself proposes. The future observation of cases of aneurism, with special reference to this point, will furnish ample data for deciding to what extent, and in what manner, the pupil is affected by the permanent suppression of the circulation through the carotid, on one or other side. In the meantime, I beg to contribute to this inquiry the following abstract of a case which seems (so far as a single carefully observed instance may be trusted) to prove that the influence exerted by an aneurism upon the pupil, through the medium of the arterial circulation, is not considerable, or even permanently appreciable. The case is in other respects, also, worthy of being recorded.

General Symptoms.—T. G., tailor, æt. 45, a bloodless, feeble, but not greatly emaciated person, was at intervals under my care for many months during the end of last year, and during the present spring and summer. He complained chiefly of a sensation, which he termed "breathlessness," but which, on examination, proved to be more similar to the angina pectoris of Heberden. His voice was husky and high-pitched; nevertheless he had no severe laryngeal symptoms, and no dysphagia; his chest expanded readily and fully, and there was no lividity. He complained of pain about the left arm and shoulder, very much increased by stretching the left arm upwards, or by bending the neck towards the right side. In sitting he stooped very considerably; when recumbent, he was often seized with paroxysms of suffering. Angina-like paroxysms were also readily brought on by agitation, and even by any kind of examination into his case. The feet were œdematous. The following facts were ascertained on physical examination:

Physical Signs.—In the main trunks of the left arm, and in the left carotid, the pulse was entirely suppressed. In the *superficialis volae* of the left side, pulsation could sometimes be discovered. The carotid and subclavian of the right side pulsated with extraordinary strength and fulness, the pulsation being attended by a vibrating thrill and a whiffing murmur.

At the upper sternum, from the middle of the clavicle on the left side to one and a half inches beyond the sterno-clavicular articulation on the right, there was dull percussion, merging into the cardiac dulness at the third left costal cartilage. The cardiac dulness at the level of the nipple was upwards of four inches by light percussion, and extended to the right of the sternum.

At the upper sternum there was a short systolic murmur, of a somewhat rasping character, but distant and inarticulate. The second sound was suppressed, or nearly inaudible, over the whole upper sternum. At the lower sternum there was heard, with each sound of the heart, a very loud, distinct, and articulate murmur,—that of the second sound, however, predominating, both in length and loudness, over the first. The second sound itself was entirely lost in the murmur. At the apex of the left ventricle, the first sound was heard, accompanied by a very loud and distinct murmur,—the second sound being inaudible, and without murmur.

The vertebral column was considerably bent in the cervical and dorsal region, but nowhere angular unless at the sixth and seventh dorsal, where it was a little more prominent than elsewhere. No localized dulness of percussion along the spine ; but the left back, near the spine, was generally a shade more dull than the right.

At the left apex, before and behind, the respiration was much diminished ; elsewhere, natural. A blowing murmur was heard along the aorta, in the left back.

The pupils throughout the disease, were found, on repeated examination, perfectly similar in size, perfectly contractile, and apparently quite natural.

Progress of Case.—After a protracted illness, this patient finally succumbed to the ordinary symptoms of valvular heart disease. He had little or no cough or expectoration, but the dropsy increased and gained the chest, causing great orthopnœa. During the last week of life, the right side of the face was very dropsical, as also the parietes of the chest and back. The conjunctiva of the right eye was so much relaxed, as to form folds infiltrated with serum, which protruded between the eyelids, and gave the face an exceedingly distressing appearance. Ultimately he sunk exhausted, in the course of July, 1855.

Post-Mortem Examination.—The body was examined on the second day after death. The majority of the organs were free from considerable disease. The lungs were highly œdematous, and the pleuræ contained fluid on both sides. The heart was enlarged and dilated ; it may probably have weighed 20 ounces or more ; but being removed along with the aneurism and the half of the sternum, it was not weighed. The upper sternum, where dull percussion existed during life, was closely attached to a large tumour which arose from the aortic arch, and passed backwards so far as to have caused slight erosion of the bodies

of two or three dorsal vertebræ. The tumour was also closely attached to the apex of the left lung, but neither the air-passages nor the œsophagus were in the slightest degree involved in it.

On laying open the large tumour, by an incision parallel to the sternum, and from above downwards, it was seen to involve the whole upper part of the arch, which, from its commencement at the aortic valves to its termination in the descending aorta, was dilated and atheromatous. The left half of the dilated portion was filled with very firm laminated clots of pale fibrine, which passed from the sternum to the back part of the sac, completely overlaying and blocking up the openings of the left carotid and subclavian trunks. The innominate artery was perfectly free, but did not appear enlarged in proportion to the force and fulness of its pulsation during the life of the patient.

The aortic valves were incompetent. On examination, their defective action was found to proceed from a separation, to the extent of about an eighth of an inch of the two posterior segments, which were a little thickened at this part, but otherwise normal. The divergence of the segments appeared to be the result of a yielding of the arterial wall at this point ; immediately above and immediately below the segments in question, an opening led into a small aneurismal sac. The upper of these aneurisms would have held a large filbert ; it communicated with the aorta by an oval opening half an inch in diameter. The lower sac, which had a somewhat smaller orifice towards the endocardium, passed directly backwards to the left auricle, with which it communicated by an opening which admitted an ordinary catheter with ease.

The mitral valve was obviously incompetent, owing to the rigidity of that portion of its curtain nearest the aortic orifice. Here, also, a small sacculated dilatation of the endocardium was formed, bordered on the one side by the muscular substance of the heart, and on the other by the rigid portion of the mitral valve.

The rest of the heart, though enlarged, presented no valvular or other deformity.

In commenting upon this case during the life of the patient, I repeatedly called the attention of the students, who saw it with me in the Infirmary, to the state of the pupils, in connection with the position of the aneurism, and the obstruction to the circulation in the left carotid. That no appreciable *permanent* effect upon the pupil is produced

by interruption to the carotid circulation *per se*, is, I think, sufficiently proved by this single instance.

In regard to the murmurs, I remarked, that while it was by no means impossible that one or both of them might be generated in the aneurismal sac, there was a much greater probability that they were due, in this instance, to valvular disease of the heart itself. The existence of such disease seemed presumable from the great amount of hypertrophy and dilatation of which there was evidence; while the ordinary rules of physical diagnosis permitted us to refer the systolic murmur in great part to the mitral orifice. That the diastolic murmur heard at the lower sternum, was due to the aortic regurgitation, appeared probable from its great intensity and prolongation as compared with the systolic bruit, which is usually the louder of the two murmurs occasionally heard in aortic aneurism. Lastly, the faintness of the sounds over the sac, as compared with their loudness and distinctness at the lower sternum, I presumed to afford proof that some thick mass of substance, of a kind calculated to intercept the vibrations of sound, lay between the current of blood in the sac and the surface. This condition was found to be fulfilled in the fibrine, which lay in layers over the upper sternum, to the thickness of more than an inch, and which, as I presume, acted as a muffler to the sounds diffused in that direction. Of the facts, at least, as above stated, there can be no doubt.

