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THE

THE SYDENHAM SOCIETY

INSTITUTED

MDCCCXLIII

LONDON

MDCCCXLVI.

William Hewson, F.R.S.

THE WORKS

OF

WILLIAM HEWSON, F.R.S.

EDITED

WITH AN INTRODUCTION AND NOTES

BY

GEORGE GULLIVER, F.R.S.
SURGEON IN THE ROYAL REGIMENT OF HORSE GUARDS.

LONDON
PRINTED FOR THE SYDENHAM SOCIETY
MDCCCXLVI.

C. AND J. ADLARD, PRINTERS,
BARTHOLOMEW CLOSE.

EDITOR'S PREFACE.

Although Hewson's merit, as a physiologist, is generally admitted, his writings have been unjustly neglected, and are now so scarce that a complete copy of them is not to be found in the stock of any London bookseller, nor even in some of our best libraries, as that of the British Museum.

Hence a new edition of Mr. Hewson's Works will be both an acceptable present to physiological literature and a just tribute of respect to his memory.

His Plates, which have all been carefully engraved on this occasion, will always be interesting. It is true that some of them might now be displaced by others containing more exact information in the same space; but then historical truth would suffer, and the conscientious inquirer would still be compelled to consult the old copies.

In the Introduction, an account is given of Hewson's life and writings, with a careful survey of the facts ascertained and opinions held concerning the blood by his predecessors and contemporaries, followed by an estimate of the value of his Inquiries. This course appeared necessary in justice to the old physiologists, and well calculated to afford an interesting and instructive view of the progress of research in a most important subject, about the history of which so much error prevails.

An attempt has been made to place the matters to which the Notes relate on a level with the present state of knowledge; while in them also the historical method has generally been kept in view, because it is useful and pleasing, in a work of this nature, to mark the footsteps of science and the names of its cultivators.

For most generous and valuable aid in the course of his labours, the Editor has now the grateful duty of returning his best thanks to his friend Dr. Davy, whose elaborate Researches have so much advanced our knowledge of the blood.

The Editor's Notes are placed beneath lines and referred to the Text by numbers within parentheses.

CONTENTS.

PART II.

LYMPHATIC SYSTEM.

INTRODUCTION,

ON THE

LIFE AND WRITINGS OF HEWSON,

WITH A HISTORY OF

THE COAGULATION OF THE BLOOD.

INTRODUCTION.

For the little that is known of the private life of William Hewson, we are indebted to the affectionate care of his widow, who left a manuscript memoir of him, which was sent by her son, Dr. Thomas Tickell Hewson, with a letter from Philadelphia, dated October 28, 1804, to Dr. Lettsom, and afterwards published in the life of this amiable physician by Mr. Pettigrew.[1] There is also a short account of Mr. Hewson by Dr. Lettsom,[2] compiled from Mrs. Hewson's papers. The following letter was written by her to Dr. Samuel Foart Simmons, who published it in his Life of Dr. Hunter.[3] The letter was also partly inserted in Dr. Hahn's Preface to the Latin edition of Hewson's Works, by Dr. J. van de Wynpresse.[4]

Sir,—I should think myself bound to grant any request introduced with Mr. Watson's name; but that which you make in the letter I received yesterday needed no such introduction. A tribute paid to the memory of Mr. Hewson is highly gratifying to me, and I can have no employment that will give me more satisfaction than that of assisting in any degree to the spreading of his fame.

You say you are not unacquainted with the general history of Mr. Hewson's life, and you speak of him in terms which show that you are not unacquainted with his character. Had you been among the number of his friends, you would bear

[1] Memoirs of the Life and Writings of the late John Coakley Lettsom, M.D., 3 vols. 8vo, London, 1817.

[2] Transactions of the Medical Society of London, vol. i, art. 2, 8vo, Lond. 1810.

[3] An Account of the Life and Writings of the late William Hunter, M.D., p. 39, 8vo, London, 1783.

[4] Guil. Hewsoni Opera Omnia, 8vo, Ludg. Batav. 1795.

testimony to his private virtues, which rendered him no less dear to his family and associates, than his talents made him respectable in the world.

Mr. Hewson was born at Hexham, in Northumberland, on the 14th Nov. O.S., 1739. He received the rudiments of his education at the grammar-school in that town under the Rev. Mr. Brown. His father was a surgeon and apothecary in the place, and much respected in that neighbourhood. With him Mr. Hewson acquired his first medical knowledge. Being ambitious to increase that knowledge, he placed himself first under an eminent surgeon in Newcastle, Mr. Lambert, and afterwards resided some time at London, Edinburgh, and Paris. His subsequent acquirements are sufficient to prove that he visited those places with a true love of science and desire of attaining eminence in his profession.

I became acquainted with him in the year 1768. He was at that time in partnership with Dr. Hunter. Some similarity in our dispositions created a mutual esteem, and the equality of our situations made our union desirable in point of prudence. I had five months the start of him in age, no pretensions to beauty, nor any splendid fortune; yet I believe he was satisfied with the choice he made. We were married July 10, 1770. I brought him two sons. The elder was just three years old when Mr. Hewson died, which was on the 1st of May, 1774, and I was delivered of a daughter on the 9th of August following. His last moments of recollection were embittered by the idea of leaving me with three children scantily provided for. The trial of my fortitude was different; the loss of affluence I did not feel for myself, and I thought I could bring up my children not to want it. However, by the death of an aunt, who left me her fortune, I became reinstated in easy circumstances, and am enabled to give a liberal education to my children, who I hope will prove worthy of the stock from which they grew, and do honour to the name of Hewson.

Mr. Hewson's mother is still living at Hexham, and has one daughter, the youngest and only remaining child of eleven.

His father died in 1767; and having had so large a family, it will be readily supposed he could not give much to his son, so that Mr. Hewson's advancement in life was owing to his own industry.

A better son and husband, or a fonder father than Mr. Hewson never existed. He was honoured with the friendship of many respectable persons now living, and the late Sir John Pringle showed him singular marks of regard.

Mr. Hewson's manners were gentle and engaging; his ambition was free from ostentation, his prudence was without meanness, and he was more covetous of fame than of fortune.

You will, I trust, readily forgive me, if I have been more prolix than you desired. It would be no easy matter to relate bare facts without some comment on such a subject.

I am, Sir, your most obedient humble servant,

MARY HEWSON.

Kensington, Aug. 30, 1782.

Mr. Hewson's mother, whose maiden name was Heron, was a native of Hexham, and allied to several respectable families in that town. She had eleven children, of whom he and three daughters only remained in 1767, when his father died.

In the autumn of 1759 Mr. Hewson came to London, lodged with Mr. John Hunter, and attended Dr. William Hunter's Anatomical Lectures at a house in Covent Garden. Hewson's diligence and skill soon recommended him to the favorable notice of the brothers; and when Mr. Hunter went abroad with the army, early in 1761, he left to Mr. Hewson the charge of instructing the other pupils in the dissecting-room, by which means, as Mrs. Hewson remarks, he gained money at an age when most students in surgery are only spending it.

He entered himself a pupil at Guy's and St. Thomas's Hospitals; and attended Dr. Colin Mackenzie on midwifery, and Dr. Hugh Smith on physic. In gratitude to the liberal confidence of his father, Hewson was justly careful of his money, and this prudence continued through life, though not so as to check the growth of generosity; for no man ever exercised his profession with less avidity of gain; he disdained every species of meanness, and possessed a judicious liberality which elevated his character. His father had the happiness of living to reap the fruit of this care.

Mr. Hewson took with him recommendatory letters from Dr. Hunter and Sir John Pringle to the professors at Edinburgh, and studied there until the winter of 1762, when he returned to London, entered into partnership with Dr. Hunter, gave some of the lectures, and had a share of the profits. Their anatomical school was in Litchfield street, where Mr. Hewson lived, and took some of the pupils to board with him.

In the summer of 1765 Mr. Hewson went to France, and returning through Flanders and Holland, arrived in London in time for the anatomical lectures.

He went to the sea-coast of Sussex in the summer of 1768 to make experiments upon fish. His papers on the lymphatic system of oviparous vertebrate animals were laid before the Royal Society during the following winter, when he was made a Fellow of that body. The date of his election was March 8, 1770.[1] To his recommendatory certificate, read December 7, 1769, and preserved in the archives of the Royal Society, eleven signatures were affixed, among which may be noticed, John Pringle, William Hunter, B. Franklin, M. Maty, J. Turton, and James Ferguson.

In 1769 Dr. Hunter finished his building in Windmill street, where Hewson had a small apartment allotted to him. They continued the lectures in partnership, dividing the profits equally, Hewson giving more of the lectures than he had formerly done. Though he did not seek much practice in surgery and midwifery, his manners were so engaging and he showed so much skill, that the profits of these branches of his business were not inconsiderable.

The Windmill-street School no longer exists; but it will be preserved from oblivion by the names of the eminent men who lectured there. Among these the future historian of anatomy and physiology in England will have to commemorate William Hunter, Hewson, Cruikshank, Sheldon, Baillie, Brodie, Charles Bell.

Hewson, soon after his marriage in 1770, took a house near Dr. Hunter, and they continued the lectures together during the winter of 1771. The partnership was soon afterwards dissolved; and Mr. Hewson began to lecture on his own account, September 30, 1772, in Craven street, where he had built a theatre adjoining a house which he intended for the future residence of his family. Before he began this course of anatomy, he

[1] Journal Book of the Royal Society, fol. MS. vol. xxvii, p. 321.

gave a lecture, to which he invited many men of science, on the uses of the spleen and thymus. He commenced his lectures with great applause, and before the end of the course succeeded so well, that he had more than half the number of pupils that he and Dr. Hunter had when in partnership. Mr. Hewson's second course of lectures, as well as those which he delivered during the winter of the following year, were quite as successful.

From the manner in which Dr. Hunter[1] and Mr. Hewson[2] wrote of each other, it is pleasing to suppose that they did not finally retain any acrimonious feeling from the disagreement which had led to their separation. This subject may be dismissed with the following letter by Dr. Franklin, for which I am indebted to the kindness of Mr. Hunter Baillie, who gave me the copy from the original in his rich collection of autograph letters, many of which, as is probably the case with the present one, have not been published.

To Dr. William Hunter.

Craven Street, Oct. 30, 1772.

Sir,—I should sooner have answered your questions, but, that in the confusion of my papers, occasioned by removing to another house, I could not readily find the memorandums I had made during the unpleasant time in which as a common friend I was obliged to hear your and Mr. Hewson's mutual complaints. I have now found one of those memorandums, dated Aug. 23, 1771, the same which I afterwards read to you, containing your idea as then expressed of the terms on which you were to proceed together. The following is an extract from it: "Dr. Hunter expects Mr. Hewson should go on with his business as usual, during the remainder of the term they are to continue together; and during that time should make preparations at Dr. Hunter's expense, such as the Dr. should direct to be made, and others, so that those directed are not neglected or omitted; that as the preparations are to be the Doctor's sole property, and at his absolute disposal, so that

[1] Introductory Lectures to his last Course of Anatomy, p. 60, 4to, Lond. 1784.

[2] On the Lymphatic System, p. 166 of this volume.

if he afterwards should give any of them to any person (which he could not be understood to promise) such gift is to be considered as the sole effect of his good-will." Some time last summer, after your parting, you wrote me a note, which I cannot at present find, but I think the purport of it was that you had some preparations which you could spare, and were disposed to give me, desiring I would call and look at them; I did so and accepted them; I apprehended it to be your supposition in giving them to me, that as I had no use for them, I should probably give them to Mr. Hewson, which I immediately did. Having said this, I must add, in justice likewise to Mr. Hewson, that his conception of the original agreement between you was that he had a right to make such preparations for himself, your business and the common interest of the partnership not being neglected. Here you had differed in opinion; but came to a kind of compromise expressed in a paper that I handed between you, a copy of which I have obtained from Mr. Hewson to send you.

I am, Sir, your most obedient and most humble servant,

B. Franklin.

Early in 1774 Hewson had every reason to be gratified with his position. "In viewing the situation of our associate at this period," says Dr. Lettsom, "the most gratifying prospects presented themselves, where genius and industry were rewarded with success, and domestic amities with felicity. The theatre in which he delivered his lectures and expounded his doctrines was crowded with men of science, as well as with pupils, to listen to a youth grown sage by experimental researches." In short, Hewson was now surrounded by the blessings of life. He had a kind and just wife who had borne him two lovely sons; his favorite sister lived with him; his success in teaching was no longer doubtful; and his practice in surgery and midwifery had so much increased as to give him the fairest prospect of providing well for his family.

But this happiness was soon to end. He was seized with a fever occasioned by a wound received in dissection, which proved fatal on May-day, 1774, after a short illness, in the thirty-fifth year of his age.

In the second volume of the 'Medical and Philosophical

Commentaries' the following notice was inserted: "On Sunday, May 1st, died at London, Mr. William Hewson, Fellow of the Royal Society, and Teacher of Anatomy. From those ingenious publications with which he has already favored the public, his untimely death will be regretted by every sincere lover of science." Dr. Franklin[1] wrote from Craven street on the 5th of the same month, "Our family here is in great distress. Poor Mrs. Hewson has lost her husband, and Mrs. Stevenson her son-in-law. . . . . . . He was an excellent young man, ingenious, industrious, useful, and beloved by all that knew him. He was just established in a profitable, growing business, with the best prospects of bringing up his young family advantageously. They were a happy couple. All their schemes of life are now overthrown."

He was buried at St. Martin's-in-the-Fields, as appears from the following notice in the Register, kept in the church, of burials in that parish: "May 6, 1774, William Hewson, a man." I know not that any monument was afterwards erected to his memory, nor indeed that he ever had an epitaph. I could not find his grave.

Mrs. Hewson describes him as slender in form, above the middle stature; with a good air and a pleasing countenance, expressive of the gentleness and sagacity of his mind. Her face was thought to resemble his.

I have not been able to learn that there is any painting of him. There is an indistinct representation of his bust in the background of the pleasing picture by S. Medley, in the library at Bolt court, of the early members of the Medical Society of London, established in 1773, the year before Hewson's death. But his countenance is so little known, that the engraving of him given in this volume will be an acceptable addition to the portraits of British physiologists. It is from a mezzotinto in the possession of Mr. John Quekett, probably the same as that spoken of by Dr. Franklin, as follows, in a letter which he wrote to Mrs. Hewson from Passy, in June, 1782:—"I forget whether I ever acknowledged the receipt of the prints of Mr. Hewson. I have one of them framed in my study. I think it very like."

[1] Franklin's Works, edited by Jared Sparks, vol. viii, p. 121, 8vo, Boston, 1840.

Of a woman so exemplary as Mary Stevenson, so well worthy to be the wife of Hewson, it is right to preserve some memorial. This, indeed, is to be found in the affecting and excellent tribute which she has left to his memory. But we have still better testimony of her merits. Her second son, Dr. Thomas Tickell Hewson, in his letter to Dr. Lettsom, dated October 28, 1804, has described the gratitude of her children for her tenderness as a mother. She had been upon terms of the warmest friendship with Dr. Franklin since she was eighteen years of age. That eminent philosopher resided with her mother, Mrs. Margaret Stevenson, at No. 7, Craven street, Strand, during the fifteen years of his abode in London. Miss Stevenson lived mostly in the country with her aunt, Mrs. Tickell. When Miss Stevenson thought of marrying Hewson she consulted Franklin, and how highly he esteemed her may be seen by the letter, dated May 31, 1770, which he wrote to her on that occasion. She was also acquainted with Dr. Hawkesworth, whose grave I have seen in the churchyard at Bromley. Writing to her from Craven street, June 7, 1762, Franklin says, "I am glad to hear that you are about to enjoy the happiness of seeing and being with your friends at Bromley. My best respects to the good Doctor and Mrs. Hawkesworth;" and again, January 9, 1765, "Your good mamma and myself are both of opinion that the Christmas gambols at Bromley last a great deal too long." On the 27th of January, 1783, he writes to her from Passy: "In looking forward, twenty-five years seem a long period, but in looking back, how short! Could you imagine, that it is now full a quarter of a century since we were first acquainted? It was in 1757. During the greatest part of the time I lived in the house with my dear deceased friend, your mother; of course you and I conversed with each other much and often. It is to all our honours, that in all that time we never had among us the smallest misunderstanding. Our friendship has been all clear sunshine, without the least cloud in its hemisphere. Let me conclude by saying to you, what I have had too frequent occasions to say to my other remaining old friends. The fewer we become, the more let us love one another."

She passed the winter of 1783 to 1784 with him at Passy, where her intelligence and amiableness were particularly noticed. Franklin had not only taken great care in the directing of her studies, but he addressed some of his best letters on philosophical subjects to her. After she had lost her mother, in 1783, he frequently expressed a wish that she would become his neighbour in America. Accordingly, in 1786, she went with her children to Philadelphia, where she lived until 1792, and then removed to Bristol, in Pennsylvania, where her eldest son, William, had established himself, and where she closed a well-spent life on the 14th of October, 1795. Her second son, Thomas Tickell Hewson, who was an eminent physician, and her daughter, Mrs. Caldwell, were both living at Philadelphia in 1837. Her son William died at Vera Cruz in 1802.[1]

In the hope that some further information might be obtained from America concerning Mr. Hewson or his descendants, the printing of this sheet has been long delayed; but my inquiries have only elicited that his son, Dr. Hewson, is at present the respected President of the College of Physicians at Philadelphia.

We are now to consider Mr. Hewson's writings. In 1767 his paper on the Operation of perforating the Pleura in cases of Air in that Sac, was communicated by Dr. Hunter and published in the third volume of the 'Medical Observations and Inquiries.'

The second Dr. Monro had before, in his lectures, proposed the same operation, of which, when Hewson was informed, he made a suitable acknowledgment to Dr. Monro, and published it in the Appendix to the first and second editions of the 'Inquiry into the Properties of the Blood.'

Hewson's three papers on the Lymphatic System in Birds, Amphibia, and Fishes, appeared in the 'Philosophical Transactions' for 1768 and 1769, and form the substance of the fourth, fifth, and sixth chapters of the Second Part of his 'Experimental Inquiries,' published but a short time before his death in 1774, and dedicated to Dr. Franklin, who had shown Hewson great regard since his marriage to Miss Stevenson. The first paper was read before the Royal Society on the 8th of December, 1768; and the discovery of the vessels which correspond in the lower vertebrate animals to the lacteals of mammalia was claimed by

[1] Franklin's Letters and Miscellaneous Papers, with Notes, by Jared Sparks, 12mo, London, 1833; and the Works of Benjamin Franklin, with Notes and Life of the Author, by Jared Sparks, 10 vols. 8vo, Boston, 1840.

Dr. Monro, in a letter read before the same Society on the 19th of January, 1769, and in a separate publication.[1] A bitter controversy followed. Hewson's answer to Dr. Monro was given in the Appendix to the first and following editions of his 'Inquiry into the Properties of the Blood,' and Dr. Monro's rejoinder in the seventh chapter of his large work on the 'Structure and Physiology of Fishes,' eleven years after Hewson's death.

After the supposed discovery by Dr. Hunter or Dr. Monro, that absorption in the animal body is exclusively performed by the lymphatic vessels, the question of the existence of these vessels in birds, reptiles, and fishes appeared of the highest importance; and the supporters of this doctrine of absorption felt the necessity of showing that these animals possess a lymphatic and lacteal system, which was at that time disbelieved by many anatomists; and even by Dr. Monro, as late as 1758.[2]

But the question has now lost much of its interest, because the lymphatic vessels are no longer held to be the exclusive agents of absorption;[3] and the main point contended for by Dr. Monro and Mr. Hewson, to wit, the honour of the first discovery of the lacteal vessels in oviparous vertebrate animals, does not strictly belong to either of the disputants; for the lacteals of a fish were observed above a century before by Thomas Bartholin,[4] though his description was alloyed with the old error, that they terminate in the liver. It was well remarked by Mr. Abernethy,[5] that all our knowledge of the absorbing vessels has been obtained by fragments, and that our future acquisitions must be made in the same manner.

But whatever may be thought now of the question of discovery, it must be allowed that the lymphatic system of the

[1] 'A State of Facts concerning the first Proposal of performing the Paracentesis of the Thorax, on account of Air effused from the Lungs into the Cavities of the Pleuræ; and concerning the Discovery of the Lymphatic Valvular Absorbent System of Vessels in Oviparous Animals, in Answer to Mr. Hewson; by Alexander Monro, Physician and Professor of Physic and of Anatomy in the University of Edinburgh.' 8vo, Edinb. 1770.

[2] Observations, Anatomical and Physiological, p. 57, 8vo, Edinb. 1758.

[3] See Note LXXXII, page 179.

[4] De Lacteis Thoracicis in Homine Brutisque nuperrimè Observatis, Historia Anatomica, p. 70, 12mo, Lond. 1652.

[5] Philosophical Transactions, 1796, p. 33.

lower vertebrate animals was more completely exhibited by Hewson than by any of his predecessors or cotemporaries.

When Dr. Monro's letter, printed at page 96 of this edition, was read before the Royal Society, there was also read a letter from Mr. Hewson; whereupon it was remarked by the president, Mr. West, "that he supposed the Society would think themselves obliged for these, as they do for all ingenious communications; at the same time that they would undoubtedly adhere to the constant rule of the Society, not to interfere by giving judgment in any matter of dispute which should be brought before them, and may afterwards become questionable in the learned world."[1]

On the 16th of November, 1769, ten months after Dr. Monro's claim had been read, the reading of Mr. Hewson's two papers on the lymphatic system of amphibia and fishes was completed, and the following notice was entered in the Journal Book of the Society:[2] "Mr. Hewson's descriptions were greatly illustrated by the exhibition of a series of preparations taken from turtles and divers fishes, wherein these vessels were injected and shown to the naked eye in their rise, progress, communications, and insertions, to the great satisfaction of the Society. Thanks were returned to Dr. Hunter and Mr. Hewson for these very ingenious communications." They were communicated to the Society by Dr. Hunter.

On the 22d of November, 1770, there was a council of the Society, composed of no less than fifteen members, including the president, when Sir Godfrey Copley's gold medal was awarded by ballot to Hewson, "for his papers on the lymphatic system in birds, amphibious animals and fishes."[3] Among the names of members present at that council, I find the Honourable Daines Barrington, the Honourable Henry Cavendish, Mr. Maskelyne, Dr. Maty, and Dr. Watson.[4] The medal was delivered to Mr. Hewson at the anniversary meeting of the Society on the 30th of the same month, when his researches were warmly praised by the president, from whose address on the occasion the following extracts are taken.

"The discoveries which Mr. Hewson has made are in a very

[1] Journal Book of the Royal Society, fol. MS. vol. xxvii, pp. 161-3.

[2] Vol. xxvii, pp. 241-44.

[3] Ibid. p. 397.

[4] Minutes of the Council of the Royal Society, fol. MS. vol. vi, pp. 84-5.

high degree interesting, not only on account of his investigation of parts of the animal system before scarce known, not only on account of the clear idea which his accurate descriptions and elegant preparations have enabled us to form of the parts discovered, but also that he has thereby been enabled to confirm that hypothesis which had been, with great show of reason, entertained, that the lymphatics are the only absorbent vessels.

"He has not only discovered the existence of this system in the classes of birds and fish, but he has been enabled to follow the course of them (the lymphatics) to an astonishing minuteness, and proved their beautiful disposition on the intestines to ocular demonstration."[1]

It was also stated by the president, that he and the council, in adjudging the medal to Mr. Hewson, had considered chiefly what person had, within the year 1769, "contributed most to the advancement of science and useful knowledge." Hewson's papers were highly commended by many of his contemporaries, especially by Dr. Hunter,[2] Mr. Sheldon,[3] and Dr. Lettsom.[4]

Hewson's three papers on the Properties of the Blood were published in the 'Philosophical Transactions' for 1770; and form the first three chapters of the First Part of the 'Experimental Inquiries.' Before the publication of the second edition, in 1772, of the 'Inquiry into the Properties of the Blood,' it underwent, according to Mrs. Hewson,[5] a critical examination by Sir John Pringle, to whom it was afterwards dedicated. The first, or duodecimo edition was published in 1771. There is a third edition, merely a reprint, in 1780, of the second.

His observations on the Red Particles of the Blood, which appeared in the 'Philosophical Transactions' for 1773, were republished by Magnus Falconar in the first chapter of the Third Part of the 'Experimental Inquiries.' An imperfect abstract of Hewson's Observations on the Uses of the Lymphatic Glands, Thymus, and Spleen, was published, without his concurrence, in the first volume of the 'Medical and Philosophical Commentaries;'[6] and in July, 1773, he gave a short account of his views on those

[1] Journal Book of the Royal Society, fol. MS. vol. xxvii, pp. 396-9.

[2] Two Introductory Lectures to his last Course of Anatomy, p. 60, 4to, Lond. 1784.

[3] History of the Absorbent System, pp. iii, iv, 4to, London, 1784.

[4] Transactions of the Medical Society of London, vol. i, p. 61, 8vo, Lond. 1810.

[5] Mr. Pettigrew's Life of Dr. Lettsom, vol. i, p. 145.

[6] By a Society in Edinburgh, 8vo, London, 1773.

subjects, in a letter to Dr. Haygarth, which appeared in the third volume of the same work.