The sources of the collateral circulation in this case would have formed an interesting subject for anatomical research; but this inquiry could not, under the circumstances, be undertaken.

The opening of the small aneurism into the left auricle had produced, apparently, no distinctly appreciable clinical result.

CASE OF A SMALL ANEURYSM OF THE FIRST PART OF THE ARCH OF THE AORTA, OPENING INTO THE PULMONARY ARTERY AND CONUS ARTERIOSUS OF THE RIGHT VENTRICLE. WITH REMARKS ON THE GENERAL SUBJECT.

Glasgow Hospital Reports, 1899, *vol. ii. p.* 1.

This admirable account of a somewhat uncommon condition is of much value. It brings into prominence many of the difficulties inseparable from the presence of a small aneurysm at the origin of the aorta, and it emphasises the contrast existing between aneurysms which involve the superior vena cava on the one hand, and those which implicate the pulmonary artery on the other. The paper contains a valuable scientific appreciation of the contributions of Wade, Walshe, Roberts, and Ord upon lesions of this nature, and in this situation. It may be added that the superficial resemblance of the physical signs in such affections to those attending persistence of the ductus arteriosus forms the subject of a clinical lecture by myself,[1] *in which the following paper was fully discussed.*

In the last volume of the *Transactions of the Glasgow Clinical and Pathological Society*, under date 10th May, 1897,[2] I brought before the Society a collection of specimens from the Glasgow Museums, including all which I found there available as "illustrating aneurysms of the arch of the aorta which either opened into, or pressed upon, the vena cava superior, or the pulmonary artery." The collection

[1] *Medical Press and Circular*, 1906, new series, vol. lxxxi. p. 572.

[2] See also *Glasgow Medical Journal*, 1897, vol. II. p. 120.

amounted to twelve cases in all (not all strictly conformable to the title), and it left upon the mind the general impression that such cases are not only rare, but that even when thus collocated they do not contribute very much, in most instances, towards the precise clinical history and diagnosis of such lesions; except indeed in one case which I myself communicated to the *Lancet* of 22nd June, 1889, p. 1233, where the opening was into the superior vena cava, with well marked cyanosis and dropsy, and with dilated veins generally limited to the upper part of the body. The diagnosis of aneurysm in this case was not very difficult, and there was even a fact in the history which may have corresponded with the period of the rupture into the vein—viz. a "feeling as if something had given way on the left side, rather to the left of the cardiac apex" (and therefore not precisely in the situation of the actual rupture); the feeling, however, was not one of local pain, but attended by faintness and cold sweat, succeeded almost immediately by the swelling and the other phenomena observed. There were in this case loud murmurs (V.S. and V.D. in rhythm) heard over the whole front, but with mainly the distribution of the aortic double murmur. No considerable doubt was in this case entertained of the existence of an aneurysm involving the vena cava, the diagnosis being afterwards verified by post-mortem examination. The clinical facts in this case were very completely, though concisely, stated in the article alluded to; but it is obvious that when an opening takes place into the pulmonary artery or right ventricle, the localization of the anasarca as above described, is not to be expected, and the diagnosis is therefore, or at least may be, much more difficult. [See paragraph appended to this article, page 787.] The following case, therefore, having been most carefully recorded with a view to clinical teaching, and having been under observation for a sufficient time to be repeatedly observed and considered with a clinical class, appears to be a fitting contribution to the Glasgow Hospital Reports. I am greatly obliged to my friend and old pupil, Dr. Alexander Macphail, for his admirable drawings of the

preparation now in the museum of the Western Infirmary, which will allow all the facts that can be brought under the eye of the reader to be vividly presented, in connection with the clinical history of the case. The report in the journal, however, is so lengthy that it will be necessary to resort to a summary which, it may be said, is *verbatim* the one that was recorded and placed before the clinical class, soon after the admission of the patient, in order to facilitate remarks at lecture, and to guide as accurately as possible the conceptions of the members of the class as to the presumptive diagnosis.

" Wm. G. L., aet. 35, engineer ; admitted 16th March, 1899. Cardiac lesion of remarkably obscure origin, possibly of about twelve months' standing on admission ; but, if so, characterized *only* by a certain amount of breathlessness and discomfort on unusual exertion, for the greater part of that time. No dropsy, until less than a week before admission. No local pain, or other definite subjective sensations of cardiac distress. No haemoptysis (this negative fact continued so to the end). Almost no orthopnoea.

" Previous history of syphilis, ten years ago or more. No rheumatism. No probable history of strain. Family history unimportant.

" Cardiac murmurs of complicated character, at first interpreted as being at least partly aortic (V.S. and V.D.), with pulse more or less characteristic of aortic regurgitation.

" Well-marked hypertrophy, probably of both, certainly of the right ventricle, with heaving impulse and thrill to the hand ; but no palpitation complained of. Dulness to percussion (of heart) $7\frac{1}{2}$ inches transverse. Epigastric pulsation.

" Question raised of aneurysm or other lesion, involving the right side of the heart as well as the aortic valves ; but no positive evidence of this obtained.

" Enlargement of liver, with induration, but no marked irregularities. Other abdominal organs not apparently altered. Urine more or less albuminous throughout."

It will be observed from the above concise statement of the actual facts observed, that there was very little in the history to guide the mind to a conclusion as to the cardiac character of the case, still less as to aneurysm, or internal rupture, at least up to the time of the occurrence of general dropsy. And this peculiar insusceptibility, on the part of the patient, to impressions of cardiac suffering, was maintained throughout the treatment, almost up to the fatal issue. Again and again it fell to be remarked that even in the presence of severe and dangerous objective symptoms—including a degree of general dropsy which required repeated puncturing of the limbs and scrotum, till it was for a time marvellously relieved under *diuretin*—the mental condition of the patient was one of apparently complacent optimism, which would hardly allow of the existence, according to his own reckoning, of any gravity in the symptoms at all. Along with this very remarkable *euphoria* there was, it is true, an equally remarkable tendency to muttering delirium, without fever or any other apparent cerebral complication, but suggestive of danger had it tended (as was regarded probable) either in the direction of coma or of exhaustion. During nearly ten weeks of observation, however, his state in this respect remained the same, noisy and apparently very restless, especially at night, but yet always able to pull himself together, as it were, when addressed, and even up to within an hour or two of his death responding to questions, as if he felt " better " or " very well," as the case might be. His pulse was always unduly rapid (98 to 116, but always regular, increasing in rapidity latterly to 138), and his respiration appeared more or less laboured, but without any marked orthopnoea, or any considerable lividity until near the end. The treatment was mainly on a diuretic plan (salines, digitalis, diuretin, spartein, caffein, etc.), and was, up to a certain point, successful, the diuretin in particular taking great effect on the quantity of the urine, and for a time on the dropsical condition. It was found impossible, however, by any combination of these remedies, to prevent recurrence of the dropsy, and ultimately drainage by

Southey's tubes and by incisions had to be employed—again for a time, with apparently good results—showing a very remarkable vital resistance to what seemed, all along, a most dangerous combination of symptoms. It is not desirable, however, here to go into more details of these fluctuations in the general state of this patient. The following extracts from the reports will bear chiefly on the objective cardiac changes, and particularly the murmurs.