Concerning the coagulation of the blood, so admirably illustrated by Hewson's labours, it is remarkable that the historical part has been strangely neglected. From this remark the interesting notices by Dr. Davy[1] must be excepted; but they are only sufficient to show, what it is probable he intended, that further inquiry on the subject is wanted. In short, so imperfect are the records of this branch of science, that physiological literature, overflowing about the red particles, is yet destitute of a connected history of the spontaneously coagulable principle of the blood. This, up to the year 1832, I shall now attempt to supply. It will be a curious chapter in physiological history, showing the uncertainty of fame and the slowness of justice; the delusion of hypothesis and the advantage of observation and induction. Such a narration is moreover absolutely necessary to enable us to form a correct estimate of the merits of Hewson, and of some of his contemporaries, as original inquirers. He entertained a just view of the nature of false membranes on serous surfaces;[2] but as he has given no opinion concerning the organization of the blood-clot or of the fibrin, it is unnecessary on the present occasion to consider the important researches of Mr. Hunter on this point, the more especially as they are familiarly known in the physiological schools of Britain.

Aristotle[3] seems to have ascribed the coagulation of the blood to the presence of a fibrous matter. It is not certain whether he considered this matter as existing in the circulation, or as liquid there, andformed in the blood after it is extravasated. He noticed that it will not coagulate if the fibres be removed.

Harvey[4] supposed that the living blood contains none of the parts found in it after death. These he described as a cruor of red and white portions, one dense and fibrous, the other ichorous and serous, the fibres connecting the whole. It may be inferred that he was acquainted with the effect of heat upon serum,—and indeed it is not improbable that Aristotle was.

[1] Researches, Physiol. and Anat. vol. ii, p. 49, 8vo, London, 1839.

[2] See Note LXXII, page 162.

[3] Oper. om. I, p. 457, fol. Aureliæ Allobrogum, 1605.

[4] De Generatione Animalium, pp. 159-60, 4to, Lond. 1651.

Dr. Willis[1] describes the filaments of the crassamentum as joined together or concreted into a parenchyma. He knew that serum is coagulable by heat and by some acids, and that it is the lightest part of the blood.

Malpighi[2] washed away the colouring matter of the clot, and obtained the whitish fibrous part; he insisted that polypi of the heart are composed of this fibrous matter, and not of coagulated serum as was then generally supposed. He examined the polypi and the pale substance of the blood-clot with a microscope, and found them both made up of a fibrous texture or network, adding that the buffy coat, which he calls pellea crusta, has probably the same structure. His discovery of the blood corpuscles is well known.

Lower[3] gave a tolerably accurate account of the glutinous fluid to which the coagulation of the blood and some of its morbid appearances are owing. He was acquainted with the effect of heat in coagulating the serum of the blood, lymph, and the fluid of the pericardium.

The observations of Borelli[4] are still more precise. He described the compound nature of the clot and of the serum; the clot as consisting of white fibres or reticulated membranes, the serum as composed partly of matter coagulable by heat, and partly of water impregnated with salts. He concluded, from microscopical observations on the fibres and on the capillary vessels, that what he describes as the white, glutinous, and spontaneously coagulable matter of the blood, is liquid in the living body.

Robert Boyle[5] describes the blood as dividing itself into a fluid or serous and a consistent and fibrous part. He knew that serum is coagulated by spirit of wine, by sublimate, and by heat.

Dr. Samuel Collins[6] states that the blood is composed of "a crystalline liquor and red crassament, which coagulates when

[1] Diatribæ duæ Med. Phil. de Febribus, cap. i, pp. 13, 14, 8vo, Lond. 1659.

[2] De Viscerum Structura, accedit Dissertatio de Polypo Cordis, p. 156, 4to, Bononiæ, 1666.

[3] Tractatus de Corde, pp. 129-30, and 136, 12mo, Lond. 1669.

[4] De Motu Animalium, prop. cxxxii, p. 264, Op. Posth. pars alt. 4to, Romæ, 1681.

[5] Natural History of Humane Blood, pp. 67, 73, 93, 12mo, London, 1684.

[6] System of Anatomy, vol. i, p. 42; vol. ii, pp. 747, 756-7, fol. London, 1685.

extravasated, thereby gaining a more solid consistence, produced by manifold fibres." The blood-clot he regarded as made up of minute white filaments, or thin membranes inclosing the red particles. He had a correct idea of the nature of polypi of the heart; and his elaborate descriptions of the intimate structure of the buffy coat, and of the arrangement of the parts composing the clot of buffy blood, show how accurately he had examined these subjects for himself, even if he did get his first knowledge of them from Malpighi.

Bidloo[1] figured and described little fibres and the red corpuscles in a drop of blood, stating that the fibres, after exposure for a while, become very tough, tensile, and like network. He calls the corpuscles globosæ vesiculæ. William Cowper[2] repeated Bidloo's plate and description, but thought that the fibres were accidental, and described the blood as consisting only of two parts, the serous and globular.

Verduc[3] calls the serum serosity, stating that it is the same as the lymph, or dew of the blood. He says that it is to a mucilage mixed with the red part that the blood owes its more or less easy coagulation, and the clot its consistency and union, while the coagulation of the blood is caused by the collecting together and the flattening of the little globules.

Gulielmini[4] examined coagulated blood microscopically, and described it as composed of whitish fibres and red globules; and Michelotti[5] attests that the crassamentum consists not of globules only, but also of most minute and tough fibres.

Sydenham[6] describes the buffy coat as solid and fibrous, the fibres perhaps formed of the sanguineous or red part divested of its colouring matter. Boerhaave[7] regarded the fibrous part of the blood as chains of globules; Haller[8] mentions fibres generated from the red portion; Quesnay[9] ascribes the buffy coat

[1] Anatomia Humani Corporis, tab. 23, fig. 16, fol. Amstelædami, 1685.

[2] Anatomy of Humane Bodies, revised and published by C. B. Albinus, tab. 23, fig. 16, fol. Leyden, 1737.

[3] Traité de l'Usage des Parties, tom. i, pp. 182, 184, 185, 188, 12mo, Paris, 1696.

[4] De Sanguinis Natura et Constitutione, p. 54, 8vo, Venetiis, 1701.

[5] De Separatione Fluidorum in Corpore Animali, p. 286, 4to, Venetiis, 1721.

[6] Opera Omnia, p. 247, Imp. Soc. Syden. 8vo, Lond. 1844.

[7] Academical Lectures, §§ 221, 223, vol. ii, 8vo, London, 1743.

[8] Primæ Lineæ Physiologiæ, § cxlvi, 8vo, Gott. 1780.

[9] Traité de la Saignée, pp. 415-16, 8vo, Paris, 1750.

to a change in the red corpuscles; and Bordenave[1] had a similar opinion. These doctrines foreshadow either those of Sir E. Home, or of Mr. Jones and Dr. Simon, mentioned at pages xxxi and xli of this Introduction, and in the Notes I, XVIII, and CXVIII.

Ruysch[2] obtained the fibrous matter from the blood-clot by washing, and from fluid blood by agitating it with a twig. He described the resemblance of the fibres to a true fibrous membrane, and depicted the pseudo-membrane, as he terms it, procured from his own blood and from that of a pig, on the branch of an African plant. The 'Ruyschian membrane' is often mentioned by succeeding writers.

At this time, then, the spontaneously coagulable matter of the blood had been well understood; while the fibrous structure of fibrin was thoroughly described, and often with the aid of the microscope. But the elementary molecules, and the cells or organic germs in the fibrin were overlooked; for although Leeuwenhoek[3] probably saw either some of these or the pale floating globules of the blood,[4] his description is obscured by hypothetical errors. The same objection occurs with regard to the observations made upwards of a century afterwards by Sir Everard Home and Mr. Bauer,[5] though they described and even accurately measured the pale globules both of the buffy coat of the blood and of the coagulated lymph in inflamed parts; and yet, to suit a preconceived hypothesis, concluded with the error that these globules are smaller than the red corpuscles.

In Britain, Keill,[6] Jurin,[7] Thomas Morgan,[8] John Cook,[9] Arbuthnot,[10] William Cowper,[11] Martine,[12] and Langrish,[13]

[1] Essai sur la Physiologie, tom. i, p. 155, 8vo, 4ème édit. Paris, 1787.

[2] Thesaurus Anat. Sept. No. xxxix, tom. ii, p. 19, tab. iii, fig. 6, 4to, Amstelædami, 1707.

[3] Philosophical Transactions, 1675, vol. x, p. 380.

[4] For the observations of Senac and Hewson on this point, see Note CXLVI, p. 282.

[5] Philosophical Transactions, 1820, pp. 3-5.

[6] Essays on several parts of the Animal Œconomy, p. 96, 2d edit. 8vo, Lond. 1717.

[7] Philosophical Transactions, 1719, vol. xxx, p. 1000.

[8] Philosophical Principles of Medicine, pp. 108, 110, 177, 251, 2d edit. 8vo, London, 1730.

[9] Essay on the Whole Animal Œconomy, vol. ii, pp. 18, 19, 8vo, London, 1730.

[10] Essay concerning Aliments, pp. 121, 163, 8vo, London, 1731.

[11] Anatomy of Humane Bodies, tab. 23, fig. 16, fol. Leyden, 1737.

[12] Medical Essays and Observations, vol. ii, pp. 77, 86, 8vo, Edinburgh, 1747.

[13] Theory and Practice of Physic, pp. 66-7, 8vo, London, 1735.

regarded the coagulation of the blood as caused simply by the running together of the red corpuscles; and that the crassamentum is formed merely of an aggregation of them was also the opinion of William Northcote[1] and of Dr. Marmaduke Berdoe.[2] Berdoe called the serum serosity. Jurin, Morgan, and Northcote used the terms serum and lymph synonymously. Martine denied the existence of the fibres described by Malpighi in the blood-clot; Cowper and Northcote expressed a similar opinion. Huxham[3] stated that the buffy coat is caused by the heat in an ardent fever turning the serum into a jelly; Lister,[4] that the serum becomes a stiff jelly by a little standing; and although Dr. Butt[5] described the coagulable lymph, and so called it, he confounded it with serum and with the white of egg. Dr. Francis Home[6] described the blood as consisting of crassamentum, serum, and lymph, mentioning, as the properties of the latter, only those of the serum. To the third and last volume of the 'Bibliotheca Anatomica,' published in London in 1714, in 4to, some of the ablest men of the day, as Keill, Drake, and Verheyen, contributed essays on the blood, which are remarkable for errors similar to those above noticed.

On the Continent, Leeuwenhoek[7] seems to have considered the blood as composed only of globules and serum, and the former as the spontaneously coagulable part. Boerhaave,[8] and his commentator Van Sweiten,[9] Haller,[10] and Marherr,[11] described coagulation as a mere conjunction of the red globules, and the clot as nothing but a cohering mass of them, save some

[1] Anatomy of the Human Body, p. 425, 8vo, London, 1772.

[2] An Essay on the Nature and Circulation of the Blood, p. 18, 8vo, London, 1772.

[3] Essay on Fevers, 6th edit. p. 36, 8vo, London, 1769.

[4] Philosophical Transactions, 1672, vol. vii, p. 5137.

[5] De Spon. Sang. Sep. pp. 514-15, 4to, Edin. 1760; in Sandifort. Thesaur. tom. ii.

[6] An Inquiry into the Nature, Cause, and Cure of the Croup, p. 39, 8vo, Edinb. 1765.

[7] Philosophical Transactions, 1675, vol. x, p. 380; and for 1700, vol. xxii, p. 450; and Select Works, tr. by Sam. Hoole, vol. i, p. 89, 4to, London, 1800.

[8] Prælectiones Academicæ, ed. et Notas additit Alb. Haller. vol. ii, § ccxxvii, 8vo, Gottingæ, 1740.

[9] Aphor. § 93.

[10] Deux Mémoires sur le Mouvement du Sang, pp. 21-2, 8vo, Lausanne, 1756; Primæ Lineæ Physiologiæ, §§ cxxxvii, cxxxviii, cxliv, cxlvii, 8vo, Gottingæ, 1780.

[11] Prælectiones in Herm. Boerhaave Institutiones Medicas, tom. ii, pp. 254-55, 8vo, Viennæ et Lipsiæ, 1772.

intermixed serum. Distinct from this fluid, these eminent men had no knowledge of the coagulable lymph; the same remark applies to most of the British writers just mentioned; and, like some of them, Haller and Marherr believed that the buffy coat is caused by a change in the serum. Schwenke[1] supposed the nature of that crust to be intermediate to the serum and cruor; and Gaber,[2] even while attesting the identity of the crust with the matter left after washing the blood-clot and with Ruysch's membrane, speaks of them as identical with the coagulable matter of the serum, quoting, in support of this opinion, Sauvages, de Haen, and Quesnay. Yet Marherr, amid the vagueness in which he joined, after referring to the opinion of those who held that there is a fibrous matter in the blood, more correctly states that the fibres, when obtained by washing from the clot, are formed from particles which were previously fluid. Errors similar to those above noticed are contained in the 'Dictionnaire Raisonné d'Anatomie et de Physiologie,' tom. ii, pp. 381 and 384, 8vo, published at Paris in 1766. While such were the current opinions, there were some writers, now to be noticed, who had a clearer knowledge of the blood.

Petit[3] declared it to be generally known, that all the parts of the blood are not susceptible of coagulation; that it at first coagulates entirely, but after a while the serum separates from the clot, as whey does from curdled milk. He always uses the term serosity for the serum, as Verduc before and many other writers afterwards did. After stating that this is not the coagulable part, he says that the next parts are the lymphatic and the globular; and then gives a just view of the disposition of the several parts of the blood in the heart and great vessels after death, distinguishing the white and lighter clot of lymph from the red and heavier globular part, and so accurately deducing the effect that the position of the body after death has on their relative situation.[4] In bleeding from the foot, he described the diffusion of the colouring matter throughout the water, and the separation of the white part. He declared

[1] Hæmatologia, p. 158, 8vo, Hagæ Com. 1743.

[2] Mélanges de Philosophie et de Mathématique de la Société Royale de Turin, pour les années 1762-1765, pp. 167, 168, 170-72, 4to, 1766.

[3] Histoire de l'Académie Royale des Sciences, ann. 1732, pp. 392-96, 4to, Paris, 1735.

[4] See Note XIII, pp. 23-4.

that the red portion forms no clot without the containing or enveloping white lymph, and showed that the different degrees of consistency of different parts of the clot depend on the proportion of the white matter, which is most solid when it coagulates alone; next adding, that if it were possible for the blood to be fluid until all the lymph rose to the surface, this would form the only clot, while the globules and serum would remain fluid. He concluded that the lymph is the only part of the blood susceptible of self-coagulation.

Quesnay[1] observed that the clot is composed of little white filaments, which he called the fibrous lymph, and of red globules, the lymph condensing when let out of the blood-vessels, and retaining the globules in its interstices. He correctly noticed and drew the right conclusions from the effects of whipping fresh blood with twigs; on which point Senac's[2] knowledge was afterwards less accurate. Describing the buffy coat, Quesnay[3] remarked that the blood is full of many glairy humours, which in cooling form the crust; that this is of the nature of lymph; and that it collects, from being in a dissolved state, on the surface of the blood during inflammation, but does not so rise at the beginning of the disease, before the humour has come to such a state of dissolution. Hence he concluded, as Hewson afterwards did from a set of ingenious experiments, I believe[4] incorrectly, that the floating of this humour on the blood, far from showing it to be thickened, proves that its fluidity is increased. Quesnay also stated, in anticipation of a very recent doctrine,[5] and of an older and more generally admitted one,[6] that the humour appears to be formed from the molecules, as he calls the red corpuscles, destroyed and reduced to a glaire by the action of the arteries; and that this humour abounds in acute diseases, at the expense of the molecules, sometimes to such an extent that the red part of the blood is much diminished.

[1] Principes de Chirurgie, pp. 31, 32, 34, 8vo, Paris, 1746.

[2] Traité du Cœur, tom. ii, p. 303, 4to, Paris, 1783.

[3] Traité de la Saignée, pp. 402, 405, 406, 415, 416, nouv. éd. 8vo, Paris, 1750.

[4] See Notes XXI, XXIII, and XXIX.

[5] See the observations of Mr. Wharton Jones, and Dr. Simon, on the transformation of the red corpuscles into fibrine, Notes I and CXVIII.

[6] See the observations of Mr. Hey and others on the increase of the fibrine in the blood during inflammation, Note I.

Senac[1] described the whitish substance which congeals of itself, becomes tenacious, and forms the buffy coat of the blood.[2] He terms the pale part indifferently a white oil, lymphatic matter, white or whitish substance, and coagulated lymph,[3] but never coagulable lymph. He correctly explained the formation of the buffy coat;[4] and separated the lymph, by washing, from the blood-clot, as Malpighi had done, and from fluid blood by whipping it with twigs, after the method of Ruysch, observing that the latter operation hinders coagulation by removing the concreting principle or bond of the other parts of the blood, and that the red part alone cannot unite into a compact substance; though he also speaks of the coagulation of the red globules,[5] yet not without combating the errors of Leeuwenhoek.[6] He observed that the lymph forms clots separate from the red corpuscles in aneurisms; that it closes the ends of the great blood-vessels after amputations,[7] and that it differs from the white of egg and from serum in the property of coagulating spontaneously.[8] After stating that the red particles may be dissolved and become white,[9] he gave reasons for objecting to the opinion of some writers, perhaps alluding to Sydenham and Quesnay, that the white part of the blood is formed of the red, deprived of its colouring matter.[10]

Examining the lymph microscopically, Senac found no globules in it, but merely branches or irregularly connected molecules, the concretion resembling what is observed in a plate of scarf-skin.[11] He correctly states, that the coagulated lymph forms a kind of membrane, like a true reticular tissue, but incorrectly ascribes the net-like appearance to other fluids coagulating with the lymph, and visible with the aid of the solar microscope.[12]

[1] Traité de la Structure du Cœur, 4to, Paris, 1749; and 2d edit. 4to, Paris, 1774, which appeared also in 1783, with a new title-page only.

[2] Ed. 1749, tom. ii, p. 91; and 2d edit. tom. ii, p. 284.

[3] Ed. 1749, tom. ii, pp. 91, 449, 453, 75, 96.

[4] Ed. 1749, tom. ii, pp. 90, 92, 124, 447; and 2d edit. tom. ii, pp. 285, 299, 414.

[5] Ed. 1749, tom. ii, pp. 92-3, 447-49, 129.

[6] Ed. 1749, tom. ii, pp. 659, 91.

[7] Ed. 1749, tom. ii, p. 94.

[8] Ed. 1749, tom. ii, p. 95.

[9] Ed. 1749, tom. ii, p. 450.

[10] Second edit. pp. 413-14.

[11] Ed. 1749, tom. ii, p. 660; and 2d edit. tom. ii, p. 280.

[12] Ed. 1749, tom. ii, pp. 92, 449, 452; and 2d edit. tom. ii, pp. 285, 415.

He gave an excellent description, like Malpighi's, of the blood-clot;[1] but added a fanciful explanation of the reticular or membranous structure of the buffy coat, and of polypi of the heart, maintaining that the lymphatic juices do not coagulate into an uniform substance like cheese, because they are compounded with many others; that the molecules concreting first, act as wire-drawers to produce the filaments, and the red parts as balls or grains, between which the congealed juices are continued as fibres.[2]

The serum he called serosity, as Verduc and Petit had before done, and represented it as the vehicle of all the other matters of the blood.[3] He truly observed, in anticipation of Mr. Hunter's[4] experimental proof, that heat cannot be the cause of the fluidity of the blood of fishes living in iced water during winter;[5] yet, following the experiments or opinions of Schwenke, states it to be a fact, that a temperature above 96° keeps the blood liquid;[6] that its liquidity is preserved by agitation in a bottle; that a large quantity of nitre or of sea-salt coagulates the blood when kept still, though it remains fluid if agitated;[7] next, that these salts prevent coagulation;[8] and finally, that nitre, injected into the veins, coagulates the blood.[9]

The second edition of Senac's work contains many of the crudities of the first, and several improvements in matters which had been made clear by the experiments of Hewson. Thus, although Senac retained his vague notion of several spontaneously coagulable matters in the blood, to wit, lymph, fat, mucous matter, and gelatinous juices,[10] he introduced a statement, not in the first edition, that it is the lymph only which coagulates;[11] and to prove, according to divers writers, that the exclusion of air makes the blood less susceptible of coagulation, he details an experiment of his own,[12] inconsistent with the accurate knowledge he had formerly shown of the effect of re-

[1] Ed. 1749, tom. ii, p. 449; and 2d edit. tom. ii, p. 415.
[2] Ed. 1749, tom. ii, p. 450; and 2d edit. tom. ii, pp. 415, 416.
[3] Ed. 1749, tom. ii, p. 104; and 2d edit. tom. ii, p. 292.
[4] Works, edited by Mr. Palmer, vol. iii, p. 26, 8vo, London, 1837.
[5] Ed. 1749, tom. ii, p. 133; and 2d edit. tom. ii, p. 302.
[6] Ed. 1749, tom. ii, p. 133.
[7] Ed. 1749, tom. ii, p. 134.
[8] Ed. 1749, tom. ii, p. 136.
[9] Ed. 1749, tom. ii, p. 459.
[10] Second edit. tom. ii, pp. 285, 414, 416.
[11] Second edit. tom. ii, p. 302.
[12] Second edit. tom. ii, p. 303.

moving the coagulable lymph from the blood by whipping it with twigs. In the second edition, too, he dwells on the evidence of the flat shape of the corpuscles,[1] and omits the vague observations which he had given in the first edition on the effects of neutral salts, and on the fluidity of the blood being preserved by a heat of 96°.

The doctrine of Leeuwenhoek was taught by the first Dr. Monro, at Edinburgh, up to the year 1758; and the first lectures of his son[2] and successor were employed in refuting this system, and in proving the distinction between the different parts of the blood.

Dr. J. M. Butt[3] followed Malpighi in the description of the blood-clot, adding that Senac aptly named the fibrous part coagulable lymph, which also forms the part commonly called the inflammatory crust. Yet Dr. Butt says the serum is composed of coagulable lymph, watery fluid, and saline matter. He stated that if serum be exposed to a heat of 150°, it concretes into an uniform mass, from which exudes, after it is cut into thin slices, a watery saline liquor, called serosity by Senac. I have not met with an expression exactly equivalent to coagulable lymph in Senac's work, though, as I have already noticed, he mentions coagulated lymph, and lymph which coagulates of itself; and he used the word serosity merely for the serum. Sir John Pringle[4] mentioned, in a note to a paper read before the Royal Society, February 13, 1752, that the inflammatory crust is the same part of the blood as that called by M. de Senac the white matter which coagulates of itself.

Gaubius[5] distinguished the three proximate constituents of the blood. He described the fibre as forming the basis of the clot, and as exceedingly different from the red part and from the serum, especially in the power of coagulating spontaneously. The inflammatory diathesis he attributed to a mucus generated in the serum and coagulating in the manner of the fibre; and

[1] Traité du Cœur, 2d edit. tom. ii, p. 276-77.

[2] Essays and Heads of Lectures, &c., and Memoir of Dr. Alexander Monro secundus, by his Son and Successor, p. xiv, 8vo, Edinburgh, 1840.

[3] De Spontanea Sanguinis Separatione, pp. 504, 514, 521, 510, 4to, Edinb. 1760; in Sandifort. Thesaur. Dissert. 4to, Roterodami, 1769.

[4] On the Diseases of the Army, 5th edit. App. p. lxxiv, 4to, London, 1765.

[5] Institutiones Pathologiæ Medicinalis, §§ 339, 344, 340, 361, 367, 280, 345-47, 8vo, Leid. Bat. 1758.

the pleuritic coat to an undue tenacity of the fibre, connected with an increase of its quantity in proportion to the serum, and with a condensation of the serum into the fibre. He regarded the constitution of blood and milk as so nearly alike, that the one might be called red milk, and the other white blood; comparing the serum of blood to whey, the red part to cream, the fibre to curd, and the red corpuscles to oily globules like those of milk.

Dr. Richard Davies[1] observed that the inflammatory pellicle is formed by the coagulation of a fluid, a gluten natural to the blood, rising to the surface, and exactly similar to the concreted substance obtained from the blood by stirring it with a tube. He washed the crassamen, as he calls it, with water, and described the remaining gluten as resembling a congeries of pellucid membranes and fibres (pp. 5-9), expressly declaring, in opposition to Jurin, that the red globules do not possess any strong cohesion to form a mass or compact body, without the interposition of the gluten. (pp. 11, 12.) This, Davies adds, acquiring tenacity by cold and rest, causes coagulation; and then by its concreting force, presses out the serum, so that "the denser the texture of the gluten is, the more serum it presses out;" and when no serum separates from the blood, it usually remains a tender coagulum, not from a defect of serum, for then the coagulum would be dense, but from the weakened contraction of the glutinous parts. (pp. 15-19.)

The inflammatory pellicle he ascribed, as Quesnay before, and Hewson afterwards did, to a preternatural attenuation or fluidity of the gluten, permitting the red globules to subside while it rises with the serum to the surface. Here the gluten exerting its full cohesive force, becomes a firm membrane, or more compact body than that part of the clot which has its cohesion broken by the intervention of the red globules. Thus the gluten squeezes out the serum and compels the crassamen into a more regular form. (pp. 23-24.) He fixes the specific gravity (p. 13) of the serum at 1·026, of the pellicle of inflamed blood, as he terms the buffy coat, at 1·056, and of the crassamen at 1·084, adopting Jurin's estimate, with some reservation, of the red globules at 1·126.

[1] Essays to promote the Experimental Analysis of the Human Blood, by Richard Davies, M.D., late Fellow of Queen's College in Cambridge, 8vo, Bath, 1760.

Dr. Davies seems to claim for himself the discovery of the gluten, ignorant perhaps, like some of his successors, of the facts ascertained by the earlier observers. Discussing the sources of error in Jurin's estimate of the weight of the red corpuscles, Dr. Davies says, "These objections could not occur to Dr. Jurin, because the gluten had not been discovered as a constituent part of the blood. So many errors therefore are detected and removed in consequence of this one discovery" (pp. 15-16); and mentioning, towards the end of his Essay, the want of success before made in these studies, he adds, "One difficulty at least is hereby removed, in the manifest exhibition of the three distinct portions of the blood." (p. 51.)

Dr. William Hunter[1] at the end of the year 1759 was well acquainted with these portions, especially distinguishing the spontaneously coagulable principle, which, like Borelli and Davies, he termed gluten, but asserted that it was "formerly falsely called fibrous." The word gluten was often used for the same part still later, as by William Hunter's pupil, Dr. Hugh Smith,[2] who had a similar knowledge of the blood, by the reviewer[3] of Mr. Hey's work, and by Dr. Henry.[4] There is no evidence that Dr. Hunter's knowledge of the properties of the gluten was of an earlier period than that above noted, and it is the exact date afterwards mentioned incidentally by himself.[5] Two years before, while carefully describing the contents of an aneurism,[6] he used no term which shows that he was then familiar with the coagulable lymph; but says that the clot, "towards its outside, was as hard and as tough as a cake of glue that had been soaked in cold water, and of a cineritious complexion; towards its inside, it was more tender and of a redder colour. The laminæ of which it was composed were thin as paper, regular, and did not readily separate from each other, especially in the tougher external part of

[1] Anatomical, Physiological, and Chirurgical Lectures, p. 4, 4to, MS. 1759, in the Library of the Royal Medical and Chirurgical Society, London, Press mark B, i, 17.

[2] Essays on the Nature and Circulation of the Blood, pp. 12, 18, 12mo, London, 1761.

[3] In the Medical and Philosophical Commentaries, by a Society in Edinburgh, vol. vi, p. 380, 8vo, London, 1779.

[4] Epitome of Chemistry, p. 125, 12mo, London, 1801.

[5] Medical Commentaries, Part I, pp. 39, 41, 4to, London, 1762.

[6] Medical Observations and Inquiries, by a Society of Physicians in London, vol. i, pp. 347, 348, 8vo, London, 1757.

the coagulum." He asks, in endeavouring to account for the erosion of bone in contact with an aneurism, whether the blood has the property of dissolving bony matter, and mentions what he regarded as evidence in the affirmative.[1] Again, in his important Remarks on the Cellular Membrane,[2] read Oct. 31, 1757, and published in 1762, he says, "in adhesions of parts, that follow in consequence of inflammation, we observe that the surfaces are first glued together by a *mucus*,[3] and every wound that is healing is naturally covered by a bed of soft mucus in which the vessels shoot." He used the term lymph for "the interstitial fluid of living bodies." Describing the effects of inflammation of serous membranes, and the pus which is formed without any breach in the solids, he writes, "it is only a sort of inspissated serum, or an inflammatory exudation . . . the containing surface is more or less covered with a glutinous concretion or slough of the same colour as the fluid . . . but still the surface covered by these sloughs is without ulceration or loss of substance."

Thomas Houlston[4] was acquainted with the three parts of the blood, and he used the term coagulable lymph in the same sense as it has long since been employed. He refers to the experiments of his friend Hewson as to the heat which coagulates this lymph, and concludes that the blood is not so viscid in inflammation as in the natural state. Mr. Houlston's dissertation is dedicated to

Dr. George Fordyce, who remarked, among other symptoms of the inflammatory diathesis, that the blood when drawn is more fluid, so that the red globules fall to the bottom and the coagulable lymph forms the buff or crust on the top.[5] He afterwards[6] described the coagulable lymph more fully, to wit, fluid in the circulation at any degree of heat between 30° and 120° of Fahrenheit's thermometer; coagulating when taken

[1] Medical Observations and Inquiries, by a Society of Physicians, vol. i, pp. 344, 345, 8vo, London, 1757.

[2] Medical Observations and Inquiries, vol. ii, pp. 28, 48, 61, 62.

[3] The italics are Dr. Hunter's or his printer's. The word mucus was used in its modern restricted sense by Dr. Arbuthnot, in his Essay on Aliments, p. 183, published in 1731, several years before Dr. Hunter wrote.

[4] Diss. Med. Inaug. de Inflammatione, pp. 11, 12, 14, 4to, Lugd. Bat. 1767.

[5] Elements of the Practice of Physic, p. 28, Part II, 8vo, Swan, London, 1768.

[6] Ibid. pp. 4, 6, Part I, 8vo, Johnson and Payne, London, 1770.

out of its vessels, whether in motion or at rest, exposed to the air or not, in the heat of the human body or in any other degree of heat; continuing fluid for more than three hours when retained in the blood-vessels, in motion or at rest, in any heat between 30° and 120°; and its coagulation prevented by saturating the whole blood with common salt and perhaps by some of the other neutral salts. His account of the formation of the buffy coat is similar to Hewson's, even stating that it will occur on blood which has some time been stagnated in its vessels before it is let out. Dr. Fordyce had a perfectly clear knowledge of the distinction between the coagulable lymph, the red corpuscles, and the serum. Fordyce and Houlston are probably alluded to in Hewson's note to the end of the second chapter of the first part of his 'Experimental Inquiries,' p. 41, where he says that his facts had been mentioned in his lectures ever since 1767 or before. A creditable contemporary writer, Dr. David Macbride[1] first quotes Fordyce concerning the buffy coat, but afterwards[2] corrected himself by adding that "the late Mr. Hewson was the first that accounted for the formation of the buff in the manner above described:" and Dr. Foart Simmons,[3] after having quoted the same observation on the authority of Dr. Fordyce, concluded by giving it entirely to Mr. Hewson. In the library of the medical department of the army, at Chatham, there is a quarto copy of manuscript notes from Dr. Fordyce's Lectures on the Practice of Physic, marked on the cover, "R. Scott, 1764." The term coagulable lymph is correctly used in that manuscript for a part of the matter which causes the swelling in inflammation, and incorrectly for the lateritious sediment of the urine in the same disease.

Hewson removed the coagulable lymph in its fluid state from the blood, and proved that the coagulation of the lymph is quite independent of the other parts. His experiments on the effects of various temperatures on its coagulation are very interesting and original; by freezing the blood he suspended its coagulating power, and restored it by thawing. To enable

[1] A Methodical Introduction to the Theory and Practice of Physic, pp. 229-30, 4to, London, 1772.

[2] Op. cit. 2d edit. vol. i, p. 294, 8vo, Dublin, 1777.

[3] Elements of Anatomy, from the French of M. Person, note to p. 327, 8vo, Lond. 1775; and 2d edit. 1781, p. 345.

him the better to investigate the properties of the lymph, apart from the red corpuscles, he kept it fluid by neutral salts, and proved that it would yet coagulate after the addition of water. He showed from precise observations how much longer coagulation is in taking place when the blood is stagnated, either artificially or from natural causes, in its own vessels, than when it is removed from them. His experiments on the comparative rate in the sinking of the red corpuscles in what has since been called the liquor sanguinis and in the serum were highly novel, ingenious, and correct.[1] He described accurately the part performed by the coagulable lymph in diseases; how the fluid of dropsies differs from that lymph and from the serum; and carefully examined the properties of the fluid of the lymphatic vessels and of the serous sacs. The theoretical conclusions to which he was led by those inquiries, on the secretion of lymph into serous sacs and on the pathology of the lymphatic system, contain the sum of nearly all that is at present known on those subjects.

In considering the labours of Hewson in connexion with the facts observed and the errors held by his predecessors and contemporaries, it must be recollected that the speculations connected with Leeuwenhoek's microscopical researches for many years supplanted accurate experimental inquiries into the properties of the blood; so that the fibrin was either forgotten or confounded with the serum, and a fanciful importance was given to the red corpuscles. When the errors consequent on this state of things began to wane, the blood sunk into neglect. Accordingly, the just observations of Malpighi, Lower, and Borelli, were lost for the greater part of a century, while the coagulation of the blood was ascribed to a mere cohesion or running together of the red corpuscles, and the formation of fibrinous clots to a change in the serum. These opinions were held in Britain by the best writers to the year 1760, and on the Continent by the most distinguished physiologists, as Haller and Marherr, up to or even beyond 1771, the date of the first edition of Hewson's 'Inquiry into the Properties of the Blood.' But there were exceptions. The knowledge of Petit, Quesnay, Senac, and Gaubius, was unquestionably in advance of that current in their day; yet they added but little to the facts of

[1] See Note XXIII, p. 40.

the older observers, so vaguely discussed by Haller,[1] so ably used in the masterly memoir of Petit; and some of the opinions set forth by Quesnay and Senac, partook of that general crudity which proved how necessary it was that the properties of the blood should be studied anew by the experimental method.

The merit of clearly apprehending the importance of such an inquiry and of supplying an outline of it, justly belongs, I believe, to Richard Davies. His 'Essay,' considered as a demonstration of the distinctive characters of the three proximate principles of the blood, is admirably decisive. Yet it seems to have fallen dead-born from the press, and his labours have been hitherto left to oblivion. Why he has suffered this injustice it might be vain or painful to inquire. There is no reason to believe that he derived his knowledge from Dr. William Hunter; on the contrary, this distinguished anatomist may rather have gained his knowledge of the blood from Davies. There is a copy of his 'Essay' in Dr. Hunter's library at Glasgow. It appears probable, from what has already been shown, that Dr. Hunter did not entertain his opinions before the end of the year 1759; his denial of the fibrous structure of fibrin would lead us to suppose that he had not then studied it very carefully, and we are not sure that he ever did experimentally. Now it is certain that Dr. Davies had made experiments on the blood at least as early as 1748, because he gave the results of his own observations on the comparative weights of the serum, buffy coat, and crassamen, in his 'Tables of Specific Gravities,' communicated to the Royal Society by its then president, Martin Folkes, and published in the 'Philosophical Transactions' of that year. Davies there mentions the buff as "sanguinis humani cuticula alba," whence it might be imagined that he had not then examined its nature. But in his 'Epistle to the Reverend Doctor Hales, being Introductory to the Essays on the Blood,' Davies says that he had drawn the principal lines of the first essay full twenty-five years before, and that he hopes the publication of it may engage younger eyes and younger minds in such studies, adding, in a postcript, dated March 1, 1759, that the first essay was then in the press. I know not that he ever published another. Twenty-five years before the date of the epistle to Hales would be in 1734, when Dr. Hunter was but sixteen years of age.

[1] Elementa Physiologiæ, tom. ii, pp. 42, 125, 4to, Lausannæ, 1760.

May not, therefore, Dr. Davies be considered as having revived and established that correct knowledge of the distinct characters of the three parts of the blood which was taught by Hewson, Fordyce, and the Hunters? Certain it is that there was a general ignorance on the subject in England when Davies wrote his Essay. Nor was the gloom in which the truth had been hid for nearly a century dispelled by the fitful lights of the able French physiologists before mentioned. Hence the honour of displacing the current errors by accurate doctrines fairly settled on experimental proofs would appear to belong, as Dr. Davy observes, chiefly to our countrymen.

Among these Mr. Hewson was unrivalled, and has perhaps never been surpassed, for the skill with which he prosecuted this right method of inquiry. His work on the 'Properties of the Blood' is a model of experiments nicely devised, well executed, and clearly told; often original, ever instructive; a happy combination of precision and simplicity, and an admirable reflection of the character of his mind.

Nothing can be plainer than the proofs given by Hewson that the coagulation of the blood is alone dependent on the fibrin; nor is there a jot of evidence that either he or Davies ever supposed that the red corpuscles have anything whatever to do with that coagulation. On the contrary, Hewson was wont to obtain them in the entire state from the clot; he had well established that their form in the blood is preserved by the saline matter of the serum, and by watery solutions of neutral salts out of the body; while the mixture of these salts with the blood was one of the means by which he separated the fibrin, as Müller afterwards did, from the red corpuscles.

There was, indeed, an old and discarded hypothesis, already noticed, entertained by Sydenham, Quesnay, and Bordenave, that the fibrin of the blood is formed of the colourless matter of the red corpuscles. Sir Everard Home[1] and Mr. Bauer revived this opinion in the more specious form, that the fibres of fibrin and of muscle are composed of rows of the nuclei of the corpuscles divested of their coloured envelopes. A similar doctrine was soon afterwards brought forward in France by Prevost and Dumas,[2]

[1] Croonian Lectures, Philosophical Transactions, 1818 and 1820.

[2] Examen du Sang, Annales de Chimie et de Physique, tom. xviii et xxiii, 8vo, Paris, 1821 and 1823.

supported and extended by Milne Edwards,[1] and adopted by Dutrochet,[2] Beclard,[3] and others.

But it never took root in Britain, where, although Davies was forgotten, the accurate conclusions of Hewson remained current in the schools and among the best independent observers. Thus Dr. Davy[4] always considered the three parts of the blood distinctly; he defined coagulation to be a change from the liquid state of fibrin to the solid, and showed that fibrin is viscid before it becomes firm, so accounting for morbid adhesions. This change in the fibrin from a liquid to a solid, was the chief fact from which the supposed production of heat in coagulation was inferred, originally, I think, by Fourcroy. Dr. Gordon,[5] Mr. Wilson,[6] Dr. Babington,[7] and others, plainly distinguished the three parts of the blood; and we have the excellent testimony of Dr. Sharpey[8] that Dr. Gordon was in the habit of giving, in his lectures, an account of serum and fibrin being the products of the coagulation of the transparent blood-fluid, as the common opinion of physiologists. In like manner Mr. Abernethy, in his lectures, when I was his pupil, used to distinguish the different parts of the blood, just as Mr. Hunter[9] had written after Davies, Dr. Hunter, and Hewson. Moreover, the exact observations of Dr. Hodgkin and Mr. Lister[10] were quite against the views of Sir Everard Home and Dr. Milne Edwards; and in 1829 we find an eminent Teacher[11] declaring that the new theory had been proved in this country to be erroneous.

In Germany the difference between the corpuscles of the

[1] Répertoire Général d'Anatomie et de Physiologie, tom. iii, 4to, Paris, 1827.

[2] Recherches sur la Formation de la Fibre Musculaire, Annales des Sciences Naturelles, tom. xxiii, 8vo, Paris, 1831.

[3] Elémens d'Anatomie Générale, pp. 81-3, 8vo, Paris, 1827.

[4] Tent. Exp. quædam de Sang. Comp. p. 15, 8vo, Edinb. 1814; Journal of Science and the Arts, ed. at the Royal Institution, vol. ii, p. 248, 8vo, Lond. 1817; Philosophical Transactions, 1822, pp. 273-4.

[5] Outlines of Lectures on Human Physiology, p. 60, 8vo, Edinburgh, 1817.

[6] Lectures on the Blood, pp. 28 et seq., 8vo, London, 1819.

[7] Medico-Chirurgical Transactions, vol. xvi, pp. 293 et seq., 8vo, London, 1830.

[8] Müller's Physiology, tr. by Dr. Baly, vol. i, p. 110, 8vo, London, 1838, 1st edit.

[9] Works, ed. by Mr. Palmer, vol. iii, pp. 17, 20, 24, 8vo, London, 1837.

[10] Philosophical Magazine, vol. ii, pp. 131 et seq., July to December, 1827.

[11] Mr. Grainger, Elements of General Anatomy, p. 46, 8vo, London, 1829.

blood and the globules of the tissues was insisted on by Tiedemann;[1] and Burdach[2] resisted the hypothesis that the fibrils of fibrin are composed of globules; while in France De Blainville[3] declared that such a structure was seen only by the dupes of an optical delusion.

But this delusion must have been widely spread on the Continent. Besides the writers already cited, Professors Müller[4] and Wagner,[5] and Dr. Milne Edwards,[6] state that it was generally believed that coagulation is produced by a running together of the red corpuscles, before Berzelius announced that the fibrin is liquid in the flowing blood, and becomes solid in the clot. This last opinion, as I have before shown, was both a very old and a very new one when that eminent chemist advanced it. That it will ever be commonly known as a fact established long ago is almost too much to hope; for M. Denis[7] has even lately declared that he was the first to admit the liquidity of the fibrin in the circulating blood!

Professor Müller witnessed coagulation in the liquor sanguinis after he had filtered it from the red corpuscles of the frog;[8] and he preserved for a while the fluidity of the blood of man and other vertebrate animals with carbonate of potash, so that the corpuscles sunk, and the liquid fibrin became solid above them.[9] Of these experiments, the first was new and ingenious in form; the second was of an old sort and more complicated than some of Hewson's; but neither was required to demonstrate the accuracy of his views, because he had already done that by the most simple, direct, and conclusive experiments.

And even if the researches of Hewson and of his predecessors and successors were to be set at naught, British physiologists had, before Professor Müller's experiments were known here, generally adopted Dr. Babington's term, liquor

[1] Physiology, tr. by Dr. Gully and Dr. Lane, vol. i, p. 397, 8vo, London, 1834.

[2] Traité de Physiologie, tr. par Jourdan, tom. vi, pp. 34, 59, 8vo, Paris, 1837.

[3] Cours de Physiologie Générale et Comparée, tom. i, p. 234, 8vo, Paris, 1829.

[4] Physiology, tr. by Dr. Baly, vol. i, pp. 109-10, 1st edit.

[5] Physiology, tr. by Dr. Willis, p. 264, *note*, 8vo, London, 1844.

[6] Cyclopædia of Anatomy, vol. i, p. 413, 8vo, London, 1836.

[7] Essai sur l'Application de la Chimie à l'étude physiologique du Sang de l'Homme, pp. 67, 356, 8vo, Paris, 1838.

[8] Annales des Sciences Naturelles, tom. xxvii, pp. 222-4, 8vo, Paris, 1832.

[9] Physiology, tr. by Dr. Baly, vol. i, p. 112, 1st edit.

sanguinis, for the fluid mixture of fibrin and serum in the circulating blood.

In the London schools Mr. Hunter has long been regarded as the great authority on the coagulation of the blood; and to crown the confusion, the discovery of the leading property of what he always considered as the most important part of the blood, the coagulable lymph, has been awarded to him. Thus the Hunterian Professor, Mr. Owen,[1] asserts that "Hunter subjects the blood both to mechanical and chemical analysis, and endeavours to determine the characteristic properties of its different constituents. It was not known in his time upon which of these constituents the act of coagulation depended. Hunter took advantage of a case in which the red globules subsided, as in some cases they do, more rapidly than usual, and skimming off the superincumbent colourless fluid, found that the fibrin, as it is now termed, immediately coagulated and formed a colourless clot. A subsequent erroneous theory, which attributed the act of coagulation to the red globules, has recently been set aside by the application of an ingenious process for artificially separating the fibrin from the blood-discs before coagulation takes place, and the opinions of Hunter on this point have been fully established by the experiments of Müller."

Now Mr. Hunter was well acquainted with Hewson's work on the 'Properties of the Blood,' in which this simple experiment of skimming off the liquor sanguinis was detailed, with all the legitimate deductions from it, many years before 1794, when Mr. Hunter's 'Treatise on the Blood' was published; and how much longer the fact which the experiment proves had been known, may be gathered from the foregoing narrative. Besides, an eminent English surgeon[2] had commented on and frequently repeated Mr. Hewson's experiment in 1779.

I know not that Mr. Hunter ever claimed for himself the discovery of any one of the three constituents of the blood. The serosity,[3] of which he thought himself the discoverer, and the paleness which he is said, by Mr. Owen, to have discovered in the blood of the early embryo of vertebrate animals,[4] had

[1] Hunter's Works, vol. iv, p. xii, 8vo, London, 1837.

[2] Mr. Hey, Observations on the Blood, pp. 10-12, 8vo, London, 1779.

[3] See Note LIV, p. 79. [4] See Note CII, p. 222.

both been described long before Mr. Hunter published on those subjects. After stating that the coagulation of the blood out of the circulation would seem to be unconnected with life,[1] Mr. Hunter[2] says that he conceives the simple act of coagulation, apart from its causes, to be an operation of life, proceeding upon the same principle as union by the first intention, adding that the coagulation of the blood appears to be that process which may be compared with the action of life in the solids. He gave an excellent description of the toughness and elasticity of a clot of fibrin, and of its fibrous and laminated structure; and when he inferred how a membrane may be formed of that part of the blood, he was entering on a most interesting and philosophical inquiry, which he pursued with a perseverance and success very characteristic of his ardent genius. In blood removed from the body, he proved, experimentally, that no heat is produced during the act of coagulation; and he described, in anticipation of some of the most recent observations, the aggregation of the red corpuscles and the consequent mottled appearance of the blood during the formation of the buffy coat.[3]

About the time when Mr. Hunter's 'Treatise on the Blood' appeared, the term fibrin was introduced by the French chemists, and used in its present sense by Fourcroy, in his 'Système des Connaissances Chimiques,' published at Paris in 1801.

The old observations on the fibrous structure of fibrin have often since been revived, and sometimes insisted on as distinguishing it from coagulated albumen, particularly by Dr. Henry,[4] Mr. Dowler,[5] and Dr. Alison;[6] while the fibrils of fibrin have been examined again with the aid of the microscope by Lauth,[7] Magendie,[8] and many others. Heidmann[9]

[1] See Note XII, p. 21.

[2] Works, ed. by Mr. Palmer, vol. iii, pp. 20, 34, 113, 23-24, 35-36; and vol. i, p. 235.

[3] See Notes XXI, XXIII, and XXIX.

[4] Epitome of Chemistry, p. 125, 12mo, London, 1801.

[5] Medico-Chirurgical Transactions, vol. xii, p. 87, 8vo, London, 1822.

[6] Outlines of Physiology and Pathology, p. 43, 8vo, Edinburgh, 1833.

[7] L'Institut, Journal Général des Sociétés et Travaux Scientifiques, No. 70, p. 301, 4to, Paris, Sept. 13, 1834.

[8] Lectures on the Blood, Lancet, 1838-39, vol. i, pp. 460, 255.

[9] Noticed in Tiedemann's Systematic Treatise on Comparative Physiology, tr. by Drs. Gully and Lane, vol. i, p. 150, 8vo, London, 1834.

described motions in the fibrils during the coagulation of fresh blood.

Magnus Falconar[1] published the third part of the Experimental Inquiries in 1777. He has explained his share of the work in the preface. He appears to have written the last four chapters with great care and fidelity; but it is probable that Hewson would have altered some of the statements, had he ever seen them.

Hewson's observations are more exact and complete than any that had before been made on the red corpuscles. He finally exploded the error of Leeuwenhoek that those of the mammalia are spherical, by proving their flat figure, and showing that they may accordingly be seen laid one against another like coins. He proved that water renders them spherical, that their flat shape is preserved by the saline matter of the serum, or by a weak solution of a neutral salt, and that they are contracted or shrivelled by a strong solution. His demonstration of the nucleus and vesicle in the corpuscle of lower vertebrate animals is excellent. He observed the important facts, now generally admitted, that there is a difference of character among the corpuscles of the same blood; that they are larger in the embryo than in the adult; and that they split into an irregular number of pieces, under certain circumstances, from the circumference to the very centre, while each of these portions retains its red colour, contrary to a leading tenet of Leeuwenhoek's hypothesis.

He was the first to examine accurately the fluid of the lymphatic system, of the thymus and of the vessels which run from it into the veins at the lower part of the neck. He described the corpuscles common to all these fluids, and saw the pale globules in the blood. The lymphatic vessels just mentioned of the thymus, he concluded, like Sir Astley Cooper

[1] Mr. Falconar was born at Cheltenham, in Gloucestershire, in November, 1754. He married Hewson's sister, and died of pulmonary consumption at Bristol, March 24, 1778, at the age of 24. He was a man of great application and dexterity, and a good speaker. The sale of his collection of anatomical preparations, which included those made by Hewson, produced upwards of £900. (Dr. Simmons's Life of Dr. Hunter, p. 58.) Mrs. Hewson (Mr. Pettigrew's Life of Dr. Lettsom, vol. i, p. 146, of the Correspondence) says that her husband desired on his death-bed that he should be succeeded in the Anatomical School by Mr. Falconar.

afterwards, to be its excretory ducts;[1] and he formed the same opinion of the lymphatics of the spleen. He observed the ruddy colour produced by the presence of blood-corpuscles in the lymph of this organ; and although he concluded that the office of the spleen is to form their red envelopes, he believed that it is not the only part destined to this function, since he confirmed the truth of the old observation, that the spleen may be removed without a permanent ill effect, and that the blood-corpuscles may be perfectly formed in the lymphatic system generally. Similar views as to the office of the spleen have been lately arrived at by inquirers,[2] probably still ignorant of Hewson's labours. His researches concerning the use of the glands without regular excretory ducts show the marks of an active and ingenious mind, and I believe first indicated a rational method of research into this important and recondite branch of physiology. His observations on the lymphatic glands are in the same spirit; and his doctrine of central particles,[3] so long looked upon as visionary, was a step far in advance of the age in which he lived.

The relation between the lymph-globules and the red corpuscles of the blood, originally announced by Hewson, has been advanced anew from independent observations by some of the best physiologists on the Continent. Professor Wagner[4] agrees with Nasse in concluding that the lymph-globules are free nuclei of the blood-corpuscles. Mulder[5] assumes that these corpuscles originate from those of lymph; and Schultz, Henle, and Simon[6] entertain the same opinion.

Yet, of all Hewson's able contemporaries and immediate successors, Magnus Falconar alone perceived the importance of this subject, and entered on the new field of inquiry with an honest zeal, unfortunately arrested by a death still more premature than that of Hewson. The conduct of Falconar in the use which he made of the materials intrusted to him by his deceased friend was every way just and generous, nobly

[1] See Note CXXVII, p. 261.

[2] See Note CXXXIII, p. 273. [3] See Note CXXII, p. 254.

[4] Elements of Physiology, tr. by Dr. Willis, p. 253, 8vo, London, 1844.

[5] Chemistry of Vegetable and Animal Physiology, tr. by Dr. P. F. H. Fromberg, Part II, p. 344.