Owing to the question of an aneurysm being throughout present to the mind, particular attention was given to the percussion of the manubrial and extra-cardiac area, but no definitely dull space such as would indicate a sacculated or general dilatation of the arch, could at any time be detected. The cardiac dulness, on the other hand, was manifestly extended laterally, and quite as much towards the right as towards the left. A distinct systolic thrill was felt over the right ventricle, and epigastric pulsation was distinctly present. "At the apex, a loud V.S. murmur is heard" (first report after admission), "long and blowing, and a much fainter and shorter V.D. The V.S. becomes still louder towards the sternum, and reaches maximum intensity just over the tricuspid area, where also the V.D. is very distinct. Both murmurs are audible at the aortic cartilage, the V.S. being much less intense here than at the lower division of the sternum, and scarcely at all conducted into the neck, while the V.D. attains its maximum over the second right cartilage, and is not at all heard over the cervical vessels. No capillary or venous pulse can be demonstrated."

At this time my own conviction, founding on several very careful personal observations, was that there was probably aortic valve-disease, obstructive and regurgitant, *but also, probably, something more.* The pulse was fairly characteristic of aortic regurgitation; but the peculiar distribution of the murmurs, the thrill over the tricuspid area, the epigastric pulsation and enlargement of the liver, the great amount and persistence of the dropsy, all pointed to the implication of the right ventricle to a much greater degree than is usual in aortic disease, even when of some consider-

able standing. In this way the question of aneurysm, though not a matter of evidence, was distinctly before the mind from the first, and was thus introduced as a speculative diagnosis into the summary above given ; and, amid all the changes taking place in the general condition as above indicated, nothing in the physical signs, or the disposition of the murmurs, occurred to suggest any change in the essential lesion, until, perhaps, seven or eight weeks, or more, after admission, when a marked increase in the complexity of the murmurs began to be observed, but without any corresponding, or at least any sudden, change in the symptoms. On 7th May I endeavoured to convey this, admittedly puzzling, change, as follows : " It has seemed to Professor Gairdner in some of the more recent observations, that there is a change in the quality and even in the rhythm of the murmurs ; and this at one observation suggested the possibility of some part of them (at least) being of exocardial origin (the point was, in fact, submitted to a number of good observers). On careful observation to-day, however, it does not appear probable that this is the case ; but, on the other hand, the extremely loud and all but continuous murmurs heard over the right ventricle seem to Dr. Gairdner much more striking, and also more difficult of interpretation, than what was originally observed and reported. At the right of the sternum they are not so overpoweringly loud, and are also more rhythmic, than elsewhere ; and over the aortic area V.S. and V.D. murmurs, very much of the usual character in aortic disease, can still be separately distinguished ; but, on the other hand, the loud, almost roaring, hollow murmurs heard midway between the left nipple and the middle line are such as to confuse the ear as to the rhythm, more especially as the sounds (apart from the murmurs) are entirely lost, and what must be presumed to be the V.S. and V.D. murmurs (see above) are here quite continuous with one another. Various hypotheses have been entertained since this patient's admission, to account not only for the peculiarities of the murmurs but also for the obstinacy of the dropsical symptoms, and the evident great predomi-

nance of the right side of the heart in a case where the murmurs and the pulse seemed to indicate aortic disease; the question was entertained of aneurysm, possibly communicating with the right ventricle or pulmonary artery; but, on the other hand, the absence, or slight degree, of cyanosis seemed unfavourable to that view; and nothing in the percussion at the base, or the palpation in the jugular fossa corresponds with the idea of an aneurysm of the first part of the arch. Now, however, with these extremely complex murmurs heard over the right ventricle, it seems difficult to exclude some lesion of an unusual kind involving the right side of the heart; but, so far as can be observed, the pulmonary artery is not particularly involved in it, and wherever the V.S. and V.D. can be separately distinguished, the inference would rather be that they are of aortic origin."

The above note, made sixteen days before the patient's death (which occurred on May 23rd), and used for direct bedside instruction at a time when the patient was quite in a state to bear examination by a considerable number of persons, was intended specially to convey to the whole clinical class not only the absolute facts, but also the tendency of opinion on the facts, as to the changes observed in these murmurs. The diagnosis, it is true, is not perfectly accurate, inasmuch as it implies, negatively, that there was no evidence of the pulmonary artery being specially implicated, while it points, very decidedly, to the right ventricle as the seat either of rupture, or pressure by the aneurysm which, notwithstanding the absence of clear evidence by percussion, etc., had all along been regarded as not improbably existing. The presumption, to my own mind, was that a rupture directly into the pulmonary artery (or the conus arteriosus) would not only have been attended by much more considerable cyanosis (as in one or two other cases in the series above referred to), but also that in that case the murmurs would have been likely to be more strictly localized and centred, over the first and most superficial portion of the pulmonary artery itself. Nevertheless, the facts are as stated, and the difficulties here more or less successfully encountered show

that the absolute diagnosis of varicose internal aneurysm, and of the seat of rupture (except perhaps in the superior cava), must still be regarded not only as among the rarities but also among the difficult problems of physical diagnosis. In the present instance, the solution was probably as nearly attained as was possible under the circumstances.

The post-mortem examination (well illustrated as to the essential facts by Dr. Alexander Macphail's excellent sketches) gave, in summary, the following results :

" The pericardial sac contained about 6 oz. of clear serous fluid.

" The left lung was bound down by fibrous adhesions all over. The right pleural cavity contained about 30 oz. of fluid.

" *Heart.* Aortic orifice is slightly incompetent. The commencement of the aorta shows some patches of atheroma. Behind the anterior cusp of the aortic valves a comparatively wide orifice communicates with an aneurysmal sac which, passing at first to the right, bulges into the pulmonary artery. The right anterior cusp of the pulmonic valve is stretched over the sac, and is almost obliterated. There is an aperture of communication, about $\frac{1}{8}$ inch in diameter, with the right ventricle, about half an inch below the attachment of the cusps. In the aorta, posterior to the aneurysmal opening, is another small depression, apparently a commencing aneurysm. The coronary arteries, which show some patches of atheroma, are quite uninvolved at their orifices. The mitral segments show some diffused thickening, but are otherwise normal. The pulmonic valve, with the exception noted above, is normal, as is also the tricuspid. The left ventricle measures $3\frac{3}{4}$ inch in length, and is from $\frac{3}{4}$ to $\frac{7}{8}$ inch in thickness. The right ventricle is also hypertrophied, the wall being $\frac{1}{4}$ inch in thickness. Both auricles are dilated, and contain some ante-mortem clot.

" There is venous congestion of the lungs, kidneys, liver, and spleen.

" The aorta, both thoracic and abdominal, shows numerous raised patches of atheroma, pretty equally distributed ; these are soft, but show no trace of ulceration or calcification.