[6] Animal Chemistry, translated for the Sydenham Society by Dr. Day, vol. i, pp. 120-21, 8vo, London, 1845.

repaying Hewson's confidence, and affording an honorable example of integrity, which, in matters of science, has sometimes been disregarded with less temptation of gain and impunity, though with a sacrifice of that higher feeling which must have warmed the breast of Mr. Falconar.

Had these two ingenious men lived to continue the inquiry which they had so successfully begun, it is not improbable that the important results obtained by Schwann would long since have been anticipated in this country; for, as I have elsewhere[1] remarked, the researches of the German physiologist show that what Hewson propounded of the blood-corpuscles particularly is applicable to the tissues generally, and that the cell-nucleus of the present day is but another term for the central particle of Hewson.

Hewson's style has generally the merit of precision and clearness, so essential to scientific narrative. From his matter he never wanders into the airy regions of mere speculation; although he had sometimes the fault, not uncommon in young and ardent minds, of pushing his conclusions beyond their just limits. But, even then, his reasoning is seldom wrong according to the facts upon which it is founded; and the means of observation and the state of knowledge at the time were scarcely sufficient to extend his premises.

Upon the whole, Hewson was a great benefactor to science; and if his works are not weighty and comprehensive enough to place him in the first class of physiologists, he is certainly entitled to the most exalted rank in the second, and to a high station among the eminent British Worthies.

[1] Appendix to the English edition of Gerber's Anatomy, p. 100, 8vo, London, 1842; and Note to Dr. Willis's translation of Wagner's Physiology, p. 250.

LIST OF HEWSON'S WRITINGS.

1. The Operation of Paracentesis Thoracis, proposed for Air in the Chest; with some Remarks on the Emphysema, and on Wounds of the Lungs in general. By Mr. William Hewson, Reader of Anatomy. Communicated by Dr. Hunter; read June 15, 1767.—Medical Observations and Inquiries, by a Society of Physicians in London, art. xxxv, vol. iii, pp. 372-96, 8vo, London, 1767.

2. An Account of the Lymphatic System in Birds. By Mr. William Hewson, Reader in Anatomy: in a Letter to William Hunter, M.D., F.R.S., and by him communicated to the Society. Received October 3, 1768; read December 8, 1768.—Philosophical Transactions for the year 1768, vol. lviii, pp. 217-26.

3. An Account of the Lymphatic System in Amphibious Animals. By Mr. William Hewson, Lecturer in Anatomy: in a Letter to William Hunter, M.D., F.R.S., and by him communicated to the Society. Received June 19, 1769; read November 9, 1769.—Philosophical Transactions for the year 1769, vol. lix, pp. 198-203.

4. An Account of the Lymphatic System in Fish. By the same. Received June 19, 1769; read November 16, 1769.—Philosophical Transactions for the year 1769, vol. lix, pp. 204-15.

5. Experiments on the Blood, with some Remarks on its Morbid Appearances. By William Hewson, F.R.S. Received May 17, 1770; read June 14 and 21, 1770.—Philosophical Transactions for the year 1770, vol. lx, pp. 368-83.

6. On the Degree of Heat which Coagulates the Lymph, and the Serum of the Blood; with an Inquiry into the Causes of the inflammatory Crust, or Size, as it is called. By the same. Received May 7, 1770; read November 15, 1770.—Philosophical Transactions for the year 1770, vol. lx. pp. 384-97.

7. Further Remarks on the Properties of the Coagulable Lymph, on the Stopping of Hæmorrhages, and on the Effects of Cold upon the Blood. By the same. Received July 7, 1770; read November 22, and December 6, 1770.—Philosophical Transactions for the year 1770, vol. lx, pp. 398-413.

8. On the Figure and Composition of the Red Particles of the Blood, commonly called the Red Globules. By Mr. William Hewson, F.R.S., and Teacher of Anatomy. Read June 17 and 24, 1773—Philosophical Transactions, 1773, vol. lxiii, pp. 303-23.

9. An Experimental Inquiry into the Properties of the Blood, with some Remarks on its Morbid Appearances; and an Appendix, relating to the Discovery of the Lymphatic System in Birds, Fish, and the Animals called Amphibious. By William Hewson, F.R.S., and Teacher of Anatomy. 12mo. Printed for T. Cadell, in the Strand, London, 1771. A second edition, the above title being preceded with 'EXPERIMENTAL INQUIRIES, PART THE FIRST,' was printed in 8vo, for T. Cadell, in the Strand, London, 1772; and a third, merely an exact reprint of the second, for J. Johnson, St. Paul's Churchyard, London, 1780.

10. Experimental Inquiries, Part the Second, containing a Description of the Lymphatic System in the Human Subject, and in other Animals. Illustrated with Plates. Together with Observations on the Lymph, and the Changes which it undergoes in some Diseases. By William Hewson, F.R.S., and Teacher of Anatomy. Printed for J. Johnson, No. 72, St. Paul's Churchyard, 8vo, London, 1774.

11. Experimental Inquiries, Part the Third, containing a Description of the Red Particles of the Blood in the Human Subject and in other Animals; with an Account of the Structure and Offices of the Lymphatic Glands, of the Thymus Gland, and of the Spleen; being the remaining part of the Observations and Experiments of the late Mr. William Hewson, F.R.S., and Teacher of Anatomy. By Magnus Falconar, Surgeon and Teacher of Anatomy. London, printed for T. Longman, No. 39, Paternoster Row. 8vo, 1777.

12. A Letter from the late Mr. William Hewson, F.R.S., and Teacher of Anatomy in London, to Dr. John Haygarth, Physician in Chester.—Medical and Philosophical Commentaries, by a Society in Edinburgh, vol. iii, pp. 87-93, 8vo, London, 1775.

EXPERIMENTAL INQUIRIES.

PART I.

AN INQUIRY

INTO

THE PROPERTIES OF THE BLOOD.

WITH

REMARKS ON SOME OF ITS MORBID APPEARANCES;

AND

AN APPENDIX

RELATING TO THE DISCOVERY OF THE LYMPHATIC SYSTEM IN BIRDS, FISH, AND THE ANIMALS CALLED AMPHIBIOUS.

BY WILLIAM HEWSON, F.R.S.
AND TEACHER OF ANATOMY.

LONDON:
MDCCLXXII.

Vere scire, est per causas scire.

LORD BACON.

TO

SIR JOHN PRINGLE, Bart.,

PHYSICIAN TO HER MAJESTY.

Sir,—I have taken the liberty to address you upon this occasion, not so much that I might pay a compliment to your merit, as that I might boast of your friendship: it is impossible that I should add anything to your reputation by displaying your abilities; but you may add greatly to mine, even by detecting my mistakes. You have done me the honour to approve this little Essay, and I am ambitious to tell the world that it owes much both to your knowledge and your kindness; which have concurred to suggest many useful hints and judicious corrections, that make it more worthy of attention, and afford me a public opportunity of assuring you that I am, with the truest respect and esteem,

Sir,

Your most obedient,

And most obliged humble servant,

WILLIAM HEWSON.

PREFACE.

The knowledge of the human frame, the preservation of health, and the cure of diseases, are objects of too great importance to mankind for the author of these sheets to doubt that any attempts to promote them, how small soever, should not meet with a candid and indulgent reception from the public. An Inquiry into the Properties of the Blood, it is presumed, will be thought, in a particular manner, interesting, since there is no part of the human body upon which more physiological reasoning is founded, nor any from which more inferences are drawn for the cure of diseases. And, as the Inquiry is made by Experiments upon the Blood as near as possible to the state in which it circulates in the vessels, it is hoped that the conclusions made from them will stand the test of a candid examination, and lead to further observations and improvements.

Since the publication of the first edition, some new Experiments have been made, and a new chapter has been added, which contains a recapitulation of the principal facts and conclusions that are met with in this Essay. These additions are between pages 61 and 82.

The Appendix is a vindication of the Author's right to the discovery of the Lymphatic Vessels, in opposition to the claim of the learned Dr. Alexander Monro, Professor of Anatomy in the University of Edinburgh.

PROPERTIES OF THE BLOOD.

CHAPTER I.

OF THE SEPARATION OF THE SERUM; THE COLOUR OF THE CRASSAMENTUM; AND OF THE CAUSES OF THE COAGULATION OF THE BLOOD.

When fresh blood is received into a basin, and suffered to rest, in a few minutes it jellies or coagulates, and soon after separates into two parts, distinguished by the names of crassamentum and serum. These two parts differ in their proportions in different constitutions: in a strong person the crassamentum is in greater proportion to the serum than in a weak one; and the same difference is found to take place in diseases; thence is deduced the general conclusion, that the less the quantity of serum is in proportion to the crassamentum, bleeding, diluting liquors, and a low diet, are the more necessary; whilst in some dropsies, and other diseases where the serum is in a great, and the crassamentum in a small proportion (1), bleeding and diluting would be highly improper. As

(1.) It is not easy to ascertain exactly the proportion of serum, because, as Dr. Davies[a] observed, the more the fibrin contracts the more serum it presses out. By adding weak saline solutions or urine to blood newly drawn, I found that the size of the crassamentum was increased and the quantity of fluid separated decreased.[b] Dr. Davy[c] has

[a] Essays on the Human Blood, p. 18, 8vo, Lond. 1760.

[b] Edin. Med. & Surg. Journ. lxiv, 369-71.

[c] Researches Phys. and Anat. ii, 28.

it is therefore supposed useful to attend to the proportions of these parts in many disorders, and even to take indications of cure from them, it has been an object with those who have made experiments on the blood, to determine the circumstances on which its more perfect separation into these two parts depends; it being obvious, that till this be done, our inferences from their proportions will be liable to considerable fallacies. Two of the latest writers on this subject agree, that if the blood, after being taken from a vein, be set in a cold place, it will not easily separate, and that a moderate warmth is necessary: this is a fact that is evinced by daily experience.

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clearly shown that, in animals blooded to death, the specific gravity of the blood which flows last is diminished, and that there is not a greater difference in the specific gravity of the whole mass of such blood than in the serum alone; whence he infers that the watery part is increased in the blood which flows last. After bleeding a man into two basins, I have several times found the serum of the last drawn blood specifically lightest.

Dr. Davy's observations[d] seem to show that in acute inflammatory diseases the blood differs very little in specific gravity from healthy blood, while in chronic diseases it is comparatively dilute and of low specific gravity. And although he infers from his first table that the proportion of animal matter is increased, and the proportion of water diminished, still the details of his last table rather favour the conclusion that there is no constant relation between the presence of the buffy coat and the proportion of fibrin in the blood. But the old opinion of Quesnay,[e] Gaubius,[f] and Hey,[g] that the fibrin is increased in the blood during inflammation, has been confirmed by Thackrah,[h] and adopted by Dr. Benj. Babington,[i] Mr, Grainger,[j] and by later writers generally. Mr. Wharton Jones[k] and Dr. Simon observe, that there is also a diminution of the red corpuscles, which they believe are more quickly turned into fibrin[l] during inflammation than in health; as was also the opinion of Quesnay and Bordenave, referred to, as well as the somewhat similar notion of Sydenham, in the Introduction.

Consult, for further details on the relative proportion of the three parts of the blood in disease, M. Andral's 'Essai d'Hematologie Pathologique,' 8vo, Paris, 1843; the observations of Becquerel, Rodier, Simon, Marchand, Nasse, Enderlin, and others, given in the English version of Simon's 'Animal Chemistry,' published in 1845, by the Sydenham Society.

[d] Res. Phys. and Anat. ii, 31, 34, 39, 42.

[e] Traité de la Saignée, p. 415, 8vo, Paris, 1750.

[f] Instit. Pathol. §§ 366-7, 8vo, Leid. Bat. 1763.

[g] Obs. on the Blood, p. 21, 8vo, Lond. 1779.

[h] Inquiry into the Nature, &c. of the Blood, new ed. by Wright, p. 206-11, 8vo, Lond. 1834.

[i] Cyclopædia of Anatomy, i, 420.

[j] Elements of General Anatomy, p. 41, 8vo. Lond. 1829.

[k] Report, §§ 19, 20, 21, 34, 35, Brit. and For. Med. Rev. No. xxxv.

[l] See Notes xviii & cxviii, and Introd.

They likewise say, that the heat should be less than that of the animal, or than 98° of Fahrenheit's thermometer; and that, if fresh blood be received into a cup, and that cup put into water heated to 98°, it will not separate; nay, they even say, that it will not coagulate; but this, I am persuaded from experiments, is ill founded (II).

EXPERIMENT I.

A tin vessel, containing water, was placed upon a lamp, which kept the water in a heat that varied between 100° and 105°. In this water was placed a phial, containing blood that instant taken from the arm of a person in health; the phial was previously warmed, then filled, and corked to exclude air. In the same water was placed a teacup half full of blood, just taken from the same person; a third portion of the blood was then received from the same vein into a basin, and was set upon a table, the heat of the atmosphere being at 67°. Now, according to their opinion, the two former should neither have coagulated nor separated, when that in the basin began to separate; but, on the contrary, they were all three found to coagulate nearly in the same time; and those in the warm water not only did separate as well as the other, but even sooner.

EXPERIMENT II.

The same experiment was repeated on the blood of a person that laboured under the acute rheumatism, whilst the heat of the atmosphere was no higher than 55°, and that of the warm water was 108°; and the result of this experiment was not only a confirmation of what was observed in the first, but it even showed, that this degree of heat was so far from lessen-

(II.) Dr. Butt[a] says, that blood taken from a vein remains fluid if kept at the heat of the living body, and that at about 32° the blood concretes into an uniform mass, but does not separate. Schwenke[b] has some like erroneous observations as to the effect of heat. A moderate warmth is not essential, though favorable, to the separation of the blood; it will take place at from 40°-44°. The effects of different temperatures on coagulation are given in Notes III, XI, and XVI.

[a] De Spont. Sang. Sep. Sect. I, cap. ii, 4to, Edin. 1760, in Sandifort. Thesaur. t. ii.

[b] Hæmatologia, pp. 89-90, 8vo, Hag. Com. 1743.

ing, that it increased the disposition to coagulate; for the blood in the cup and in the phial was not only congealed, but the separation was much advanced before the whole of the blood in the basin was coagulated. Thence I am led to conclude, that the separation of the blood in a given time, is in proportion as the heat in which it stands is nearer to the animal heat, or 98°; or greater in that heat than in any of a less degree (III). And I am confirmed in this inference by

(III.) Mr. Hewson has clearly shown that a high temperature promotes and a low one retards coagulation. Mr. Hey[a] says, that a heat equal to that of the living body is most favorable to the separation of the serum and clot; and Professor Burdach,[b] quoting Hewson, Schrœder, and Thackrah, asserts that this heat is most favorable to coagulation. But it would appear to be more hastened by a higher temperature. In Hewson's experiments VI-IX, blood in the veins just removed from dogs, coagulated completely at 120°-125° in eleven minutes, remained fluid after that time, when the heat was not raised above 114°; and when kept at rest in the vein in the living animal was not wholly coagulated in less than two or three and a quarter hours. Mr. Hunter[c] found that blood removed in sections of the jugular veins from dogs, and heated to about 120°, coagulated five minutes sooner than when kept at its natural heat; and that the contraction of muscle was hastened by a temperature of 125° and of 98°. In Mr. Thackrah's experiments,[d] blood removed in the jugular veins from living dogs, did not coagulate at the atmospheric temperature in twenty minutes, nor in an hour at 98-100°; and blood, in portions of the umbilical cord, detached from a living child, and placed in water at 100-110°, was fluid and of the natural consistence at the end of fifteen minutes, and found to contain clots after thirty-five minutes. Of blood abstracted as usual, he believed that a temperature of 120-130° considerably hastens coagulation; and that 100-110° generally does so, but in a less marked manner. Sir Charles Scudamore[e] found that blood drawn from a person affected with pleurisy coagulated quicker at 120° than at 98°. Dr. Davy's experiments[f] dispose him to infer that a heat of 120° immediately renders the blood more liquid and accelerates coagulation; that it is rather retarded than quickened at 100°; more rapid at 80° or 90° than at 100°, and less so than at 120°. Mr. Hunter[g] observed that a cup of blood put into water at 150° coagulated much quicker than at 48°, whence he infers that heat acts as a stimulus upon the blood, adducing the experiment "as one of the proofs of the living principle of the blood, where it is contrasted with a similar experiment on living muscles." Mr. Prater[h] says, that blood is kept permanently fluid by a heat of 140-150°.

The following is a brief synopsis of my own experiments. Fahrenheit's

[a] Obs. on the Blood, p. 39.
[b] Physiologie, tr. par Jourdan, t. vi, p. 45.
[c] Works, ed. by Palmer, iii, 26, 110.
[d] On the Blood, ed. 1834, Exp. 44, 45, 50, 51, 52, 56.
[e] On the Blood, p. 20, 8vo, Lond. 1824.
[f] Res. Phys. and Anat. ii, 78.
[g] Works, ed. by Palmer, iii, 144.
[h] Exp. Inq. in Chemical Physiol. Part I, pp. 12 et seq. & 73, 8vo, Lond. 1832.

experiments hereafter to be related, where the blood in the living animal, whilst at rest, was found both to coagulate and to separate.

It is well known, that the crassamentum consists of two parts, of which one gives it solidity, and is by some called the fibrous part of the blood, or the gluten, but by others with more propriety termed the coagulable lymph; and of another,

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thermometer, as usual, was employed. The blood experimented on was taken from the jugular veins of troop horses; it was put into tubes of thin glass, about three eighths of an inch in diameter, and then plunged into water heated to the required degrees. Different neutral salts were used; but most commonly half an ounce of Glauber's salt to six ounces of blood, being the mixture mentioned by Hewson.

a. It is extremely difficult to ascertain the precise degree of heat most favorable to the coagulation of a spontaneously coagulable fluid; but one cause of discordance as to the effects of higher temperatures has arisen from different methods of experimenting. A cup of blood put into water at 150°, as in Mr. Hunter's experiment, will coagulate before acquiring that heat, whilst blood in a thin tube, in the same water, may have its property of spontaneously coagulating quite destroyed.

b. Different portions of blood were subjected to every degree of heat, from 139° to 151°, and in every trial the blood was fluid at the end of twenty miuutes. It never coagulated regularly, though it did sometimes partially; and at the higher degree there was, towards the bottom of the tube, a grumous, and sometimes a stiff and brownish paste, chiefly of corpuscles, leaving no fibrin behind when washed in a linen bag. The coagulable lymph was liquid at the top of the blood in the tube.

c. Some liquor sanguinis skimmed from fluid blood, and kept at a heat of 139° for twenty minutes, never coagulated. Another portion of liquor sanguinis at 148-150° was fluid at the end of twenty-three minutes, but coagulated in twenty-eight, when it was more opaque than a portion of the same fluid which had coagulated in the air at 60° in fourteen minutes.

d. At a heat of 122°, 123°, 124°, 125°, and 126°, coagulation of blood, and of liquor sanguinis separately, was hastened about two minutes, and rather more at the lower than at the higher degrees.

e. A mixture of blood and salt, or of liquor sanguinis and salt, will not coagulate at any degree of heat short of that which coagulates the albumen, commencing at about 125°.

f. It is true that in salted liquor sanguinis at 124-127°, though a great part of the mixture remains fluid, a little curdy, flaky, filamentous, and opaque precipitate forms, which is probably what Hewson describes in his Experiments VII and IX as coagulation; though it differs from the spontaneous setting or jellying into a transparent mass of a mixture of salted liquor sanguinis and water.

g. As shown in Note XVII, serum mixed with Glauber's salt coagulates which gives the red colour to the blood, and is called the red globules. These two parts can be separated by washing the crassamentum in water, the red particles dissolving in the water, whilst the coagulable lymph remains solid. That it is the coagulable lymph, which, by its becoming solid, gives firmness to the crassamentum, is proved by agitating fresh blood with a stick, so as to collect this substance on the stick, in which case the rest of the blood remains fluid[1].

The surface of the crassamentum, when not covered with a size, is in general of a more florid red than the blood was when first taken from the vein, whilst its bottom is of a dark colour, or blackish. This floridness of the surface is justly attributed by some of the more accurate observers to the air with which it is in contact; for, if the crassamentum be in-

[1] It may be proper to mention here, that till of late the coagulable lymph has been confounded with the serum of the blood, which contains a substance that is likewise coagulable. But in these sheets, by the *lymph*, is always meant that part of the blood which jellies, or becomes solid spontaneously when blood is received into a basin, which the coagulable matter that is dissolved in the serum does not; but agrees more with the white of an egg, in remaining fluid when exposed to the air, and coagulating when exposed to heat, or when mixed with ardent spirits, or some other chemical substances.

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at a lower heat than pure serum; and the less water there is in serum, or in other words, the greater the proportion of albumen, the lower is the temperature required for its coagulation. Liquor sanguinis kept fluid by salt is only affected by heat like a concentrated serum.

h. A fresh mixture of liquor sanguinis and salt, diluted with four or five parts of distilled water, generally coagulates in less than twenty-five minutes at 65°; its coagulation is hastened by any degree of heat upwards to 123°; most so from 96° to 114°. From 124° to 127° its power of spontaneously coagulating is completely destroyed; and its transparency and fluidity are not lessened till the heat be raised enough to produce the albuminous precipitate described above, at *f*.

i. When the red corpuscles are mixed with the salted liquor sanguinis diluted with water, coagulation is quickened at all temperatures, just as adding corpuscles to pure blood hastens its coagulation, as shown in Exp. 57-63, Edin. Med. and Surg. Journ. vol. lxiv, p. 374.

k. As coagulation is slower in the liquor sanguinis apart from the corpuscles than with them, the retarded coagulation often observed in buffy blood seems to be an effect, and not, as has been so commonly supposed, a cause of the separation of the corpuscles and fibrin.

The effects of a low temperature and of freezing on the coagulation of blood, and on the contractility of muscle, are mentioned in Note XI.

verted, the colours are changed, at least that which is now become the upper surface assumes a more florid redness. This difference of colour, others have endeavoured to explain from the different proportions of the red particles, or globules as they are called, which, say they, being in a greater proportion at the bottom of the crassamentum, make it appear black; but, if inverted, the globules then settle from the surface which is now uppermost, and that becomes redder. But this, I think, is not probable; for the lymph in the crassamentum is so firmly coagulated, as to make it too dense to allow of bodies even heavier than the red particles to gravitate through it; for example, gold. That air has the power of changing the colour of the blood, has been long known; and the following experiment shows it very satisfactorily, and hardly leaves room to refer the appearance to another cause (IV).

(IV.) Schwenke[a] entertained the error mentioned in the text, that the heavier corpuscles sink through the clot, while the lighter ones rise to its middle and surface.

Fracassati[b] noticed the effect of air in making the clot more florid. Harvey[c] admitted that the blood from an artery of a living animal is more florid than from a vein, which he ascribed to accidental circumstances, for he believed that the colour of the two kinds of blood is essentially the same. Lower[d] specially proved the difference of colour, correctly inferring from experiments that the change is effected in the lungs, and further supporting this inference by observing that air produces just the same florid hue on the surface of the blood-clot out of the body. Mayow[e] fully admitted Lower's views; and attributed the change of colour suffered by the blood in passing through the lungs to its abstracting from the atmosphere a nitro-aerial spirit, many of the properties of which, as he described them, belong to oxygen. Senac[f] disbelieved that air affects the colour of the blood; he attributed the change of colour to the more or less density and cohesion of the corpuscles, and treated Mayow with improper contempt. Dr. William Hunter[g] taught the difference of colour between arterial and venous blood. Haller[h] opposed Lower's conclusions; and even declared that he had a hundred times assured himself, in dogs, that there is no difference of

[a] Hæmatologia, 8vo, Hagæ Com. 1743, p. 116.
[b] Phil. Trans. 1667, ii, 492.
[c] Opera om. 4to, Lond. 1766, p. 115.
[d] De Corde et de Motu Sang. p. 170 et seq. 8vo, Lond. 1669.
[e] Tractatus quinque Med. Phys. cap. viii, p. 114, 8vo, Oxon. 1674.
[f] Traité de la Structure du Cœur, tom. ii, pp. 86-7, ed. 1749.
[g] MS. copy of Lectures, pp. 4-5, 4to, 1759; Press mark, B. i. 17, in Lib. Med. Chir. Soc.
[h] Elem. Phys. Lib. VI, Sect. III, § xvii; Primæ Lin. Phys. § 155, 8vo, Gott. 1780; et Deux Mém. sur le Sang, 8vo, Lausanne, 1756, pp. 190-1.

EXPERIMENT III.

Having laid bare the jugular vein of a living rabbit, I tied it up in three places; then opening it between two of the ligatures, I let out the blood, and filled this part of the vein with

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colour between the blood of the pulmonary artery and vein; and though he observed that the blood of the crural artery is brighter than that of the vein, he denied that the difference of colour is caused by the action of the lungs. Cigna[i] refuted the error that the lower part of the blood-clot is darkened by the sinking of the heavier and blacker corpuscles. He gave some experimental proofs of the brightening effect of air on the colour of the blood; but believed that the air is not introduced through the lungs, their chief office being, as he thought, for exhalation and to cool the blood. Dr. Priestley[j] ascertained the correctness of Lower's results, and that air will act on the blood through a moist membrane. Priestley also proved[k] that the brightening of the blood is produced by the oxygen only, and that carbonic acid, hydrogen, and azote, have the contrary effect.

More recently, Dr. Stevens[l] has arrived at the very different conclusion, that the bright colour of arterial blood is caused chiefly or entirely by the agency of the salts of the serum on the hematozine; and that oxygen changes the colour of blood from venous to arterial, merely by removing the carbonic acid which is the cause of the dark hue of venous blood. Dr. Turner[m] fully adopts these views; and Mr. Hoffmann[n] is also favorable to them, chiefly because he found that salt alone will render black blood florid, while air without salt will not produce the effect, as he illustrates by a bit of clot rendered dark by distilled water. I find that the clot, darkened by washing with water, is also made florid by sugar, though less brightly and quickly than by the salt. Dr. Christison,[o] on agitating red corpuscles and serum of blood with atmospheric air, always found that oxygen disappears while carbonic acid is formed; but, owing to the strong solvent power of the serum on this acid, he believed that more of it was formed than appeared in the residual air. In all these experiments venous blood acquired a bright vermilion hue, and arterial blood had its florid tint heightened. Dr. Davy[p] obtained the same results as to the absorption of oxygen by the blood and the consequent brightening of its colour. But in the residual air he could detect only a trace of carbonic acid, not exceeding one per cent.; and none at all when, instead of common air, pure oxygen was agitated with the blood. He observed that venous

[i] Miscel. Phil. Math. Taurinensis, t. i, 4to, 1759; et Mélanges de la Soc. Roy. de Turin, tom. v, 1770-3.

[j] Phil. Trans. 1776, lxvi, 239.

[k] Exp. and Obs. on different kinds of Air, 8vo, Birmingham, 1790, iii, 363.

[l] On the Blood, p. 27, 8vo, Lond. 1832; Phil. Trans. 1835, p. 352, et seq.

[m] Elements of Chemistry, 5th ed. p. 972, 8vo, Lond. 1834.

[n] Lond. Med. Gaz. xi, 1833, p. 887.

[o] Edin. Med. and Surg. Journ. xxxv, 97-100.

[p] Phys. and Anat. Res. ii, 138-9, 176–182.

air. After letting it rest a little, till the air should become warm, I took off the ligature which separated the air from the blood, and then gently mixed them, and I observed that the venous blood assumed a more florid redness, where it was in

blood when acted on by the air-pump did not acquire the arterial hue, even when carbonic acid was abstracted; and yet the florid tint was imparted to venous blood by agitating it with a mixture of oxygen and carbonic acid gases, although the blood absorbed a much larger portion of the latter than of the former gas.

The results of these two last experiments, as Dr. Davy remarks, are nowise reconcilable with the theory of Dr. Stevens, but perfectly in accordance with the older views which he controverts. Dr. C. J. B. Williams[q] gives some experiments to prove that oxygen and the salts produce the florid effect by causing the reflection of more light through the colouring matter. Dr. Davy concludes that neutral salts brighten the blood by separating the corpuscles, so that they reflect more light; and that water, acids, and other agents, darken the blood by altering the form of the corpuscles and partially dissolving the colouring matter. He remarks that hematozine is black only in mass, and red when reduced to powder, or viewed in a small portion by transmitted light.

This exactly agrees with the conclusion of Dr. Wells,[r] that air and neutral salts affect the blood just as bright vermilion is produced from cinnabar by subjecting it to minute mechanical division. He proves by an ingenious experiment, that the opacity of the blood and the reflection of light from it are increased by neutral salts. Now, if the brightening of the blood arise simply from the action of the agents on the colouring matter, it should suffer the same change when separated from the corpuscles; but this it does not; for although Dr. Stevens states that the salts strike a scarlet colour with the pure black hematozine,[s] this effect did not occur in several trials which I made by treating a solution of it either with oxygen, sugar, or neutral salts. The experience of Dr. Davy with common salt, and of Dr. Wells with air and neutral salts, was the same; and Mülder[t] found that hematozine, separated from the corpuscles, is not changed in colour either by oxygen, carbonic acid, or protoxide of nitrogen. In all my experiments the red corpuscles were reduced in size, both in breadth and thickness, by neutral salts, and in a less degree by sugar and by oxygen; while the first effect of water and of carbonic acid was to swell the corpuscles and make them more globular, though they became much smaller after the hematozine dissolved in the fluid. The observations of H. Nasse, Schultz, Henle,[u] and of Mülder,[v] are in many respects similar. In short, it would appear that it is simply to changes in the corpuscles and their state of aggre-

[q] Lond. Med. Gaz. 1835, xvi, 788.
[r] Phil. Trans. 1797, pp. 423-4, 418.
[s] Phil. Trans. 1835, p. 352.
[t] Chemistry of Veget. and Animal Phys. Part ii, p. 344.
[u] Anatomie Générale, tr. par Jourdan, t. i, pp. 461, 463, 471-2, 8vo, Paris, 1843.
[v] Op. cit. p. 339 et seq.

contact with the air-bubbles, whilst in other parts it remained of its natural colour.

There is a difference between the arterial and venous blood in colour; the former is of a florid red like the surface of the crassamentum, the latter is dark or blackish like the bottom of the crassamentum. This change in its colour is produced on the blood as it passes through the lungs, as we see by opening of living animals[1]; and as a similar change is produced by air applied to blood out of the body, it is presumed that the air in the lungs is the immediate cause of this change; but how it effects it, is not yet determined.

As the blood is changed to a more florid red in passing through the lungs, or from the venous to the arterial system, so it loses that colour again in passing from the arteries to the veins in the extreme parts, especially when the person is in health; but every now and then we observe the blood in the veins more florid than is usual, and it likewise frequently happens in venesection, that the blood which comes first out

[1] That this change is really produced in the lungs, I am persuaded from experiments, in which I have distinctly seen the blood of a more florid red in the left auricle than it was in the right. But some authors of the greatest authority say that they could not observe any such difference in a great number of experiments which they made; but this I should attribute to their having been later in opening the left auricle after the collapsing of the lungs than I was; for it seems probable, that whatever is the alteration produced on the blood in its circulation through this organ, that change cannot take place after it is collapsed.

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gation that the effect of many substances on the colour of the blood is owing.

The discordant observations as to the difference of colour between arterial and venous blood may be partly owing to the facts observed by Dr. Crawford[w] and Dr. Davy,[x] that when dogs or sheep are exposed to a temperature above 80°, the venous blood becomes more florid, and the arterial less so, than at a temperature several degrees lower. The brightening of arterial blood seems to depend on the quantity of oxygen consumed in respiration, which is greatest in cold weather. Harvey probably made many of his experiments in Italy. Dr. Davy could perceive no difference of colour between the blood of the jugular vein and carotid artery of a sheep in the hot months, at Malta. Mr. Thackrah,[y] on bleeding a man in a warm bath, observed that the blood from the basilic vein was scarlet.

[w] On Animal Heat, p. 307-8, 8vo, Lond. 1788.

[x] Researches, ii, 140.

[y] On the Blood, ed. 1834, p. 123.

is blackish (v), and that which comes afterwards is more florid: in such cases, the arterial blood passes into the veins without undergoing that change which is natural to it.

Some of the neutral salts have a similar effect on the colour of the blood to what air has, particularly nitre; thence some have attributed the difference of colour in the arterial and venous blood to nitre, which they supposed was absorbed from the air whilst in the lungs. But we know that this is a mere hypothesis, for air contains no nitre (vi). Indeed, nitre is far from being the only neutral salt which has this effect on the blood, for most of them have some degree of it. In making some experiments on this subject, I have observed a more remarkable effect which some of the neutral salts have upon the blood; and that is, being mixed with it when just received from the vein, they prevent its coagulation, or keep it fluid, and yet, upon adding water to the mixture, it then jellies or coagulates. Thus, if six ounces of human blood be received from a vein upon half an ounce of true Glauber's salt reduced to a powder, and the mixture agitated so as to make the salt be dissolved, that blood will not coagulate on being exposed

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(v.) Blood quickly becomes darker by stagnation in the living body. In bleeding a horse, if the neck be tied up a few minutes, the first flowing blood from the vein will be very dark coloured. Two hours after putting two ligatures on the femoral artery of a dog, I found the included blood as dark as that of a vein. Mr. Hunter[a] noticed a darkening of the arterial blood merely from the stagnation produced by a tourniquet. At the temperature of the body changes take place very rapidly in the blood. Its darkening from stagnation may perhaps be owing to the conversion of a little oxygen, in contact with the red corpuscles, into carbonic acid.[b]

(vi.) Perhaps Mayow's nitro-aerial spirit, mentioned in Note iv, may be here confounded with nitre. But when Hewson wrote, there was an opinion current that the air in many places is impregnated with nitre, which he correctly opposes as a mere hypothesis. The knowledge then possessed of the atmosphere was very vague. Oxygen, Dr. Priestley's dephlogisticated air, was discovered by him in August 1774, three years after the publication of this part of Hewson's works. Among the earlier observers on the effects of salts on the colour and coagulation of the blood, were Dr. Freind,[c] J. Handley,[d] and Thomas Schwenke.[e]

[a] On the Blood, Palmer's ed. p. 88.
[b] See Dr. Davy's Researches, ii, 210.
[c] Emmenologia, p. 160, §§ 1, 12, 13, 14, 8vo, Oxon. 1703.
[d] Mec. Essays on the Animal Economy, p. 8, 8vo, Lond. 1721.
[e] Hæmatologia, pp. 106, 145 et seq. 8vo, Hagæ Com. 1743.

to the air, as it would have done without the salt; but if to this mixture about twice its quantity of water be added, in a short time the whole will be jellied or coagulated, and on shaking the jelly, the coagulum will be broken, and the part so coagulated can be now separated as it falls to the bottom, and proves to be the lymph.

In these mixtures of the blood with neutral salts, the red particles readily subside, (especially if human blood be used) and the surface of the mixture becomes clear and colourless; and being poured off from the red part, it is found to contain the coagulable lymph, which can be separated by the addition of water.

I have tried most of the neutral salts, and have made a table of their effects on the blood; but it is not necessary to trouble the reader with it, as we do not see what use it could be in medicine; for we must not conclude that their effects within the body would be the same as out of it.[1] Indeed,

[1] The salts which keep the blood fluid by itself, and yet allow it afterwards to jelly on being mixed with water, are, sal Glauberi verus (sulphate of soda); sal digestivus Sylvii (chloride of potassium); sal communis; nitrum commune; nitrum cubicum (nitrate of soda); sal diureticus (acetate of potash); borax; the salt made of vinegar and the fossil alkali; and the salt made with vinegar and chalk.

The following salts likewise keep the blood fluid, but do not allow it to jelly when mixed with water: Tartarus vitriolatus (sulphate of potash); sal Epsomensis; sal ammoniacus communis; sal ammoniacus nitrosus; sal rupillensis (tartrate of potash and soda): but alum, on being mixed with blood, coagulates it immediately (VII).

(VII.) Dr. Davy[a] found that various salts and vegetable matters which prevent or retard coagulation, commonly have their influence destroyed by dilution with water; and he believes that blood kept fluid by those agents would coagulate in every instance, if the due proportion of water were added. He shows that the blood may be preserved either liquid or viscid, by the neutral salts, for days or weeks, without losing the power of coagulating and of contracting when properly diluted with water; in the liquid state resisting putrefaction, and yet readily yielding to it after coagulation. Some horse's blood, which I had kept fluid with nitre for fifty-seven weeks, readily coagulated when diluted with water.

Mr. Prater, too,[b] supposes that all the neutral salts only suspend coagulation; and he has proved that blood which has been kept liquid, either by Epsom salt or carbonate of potass, will coagulate after the addition of certain proportions of water. He suggests, from a trial with muriate of soda, that a small proportion of all the neutral salts

[a] Researches, ii, 101–2.

[b] Exp. Inq. in Chem. Phys. part i, 8vo, Lond. 1832, pp. 59, 63, 32, 128, 168, 174.

these experiments, as well as some others, were not made so much with a view of any immediate application to medicine, as to determine the properties of the blood chemically; for, having set out with a persuasion, that a more particular acquaintance with the properties of this fluid was necessary before we could arrive at the knowledge of some of the animal functions, such as the manner in which the bile and other secreted fluids are formed, I therefore was anxious to throw some more light on this subject. With this view I have made some experiments even on living animals, being convinced that my inquiries would not otherwise be satisfactory.

When blood is thus kept fluid by Glauber's salt, it still retains its property of being coagulable by heat, and by other substances as before, air excepted. This method of keeping the blood fluid may therefore be useful, by affording an opportunity of making some experiments upon it, which we could not otherwise do from its coagulating so soon when taken from the vessels.

This property of one of the neutral salts has been long

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will hasten coagulation, though they prevent it in greater quantity. Dr. Simon[c] mentions, quoting Hamburger, that carbonates and acetates, in all degrees of concentration, prevent the coagulation of the blood; while the sulphates, tartrates and borates, in strong solution retard and in weak solution accelerate coagulation. In my experiments[d] with solutions of common salt, five and ten grains to an ounce of water, coagulation was somewhat retarded, but most so by the weaker solution; and an ounce of mucilage, having either five or ten grains of salt dissolved in it, likewise retarded coagulation, though less so than the mucilage without the salt. The contraction of the clot was generally much diminished by the aqueous saline solutions, but not by the mucilaginous saline solutions. Mr. Prater observed that either a saturated or a very weak solution of common salt, prevents the contraction of a portion of muscle removed from an animal just killed, and that the addition of water does not restore the contractile property. Mr. Ancell[e] has given a copious table of the effects of various substances on the coagulation of the blood.

The effect of neutral salts in preserving the fluidity of the blood is probably dependent on the solvent power, which M. Denis[f] has shown them to possess over the fibrin. But this requires further inquiry.[g]

[c] Animal Chem. i, 116, tr. by Dr. Day, 8vo, Lond. 1845.

[d] Edin. Med. and Surg. Journ. vol. lxiv, 370. Exp. 20-28.

[e] Lectures on the Blood, Lancet, vol. i, 1839-40, p. 522.

[f] Essai sur le Sang, 8vo, Paris, 1838, pp. 71-2.

[g] See Notes LVI, XII, XLIV, XVIII.

known amongst those whose who prepare the blood of cattle for food; for it has long been a practice with such people, to receive it into a vessel containing some common salt, and to agitate it as fast as it falls, by which means the coagulation is prevented, and the blood remains so fluid as to pass through a strainer, without leaving any coagulum behind: by this means they have an opportunity of mixing it with other substances for culinary purposes.

Although the coagulable lymph so readily becomes solid when exposed to the air, yet whilst circulating it is far from that consistence: it has indeed been supposed to be fibrous (VIII), even whilst moving in the blood-vessels, but erroneously.

(VIII.) It was a question in Haller's time and before, whether the coagulable lymph be fibrous in the circulating blood.[a] And, notwithstanding the evidence to the contrary, referred to in the Introduction, Dr. Buchanan[b] has lately published some interesting observations, to show that the fibrin is not dissolved in the animal fluids, "but exists, while yet within the body, already solidified, and organized in the form of granules and vesicles; and that the process of coagulation consists simply in the aggregation of these minute granules and vesicles into a mass visible to the naked eye." Unquestionably, minute molecules and pale primary cells which floated in the fluid blood, are included in the fibrinous clot; but the delicate fibrils, which I have depicted[c] as constituting the bulk of such clots, do not appear to be formed of particles before visible by the aid of the microscope in fluid blood, or in the liquor sanguinis apart from the red corpuscles. Besides, similar fibrils may be instantly produced by chemical action in certain fluids.[d] In the clot spontaneously formed in a mixture of two varieties of perfectly transparent serum, mentioned in Notes XVIII and CXVIII, I have distinctly seen the fibrils; whilst they were not visible in the stiff, homogeneous, hyaline coagulum, formed after adding water to a fluid mixture of a neutral salt and coagulable lymph, unless occasionally when the mixture had been much more diluted before coagulation.

What is the proof that the fibrils of fibrin may not be the primordial ones of some animal textures? And, as mentioned more fully in the 'Philosophical Magazine' cited below,[c] I know not how this setting of fibrin into fibrils can be reconciled with M. Schwann's theory,[e] that all fibres arise from a transformation of cells; or with Dr. Martin Barry's,[f] that the origin of the fibrils is from the interior of the blood-

[a] Elem. Phys. tom. ii, 68-70; Senac, Traité du Cœur, ed. 1749, ii, 102-3.

[b] Proc. Glasgow Phil. Soc. No. 7.

[c] Gerber's Anat. 8vo, Lond. 1842, pl. 28; and Lond. & Edin. Phil. Mag. S. 3, vol. xxi, 1842, pp. 242-4.

[d] Gruby, Morphologia Fluidorum, 8vo, Vindobonæ, 1840, tab. 5, fig. 83; Edin. Med. and Surg. Journ. vol. lx, p. 161; and Mr. Addison, Trans. Prov. Med. and Surg. Ass. vol. xii, 249-50.

[e] Wagner's Physiol. tr. by Dr. Willis, p. 222.

[f] Phil. Trans. 1842, p. 90.

It is this coagulable lymph which forms the inflammatory crust, or buff, as it is called. It likewise forms polypi of the heart, and sometimes fills up the cavities of aneurisms, and plugs up the extremities of divided arteries. It is supposed, by its becoming solid in the body, to occasion obstructions and inflammations; and even mortifications, from the exposition to cold, have been attributed to its coagulation. In a word, this lymph is supposed to have so great a share in the cause of several diseases, that it would be a desirable matter to be able to ascertain the causes of that coagulation, either in the body, or out of it.

The blood, when received into a basin, and suffered to rest in the common heat of the atmosphere, very soon jellies or coagulates; the part which now becomes solid is the coagulable lymph, as has been shown above. The circumstances in which it now differs from what it was in the veins, are these: it is exposed to the air, to cold, and is at rest; for whilst in the body, air is excluded, it is there of a considerable warmth, and is always in motion. The question is, to which of these circumstances its coagulation whilst in the basin is chiefly owing? This question, I believe, cannot well be answered from the experiments that have hitherto been made. It has indeed been said, that the cold alone coagulated it; for, say they, if you receive blood into a basin, and set that basin in warm water, and stir the blood well, it can be kept fluid. But in the experiments from which this conclusion was made, I find there has been a deception[1]. In short, I have found that it coagulates as soon when kept warm and when agitated (IX),

[1] That is, the lymph really had been coagulated, but by the agitation had likewise been separated from the rest of the blood, and had thereby escaped notice.

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discs. Fibrin undergoes modifications in its chemical properties after its separation from the blood, being especially less susceptible of the action of acetic acid, as mentioned in the Note to Gerber's Anatomy, p. 95.

A notice is given in the Introduction, of the incorrect views of Sir E. Home and others; as well as of the singular claim of M. Denis to the discovery of the liquidity of the fibrin in the circulating blood.

(IX.) The opinion that agitation prevents coagulation was entertained by Lower,[a] and stated as a well-known fact 159 years afterwards, by Dr. Bostock.[b] Senac[c] adduced the supposed fact as a proof of the

[a] De Corde, 8vo, Lond. 1669, p. 173.
[b] Elementary System of Physiol. i, 438, 440, 2d ed. 8vo, Lond. 1828.
[c] Traité du Cœur, ed. 1749, ii, 134.

as it does when suffered to rest and to cool. As the subject seemed to me of importance, I have endeavoured to ascertain the circumstance to which this coagulation is owing by several experiments, in each of which the blood was generally exposed to but one of the suspected causes at a time. Thus, in order to see whether the blood's coagulation out of the body was owing to its being at rest, I made the following experiment:

EXPERIMENT IV.

Having laid bare the jugular vein of a living dog, I made a ligature upon it in two places, so that the blood was at rest between the ligatures; then covering the vein with the skin, to prevent its cooling, I left it in this situation. From several experiments made in this way, I found in general, that after being at rest for ten minutes, the blood continued fluid; nay, that after being at rest for three hours and a quarter, above two thirds of it were still fluid, though it coagulated afterwards. Now the blood, when taken from a vein of the same animal, was completely jellied in about seven minutes. The coagulation therefore of the blood in the basin, and of that which is merely at rest (x), are so different, that rest alone

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power of the action of the blood-vessels in keeping the blood fluid. Mr. Hewson's conclusion accords with that now proved to be correct. Mr. Hunter[d] states that coagulation is hastened by shaking the blood in a phial, with or without the exclusion of air. Mr. Thackrah,[e] Dr. Davy,[f] Sir Charles Scudamore,[g] and Mr. Prater,[h] have proved that even violent shaking of the blood does not prevent coagulation, but merely separates the fibrin into small fragments, which may be collected on a filter; and that moderate agitation rather hastens coagulation.

The effect of rest on coagulation is mentioned in Note x, and of heat in Notes III and XVI.

(x.) The effect of living tissues in retarding coagulation is mentioned in Note XIII. Mere rest has no effect whatever on the coagulation of fibrin. In cases of empyema, I have thrice seen the matter, compressing and condensing the lung and depressing the diaphragm, coagulate spontaneously when set aside in jars, although it was fluid many hours after death, and must have been in a state of comparative repose during life, for the matter completely filled the cavity. Blood sometimes remains fluid and in complete rest for several hours after death, and yet coagulates readily on exposure, as in Mr. Hewson's Exp. XVI.

[d] Works, ed. by Palmer, iii, 31.
[e] On the Blood, p. 68, ed. 1834.
[f] Researches, ii, 65.
[g] Essay on the Blood, 8vo, Lond. 1824, p. 40.
[h] Exp. Inq. in Chem. Phys. Part I, 17.

cannot be supposed to be the cause of the coagulation out of the body.

To see the effects of cold on the blood, I made this experiment:

EXPERIMENT V.

I killed a rabbit, and immediately cut ou one of its jugular veins, proper ligatures being previously made upon it; I then threw the vein into a solution of sal ammoniac and snow, in which the mercury stood at the 14th degree of Fahrenheit's thermometer. As soon as the blood was frozen and converted into ice, I took the vein out again, and put it into lukewarm water till it thawed and became soft; I then opened the vein, received the blood into a teacup, and observed that it was perfectly fluid, and in a few minutes it jellied or coagulated as blood usually does. Now, as in this experiment the blood was frozen and thawed again without being coagulated, it is evident that the coagulation of the blood out of the body is not solely owing to cold (XI) any more than it is to rest.

In the heart of a man fifteen hours after death, when the limbs were stiffened, I have seen fluid blood which coagulated after exposure. Dr. Davy[c] has made several exact observations to the same effect, one twenty-nine and several twenty-six hours after death. Dr. B. G. Babington[d] has recorded an instance of the same kind in bloody fluid from the brain. Occasionally blood is extravasated and stagnated in the living body for an indefinite time, and yet retains its fluidity, as Mr. Hunter[e] and Mr. Cæsar Hawkins have noticed. I saw a case in a soldier who had received a bruise in his loins, from his horse bolting with him over a bridge in Hyde Park; the injured part quickly swelled, evidently from effused fluid, which was let out twenty-eight days afterwards. It measured five ounces; was as liquid as blood just drawn from a vein, and coagulated in a cup in less than thirty minutes. The corpuscles were observed to be unchanged, and readily collecting together in the usual way by their broad surfaces; next day, the clot was moderately firm, scarlet at the top, somewhat contracted, and surrounded by a little serum.

The effect of agitation on coagulation is mentioned in Note IX.

(XI.) Aristotle,[a] Harvey,[b] and Sydenham,[c'] attributed the coagulation of the blood to the escape of its native animal heat or to the cold of the

[c] Researches, ii, 206, 122.

[d] Cyclopædia of Anatomy, i, 421.

[e] Works, ed. by Palmer, iii, 33.

[a] De Part. Animal. lib. ii, c. 4.

[b] De Gener. Animal. p. 404, Op. om. a Coll. Med. Lond. edit. 4to, 1766.

[c'] Op. omnia, imp. Soc. Sydenhamianæ, 8vo, Lond. 1844, p. 247.

Next, to see the effects of air upon the blood, I tried as follows:

EXPERIMENT VI.

Having laid bare the jugular vein of a living rabbit, I tied it up in three places, and then opened it between two of the

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atmosphere. Fourcroy[d] stated that caloric contributes to the fluidity of the blood, which becomes concrete in cooling. Gaubius[e] concluded that the fluidity of the blood is preserved by the heat and motion of the living body. Mr. Hewson's conclusions, that cold, so far from being the cause of coagulation, actually retards it, and that it may be suspended by freezing the blood and restored by thawing it, are now well established. Senac[f] had noticed that the blood of fishes in winter cannot be kept fluid by heat; and Mr. Hunter[g] states that he observed, off Belleisle, in 1761, the blood of a fish, at a temperature about 60°, immediately coagulate on being let out of the animal into the atmosphere at 70°. Mr. Hey,[h] Mr. Thackrah,[i] Sir Charles Scudamore,[j] and Dr. Davy,[k] all ascertained that a low temperature retards coagulation. Thackrah found that human blood remains liquid upwards of sixteen minutes at 45°, and freezes at a temperature between 20° and 30°. Dr. Davy has seen blood remain fluid for more than an hour at 32°, and not freezing until reduced some degrees lower; and he informs me that he has seen the trout active in water in which ground-ice was forming at 32°. As mentioned in Note III, he infers that a heat of 120°, which hastens the coagulation of the blood, first makes it thinner and more liquid; and he agrees with Mr. Hewson, that a low temperature, which retards coagulation, renders the blood more viscid.