" The right internal jugular vein is found thrombosed, the thrombus extending down to the junction of the subclavian vein, and partly obstructing the latter. The thrombus is of firm consistence, laminated, pale and in parts adherent to the wall." (This lesion does not appear to have been noticed during life.)

It has been already remarked that, especially in the earlier observations of the case, the radial pulses had in general the characters of those of aortic disease. The " water-hammer " character to the finger was frequently very well marked, and the sphygmographic tracings obtained (which were sometimes difficult to procure owing to the dropsy) were quite in conformity with this view, although under the influence of digitalis, etc., they varied considerably as to the extent of the primary wave and the distinctness of the dicrotic and other waves. In one tracing, obtained on 12th May (eleven days before death), the tracing was decidedly hyperdicrotic in character.

In the only case very strictly comparable with this one, contained in the above series (numbered 58 in the list of museum preparations from the Western Infirmary), the brief description given is " Aneurysm of the aorta, projecting into and perforating the pulmonary artery." In this case, which was also under my own care, the aneurysm was much larger than in the present case (" as large as the fist ") and, independently of the perforation, must have exercised a very considerable amount of direct pressure, not only upon the pulmonary artery, but upon the left auricle and auricular appendage. It was characterized, accordingly, by much more apparently grave symptoms than the present case, and particularly by " great angina pectoris and dyspnoea, with lividity, lasting more than a year ; dropsy also, which became extreme towards the close, with considerable (though not sudden) increase of the lividity and the other symptoms, and also pervigilium and orthopnoea. Death was at last sudden, but only after a very lengthened agony." In that case, however, as in the present, it was found exceedingly difficult, if not impossible, in view of the whole facts, to

determine at what precise point in the history the perforation may be supposed to have occurred. As in the present case, the murmurs on admission were V.S. and V.D. in rhythm ; and as the aorta presented signs of dilatation in its ascending portion, these were readily enough concluded to have their origin either in the valves, or in an aneurysm of that portion of the vessel. These murmurs, however, became very indistinct during the progress of the case, giving way to a V.S. murmur, which from its localization was regarded as mitral or tricuspid. " Great hypertrophy and dilatation of both ventricles, with venous pulse, became apparent in the course of observation. The liver also became enlarged, the urine scanty, of high specific gravity, and albuminous. The pulse was one of low tension, almost hyperdicrotous, with an occasional irregularity, but not complete intermission, the low tension increasing as the case proceeded." In my brief remarks appended to the case are these words : " Both the murmurs here were probably aneurysmal, inasmuch as the aortic valves were not diseased. The disappearance of the V.D. in the course of observation is not very easily explained unless as a consequence of the increasingly low arterial tension, which, again, may be referred to the communication with the pulmonary artery. There was no specific fact to indicate the time of the rupture."

In a case which has been frequently referred to, and on which some critical remarks will be found in Walshe, *Diseases of the Heart*, etc., 4th edit., p. 530, Sir Willoughby F. Wade has published (*Med. Chir. Trans.*, vol. xliv., p. 211) what seems to be one of the very few cases of aortic aneurysm in which " a communication with the pulmonary artery was recognized during life by means of physical diagnosis." In this case, as in most of those recorded, a double murmur having more or less resemblance to the usual aortic valvular murmur in rhythm existed, and Dr. Wade lays great stress on the fact that while this double rhythm was evident in the neighbourhood of the conus arteriosus and pulmonary artery, the diastolic element was not propagated to the apex, where, although a blowing murmur attended the first sound,

" the cardiac second sound was very distinct and quite natural ; no trace of murmur." Upon this, Dr. Walshe remarks that (according to Dr. Wade) " non-conduction of existent diastolic murmur downwards to the heart's apex is the key to the diagnosis of aortic communication with the pulmonary artery. But," he goes on to say, " this inference seems to me very seriously shaken by the fact that in some cases of aortic regurgitation, the attendant diastolic murmur is not transmitted to the left apex, where, on the contrary, a pure second sound is heard ; this holds true even of excessively loud basic reflux murmur. Yet more, in a case of aorto-pulmonary communication by W. Roberts, the attendant murmur (a double one) was distinctly heard at the left apex." There is no doubt, I think, that Dr. Walshe is technically right in this criticism, and that Sir Willoughby Wade has laid a stress on this particular diagnostic point which it can by no means bear. At the same time, I am not disposed to consider (as Dr. Walshe apparently does) the whole principle of the diagnosis in Dr. Wade's case, as vitiated by this error. What he actually observed was a double murmur accompanying, or rather replacing, the two cardiac sounds ; " that with the second sound being of a hissing character, and so prolonged as to continue till the commencement of the next ventricular systole " ; the special seat of these murmurs being " over the cartilage of the fourth left rib." The murmurs were widely distributed, but at this, the spot of their evidently maximum intensity, " a very considerable purring tremor accompanied the second murmur." The propagation of the murmurs along the carotids made it probable that they originated in the aorta, while their maximum intensity at the cartilage of the fourth rib (left) gave the impression that the right side of the heart (conus arteriosus or pulmonary artery) had something also to do with modifying the conduction. The peculiar quality and prolongation of the second murmur, together with its no less unusual distribution, appear to have carried the convictions of the observer by an instinct more sure than his verbal and categorical

arguments, towards the conclusion that some very unusual form of lesion, probably aneurysmal, had established a communication between the systemic and pulmonic circulations; and if so, most probably (on the ground, as stated, of numerical frequency) between the first part of the aorta and the pulmonary artery. It will be found, I think, that the tentative diagnosis indicated in the case which I have here recorded, under circumstances of greater difficulty and obscurity, was in accordance with what has just been stated as bearing on Sir Willoughby Wade's remarkable case. In this latter case, moreover, there was, in the opinion of Dr. Wade, a distinct probability arising from the history, that the first rupture of the aneurysm into the pulmonary artery (there was a second, apparently of more recent date, and attended by much more severe symptoms, into the right ventricle) had originated in a severe effort made to avoid being run over (the patient was a railway porter), which had produced considerable faintness at the time. In my present case, as will have been seen above, there was nothing in the history which, even after knowledge of all the facts, could be construed as having such significance. It is rather remarkable that in only one of the cases in the Glasgow museums is there any history of sudden accident or illness, such as might be expected, and has often been described, as the consequence of rupture of an aneurysm into some other portion of the organs of circulation. This was my case of rupture into the vena cava superior, already alluded to.