Mr. Hunter[l] froze some blood rapidly by subjecting it to a cold below zero; after thawing the blood, it became fluid, and then coagulated, he believed as quickly as if it had not been frozen. He found that portions of muscle removed instantly from a live frog, and from a bullock just knocked down, contracted after having been frozen and thawed in the same manner, though irritation did not produce any sensible motion in the fibres. "This," he says, "is exactly similar to the freezing of blood too fast for its coagulation, which, when thawed, does afterwards coagulate, as it depends in each on the life of the part not being destroyed." And he observed that a high temperature acts alike as an excitement to the coagulation of blood and to the contraction of muscle, "both apparently depending on the same principle, namely, life." Although Hunter never saw the life of a whole animal, which had been frozen, return by thawing, Dr. Davy[m] has shown that the

[d] Journal de Physique, 4to, Paris, 1749, tom. xliv, p. 382.
[e] Institutiones Pathologiæ, § 336, 8vo, Leid. Batav. 1758.
[f] Traité du Cœur, ii, 133, ed. 1749.
[g] Works, ed. by Palmer, iii, 26.
[h] Obs. on the Blood, 8vo, Lond. 1779, p. 34.
[i] On the Blood, ed. 1834, p. 67.
[j] Essay on the Blood, p. 21.
[k] Researches, ii, 75.
[l] Works, ed. by Palmer, iii, pp. 109-13.
[m] Researches, vol. 2, p. 121.

ligatures, and emptied that part of its blood. I next blew warm air into the empty vein, and put another ligature upon it, and letting it rest till I thought the air had acquired the same degree of heat as the blood, I then removed the intermediate ligature, and mixed the air with the blood. The air immediately made the blood florid, where it was in contact with it, as could be seen through the coats of the vein. In a quarter of an hour I opened the vein, and found the blood entirely coagulated; and as the blood could not in this time have been completely congealed by rest alone, the air was probably the cause of its coagulation.

From comparing these experiments, may we not venture to conclude, that the air (XII) is a strong coagulant of the blood,

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common medicinal leech may be restored to life after having been completely frozen; Lister, Bonnet, and others[n] have observed the same fact in the larvæ of insects; and Rudolphi,[o] in the Filaria capsularia.

Dr. Davy has proved that the blood of a fowl may be thrice rapidly frozen and thawed, and yet retain its coagulating power. He also froze some human venous blood, which coagulated in about eight minutes after it was gradually and completely thawed; and on the following day there was a distinct separation of serum from the clot. The results obtained by Mr. Thackrah[p] to the contrary, are of no weight against the mass of positive testimony above quoted. Mr. Hewson states at pp. 25-26 how he varied his experiments on the freezing of blood.

The effects of a high temperature in hastening the coagulation of blood and the contraction of muscle are mentioned in Notes III and XVI.

(XII.) It follows directly from Mr. Hewson's experiments, that though air may promote, it is not the cause of the coagulation of the blood. This he knew; for he observed that it coagulates when kept at rest in the veins of living dogs and rabbits, in the sacs of aneurisms, and in the heart and blood-vessels after death; while, in some of his experiments, he found that blood when less exposed to the air coagulated sooner than when more exposed, which he ascribed to the retarding influence of cold on coagulation. It has long been known that air may be gradually introduced into the veins of living animals without any ill effect.[a] Sproegel,[b] after killing dogs by blowing air into the veins, states that the blood was fluid, even more so than natural, contrary to the assertion which some persons had made that it is coagulated; and

[n] Carpenter's Gen. and Comp. Phys. p. 175, 8vo, Lond. 1841.

[o] Entoz. Hist. Nat. 8vo, Amstel. 1809, ii, 62.

[p] On the Blood, ed. 1834, pp. 37, 67, 68.

[a] Haller, in Boerhaav. Prelect. Acad. 8vo, Gott. 1740, ii, 108; Nysten, Recherches de Physiol. 8vo, Paris, 1811, p. 34; Majendie, Physiol. tr. by Milligan, 4th ed. 8vo, Edin. 1831, p. 445.

[b] Exp. circa varia venena in vivis animalibus inst. § xliii, Gott. 1753, in Haller. Disputat. ad Morb. t. vi, 4to, Lausan. 1758.

and that to this its coagulation when taken from the veins is chiefly owing, and not to cold, nor to rest?

But although it appears from these experiments that the coagulation of the blood in the basin is owing to the air alone;

Thackrah,[c] in similar experiments, in which the air was well mixed with the blood, says that it either remained fluid or had its coagulation retarded; but these conclusions require further examination.

Mr. Hunter's statement,[d] that blood coagulates more readily in vacuo than in the open air, is negatived by Dr. Davy's experiments,[e] from which it results that the rate of coagulating is much the same under the exhausted receiver as in the atmosphere, certainly not quicker, but rather slower, which he suggests may be owing to the cooling effect of exhaustion. From the experiments of Sir Astley Cooper and Mr. Thackrah,[f] it appears that blood, when removed from living dogs in such a manner as to cut off all communication with the atmosphere, coagulates in ten or fifteen minutes. In some similar experiments which I made, coagulation was generally, but not invariably, slower than in blood received through the air into a tube. Exclusion of air then from the blood, merely retards coagulation; and this agrees with the observations of Dr. Babington[g] and Dr. Davy[h] on the coagulation of blood covered with oil.

It results from Sir Humphrey Davy's experiments,[i] that coagulation is neither materially hastened nor retarded by nitrogen, nitrous gas, common air, oxygen, nitrous oxide, carbonic acid, or hydrocarbon. Dr. Davy's observations[j] on the effect of carbonic acid and of oxygen are similar. Neither the cause of the coagulation of the blood, nor of its fluidity in the living animal, is yet known. All that has been clearly determined is, that certain conditions or substances either hasten or retard, suspend or prevent coagulation; and some of them would appear to have directly opposite effects in different proportions, as noticed in Note VII.

The causes usually given, on the authority of Mr. Hunter,[k] as altogether destroying the coagulable property of blood and the contractility of muscle, are some of them so doubtful that they all require to be examined anew. Thus, in a man killed by lightning, Dr. Davy[l] observed some soft coagulum in the heart, and that the fingers were rigid, although the examination was not made until the body was rather advanced in putrefaction. Sir Charles Scudamore[m] invariably found the blood coagulated as usual in animals killed by electricity. Sir B. Brodie[n]

[c] On the Blood, pp. 76-7, ed. 1834.
[d] Works, ed. by Palmer, iii, 27.
[e] Researches, ii, 89.
[f] On the Blood, pp. 77, 83, ed. 1834.
[g] Med. Chir. Trans. xvi, 298.
[h] Researches, ii, 90.
[i] Res. on Nitrous Oxide, 8vo, Lond. 1800, pp. 380-1.
[j] Researches, ii, 86.
[k] Works, ed. by Palmer, iii, 34, 114.
[l] Researches, ii, 71.
[m] Essay on the Blood, p. 126, 8vo, Lond. 1824.
[n] Lectures on Pathology and Surgery, 8vo, Lond. 1846, p. 102.

for cold has no such effect, nor has rest in a sufficient degree, because the coagulation of the blood in the basin takes place in a few minutes, whilst that which is merely at rest in the veins is not completely coagulated in three hours or more. Yet the blood is in time completely coagulated merely by its being at rest in the veins; but then in this case it coagulates in a different manner from what it does in the basin; and as

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found that the irritability of the muscular fibre was not destroyed in a guinea-pig which had been instantly killed by an electric shock.

Dr. Andrew Smith[o] commonly saw coagulated blood in the hearts of antelopes run down by dogs. In a hunted hare Dr. Davy saw, he informs me, some coagulated blood. So did I in one that had been run for thirty-five minutes and then killed by the Windsor harriers; it was immediately gutted, and I examined it four hours afterwards. After pressing blood into the jugular veins, portions of them were removed and laid aside with the contained blood; it never coagulated. There were some small but distinct clots in both ventricles of the heart, all the cavities of which were otherwise empty. In the left pleura there was upwards of a quarter of an ounce of thick yet fluid blood, which never clotted; but in which, when mixed with water and filtered, some small fibrinous flakes were found. The heart was hard and contracted; the limbs, especially the hind ones, were quite rigid. Besides, I have been assured by old and observant sportsmen, that when a fox has been run very hard he stiffens quickly, in the hind limbs especially, so that you may hold out his body by them horizontally. Finally, as to a blow on the stomach: In a cat killed by a kick, which ruptured the stomach and liver, I found coagulated blood in the heart, and the limbs rigid, seventeen hours after death. Some cases in which the blood did not coagulate after death are mentioned in Note XXXIII.

Mr. Hunter,[p] conceiving coagulation to be an act of life, maintained that the blood coagulates by virtue of its living principle. If we admit this hypothesis, we must also admit that we can pickle the life (see Note VII); that it is preserved after repeated freezing and thawing (see Note XI); and, as Dr. Davy remarks,[q] that the blood may remain alive many hours after the death of the body, when the muscular fibre has lost its irritability, the limbs have stiffened, and even partial decomposition has begun (see Note X). Besides, a mixture of two varieties of perfectly clear serum will coagulate spontaneously, as I have witnessed after filtering them four days after they were drawn from the living human body (see Note XVIII); and M. Denis[r] states that fibrin may be dried and powdered, and yet possess the power of self-coagulation when dissolved in a neutral salt and diluted with water.

[o] J. Davy's Researches, ii, p. 74.
[p] Works, ed. by Palmer, iii, pp. 34, 113.
[q] Researches, ii, 122.
[r] Essai sur l'Application de la Chimie à l'Etudie physiologique du Sang de l'Homme, p. 72, 8vo, Paris, 1833.

it probably is in this way that the blood is coagulated in the body, I have been more particularly attentive to it, and have endeavoured to determine by experiment how it takes place. With this view I have several times repeated Experiment the Fourth, which was made with a view to determine whether the blood would coagulate by rest. In the first trial, the vein was not opened till the end of three hours and a quarter; and just before it was opened I had observed through its coats, that the upper part of the blood was transparent, owing to the separation of the lymph. On letting out this blood, it seemed to me entirely fluid: a part indeed had been lost, but the greatest part was collected in the cup, and which afterwards coagulated as blood commonly does when exposed to the air. From this experiment I imagined that the whole had been fluid; but from others made since, I am persuaded that the part which was lost had been coagulated; for, from a variety of trials, I now find, that though the whole of the blood is not congealed in this time by rest alone, yet a part of it is. But as it would be trespassing too much on the reader's time to relate every experiment I have been obliged to make for this purpose, I shall only mention the general result of the whole.

After fixing a dog down to a table, and tying up his jugular veins, I have in general found that on opening them at the end of ten minutes, the blood was still entirely fluid, or without any appearance of coagulation.[1] If they were opened at the end of fifteen minutes, at first sight it also appeared quite fluid; but on a careful examination I found sometimes one, and sometimes two or three small particles about the size of a pin's head, which were coagulated parts of the blood. When opened later than this period, a larger and larger coagulum was observed; but so very slowly does this coagulation proceed, that in an experiment where I had the curiosity to compare more exactly the clotted part with the unclotted, I found, after the vein had been tied two hours and a quarter, that the coagulum weighed only two grains; whilst the rest of the blood, which was fluid, on being suffered to congeal, weighed

[1] I say *in general* it was fluid at the end of ten minutes; but I must likewise mention that in one dog I found two very small particles of beginning coagulation, even at this period; yet in another I could not observe any such appearance, even at the end of fifteen minutes.

eleven grains. I can advance nothing farther in this part of my subject with precision. Nor can I pretend exactly to determine the time at which all the blood between the ligatures is coagulated. I have indeed opened such a vein at the end of three days, when I found a thin white coagulum, which was a mere film; the serum and red particles having disappeared. But the whole is undoubtedly congealed long before this period. The manner in which the blood coagulates, when at rest in the body, has appeared to me curious, and therefore I have taken the more pains to discover how it happens, especially as it may assist us in judging whether or no it coagulates in the heart, so as to form those substances called polypi (XIII). The

(XIII.) Blood generally coagulates much more slowly in contact with the living parts than when removed from them, an effect which Mr. Thackrah[a] ascribes, I believe hypothetically, to the influence of the nerves in keeping the blood fluid. Mr. Hunter[b] noticed that the contact of blood with living vessels retards its coagulation. In the experiments of Sir Astley Cooper and Mr. Thackrah[c] blood kept at rest in a living vein did not coagulate in an hour, while blood similarly confined in a dead vein, excluding air, was firmly concreted in a quarter of that time; and blood from the same dogs, cats, and rabbits, used in Mr. Thackrah's experiments, began to coagulate in from two to four minutes when abstracted and set apart in the usual manner.

I made some experiments at Chatham, in August, 1837, on the coagulation of blood confined in the jugular veins of living dogs, disturbing the parts no more than was necessary to apply the ligatures. Twice only, out of many trials, were small clots observed as early as two hours after the operation. Coagulation generally did not commence before the expiration of three hours; half an hour later there was commonly a central clot about one fourth the size of the imprisoned blood; and the remaining three fourths coagulated in two, six, and seven minutes, in three different instances, when received into a watch-glass. In one trial, the confined blood was wholly fluid at the end of five hours; in another, after seven hours, the clot was no larger than that just mentioned, and the fluid part of the blood coagulated on exposure in five minutes. In nine hours the blood was completely coagulated in the right vein of one dog, and only partially so in the left vein of another; and the fluid portion concreted when exposed in less than eleven minutes. After eighteen hours, in two trials the blood was about half coagulated, and in a third, completely so. At the end of twenty-four hours, coagulation was complete in four trials, and incomplete in one.

The pale masses of fibrin called polypi are formed in the heart after

[a] On the Blood, ed. 1834, pp. 80, 91.
[b] Works, ed. by Palmer, iii, 29.
[c] On the Blood, ed. 1834, p. 85; and Prater, p. 21.

above-mentioned times will, I believe, be found to be those at which the blood congeals in the veins of healthy dogs: and as I have found, by experiments, that the blood of a dog and of the human subject in health jellies out of the body, nearly in the same time, that is, it begins in three or four minutes, and is completed in seven or eight; I should therefore conclude that the blood in the veins of the human body coagulates nearly at the same period with that of a dog (XIV). But it may be necessary to add here that, from experiments which I have made, I have reason to believe that the time at which the blood coagulates, is different in different constitutions, and

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death, the sinking of the corpuscles being favoured by their increased tendency to aggregation in connexion with disease, and by the slowness with which the blood coagulates (see Notes X and XXIII). Mr. Paget[d] has shown that the disposition of the parts of the blood in the dead body is the same as if the blood had coagulated as slowly and separated in a basin; and that the clot is so regularly pale in its upper part, and coloured by the red corpuscles at the bottom, that the fact might be available in medico-legal inquiries as to the position of the body in the first hours after death. But the separation of the corpuscles is often imperfect, or does not take place at all, as I have witnessed in some cases of sudden death. Dr. Davy[e] found the clot broken up in the ventricles, as if its coagulation had preceded death. The relative position of the fibrin and coloured matter in the heart and great vessels after death, did not escape the notice of the accurate Petit.[f]

Fibrinous clots are occasionally formed in the heart and blood-vessels, and become softened there, during life. The softening is generally, but not always, in the centre of the clot. Coagulation of blood in the veins, often followed by softening of the clot, is a common disease in the human subject, especially in connexion with œdema of the lower limbs. This softening of fibrin is very frequent in the veins of horses. It does not appear to be of an inflammatory nature, though I apprehend that it has been generally confounded with phlebitis.[g]

(XIV.) I have found that the blood of a dog generally coagulates rather sooner than that of man. Mr. Thackrah[a] infers from his experiments that coagulation commences sooner in the blood of small and weak animals than in that of the large and strong. Dr. Blundell[b] states that the blood of the dog, sheep, and ox, coagulates sooner than the human blood.

[d] Lond. Med. Gaz. i, 1840-1, p. 613.
[e] Researches. ii, 196.
[f] Hist. de l'Acad. Roy. des Sciences, année 1732, p. 393.
[g] On the Softening of Fibrin, Med. Chir. Trans. vol. xxii.
[a] On the Blood, ed. 1834, p. 154.
[b] Med. Chir. Trans. ix, 67.

in different diseases. For though the blood of a person in health is completely coagulated in seven minutes after it is taken out of the veins, yet in some diseases, I have found the blood fifteen or twenty minutes, nay, even an hour and a half, before it was completely jellied.

As we see in the above-related experiments, that the blood coagulates in the body when suffered to rest for a little time, is it not probable that to this cause its coagulation in those true aneurisms, which are attended with a pouch, are owing?[1] For in such enlargements a part of the blood is without motion, which will congeal when at rest, and in contact with the sac; and thus one layer may be formed; and the sac afterwards enlarging, another portion of the blood will then be at rest; and so a second layer may be formed; and thence probably is the origin of those laminated coagula met with in such sacs.

Likewise, to the blood's being at rest, is probably owing its coagulation in the large arteries which are tied after amputation, or other operations; for after most of such ligatures there will be a part of the artery impervious, in which the blood can have no motion. The coagulum after amputation might indeed be supposed owing to air; but, considering the manner in which arteries are tied whilst the blood is running from them, it does not seem probable that the air has any effect on what is above the ligature.

To the blood's being without motion in the cavity of the uterus, is its coagulation therein probably owing; hence the origin of those large clots which sometimes come from that cavity; and which, when more condensed by oozing out of the serum, and of the red globules, assume a flesh-like appearance, and have often been called moles or false conceptions.

In Experiment the Fifth, we found that the blood could be frozen and thawed again, without being coagulated: this likewise is an experiment which I have repeated several times, that I might be sure of the fact. I have also varied the experiment by sometimes putting the vein into a phial of water, and freezing the whole in a solution of sal ammoniac in snow; and sometimes I have put the vein into the solution itself; and

[1] An instance of which may be seen in the Med. Obs. and Inq. vol. i, article xxvii, fig. iii.

three or four times I have thrown it into oil, and then frozen it; but after all these trials, the result was the same (XV). The blood was always evidently fluid on being thawed, and as evidently jellied when exposed to the air.

Besides being coagulated when exposed to the air, the coagulable lymph, as well as the serum, is known to be fixed by heat; but the degree of heat has not, so far as I know, been determined. It has been supposed to require a degree almost equal to that which coagulates the serum;[1] but one much less is necessary, as will appear from the following experiments.

EXPERIMENT VII.

Having found, from a number of trials, that blood, kept fluid by means of true Glauber's salt, had its lymph coagulated by a heat of 125° of Fahrenheit's thermometer, I supposed that the degree necessary for fixing it in its natural state could not be very different from this. I therefore prepared a lamp furnace with a small vessel of water upon it; this water was heated to 125°; and then laying bare the jugular vein of a living dog, I tied it properly, cut a piece of it out, and put it into this water: after eleven minutes, I took out the vein, opened it, and found the blood entirely coagulated: thence I concluded, that 125°, or less, was sufficient to coagulate the blood of a dog. It may be necessary to observe here, that the part coagulated was only the lymph; for the serum requires a much greater heat to fix it, that is, a heat of 160°, as will appear hereafter.

EXPERIMENT VIII.

The same experiment was repeated in such a manner, that the heat never went higher than 120½°; and I found that, on opening the vein at the end of eleven minutes, the lymph was entirely coagulated, even in this heat.

EXPERIMENT IX.

I repeated the experiment with a heat no higher than 114°, and at the end of eleven minutes, the vein being opened, the

[1] See Traité du Cœur, t. ii, p. 93; Schwenke Hæmatolog. p. 138.

(XV.) See Note XI, where the effects of cold and of freezing on coagulation are detailed.

blood was found to be fluid, and in a few minutes after, being laid open to the air, it coagulated as usual. Now, as the blood in the last experiment was coagulated when the heat had never risen above $120\frac{1}{2}°$; and in this experiment was fluid, though it had been exposed to a heat of 114°; we may therefore conclude that the coagulable lymph in the blood of a dog in health is fixed in a degree of heat between 114° and $120\frac{1}{2}°$ of Fahrenheit's thermometer.

As to the degree of heat at which the lymph in human blood coagulates, I have not yet had an opportunity of trying it in a more satisfactory way, than with the mixture with Glauber's salt, in which state it coagulates at 125°. But, as we find that the human blood and that of a dog jelly nearly in the same time, when exposed to the air, I think it probable that the precise degree of heat at which the lymph of the human blood coagulates, is between 114° and $120\frac{1}{2}°$ (XVI). I have thought of making the experiment on the umbilical cord of a recent placenta, which perhaps is the most likely way of coming at the truth.

The degree of heat, at which the serum of the blood (which should not be confounded with the lymph) coagulates, is generally said to be 150°; but from my experiments I am inclined to believe it requires a greater heat to fix it. They were made in the following manner:

(XVI.) In my experiments, mentioned in Note III, a mixture of liquor sanguinis and a neutral salt appeared to be affected by heat only like a very concentrated solution of albumen. When diluted with water, and kept at a heat of 125° or 126°, there was no precipitation of albumen; but the self-coagulating power of the mixture was completely destroyed. These results agree essentially with those of Mr. Prater.[a] The heat of some animals approaches 114°. In some healthy birds at Ceylon, Dr. Davy[b] found it 111°; and 113° under the left ventricle of the heart in a soldier at Malta, three hours and a half after death, in which case the blood was liquid, and yet coagulated, as Dr. Davy informs me, on exposure to the air.

We want some exact observations on the temperature of fishes in hot springs. Sonneret is reported to have seen fishes, apparently not incommoded, in a spring as hot as 187° Fahrn., in the island of Lugon, one of the Mantilles; and Cuvier received some fishes from the waters of Cassa and Tozer, which are said to be as high as 170° Fahrn.[c]

[a] Exp. Inq. in Chem. Physiol. part i, p. 114.

[b] Researches, i, 186-7, 230.

[c] See Dr. Hodgkins's tr. of Edwards on Physical Agents, 8vo, Lond. 1832.

EXPERIMENT X.

I took a wide-mouthed phial, containing some serum, and placing a thermometer in it, I put it into water which was kept warm by a lamp underneath; and, in making this experiment with as much accuracy as I could, I found the heat required (XVII) was 160°; which is about 40° more than is necessary for the coagulation of the lymph.

(XVII.) The temperature required to coagulate the serum of different animals is not the same. Dr. Davy[a] observed a variation of 10° in the coagulating point of the serum of mammals; and it appears to vary with the proportion of water in the serum, which may be indicated by its specific gravity. Serum, too, first becomes opalescent and then viscid at lower heats than are required to make it coagulate firmly, as I have learned from experiments communicated to me by Dr. Davy, and from others made by myself. He found that sheep's serum coagulated at 168°—170°, and at 172°—174° when diluted with one third of water; that common salt, magnesia, carbonate of potash, and oxalic acid had very little effect on the heat required to coagulate the serum; that its coagulation was prevented, even at 212°, by an excess of acetic, citric, or tartaric acid; that super-tartrate of potash promoted coagulation; and that it was retarded but not prevented by lime. Calf's serum he found coagulated at 178°—180°; pig's serum at 158°, at 180° when diluted with an equal part of water, and with a sixth part of water was but slightly thickened at 160°. The same serum after having been kept six days, but quite fresh, had its colour less readily affected by heat.

Serum, sp. gr. 1031, gradually heated in a test-tube in water, from the blood of a healthy horse, I found affected as follows: at 153° slightly opalescent in reflected light; at 155° distinctly so; at 157° viscid; at 158° a jelly, not breaking when inverted, semi-transparent in transmitted light; at 164° a firm jelly, nearly opake in transmitted light; at 168° quite opake and very firm. Kept for above two hours at 120°—130,° it became glairy and viscid, remaining quite transparent. Nitre had little effect on the coagulation. But it was promoted by Glauber's salt; added in excess to the serum, this became white and opake at 144°, and curdled throughout at 146°; an ounce of serum, with seventy grains of the salt, kept fifteen minutes at 144°, became a thin semi-transparent pulp, and for fifteen minutes more at 146°, a feeble and quite opake jelly. Serum, sp. gr. 1033, from a man aged 40, blooded for pneumonia: at 156° rather opalescent in reflected light; at 158° liquidity impaired, and after forty minutes a tender jelly; at 159° coagulated firmly after twenty-six minutes, opalescent in reflected light, and quite transparent in transmitted light; at 169°, just the same: at 212° quite opake. The blood was not buffy. Pellucid serum from a hydrocele, sp. gr. 1024, became opalescent at 150°; coagulated at 160°; quite opake at 168°.

[a] Researches, Physiol. and Anat. ii, 95.

As the blood is coagulable by heat, and as the heat of an animal is increased in fevers, it has been supposed that the blood might be coagulated by the animal heat, even whilst it is circulating in the vessels; but there is little foundation for such an opinion; since the animal heat is naturally only 98° or 100°, and in the most ardent fever is not raised above 112°. (XVII*).

Serum, sp. gr. 1029, from buffy blood, tried at the same time for comparison, coagulated at 160°; was semi-transparent at 170°, but forming a much firmer clot than the serum of the hydrocele at a higher temperature. The serum from the blood was quite opake at 212.°

(XVII*) See Note XVI.

CHAPTER II.

OF THE INFLAMMATORY CRUST, OR SIZE.

I SHALL next proceed to inquire into the formation of the inflammatory crust, or size, as it is called.

This remarkable appearance is frequently met with in inflammatory disorders, and is formed by the coagulable lymph's being fixed, or coagulated, after the red particles have subsided. It has indeed been supposed to be formed from the serum of the blood; and an excellent writer on this subject seems in doubt to which of the two it should be attributed (XVIII). But that it is formed by the coagulable lymph alone, after the red particles have subsided, appears from the following experiments.

(XVIII.) Schwenke[a] thought that the buffy coat might be intermediate to the serum and cruor. Gaubius, referred to in the Introduction, says that the serum may be spontaneously converted into fibre, so as to form the buffy coat; Van Swieten[b] asks whether that part may not be formed from the serum; Gaber[c] concludes that the pleuritic crust is the same as the coagulable part of the serum; and Huxham[d] supposed that the febrile heat turns the serum into the buffy coat. Haller[e] describes the serum as being spontaneously coagulable, forming the buffy coat of the blood, polypi of the heart, and false membranes. A notice is given in the Introduction of the opinion entertained by many eminent men that the fibrin is not distinct from the serum; of others that coagulation is owing to an aggregation of the red corpuscles; and further, that these corpuscles are transformed into fibrin (see Notes I and CXVIII).

The fact probably is, as Liebig[f] states, that fibrin and albumen are isomerical substances, that is to say, they may, under certain influences, be suddenly converted into each other, as is the case with starch and sugar, their elements being the same and in the same proportions. The sudden changes in the properties of the blood during its evacuation

[a] Hæmatologia, pp. 157-8, 8vo, Hagæ, Com. 1743.