In his carefully studied, but no doubt somewhat imaginative, description of a typical case of aneurysmal communication with the pulmonary artery (816, p. 529), Dr. Walshe writes as follows: "If an individual known, or not known, to have been the subject of aortic aneurysm suddenly experience after effort a sensation of something giving way in the cardiac region, feel faint, become pale and exhibit the general characteristics of nervous shock as in the heart, followed by peculiar fluttering in the chest,—if he subsequently suffer from dyspnoea or orthopnoea, more or less cyanotic blueness

of the lips, pallor of the face, chilliness, prostration of strength, anxiety, terrible dreams, occasional nausea and vomiting, syncopal and pseudo-epileptic attacks, and become anasarcous in the lower extremities, while the lungs and liver undergo mechanical engorgement, as proved by percussion,—if all this coexist with powerful systolic thrill, limited to the second and third left intercostal spaces close to the sternum (lower than this, it is explained in a note, if the heart itself is lowered by hypertrophy), and with loud whirring murmur essentially systolic and intermittent, though sometimes inclining to continuousness (or, it may be, double murmur similarly seated), the diagnosis of sudden communication between the aorta and either the pulmonary artery or right ventricle is warranted."

No one who knows well Dr. Walshe's constant habit of verification, in even the most minute details, of all his own apparently casual statements, can do otherwise than accept this *syndrome*, or " symptomen-complex," as a good working basis for future research, in so far at least as that a case presenting the majority of these phenomena, or the more important of them, in the succession here referred to might fairly be expected to show on a post-mortem examination the lesion, or lesions, here assigned to the description. But the case which I have here placed on record shows indisputably—*first*, that there may be no sudden shock or sense of " giving way " with or without effort ; *secondly*, there may be no orthopnoea at all (this was most carefully attested in my case up to nearly the end) and certainly not more, probably much less, of dyspnoea than attends the great majority of ordinary cardiac diseases ; *thirdly*, cyanosis may be quite inconsiderable, and also a phenomenon of very late occurrence, almost a part of the agony ; *fourthly*, " syncopal and pseudo-epileptic attacks " may be altogether absent throughout. In fact, it may be quite impossible *from the symptoms alone*, even after the facts have been suspected during life and verified after death, to say at what period in the case the rupture has taken place.

As regards the physical signs, my own experience and

reading incline me to affirm that the murmurs in such cases will usually be double, the V.D. element not unfrequently predominating, as in Sir Willoughby Wade's case, and very harsh, whether or not accompanied by thrill (Dr. Walshe probably dwells too much on the "systolic" element in these cases). In the interesting and well-recorded case of the late Sir Wm. Roberts (*British Medical Journal*, 1868, vol. i. p. 421), quoted by Walshe (but not as regards this particular point), the description is as follows: "A loud harsh double murmur was heard at the mid-sternal base; or, rather, it was a single murmur consisting of two parts and covering completely both sounds. This murmur diminished rapidly in intensity towards the apex, where, however, both parts of it were distinctly audible. It was not heard beyond the apex. It was heard in moderate intensity at the ensiform cartilage. It was well conducted up the aorta and into the great vessels at the root of the neck. But the murmur was heard far more loudly at the upper margin of the cardiac dulness midway between the middle line and left nipple. At this spot it was excessively loud, harsh, and rasping; it gave the impression of being produced very near the ear. It could even be heard by the naked ear at a distance of an inch from the surface of the chest. It diminished in intensity in all directions from this centre. Over the body of the ventricle it *seemed so superficial that it deceitfully resembled a pericardial friction sound.*" I have emphasized this last statement in order that it may be compared with the notes, given above in full from my hospital journal, as to the later stages of the murmurs in the case here recorded. The question of exocardial origin, which ought always to be present to the mind in cases of very complex or anomalous murmurs, is there also raised, but only to be again dismissed, as in Sir Wm. Roberts' case.

On the whole, this description, as well as that of Sir Willoughby Wade and my own personal experience, tends to the conclusion, that while the murmurs in these cases often present a general resemblance to those of aortic valve

disease, there are usually details of differentiation which, if carefully studied, will lead up to a different diagnosis, and it may very probably be the case as (following Dr. Sansom) I have assumed in the article on aneurysm of the aorta in Clifford Allbutt's *System*, vol. vi. (p. 399), that, when the opening is into the right auricle or superior cava, the murmurs will tend towards the right of the sternum, while, when the opening is into the pulmonary artery or conus arteriosus, they will be more definitely heard towards the left. The approach to continuity of the murmurs, also discussed in that article, is well illustrated in the present case, as well as in that of Sir Wm. Roberts, although it is perhaps only in cases of communication with the great veins that we may expect, if at all, the state of matters described by Dr. Ord : " a long continuous humming murmur, never ceasing, but varying in intensity, more sonorous during systole, fainter during diastole, conducted into the neck, and heard over the whole right side of the chest posteriorly." (See article on Aneurysm above alluded to, p. 399.)

It remains to be stated, as a negative point of some importance, that murmurs of the above characteristic types may be absent, or unrecognizable, in some cases of aneurysms, either opening into the vena cava, or into the right side of the heart and pulmonary artery. In the other case of rupture into the pulmonary artery, reported in abstract in the *Transactions of the Pathological and Clinical Society*, murmurs which in the main suggested aortic valvular disease, with regurgitation, were present at the earlier observations, and afterwards completely or almost completely disappeared, leaving only a murmur, V.S. in rhythm, near the apex, which might have been, and was regarded as being probably of mitral or tricuspid origin ; and as the symptoms were those in general of advanced cardiac disease, with dilatation and hypertrophy of both ventricles, the disappearance of both, but especially of the V.D. murmur, can only be accounted for by the greatly diminished arterial tension, which was further evidenced by the markedly hyperdicrotous character of the pulse at the wrist. The murmurs, too, were regarded in

this case (after the P.M. examination) as of directly aneurysmal origin, inasmuch as the aortic valves at least were found to be intact, although it is possible, of course, that the protrusion of the aneurysm into the pulmonary artery, and the strain to which the valves of that vessel were subjected, may have had something to do with their having origin in that vessel. Cyanosis, too, was much more evident in that case than in the one to which this article is chiefly devoted.

I wish, before closing the present paper, to allude very briefly to yet another case occurring a few months later than the present, and recorded, with illustrations, in the *Glasgow Medical Journal*, vol. xlix., 1898, p. 195 (Glasgow Pathological and Clinical Society, 13th December, 1897). In this case a small aneurysm, with endocarditis, had arisen in connection with the cusps of the aortic valve (which was of course incompetent), and had ultimately caused obstruction in the pulmonic circulation by pouting into the infundibulum of the right ventricle. There was in this case no rupture into the right cavities, but (singular to state) the history was much more conformable to the idea of such rupture, and to Dr. Walshe's typical *syndrome*, than in the case on which this article is founded. There was a distinct history, not only of strain eight weeks before admission, but of a sudden development of cardiac symptoms, especially "an uncomfortable beating of the heart" *immediately* after the strain. Anginous attacks, aggravated very greatly by the slightest exertion and by alcohol, succeeded, and were maintained after this throughout the observation of the case. Tachycardia was so marked a feature of the illness (also throughout) as to excite particular attention and to determine treatment, which, however, though of the most varied kind, was almost entirely ineffectual. There was only slight oedema, and the urine was very slightly albuminous. Temperature was normal. Death was sudden, about six weeks after the apparent origin of the illness (the strain as above). There was no history of rheumatism, and none of syphilis; but it is worthy of notice that the patient's habits

were far from temperate; and as he went up for a month's training with the militia after the supposed beginning of the illness, he had abundant opportunities of observing for himself the effects not only of exertion, but of even moderate amounts of alcohol, in increasing his distress. The murmurs in this case were extremely difficult to define, but the one chiefly in evidence was regarded as probably V.D. in rhythm, and was heard best to the left of the sternum rather below the pulmonic cartilage. From the whole of the facts the following diagnosis was formed during life, and expressed to the clinical class, founding mainly on the persistent and well-marked tachycardia, and the almost indescribable difficulties in detail with regard to the interpretation of the murmurs: "The physical signs were those mainly of the valvular disease. There was, however, a well-defined suspicion of aneurysm, embodied in the clinical abstract used for lecture purposes, and stated in advance at the post-mortem examination. This suspicion arose from certain clinical peculiarities of detail: (*a*) Anomalies of murmur and sound; (*b*) suspected dulness at base (doubtful, however, and contradicted in a second report); (*c*) marked tachycardia, in excess of what is to be expected in a lesion of the aortic valves. The incompetency of the valve explained (P.M.) the double murmur, but there was this *something* in the case which vaguely suggested more."

It appears, therefore, that while the main facts of Dr. Walshe's symptomatic *syndrome* as to the history may be awanting in cases of actual rupture, the most striking of these facts may appear, in a way not unlikely to mislead in cases where no actual rupture has taken place.

[I desire to take this opportunity of noticing (although not strictly in accordance with the title of this communication) a most valuable paper in the *International Journal of the Medical Sciences* for October, 1890, by the late Dr. Pepper of Philadelphia, U.S., and Dr. J. P. Crozier Griffith, on "Varicose Aneurisms of the Aorta and Superior Vena Cava." This paper altogether escaped my notice when engaged on the article on Aneurysm for Dr. Clifford Allbutt; but from

the detailed personal record of one case, and the careful reference to and abstract of twenty-eight other cases presumed to be all that were available in medical literature up to the date in question, this painstaking investigation assumes very high rank in the literature of the subject. I infer from a reading of this memoir, as indeed is stated above : (1) That the diagnosis or rupture into the vena cava superior is usually easy as compared with that into the pulmonary artery ; (2) that the sudden onset, indicating the exact time of the rupture, is a much more frequent and prominent fact in the former than in the latter class of cases (the most distinctive symptom being the dropsy localized in the upper half of the body) ; (3) that cyanosis in a high degree, in the upper part of the body at least, and accompanying the dropsy, is much more characteristic of rupture into the V.C. than into the pulmonary artery, or even into the right cavities of the heart.]

ON THE METHODS OF PERCUSSION EMPLOYED IN EDINBURGH AND GLASGOW

With Special Reference to the importance of Minimising the Stroke in most cases in the Delimitation of Areas.

This paper is invested with a pathetic interest, seeing that it is the last which Gairdner wrote. Containing the ripe opinions, founded upon many years' experience as a physician, in respect of the important subject with which it deals, it is instinct with the inborn wisdom and abundant information which were the distinguishing characteristics of its author during his later years. Every one of us who admired and followed his teaching and methods could not but be struck by the delicacy of his touch and the accuracy of his results. There is a fear that, with all the modern aids in diagnosis, a tendency to devote less attention than formerly to this careful education of the senses may take place. While in no way yielding to anyone in estimation of all our recent advances as regards instrumental examination, it must, in my opinion, be our duty to see that those for whose education we are responsible are not allowed to run the risk of overlooking the supreme importance of thorough training in the use of hands and ears. From this point of view, the following paper is of very great value. Our recent instruments of precision should only be employed to confirm, correct, and extend the observations already made by the unaided senses. It is clear that we ought to make an attempt to estimate the arterial pressure by means of the finger, before employing the sphygmomanometer, and, in the same way, we should thoroughly explore the chest and abdomen by palpation, percussion, and auscultation, before we

obtain skiagrams and orthodiagrams. Comparison of the results furnished by modern methods with those reached by the unaided senses, serves to bring out the wonderful accuracy which may be attained by means of the older methods. The introduction of the hammer in the art of percussion by Wintrich[1] *was an unmitigated evil, especially in two directions—it prevented the attainment of proper skill by training the hands in percussion, and it led those who used it to employ too much energy in eliciting the sound; the remarks made by Gairdner upon this subject are therefore amply merited. Another question with which he deals is the unfortunate theory proposed and expounded by Piorry.*[2] *In every respect Piorry, with his mistaken belief that there was a percussion sound characteristic of every particular organ, stands in direct opposition to Skoda,*[3] *who taught that, in the examination of the body, it was the physical state, and not the structural condition, which alone could be ascertained by palpation, by percussion, and by auscultation. In this way, Skoda was undoubtedly the pioneer in the modern methods of clinical investigation.*

In the course of having to superintend the studies in the medical art of two sons in succession over a considerable term of years in Glasgow, I have had my attention directed to certain differences in the methods of Edinburgh and Glasgow tuition, of which one, at least, has seemed to be of so much importance, that I have now determined to devote this article to it, in the hope that both in Edinburgh and elsewhere matters involving fundamental principles may be more fully considered, and if possible acted on more in conformity with these physical principles, and therefore with more general uniformity and consent than hitherto.

The matter alluded to is the method to be employed in percussion, with special reference to the force of the stroke, and the conclusions to be drawn from the differentiation of results in various areas in which delimitation is of

[1] *Krankheiten der Respirations-organe*, Erlangen, 1854-67.

[2] *De la Percussion Médiate*, Paris, 1828.

Du Procédé opératoire à suivre dans l'Exploration des Organes par la Percussion Médiate, Paris, 1831.

[3] *Abhandlung über Perkussion und Auscultation*, Wien, 1844.

great consequence. It is no longer doubtful to me, judging from what has been told me, in particular by the younger of my medical sons, that in this respect the manner of delivering the stroke in Edinburgh and Glasgow differs to an appreciable degree, and the difference is probably, if not certainly, due to the general instructions which, in thirty-eight years of a professorship in Glasgow, I have had occasion to impress upon a corresponding number of classes of students, both in systematic and ward teaching. But as it has only occurred to me recently to attach this kind of importance to my own instruction, and as I should be very unwilling to over-estimate the difference in question, I am glad to be able to avail myself of words committed to the press so early as 1885, and primarily intended, not for any controversial purpose, but simply for the full clinical exposition of what seemed to emerge from the consideration of a particular case ; and I am far from asserting or claiming that the views expressed are in any respects absolutely novel or peculiar to my own teaching. It will remain for each reader of the following remarks to arrive at conclusions for himself ; but it is not too much to suppose that matters which have occupied my own attention during a long career as a teacher, and have been deliberately formulated in the oral teaching of a considerable school during so many years, may have been so dealt with under the influence of personal suggestion as to demand either some definite criticism or some rectification of the methods referred to on the part of others ; and as my own influence is now permanently withdrawn from direct teaching, it is only through an article like this that an erroneous bias existing, if any, can be further controlled.