[b] Comment. Boerhaave, Aph. § 384.

[c] De Humor. Animal. Mélanges de Phil. et de Math. de la Soc. Roy. de Turin, t. iii, p. 67.

[d] Essay on Fevers, pp. 5, 36, 6th ed. 8vo, Lond. 1769.

[e] Elem. Physiol. t. ii, pp. 125-6; Prim. Lin. Physiol. § 138, 8vo, Gott. 1780.

[f] Familiar Letters on Chemistry, p. 80, 12mo, Lond. 1843.

EXPERIMENT XI.

In the month of June, when the thermometer in the shade stood at 67°, I bled a man who had laboured under a phthisis pulmonalis for some months, and at that time complained of a pain in his side. The blood, though it came out in a small stream, yet flowed with such velocity, that it soon filled the basin. After tying up his arm I attended to the blood, and observed that the surface became transparent, and that the transparency gradually went deeper and deeper, the blood being

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are favorable to this view; but Mülder[g] asserts that there is a difference between albumen and fibrin of one equivalent of sulphur in every ten equivalents of protein. Denis[h] supposes liquid albumen to be a salino-alkaline solution of fibrin, a portion of which, precipitated during coagulation, he thinks is the fibrin of the blood. But it is well known[i] that serum is not rendered turbid by many acids. Denis says that 1000 parts of water, charged with 100th part of nitre, and 1000th of soda, dissolves about 700ths of fibrin, producing a fluid with all the properties of serum. M. Wurtz[j] gives an account of a mode of preparing albumen which is soluble without the aid of an alkali or salt.

Dr. Buchanan,[k] like the older observers, regards fibrin as a mere modification of serum; and the results of his experiments, in which a mixture of serum from blood and serum from a serous cavity was found to coagulate spontaneously, appear to me fully to warrant his conclusion, which is not inconsistent with Liebig's, and to afford a simple explanation of the formation of some of the most important healthy and morbid products. In mixtures of serum of blood with serum either from the pericardium, pleura, or vaginal tunic of the testicle, I have seen the coagulum assume various forms, not distinguishable from more permanent structures. Sometimes it would be apparently a homogeneous transparent jelly, either contracting but slightly or not at all, or so greatly as to form less than a third of the bulk of the serum; often presenting the character of a closed membrane of the utmost delicacy, with processes running towards the centre, and forming cells for the contained fluid: and occasionally a portion of the clot would be white and nearly opake, somewhat resembling the cicatricula of a fowl's egg. The ultimate structure of the membranous part was composed of most delicate fibrils, as stated in Note VIII; and faint round molecules of extreme minuteness were interspersed here and there. I may add that in several trials the fluids were filtered both before and after mixing; and that it was clearly ascertained that neither of them would coagulate spontane-

[g] Chemistry of Vegetable and Animal Physiology, 8vo, part ii, p. 314.

[h] Essai sur l'Application de la Chimie à l'Etude physiol. du Sang de l'Homme, p. 81, 8vo, Paris, 1838.

[i] J. Davy's Researches, ii, 93 et seq.

[j] Mr. Paget's Report, Br. and For. Med. Rev. vol. 21, p. 541.

[k] London Medical Gazette, xviii, pp. 50 et seq.

still fluid. I likewise observed that the coagulation first began on the surface, where it was in contact with the air, and formed a thin pellicle; this I removed, and saw that it was soon succeeded by a second. I then took up a part of the clear liquor with a wet teaspoon, and put it into a phial with an equal quantity of water; a second portion I kept in the teaspoon; and I found afterwards that they both jellied or coagulated, as did also the surface of the crassamentum, making a thick crust. On pressing with my finger that portion which was in the teaspoon, I found it contained a little serum.

From this experiment it is evident, that the substance which formed the size was fluid after it was taken from the vein, and coagulated when exposed to the air; and as this is a property of the coagulable lymph alone, and not of the serum, there can be no doubt that the size was formed of the lymph (XIX).

The following experiment, made on the blood, without exposing it to the air, likewise proves the same fact.

EXPERIMENT XII.

Immediately after killing a dog, I tied up his jugular veins near the sternum, and hung his head over the edge of the table, so that the parts of the veins where the ligatures were might be higher than his head. I looked at the veins from time to time, and observed that they became transparent at their upper part, the red particles subsiding. I then made a ligature upon one vein, so as to divide the transparent from the red portion of the blood; and opening the vein, I let out the transparent portion, which was still fluid, but coagulated soon after. On pressing this coagulum, I found it contain a

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ously alone, nor after dilution merely with distilled water. The blood-corpuscles, as mentioned in Note CXVIII, cannot be the immediate agents in the formation of fibrin in these mixtures of serum. Dr. Buchanan[1] has since found that the liquid of hydrocele will coagulate when there is added to it either some washed blood-clot, shreds of the fresh buffy coat, the same dried and powdered, the transparent clot from a blister, or muscular fibre and other tissues. He and Dr. Anderson are now satisfied that the fibrin exists in the animal fluids in a state of solution.

(XIX.) I know not that this simple and convincing experiment was ever before made. The claiming of it by Professor Owen for Mr. Hunter has been noticed in the Introduction.

[1] Lond. Med. Gaz. 1845, vol. i, pp. 617 et seq.

little serum. The other vein I did not open till after the blood was congealed, and then I found the upper part of the coagulum whitish like the crust in pleuritic blood.[1]

And that the size is merely the coagulable lymph separated by the subsidence of the red particles, will appear evident to any person who will, as Sydenham (xx) directs, move a finger or a teaspoon through the blood when he observes its surface becoming transparent; for in this case the blood that otherwise would have been sizy, will now have a natural appearance, or be without size; from the red particles being prevented from subsiding.

It has been a very generally received opinion, that inflammation thickens the blood, and makes it more ready to coagulate. Nay, some have gone so far as to say, that in those disorders where the inflammatory crust is seen, the blood is almost coagulated even before it is let out of the vein. Now I am persuaded from experiment, that the contrary of this is true; or that inflammation, instead of increasing the disposition of the blood to coagulate, really lessens it (xxi); and instead of

[1] This is not the only apparently healthful animal whose blood had a crust; I have seen it in others: whence I at first suspected that merely keeping the blood fluid for a little time was sufficient to produce this appearance; but I altered my opinion on seeing that in the greatest number of animals it did not occur; nor is it commonly met with in the hearts of those persons who die a violent death, though the blood remains longer fluid in such cases, than it does in the basin where the size appears.

(xx.) Sydenham, Opera Omnia, ed. Soc. Sydenhamianæ, 8vo, Lond. 1844, sect. 6, cap. iii, p. 248. In this case the proportion of fibrin would be the same, as the red corpuscles are only prevented from sinking by stirring them with the liquor sanguinis till it becomes viscid. And even then, according to M. Denis,[a] the separation of the corpuscles and fibrin is not entirely prevented; for he states that the buffy part is diffused throughout the clot in the form of whitish granulations; and such, too, he says, is the case when the buffy coat is prevented from forming, as he maintains, in blood taken away during syncope. After stirring up horse's blood, just when the buffy coat began to form, and preventing its formation, I did not find any buffy granules on the cut surfaces of the clot.

(xxi.) It has been generally supposed that the coagulation of the blood in inflammation is so retarded as to cause or favour the sinking of the corpuscles, and the consequent formation of the buffy coat. Mr.

[a] Essai sur le Sang, p. 287, 8vo, Paris, 1838.

thickening the blood, really thins it; at least that part which forms the crust, viz. the coagulable lymph.

Hunter,[a] Dr. George Fordyce,[b] and Dr. B. G. Babington,[c] adopted this view. Yet some recent observations are anticipated in the following passage from Mr. Hunter's Surgical Lectures, vol. i, referred to below;[a] speaking of the blood in inflammation, he says, "The blood has an increased disposition to separate into its component parts, the red globules become less uniformly diffused, and their attraction to one another becomes stronger, so that the blood when out of the vessels soon becomes cloudy or muddy, and dusky in its colour, and when spread over any surface, it appears mottled, the red blood attracting itself and forming spots of red." See Note XXIII.

I have repeatedly seen a clear layer of liquor sanguinis, about one eighth of an inch deep, appear on the surface of the blood of the horse in two minutes, after which the sinking of the red corpuscles went on with increased speed. In short, as mentioned in Note III, *i*, *k*, I believe that the slow coagulation often occurring in blood drawn during inflammation, so far from being the cause, is an effect of the separation of the fibrin and corpuscles. In two trials, I found that the liquor sanguinis remained longer fluid when separated from the corpuscles than when mixed with them.

Mr. Hewson[d] knew that the buffy coat does not necessarily follow from slow coagulation; and Dr. Davy[e] observed that in certain cases in which the inflammatory diathesis is best marked, the separation of the red particles from the blood drawn is most rapid, often occurring in one or two minutes; and that in some diseases, particularly erysipelas, the blood taken from a vein coagulates as rapidly as healthy blood, and yet exhibits the buffy coat. The observations of Schrœder Van der Kolk and Dr. Alison,[f] of Professor Hermann Nasse,[g] and of Mr. Wharton Jones,[h] are to the same effect generally. Dr. Stoker[i] has shown that blood which coagulates in one, three, and five minutes, may have a buffy coat; and yet that it may be absent from blood that is ten, twenty, or even forty minutes in coagulating. Results of the same kind from my experiments are mentioned in the 'Edin. Med. and Surg. Journal,' v. 64, p. 370.

Mr. Hewson, it will be observed, always states that in inflammation the coagulable lymph is thinned or attenuated; never that it is specifically lighter; yet he is often erroneously represented to have maintained the latter view. The consistency of the liquor sanguinis, and the state of the red corpuscles leading to the formation of the buffy coat, will be discussed in Notes XXIII and XXIX.

[a] Works, ed. by Palmer, i, pp. 234-5, 381; iii, 356.

[b] Elements of the Practice of Physic. 2d ed. 8vo, Lond. 1768, part ii, p. 30.

[c] Cyclopædia of Anatomy, i, 419.

[d] See his Exp. XVI, and XXVII.

[e] Phil. Trans. 1822, p. 271; Researches ii, 48.

[f] Out. Physiol. and Path. p. 47, 8vo, Edin. 1833.

[g] Henle, Anat. Gener. tr. par Jourdan, i, 468.

[h] Edin. Med. and Surg. Journ. lx, 309.

[i] Pathological Observations, 8vo, Dublin, 1823, pp. 37, 44.

In the first place, that inflammation really lessens the disposition to coagulate, will appear evident to every one who attends to the jellying of such blood as has a crust. For in all those cases the blood will be found to be longer in congealing, than in its natural state. To this opinion I was first led by attending to the phthisical patient's blood above mentioned; but I have since made a comparison, which seems to prove the fact. For, from a variety of experiments made on the blood of persons nearly in health, or at least who had no inflammatory disorder, and no crust on their blood, I found that after being taken from a vein, it began to jelly in about three minutes and a half. The first appearance of coagulation was a thin film on the surface near the air-bubbles, or near the edge of the basin; this film spread over the surface, and thickened gradually till the whole was jellied, which was in about seven minutes after the opening of the vein; and in about ten or eleven the whole was so firm, that, on cutting the cake, the gashes were immediately filled up by the serum, which now began to separate from the crassamentum. But in those persons whose blood had an inflammatory crust, the coagulation was much later; as will appear from the following experiments.

EXPERIMENT XIII.

I bled a woman who was seven months gone with child, and the blood was received into a basin. In five minutes after the vein was opened, a film first appeared; but this spread so slowly, that in ten minutes it did not cover the whole surface; in fifteen minutes it had nearly spread over the surface; but the rest of the blood was quite fluid, at least for some depth, and even in half an hour it was not so firmly jellied as it was afterwards. In this case there was a very thick and strong crust or size.

EXPERIMENT XIV.

Having bled a person with a violent rheumatic pain in his breast, the blood was received into three teacups, and each of them had afterwards a crust. In the first I observed the progress of the coagulation as follows: The beginning of the coagulation was not marked, but at the end of half an hour the film was not thicker than common writing-paper; and this being removed, a little of the clear lymph was taken up with a wet teaspoon, put into a clean cup, and was twenty minutes more in coagulating. Even at the end of an hour and a half, the whole of the blood was not jellied; for at this time I removed the film or pellicle, and took up a second portion of clear lymph with a spoon, and put it into a teacup, where it jellied afterwards; though this jelly was not indeed quite so firm as the crassamentum itself.

EXPERIMENT XV.

A woman, with a slight inflammation in her throat, had eight ounces of blood taken from her arm; the blood was received into a basin, and the bleeding finished in four minutes and three quarters, when a film began to form near the air-bubbles; in seven minutes a transparent size appeared over a considerable part of the surface which was quite fluid, whilst the rest of the blood was coagulating, there being now a very distinct red crust over the rest of the surface.

Now, from comparing these experiments with what has been observed of the coagulation of the blood, where there is no inflammatory crust or size, is it not evident that the blood remains longer fluid after being exposed to the air, and has less disposition to coagulate, in those cases where there is a size, than where there is none? for where there was none, it was found to coagulate completely in seven minutes; but in one of the others, where the size was very thick, it did not completely coagulate in less than an hour and a half.

The power that inflammation has in lessening the disposition of the lymph to coagulate is likewise plain from the following experiment, where the blood in the heart of a dead animal seems to have congealed very slowly.

EXPERIMENT XVI.

A dog was killed eight hours after receiving a large wound in his neck. The wound had during this time inflamed considerably. Upon opening him next morning, when he had been dead thirteen hours, a large whitish polypus was found in the right ventricle of his heart; under this was a little blood still fluid, which being taken up with a teaspoon, coagulated soon after being exposed to the air.

It may be proper to observe here, that in the hearts of animals which had died without any inflammation, I have found the blood entirely coagulated long before this time. And that from opening them at different times, I have seen it coagulate in their hearts after death, in the same gradual manner that it does in their veins, when its motion is stopped by ligatures; as related in page 22.

In the next place, that the blood is really attenuated (see Notes XXI, XXIII, and XXIX) in inflammatory disorders, where the whitish crust or size appears, is probable from the following circumstances: 1st. It even seems thinner to the eye. 2d. The red particles or globules subside sooner in such blood than in that of an animal in health. This seems proved by observing that in the above-mentioned experiments, where the blood was at rest in the veins, it was not covered with a crust, except in one or two instances, though in all those cases it remained longer fluid than the blood commonly does in a basin, after bleeding, where the crust appears. And again, the blood in the heart of an animal that dies a violent death, is not generally covered with a white crust, notwithstanding it is so late in being congealed (see Note XIII). These circumstances show, that something more than merely a lessened disposition to coagulate is necessary for the forming of the crust or size. 3d. The globules more readily subside in inflammatory cases, from the surface of the whole mass of blood, than they will afterwards do from the surface of a mixture with the serum alone, of which the following experiments are a proof. But, before I relate them, let me observe, that they were made with a view to discover whether the inflammatory crust could be owing to any other cause than to the attenuation of the coagulable lymph, and to its disposition to coagulation being lessened: and as the same appearance might be suspected to arise from an increased specific gravity in the red particles, or from the serum alone being attenuated, I endeavoured to decide the question in the following manner:

EXPERIMENT XVII.

Into a phial, marked A, I put an ounce of the serum of the blood of a person whose crassamentum had an inflammatory crust.

Into another, marked B, I poured an ounce of the serum of a person whose blood had no crust; then to each of these, I added a teaspoonful of serum, loaded with the red particles of a person whose blood had no inflammatory crust or size. In attending to them, I could not observe that the red particles subsided at all sooner in the serum of the blood that had a crust, than they did in the serum of that blood which had no crust. Thence, I conclude, that the serum (XXII) is not attenuated in those cases where the inflammatory crust appears.

(XXII.) Mr. Hunter[a] concluded that the red corpuscles of the blood during inflammation are increased in specific gravity, and the serum diminished in specific gravity. Mr. Wharton Jones[b] found that the corpuscles of buffy blood subside more rapidly in its serum than the corpuscles of blood not buffy do in its serum. But both the consistency and specific gravity of a fluid may be incorrectly indicated by the pace with which these corpuscles fall through it, as explained in Note XXIII.

The serum of the blood in inflammation is very variable in density. Mr. Thackrah,[c] finding, in some trials, that a marked increase in the proportion of fibrin was attended by a decrease in the solid matter of the serum, supposed, like the older observers referred to in the Introduction and in Note XVIII, the fibrin of buffy blood to be formed at the expense of the albumen. Dr. B. G. Babington[d] was at one time led to believe that the serum of buffy blood is always deficient in its due proportion of albumen; but he has since found this not to be the case, having met with blood thickly buffed, of which, in one specimen, the specific gravity of the serum was 1024, and in another 1040. Dr. Traill[e] found about double the usual proportion of albumen in three specimens of milky serum taken from patients labouring under inflammatory diseases. Gendrin[f] states that the serum of blood in inflammation is made more viscid by an excess of albumen, and that the serum expressed from the buffy coat contains a greater proportion of albumen than the rest of the serum. Andral[g] says he agrees with Traill and Gendrin that the proportion of albumen is nearly doubled in the serum of buffy blood. Thackrah[h] also inferred that the serum separating first from the clot, contains less solid matter than the serum which separates last.

In some observations, I found, like Dr. Davy,[i] but little connexion between the density of the serum and the presence of the buffy coat. Thus, in a man aged 33, affected with pulmonary catarrh, the blood thickly buffed, the specific gravity of the serum was 1024; in a man aged 25, blooded for iritis following venereal disease and the use of mercury, the blood slightly buffed, the serum was 1029; in a man aged 19, labouring under common continued fever, the blood thickly buffed, the serum was 1034; in a man aged 30, with jaundice, the blood thickly buffed, the serum was 1035; in a boy aged 4, the blood moderately buffed, the serum was 1025. Serum from very thickly buffed blood of a healthy mare was found to be 1031, and from similar blood of a

[a] Works, edited by Palmer, iii, 356.
[b] Br. and For. Med. Rev. xiv, 590.
[c] On the Blood, p. 212, ed. 1834.
[d] Cyclopædia of Anatomy, i, 419.
[e] Annals of Philos. 1823, vol. v, 199.
[f] Hist. Anat. des Inflammations, tom. ii, pp. 442-3, 8vo, Paris, 1826.
[g] Anat. Path. t. i, p. 534, 8vo, Paris, 1829.
[h] On the Blood, ed. 1834, pp. 41, 232.
[i] Researches, ii, 34-37.

Lastly, to see whether the specific gravity of the red globules was increased, I proceeded as follows:

EXPERIMENT XVIII.

I poured into a phial C, a portion of the serum of the blood which had no crust; and likewise into another D, a second portion of the same serum. I then added to C a teaspoonful of the same serum, loaded with red particles from the blood which had an inflammatory crust. And into D, I poured a teaspoonful of the same serum, loaded with the globules of that blood which had no crust. In viewing these, I could not observe that the globules of the blood which had an inflammatory crust subsided sooner than those of the blood which had none: whence I inferred, that the specific gravity of the red particles, or globules as they are called, is not increased in those cases where the crust appears. And, therefore, since that inflammatory crust or size seems neither owing to the serum's being attenuated, nor to an increased specific gravity in the red particles, it probably depends solely upon a change in the coagulable lymph. And what seems farther to confirm this inference, in none of these experiments did the red particles subside from the surface of the serum in twenty minutes, though, where the crust appears, they subside from the surface of the blood in half that time; so that the whole mass of blood seems to be thinner than the serum alone; or, the coagulable lymph seems to be so much attenuated in these cases, as even to dilute the serum, which at first sight appears a paradox.

May we not, therefore, conclude, that in those cases where the inflammatory crust appears, the coagulable lymph is thinner,

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healthy gelding 1035. In both trials with the blood of the horse, the serum first exuding from the clot, the serum last exuding from the clot, and the serum obtained from the buffy coat, by pressure through a linen bag, were all ascertained to be of the same specific gravity, contrary to what might have been expected from the observations of Thackrah and Gendrin. The serum was in every instance filtered before it was weighed, and its specific gravity noted at a temperature of 60°. Mr Hunter[j] found that the serum of blood from a person with an inflammatory complaint, and serum of blood in a case not inflammatory, were nearly the same respecting coagulation and the quantity of serosity, or matter not coagulable by heat. The coagulating point of serum varies with its proportion of albumen. See Notes III, *g*, XVII, and LII.

[j] Works, edited by Palmer, iii, 51.

and its disposition to coagulation is lessened? both of which circumstances contribute to the subsiding of the red globules from the surface of the blood, which then coagulating gives rise to this appearance, called the inflammatory crust or size, in the blood of pleuritic or rheumatic patients.[1]

How contrary to the conclusion, which these experiments lead us to, are the opinions of some medical writers on this subject! How frequently do we find it said, that the blood is thicker in inflammatory disorders, where that size occurs; and that a large orifice is necessary to let out the vitiated blood! That a large orifice is preferable to a small one in many cases, where such blood is found, I believe to be true; but that its advantages are owing to its letting out the thickened blood, seems improbable from what we have seen in the experiments above related: they are perhaps nearer the truth, who attribute it to the suddenness of the evacuation.

It may be proper to observe here, that this size or whitish crust is not a certain sign of inflammation (XXIII); it being

[1] This remarkable appearance might indeed be accounted for, by supposing that the lymph had ascended to the surface of the blood in those cases; but this is improbable, from considering that, in its coagulated state, it is of greater specific gravity than the serum, and sinks in it.

(XXIII.) That inflammation is not certainly indicated by the buffy coat is shown by the fact that this part forms regularly on the blood of the horse, as was probably known to Harvey.[a] It seems to have been scarcely known to Dr. Simon,[b] though noticed by Dr. Allan Thomson,[c] and more particularly by Andral,[d] Gavarret, and Delafond; the last three writers ascribe it, and the formation of the buffy coat generally, to an excess of fibrin in relation to the other parts of the blood, as likewise did some of the older observers referred to in Note I. The buffy state of the blood from women far advanced in pregnancy is well known; and Mr. Hey[e] observed the great abundance of fibrin in such blood.

Mr. Hewson inferred that the buffy coat is owing to an attenuation of the liquor sanguinis, because he found in his ingenious and I believe original experiments, that the red corpuscles sink more quickly in it than in the serum alone. The fact is undoubted; but the inference is probably incorrect. The rapid sinking of the corpuscles during the formation of the buffy coat arises simply from their clustering in such blood, and not from any thinning of the liquor sanguinis nor from retarded coagulation (see Notes XXI and XXIX). The collection of the corpuscles into piles in blood not buffy is well known (see Notes C and CIX); in

[a] De Generatione Animalium, 4to, Lond. 1651, p. 160.

[b] Animal Chemistry, i, pp. 115, 292, 340, tr. by Dr. Day, for the Sydenham Society, 8vo, Lond. 1845.

[c] Syllabus of Lectures on Physiology, p. 13, 8vo, Edin. 1835.

[d] Hematol. Pathol. pp. 28 et seq. 8vo, Paris, 1843.

[e] On the Blood, p. 66, 8vo, Lond. 1779.

often met with where there seems to be no such disease, in particular in the blood of pregnant women. And that it differs much in density in different cases; in some it is extremely firm, in others it is spongy or cellular, and contains much serum in its cells. These diversities we shall endeavour to explain hereafter, when we have laid before the reader some more observations on the coagulation of the lymph.[1]

[1] Although this essay has been so lately printed, yet most of the facts which occur in the preceding pages have been mentioned in my 'Anatomical Lectures,' ever since the year 1767; and some of them were mentioned publicly even before that time. This I thought necessary to observe, because many of them have since appeared in other publications.

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buffy blood they are still more closely connected, almost as if fused together, and the piles run into little clumps often visible to the naked eye. Now we know that coarse particles sink more rapidly in a liquid than fine ones, and the clumps of corpuscles in buffy blood represent much coarser particles than their composing single piles or separate corpuscles. The more they run together, therefore, the faster they will fall in the liquor sanguinis, so as to produce the buffy coat.

Accordingly, it was found in my experiments that, during the formation of the buffy coat, the corpuscles will at first take about two and a half minutes to sink an eighth of an inch in the liquor sanguinis, and fall five or six times faster in the next two or three minutes; and that this remarkable acceleration in the sinking of the corpuscles is connected with their increasing aggregation, appeared from the fact that their rapidity of sinking was increased by increasing their aggregation, though the means used did not attenuate the blood; and prevented or even reversed by destroying the aggregation, though by means which made the blood much thinner and lighter. There is also an acceleration, after a few minutes, in the pace with which the corpuscles of buffy blood sink in the serum, though to a less degree than in the liquor sanguinis. These observations support the views concerning the formation of the buffy coat originally given by Professor Herrman Nasse of Marbourg, adopted by Professor Henle, Drs. Carpenter and C. J. B. Williams, and arrived at, I believe independently, by Mr. Wharton Jones.

The efficacy of saline medicines in inflammation probably depends on their correcting the disordered state of the blood, by preventing or destroying the aggregation of the corpuscles, and consequently their tendency to separate from the fibrin and to accumulate in the minute vessels; and hence the application of a solution of salt to inflamed parts might prove a very useful and simple remedy.

The experiments from which the results mentioned in this Note were obtained, are detailed in my paper on Buffy Blood, 'Edin. Med. and Surg. Journ.,' v. 64, pp. 360-375, in which I have given some historical notices, and references to various writers, concerning the consistency of the liquor sanguinis and the state of the red corpuscles in inflammation.

CHAPTER III.

OF THE CAUSES OF THE INFLAMMATORY CRUST'S APPEARING AT DIFFERENT TIMES IN BLOODLETTING; OF THE STOPPING OF HEMORRHAGES; AND OF THE EFFECTS OF COLD UPON THE BLOOD.