In the *Medical Times and Gazette* of 19th December, 1885, very closely corresponding with the decease of that once eminent and excellent journal, I find that I made the following remarks in commenting upon a case in which the percussion of the abdomen had to be regulated so as to discover a certain amount of thickening of the great omentum. The condition in question was demonstrated

on many occasions in this case, and was partially illustrated by a diagram, which, however, it is not considered necessary to reproduce here. The conclusion reached was (omitting details for the present), that "there was thickening of peritoneum generally, and especially of the great omentum, by fibrinous deposits, including perhaps some fluid, but not such an amount as to give rise to physical signs as such."

On this occasion I so far diverged into discussion of the general subject as to furnish forth a report which will serve me well in reference to the present article, and from which, therefore, I take the following somewhat extended extract :—

"Upon this subject of the *minimised percussion stroke*, I should like to take this opportunity of making a few remarks incidentally, because it is one, in my opinion, erroneously, or at least, ambiguously, stated in some of your text-books ; and practical errors and confusions arising from this source are not uncommon in my experience.

"Most persons, and almost all beginners, in employing percussion for the delimitation of organs, *err by percussing too hard*. The desire is, naturally enough, to get a definite and recognisable *quantity* of sound ; and, by percussing hard, more sound is got, of course, than when, as in the observations just referred to (case of Mary Jane S.), a carefully minimised stroke is employed. But in increasing the quantity of sound so as to make it apparently or really more easily audible, you are, in most cases, *exactly in a corresponding degree reducing the value of your results*. In other words, the carefully minimised stroke gives you approximately exact definitions ; while the stronger stroke necessarily gives you less exact, or wholly inexact, limits of dull and clear areas under like circumstances ; the degree of inexactness or vagueness of the results increasing immensely with every degree of added force employed, so that, according to the quite ordinary mode of percussion used by many persons, and by almost all young or inexperienced persons, nothing like exact results can ever be obtained at all. Both in the case of Mary Jane S., for ex-

ample, and in that of Jane M., all the best results of our percussion would have wholly escaped us had we employed such a force of percussion-stroke as I observe many of you employ through an instinct and habit that it seems to take much teaching to get over.

"Piorry, the great master of *mediate* percussion (as he first called it), and unquestionably a very great man in this art, appears in his numerous writings to proceed throughout on the assumption that percussion operates directly downwards, or in the direction only of the impact, in the way of educing the characteristic sound; and accordingly he speaks always of *superficial* and of *deep* percussion (*percussion profonde*) as if they were facts of the same order and of equal exactness in the delimitation of organs. No doubt Piorry had ways of his own of attaining valuable results, both from superficial and from deep percussion, and of thus obviating practically some of the faults of his method, or, at all events, of his doctrine. But he left a legacy of fatal confusion to his successors, by teaching that for every organ or structure in the body there was a process of delimitation of its deeper relations, which he implicitly regarded as equally available for exact definition with the superficial; the means for deep percussion being simply a stronger percussion stroke, which, as I have already said, can never under any circumstances give an exact, or even approximately exact, result in delimitation at all. For it is a physical law which no amount of experience can evade or set at nought, that percussion does *not* operate directly downwards or in the direction of the impact *only*; but, in proportion to the strength of the stroke, laterally, diagonally, and in every possible direction in educing sound. And therefore, as I said before, deep percussion (so-called) is *necessarily* inexact percussion; and what you gain in volume of sound, by strengthening your stroke, you lose, and far more than lose, in definition,[1] according to a physical law which no

[1] The introduction of Wintrich's hammer in percussion was, from this point of view, a great misfortune; and after using it for some years, more or less, from my earliest initiation into physical diagnosis, I abandoned it

amount of skill can overcome. To trace this principle out into a refutation of all the nonsense that has been written in books about the deep and superficial, absolute and relative, limits of certain organs according to percussion, would occupy too much time. I will therefore pass to one consideration only, which is very directly related to the subject of this clinical lesson.

" What I mean by a *carefully minimised* percussion-stroke is this : You are to percuss so as to elicit the distinctions of sound depending on air-filled or not air-filled viscera when superficially placed in reference to the abdominal wall ; but you are not, as a rule, to strengthen your stroke beyond what is absolutely necessary for this purpose, at least when you are in search of objects such as the thin edge of the liver, which closely underlies the surface, or rather the wall of the abdomen.[1]

" When your object is (as it may very well be) to determine not *limits*, but the qualities of large masses of sound as elicited by strong percussion, remember always that what is gained in volume is more than lost in exactness of delimitation ; and that by no sort of management is it physically possible to determine accurately an edge, or limit, or any kind of definite form of an object, even half an inch below the wall of the thoracic or abdominal cavity under ordinary circumstances. Hence you can determine with tolerable accuracy the fact and the extent of a thickened omentum ; but you cannot determine with like accuracy, by percussion alone, the enlargement of a mass of mesenteric glands ; unless, indeed, they are thrusting aside the intestines, and (what is quite unusual in fact) thus becoming superficial.

entirely on this account, except in eliciting the cracked-pot sound ; because I found that the result of using it habitually was to cultivate a habit of too strong, and therefore inexact, percussion. Yet I found the hammer, in the hands of Traube and others, almost universal in Berlin in 1872.

[1] I have again and again been obliged to demonstrate to young and even to some skilful operators, that the lower edge of the liver has been shown by them as half an inch, or even an inch, too high, in consequence of the percussion stroke not being carefully minimised.

By using palpation along with percussion you can sometimes, but not always, distinguish the two conditions. About this diagnosis I have something to say further on.

"Meanwhile, observe the precautions which, in this case and in the other, we actually employed, and have reason to urge upon you as necessary in exploring the abdomen by percussion in search of thickening of the great omentum or any similar lesion. The first point is to determine the existence, or the absence, of gravitating fluid in such quantity as to affect separately the percussion. In this case our diagnosis on this point by the quite ordinary test, was in the negative. Then we had to consider that under normal circumstances the intestinal canal, in one or other of its divisions, occupies the whole anterior wall of the abdomen, giving rise to a well-defined, though not identical, tympanitic note, which is reached by a nearly equal force of percussion-stroke all over the anterior surface. If, therefore, we can find even a few points at which a very gentle tap elicits this normal quality of sound, we know that under like physical conditions the same gentle percussion-stroke and no more, ought to elicit a more or less similar tympanitic note all over; and if it is otherwise, we ought to study the facts carefully in detail, and to know, if possible, the reason why. These points or areas of quasi-normal percussion afford us the necessary data, then, for what I call *minimising the stroke.* Now, if we find that over a particular area corresponding anatomically with the area of the great omentum, or of some portion of it, there exists a dull percussion which contrasts notably with the quasi-normal areas just referred to, being absolutely without sound to a *minimised* stroke, while to a very slightly stronger stroke it gives a tympanitic percussion quite like the quasi-normal areas, to what conclusion can we possibly come but that within this area of abnormal percussion there is something interposed between the abdominal wall and the air-filled intestines; something which is thick enough to differentiate the percussion, but not so thick as to give dull percussion to a stronger stroke; which is thus, as I ex-

pressed it in the report, 'easily penetrated' by percussion? And if this 'something' is in the anatomical position of the omentum, why should it not actually be the omentum, involved in a possibly more general peritoneal morbid process, but presenting (owing to its double fold) the changes due to this in a more marked form? And although this diagnosis may be confirmed and strengthened indefinitely by palpation, showing a diminution of elastic resistance over a like area as compared with other parts of the abdomen, it may, and often does, emerge with a fair amount of accuracy and security from the percussion alone, employed in the manner now described."

The only circumstance which makes the remarks in this particular case of great value for my present purpose, is that it was one of many in which absolute accuracy of result depended in a very peculiar sense on the method employed; but it will be observed that the principles set forth as regards the importance of "minimising the stroke" have an application beyond the abdomen or the particular investigation referred to. It is these principles I am most anxious to maintain in this article, and therefore I shall endeavour in what remains of it to show that the differences above referred to between the Edinburgh and Glasgow schools in reference to percussion may possibly and indeed probably, involve questions extending to the fundamental basis of acoustic principle, on which insufficient stress has hitherto been laid. My object, in other words, is to show that wherever percussion is employed with a view to delimitation, a carefully minimised stroke (in the sense above defined) is essential to correct practice, and that it is vain to expect really accurate results unless this condition is systematically attended to. I am very far from persuading myself that the manipulations of experienced hands have not, in fact, led to fairly good results in a large proportion of cases; but for teaching purposes it seems to be most important that beginners at least should be started in the right line, and not involved in the intricacies of a terminology which is itself

demonstrably out of accord with the laws of acoustics. When, for example, the expression "deep percussion" is employed, as it is by Piorry and others, and when it is thus made to appear that the deep limits of an organ can be indicated by percussion in similarly exact terms with the superficial areas, a positively wrong direction is given to procedure, and the results cannot fail to indicate in some cases the divergence from physical laws tacitly assumed in the process. There is indeed an apparent sacrifice in respect of the amount of sound produced, but the sacrifice is a necessary one, and is amply compensated by the greater value resulting from better methods. I would instance in particular the percussion of the heart and of the lower edge of the liver as being among those which in my experience have been most perverted or rendered indeterminate by the employment of wrong methods. I trust, therefore, that this article will be accepted in the spirit in which it is intended, not as a claim of priority or undue dogmatism, but as a simple attempt to set forth principles which have been apt to be lost sight of in daily use.

The discontinuance on my part of the habitual use of "Wintrich's hammer," as referred to in the footnote to the above, is a very marked and almost unconscious result of the principles referred to, the use of the hammer having been adopted by me at the instance of the late Professor Hughes Bennett not less than fifty years ago, on its being first imported into this country, and (notwithstanding the strong temptations to its use in class teaching) discontinued, as appears in the note, in everyday use, although always retained for certain cases in demonstration, at some time prior to the year 1872, after a prolonged and varied experience.

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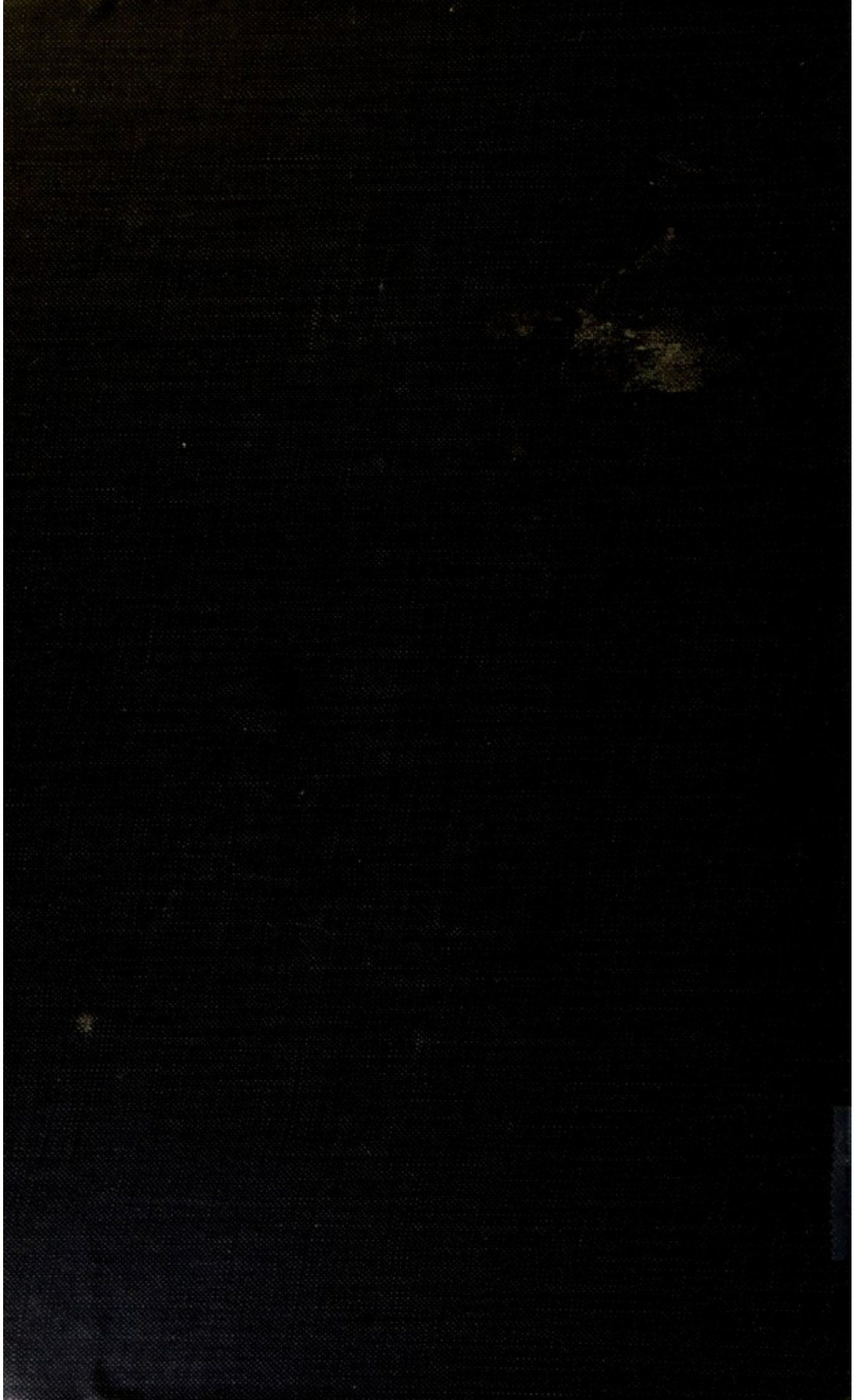
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