It has been observed by those who have written on the blood, that it sometimes happens in bloodletting, that the first cup has an inflammatory crust, whilst the last has none; but no satisfactory reason has been given for this difference. One might suppose that it was owing to some circumstance in the bleeding, such as in the different velocity with which the blood flowed into each cup, or to the last cup's being agitated so as to prevent the separation of the lymph: but I have seen it where there was no difference of this sort, nor in any other circumstance that I could observe. I therefore suspect that in such cases the properties of the blood are changed, even during the time of the evacuation; to which opinion I was led by the following experiments.

EXPERIMENT XIX.

Nine ounces of blood were taken from a woman who had been delivered two days before, and who at that time laboured under a fever, with a considerable pain in her side and in her abdomen. The blood was received into a basin, and her arm was tied up; when, on looking at the blood, I found its surface transparent for some depth, an indication of a future crust; and as her pain was not abated, and as her pulse could bear it well, I removed the ligature from her arm, and took away about six ounces more, into three teacups; but what appeared to me remarkable, although the blood flowed as fast into each of the cups as into the basin, and when full they were immediately set down on the same window, yet there was no inflammatory crust on the blood in the cups, though a very dense one on that in the basin. And again, although the blood in the basin had been taken away some minutes before that in the cups, yet it was later in being completely coagulated; as was evident on comparing them.

I had an opportunity of repeating the experiment in the evening; for the symptoms of inflammation seeming equally violent, it was thought proper by the physicians who attended her to take away more blood; which was done by opening the same orifice, when three teacups were nearly filled, and set in the same place; and it was observed, that the first had a crust, though not so thick a one as in the first bleeding; but the other two cups were without this appearance, though the blood had flowed into them even more quickly than into the first.[1]

EXPERIMENT XX.

A gentleman, who laboured under an inflammatory complaint, had about nine ounces of blood taken from his arm. This quantity was divided into four portions; the first was received into a cup, and was in measure little more than an ounce; the second into a basin, to the quantity of two ounces; the third into a cup, which held one ounce; and the fourth into a basin, to the quantity of three ounces. Each vessel was immediately placed upon the window; and it was observed that the blood in the first was latest in coagulating, and had a crust over the whole surface; that in the second had a crust only upon a part of its surface; but that in the third and fourth had none, and manifestly coagulated before either of the other two.

Now, since in these experiments the blood in the first cups

[1] As this experiment seems contradictory to some mentioned hereafter, in the last cups being filled rather sooner and yet coagulating sooner, which might be suspected to be owing to the vessel's acting more strongly at the latter part of the operation than at the beginning; it is therefore necessary to observe, that the difference in this experiment appeared to be only owing to a difference in the size of the orifice, for when the ligature was first removed, the old wound was not so much torn open as it was afterwards, when it was more enlarged in order to hasten the evacuation. But it did not, in the beginning of the operation, trickle down the arm as in Exp. XXVII, where the size of the orifice was not enlarged from the first, and yet in proportion as the operation advanced, the velocity of the blood increased; which was thence concluded to be owing to an increased action of the blood-vessels.

was later in coagulating than that in the last, and since the blood in the first cups alone had a size, is it not probable, that even during the short time taken up in the evacuation, the properties of the lymph had been changed, and that it was owing to this change (XXIV) that the size disappeared? It might indeed, at first sight, seem possible that the bleeding had only let out the vitiated part; but this is not at all likely; for, suppose a part only of the blood was vitiated, that part must have been equally diffused through the whole mass, and there is no probability of its getting out of the vessels before the rest of the blood; and consequently it ought to have appeared in the last equally as in the first cup, but it did not. Bleeding, therefore, in those cases alters the nature of the blood, not by removing the vitiated part, and giving room for new blood to be formed, as has been supposed; but probably by changing that state of the blood-vessels, on which the thinness, and lessened

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(XXIV.) The change probably consists in a dilution of the blood, as it may be supposed that the contents of the lymphatic vessels are rapidly poured into the veins as the blood flows out. The evidence originally given by Dr. Davy in his Thesis,[a] that the blood which flows last and coagulates first contains most water, is mentioned in Note I. I did not observe, in some experiments, that adding distilled water to the blood hastened its coagulation, though it was hastened by the addition of serum, and still more by the addition of red corpuscles: See Exp. 45-63, 'Edin. Med. and Surg. Journ.' vol. 64, p. 373. Mr. Prater,[b] Schrœder Van der Kolk, and Dr. Alison,[c] had before observed the same effect from diluting the blood with serum, and applied it to account for the more rapid coagulation of the last flowing blood.

On the curious subject of the change in the properties of the blood during its evacuation, there are the three following hypotheses: 1. That of Quesnay[d] and Hewson, in which vascular action is supposed to be the chief cause. 2. That of Dr. Davy,[e] in which the change is conjectured to be owing to blood from different parts of the body entering the stream flowing in any bloodletting. 3. That of Liebig, described in Note XVIII, that albumen and fibrin are isomerical substances. The second hypothesis is not incompatible with the third. Mr. Thackrah[f] has given an account of the difference in the blood from divers vessels. But some of his results are so extraordinary as to require further examination; the wide range, for instance, in the specific gravity of blood and its serum from the portal vein.

[a] Tent. Exp. quædam de Sang. comp. p. 37, 8vo, Edin. 1814.

[b] Exp. Inq. in Chem. Physiol. p. 105, 8vo, Lond. 1832.

[c] Outlines of Physiology and Path., p. 46, 8vo, Edin. 1833.

[d] Traité de la Saignée, p. 415, nouv. ed. 8vo, Paris, 1750. He refers only to the excess of fibrin in the blood in inflammation.

[e] Researches, ii, 67, 83.

[f] On the Blood, ed. 1834, p. 94 et seq.

tendency of the lymph to coagulation, depends; which surely is a very curious circumstance.[1]

From this observation we may be led to think, that it may be useful to receive the blood more frequently into small cups, instead of a basin, and to attend more carefully to the alteration produced upon it by bleeding; as we may by that means perhaps learn to determine better what quantities should be taken away in particular cases. For it would seem probable that the operation is likely to have the most effect on the disease, in those cases where the greatest change is produced by its means, on the disposition of the blood to coagulate; and of that change we can judge, by comparing the blood in the first cup with that in the last; for the first cup will nearly show the state of the blood at the beginning, and the last cup the state of the blood at the latter part of the evacuation.

It frequently happens that, instead of an inflammatory crust over the whole surface of the crassamentum, there is only a partial one, which appears in large spots or streaks. In such cases I have observed, that only a part of the blood had its disposition to coagulate lessened, as in Experiment xv, in which some of the blood remained fluid and transparent, where those streaks appeared, for some time after the coagulation had begun in other parts of the surface. Now whether in those cases there had been the same difference before the vein was opened, or whether the whole blood had not been of the inflammatory kind, before venesection, and a part of it was changed as it ran out, or as soon as the general fulness was diminished, may be a question; but the probability, I think, is much in favour of its being changed during the time of the evacuation, from what was observed in the last experiments.

When I had observed that this disposition of the lymph to coagulate was increased by bleeding, or by weakening the action of the blood-vessels, I suspected that possibly in those cases where the body was very weak, the disposition to coagulate might be so much increased, that instead of being three or

[1] That the properties of the blood can be changed by emptying the blood-vessels, is likewise proved by an experiment hereafter to be related, where the blood in an animal in health was found to have its disposition to coagulation increased in proportion as the vessels were emptied, and as the animal became weaker. It may likewise be proper to mention, that though the inference is here drawn from two experiments only, yet I have likewise observed the same appearance in other cases, which I have thought unnecessary to relate.

four minutes in beginning to do it, after it is let out of the veins (as is the case in people in health) it might coagulate in less time, or almost instantaneously; for I imagined, that unless this took place, we could hardly conceive how the blood should ever have time to coagulate in ruptured vessels, so as to stop hemorrhages, as it is believed to do. And upon this occasion I recollected a remark that I had heard, particularly from Dr. Hunter, which is, "That the faintness which comes on after hemorrhages, instead of alarming the bystanders, and making them support the patient by stimulating medicines, as spirits of hartshorn and cordials, should be looked upon as salutary; as it seems to be the method Nature takes to give the blood time to coagulate." Now as this seemed to favour my suspicion, I determined to make the experiment.

EXPERIMENT XXI.

Believing it would be sufficient for this purpose, to attend to the properties of the blood, as it flows at different times from an animal that is bleeding to death, I therefore went to the markets, and attended the killing of sheep; and having received the blood into cups, I found my notion verified. For I observed, that the blood which came from the vessels immediately on withdrawing the knife was about two minutes in beginning to coagulate; and that the blood taken later, or as the animal became weaker, coagulated in less and less time; till at last, when the animal became very weak, the blood, though quite fluid as it came from the vessels, yet had hardly been received into the cup before it congealed. I have also repeated the experiment, by receiving blood into different cups at different times, whilst the animal was bleeding to death; and though the time taken up in killing the animal was not commonly more than two minutes, yet I observed, on comparing the cups, that the blood which issued last coagulated first[1]

[1] It may be necessary to mention a circumstance that has occurred in repeating these experiments; which is, that although the last cup being taken from the animal when much reduced, always coagulated in less time than the first, yet, when four or five cups were used, the blood in them did not always coagulate precisely in the inverse order of their being filled; for sometimes the second coagulated before the third. This circumstance at first seemed contradictory to the general conclusion, but on a more careful examination, it was suspected to be owing to the struggles (or temporary exertion of strength of the vessels) of the animal, and no difference was observed in the exposition to cold or to air.

(xxv). I have observed likewise, that the blood coagulates with a different appearance in proportion as the animal becomes weaker; that which follows the knife begins to coagulate in about two minutes; it first forms a film or pellicle on the surface, which extends gradually through the whole blood, yet so slowly that its progress may be observed, especially if the pellicle be moved from time to time. But the blood that comes from the fainting animal is coagulated in an instant, after it once begins. From this circumstance, that the disposition of the blood to coagulate is increased as the animal becomes weaker, we may draw an inference of some use with regard to the stopping of hemorrhages, viz. not to rouse the patient by stimulating medicines, nor by motion, but to let that languor or faintness continue, since it is so favorable for that purpose; and also that the medicines likely to be of service in those cases, are such as cool the body, lessen the force of the circulation, and increase that languor or faintness.[1] For, in proportion as these effects are produced, the divided arteries become more capable of contracting, and the blood more readily coagulates; two circumstances that seem to concur in closing the bleeding orifices.[2]

It has been questioned whether bloodletting can be properly

[1] Besides giving stimulants and cordials to counteract the fainting, it is a common practice in many parts of England to give women, who are flooding, considerable quantities of port wine, on a supposition that it will do them service by its astringency. But surely, from its increasing the force of the circulation, it must be prejudicial in those cases. Perhaps many of the remedies called styptics might be objected to for the same reason.

[2] It has of late been proved by experiments, particularly by those of the ingenious Mr. Kirkland, that the larger arteries, when divided, contract so as to stop the hemorrhage. But the large coagula which we see in the orifices of the vessels of the uterus of those who die soon after delivery, and the stopping of hemorrhages where the blood-vessels were ruptured on their sides and not entirely divided, make me believe that contracting the bleeding orifice is not the only method nature takes to stop an hemorrhage. Her resources indeed are great, and she has often more methods than one of producing the same effect.

(xxv.) See Note xxiv. The contrary results obtained by Mr. Hey[a] are curious. Mr. Thackrah's experiments,[b] afterwards witnessed by Mr. Hey, agree with Hewson's, as Hey himself admitted.[c]

[a] On the Blood, 24 et seq. 8vo, Lon. 1779.
[b] On the Blood, ed. 1834, p. 138.
[c] Mr. Grainger's Elem. Gen. Anat. p. 41, 8vo, Lond. 1829.

recommended in hemorrhages, excepting in those that are attended with evident signs of plethora: but do not these experiments show that a vein may be opened with propriety, even where there is no plethora, in order suddenly to bring on weakness; by which the momentum of the blood may be so diminished, and the disposition of the lymph to coagulate may be so increased, as to stop the hemorrhage? For, when we consider how soon the blood-vessels contract, and adapt themselves to the quantity of blood which they contain, it seems not improbable that in some cases where the hemorrhage is not profuse, but long continued, the strength of the patient may be so recruited, that the disposition to coagulate shall not be sufficiently increased, or the extremities of the vessels sufficiently contracted, for the stopping of the bleeding; but, by emptying the vessels suddenly, this effect may be obtained, and the hemorrhage may be stopped by the loss of less blood than would have happened, had only the slow draining been continued.

Although the whitish crust so commonly seen in inflammatory disorders, has so very morbid an aspect, as might induce us to consider it as inflammatory, and to bleed repeatedly in all those cases where it occurs, yet I believe we should act improperly: for, to say nothing of pregnancy, in which the appearance is almost constant, there are few physicians that have not seen patients, who, even in such circumstances, were the worse for this evacuation. Nor need we be surprised that this should happen, considering how soon in some instances this size disappears; and if so, may we not suppose, that it may likewise soon be formed, even by a short exertion of strength in the vessels? Perhaps this was the case in the gentleman mentioned in page 43, who in less than twenty-four hours after bleeding had symptoms of great weakness.

As it appears from these experiments, that the disposition of the blood to coagulate is increased by bleeding, it may be useful to attend more to this circumstance, and to compare the coagulation of the blood in the last with that in the first cup, even in cases that are not attended with the inflammatory crust. And it may likewise be worth while to make the same comparison in those cases where every cup has a crust; which frequently happens both in rheumatic and in phthisical complaints. By these means we may judge what effect the evacuation has produced on the strength or fulness of the vessels; and may, perhaps, by inspecting the last cup, especially if it contains only a small quantity, be able to guess pretty nearly at the nature of the blood which remains in the body. In the rheumatic case mentioned in page 35, every cup contained this crust: and although the blood in the last cup coagulated in much less time than that in the first, yet, as it was later in coagulating than common, I suspected what remained in the vessels had the same disposition; but the patient recovered without repeating the evacuation.

It may be mentioned here, that I have once or twice seen blood which, when it first began to coagulate, had on its surface a red pellicle, and underneath a transparent fluid, which afterwards formed a crust. In these cases, if the red pellicle had not been removed before the rest of the blood had congealed, we might have concluded that no part of the blood had this disposition to form a white crust. This appearance, I should imagine, was owing to the blood, where in contact with the air, having coagulated before the red particles had time to subside from that part of the lymph which had its disposition to coagulation lessened.

The learned Professor de Haen has taken notice of a curious appearance of the blood, which he could not account for; but which, I presume, may be explained from some of the above experiments. His observation is, "that having bled a person in a fever, the blood was covered with an inflammatory crust, and upon examining the crassamentum in one of the cups, he found that it formed a sort of sac containing a clear fluid: this fluid being let out, and the cup set by, on examining it next morning, he observed a very firm crust covering the whole again, and extending to the bottom of the cup."[1] I once met with a case similar to this; for, having bled a person into four cups at ten o'clock in the morning, on looking at the blood afterwards, at five in the afternoon, I found the serum had not separated from the crassamentum in the first cup; but the crassamentum felt as if it contained a fluid in a bag, as Professor de Haen has described it. Upon pressing it, the fluid gushed out, and in a few minutes after being exposed to the air, coagu-

[1] Vide Rat. Medendi, cap. vi.

lated: there was however this difference in the two cases, that in mine the fluid was red, so that it formed a red crust over the first, which was white. Now this seems to have been owing to the blood's having first coagulated, where it was in contact with the air and with the sides of the cup; and the fluid which gushed out was the serum, with a part of the coagulable lymph, which still remained fluid; but, when exposed to the air, it jellied or coagulated, as it naturally does. That one part of the lymph can remain fluid after the other is coagulated, is proved by some of the preceding experiments; and I have more than once seen blood which appeared perfectly jellied soon after bleeding; yet, on cutting into the coagulum, a transparent fluid has oozed out, which afterwards jellied. And so slowly does this coagulation proceed in some cases, that, in an experiment mentioned before, a part of the blood in a dog's heart was found uncoagulated thirteen hours after death. And I have likewise distinctly observed, that in some cases where the disposition to coagulate was much lessened during the evacuation, the blood at the bottom of the cup has jellied, whilst the greatest part of the size at the top was yet fluid; there being only a thin pellicle on its surface, where it was in contact with the air (XXVI).

Another instance of a change in the properties of this coagulable lymph, which appears curious, was seen in some experiments, where I had occasion to throw the blood into water, and into oil, during the winter season, whilst the heat of the water and of the oil was no greater than 41° of Fahrenheit's scale. In all those experiments, I found that the disposition to coagulate was lessened, the blood becoming more and more viscid, but did not coagulate whilst in that degree of cold. I shall next relate those experiments.

EXPERIMENT XXII.

The jugular vein being properly tied, and then cut out from a rabbit just killed, was then thrown into water of 41° of heat,

(XXVI.) See Exp. XVI, and Notes XII, X, XIII and III, *i. k.* I have repeatedly removed a clot as soon as it was formed, and observed that it was succeeded by another. Dr. Davy[a] noticed the same fact.

[a] Researches, Physiol. and Anat. ii, 68.

and taken out at the end of half an hour; when the blood was found to be still fluid, though rather more viscid than natural; but, after being exposed to the air, it coagulated.

EXPERIMENT XXIII.

Two pieces of the jugular vein of a dog, just killed, were put into water, in which the thermometer stood at 41°; one was taken out after twenty minutes, and the other after three quarters of an hour; the blood in both was found to be fluid, and to coagulate afterwards.

As it was evident from these experiments, that the water had lessened the disposition of the blood to coagulate, I next inquired to what property in the water this effect could be owing; and to see whether water that was warmer would not have the same effect, I made the following experiment.

EXPERIMENT XXIV.

On December the 13th, I cut out two pieces of the jugular vein of another dog, immediately after his death. One piece was put into cold water, and the other into water kept warm by a lamp, so that the heat never varied more than between 90° and 100°. At the end of three quarters of an hour, that in the warm water had in it a coagulum as large as a garden-pea; but that in the cold water, being let out into a cup, was quite fluid. Twenty minutes after being exposed to the air, that which had been in the cold water was coagulating; but that from the warm water neither then nor afterwards showed any signs of farther coagulation: so that it seemed not only to have jellied whilst in warm water, but to have begun to part with its serum. From this experiment, it seems probable that the coldness was that property of the water to which the lessened disposition to coagulate was owing; but to be more sure of this, and to see whether the blood might not be kept fluid a longer time by these means, I tried as follows:

EXPERIMENT XXV.

On January the 14th, I cut out a piece of the jugular vein of another dog, and put it into oil, in which the thermometer stood at 38°. At the end of six hours it was taken out, and the red particles were observed through the coats of the vein to have mostly settled to one side. The blood was let out into a cup, and was found to be fluid; at the end of fifteen minutes above one half was still fluid; in twenty-five minutes it seemed to be quite jellied. Now as in this experiment a similar effect was produced, as when the vein was put into water, it seems probable that it was the coldness of the water, and of the oil, which had lessened the disposition of the lymph to coagulate.

EXPERIMENT XXVI.[1]

Another piece of the same vein was put into river-water, in which the thermometer stood at 38°, and was left till the next morning; when, after twenty-two hours and a quarter, it was taken out. The red particles did not seem to have subsided, as in the former experiment; but the vein being opened the blood was found to be fluid, though so viscid that it could barely drop from the vessel. The cup into which it was received was placed upon the window of a moderately warm room, and was examined carefully from time to time; but the blood never had any appearance of coagulation, on the contrary, it remained fluid till it was dried by the evaporation of the water, which happened by the next day. In this experiment the cold seemed entirely to have prevented the coagulation of the lymph: so ill-founded is the common opinion, that cold coagulates the blood.

As the lymph, on being cooled, is deprived of its power of coagulating when exposed to the air, may we not thence be led to explain that fact mentioned by Lister, that the blood of those cold animals which sleep during the winter-season, on being let out into a basin, does not coagulate? (XXVII.) And thence, as he observes, remains always fit for motion.

[1] It is necessary to observe here, that great expedition should be used in making these experiments, for, unless the vein be cut out in a few minutes after the death of the animal, the experiment may not succeed, from the blood having begun to coagulate.

(XXVII.) Mr. Hunter[a] quotes Mr. Cornish as having seen the blood of torpid bats in some degree coagulated, but soon recovering its fluidity when subjected to motion and heat: Mr. Palmer adds in a note, that Dr. Marshall Hall, in his experiments on hybernating animals, found the blood as fluid as under ordinary circumstances. M. Saissy[b] states that the blood of hybernating animals, even during the deepest lethargy,

[a] Works, ed. by Palmer, vol. iii, p. 33.

[b] Researches Exp. sur les Animaux hybernans, p. 46, 8vo, Paris, 1808.

CHAPTER IV.

SOME FURTHER OBSERVATIONS ON THE COAGULABLE LYMPH, AND ON THE SUDDEN CHANGES PRODUCED UPON IT.

If the reader has been persuaded of the common opinion, that the disposition of the blood to coagulate is increased in inflammatory disorders, it may perhaps appear to him, as it formerly did to me, a very extraordinary circumstance that the contrary should be true (see Note XXI); and likewise that the blood should in reality be the more disposed to concrete, in proportion as the body is weakened, or as the action of the blood-vessels is diminished (see Note XXIV). And as we are naturally tenacious of old opinions, and unwilling to adopt new ones till fully proved, he may suspect that there has been some fallacy in these experiments. And indeed I must acknowledge that there is, in appearance, one strong argument against my general conclusion, which is, that it has not only been remarked that the first cup has a crust, whilst the last has none, but likewise, that the second, or the third cup, alone shall have a crust, whilst the preceding ones are without it. Now this, I say, seems contradictory to what I have advanced, concerning the disposition of the blood to coagulate being increased in proportion as the body is weakened; for here in proportion as the blood is evacuated, its disposition to coagulate is lessened; since it was more sizy in the second or third cup than in the first. But, in answer to this objection, I must remark, that these cases very seldom occur; and that in general the first cups are more sizy, and are the latest

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is not coagulated. Dr. Davy has favoured me with the following extract from his note-book: "March 31, 1841, killed a tortoise, just then awaking and almost torpid; the air 50°; air of room 55°; temperature in recto 48°; blood from right auricle 50°; in left compartment of ventricle 51·5°. In the cavity of the belly a good deal of colourless fluid, which jellied on standing. But little blood in the heart and great vessels, which, when coagulated, was very tender, like the thinnest jelly. March 7, 1836, killed and examined a hedgehog; the blood coagulated slowly and feebly."

in jellying; and when the contrary takes place, or when the second or third cup is more sizy than the preceding, I am persuaded that upon a careful examination, instead of weakening, they will be found to strengthen my inference; as will appear probable by the following case, which has occurred since these experiments were published in the 'Philosophical Transactions.'

EXPERIMENT XXVII.

On the 13th of June, I visited a young man, twenty-two years old, of an athletic habit, who complained of a violent pain in his head and back, with a full strong pulse; but as he was then in a profuse sweat, which had been preceded by a shivering, it was not thought proper to bleed him, and the rather, as we were informed, that he had had a similar paroxysm two days before. But next day, finding that his fever had not left him with the sweat, and that he still had a pain in his head and back, and that his pulse, though not now full and strong, yet was quicker than natural, it was then judged necessary to take away some blood. Upon opening the vein, the blood flowed very slowly, and indeed merely trickled down his arm. Imagining that the bandage might be too tight, I slackened it, but still the motion of the blood was not accelerated. I then asked him whether he had not been afraid of the bleeding, and he told me he had; and on feeling his pulse in the other arm, I found it very low. I therefore desired him to move the muscles of his hand, which he did; but nevertheless so slowly did the blood run, that it was four minutes before I got an ounce and a half into a cup. I then stopped the orifice till another cup was brought, into which the blood ran in a full stream, to the quantity of three ounces, and that in two minutes, although the orifice was rather small, so much was its velocity now increased. Into the third cup, which likewise held three ounces, the blood ran still faster, as it was filled in less than two minutes. By this time the patient beginning to be faint, I stopped the bleeding till he could lie down on the floor, and then about three drachms more of blood were received into a fourth cup: this came away very slowly, and the bleeding stopped of itself. He drank a glass of water, and did not faint, and he appeared afterwards to be much relieved by the evacuation. Upon this blood I made the following remarks:

That which was taken away last was first coagulated, and completely too, by the time I had tied up his arm, which was in three minutes from the blood's first running into the cup.

The blood which was received into the first cup coagulated next, and as I observed by my watch, in twelve minutes from its being set down on the table.

That which was received into the second cup was the third in order as to coagulation, and was considerably later in jellying than the first; for in fifteen minutes it was not thoroughly coagulated; nay, even in twenty-two minutes a small part of it was still fluid. It was remarkable that none of these three had any size.

But the blood in the third cup differed considerably from that in the others; for in five minutes it began to appear transparent on its surface, an indication of a future size, and it was later in coagulating (see Note XXI) than that in the other cups; for even at the end of twenty-six minutes a great part of the coagulable lymph was still fluid, as appeared on removing the pellicle that covered it; but in thirty-five minutes it was completely jellied. The size in this blood was very thick and tough.

Now this case, when carefully examined, instead of being an objection to my conclusions, will, I presume, be thought a strong confirmation of them.

For, in the first place, as the blood in the third cup alone had a crust, and was much later in jellying than the rest, it strengthens my inference, that the disposition of the blood to coagulate is lessened in those cases where the inflammatory crust or size appears (see Note XXI). And as the blood ran more rapidly into this cup, it showed that the heart and blood-vessels had begun to act with greater force, and therefore confirmed the opinion, that in proportion as these act more strongly, the disposition of the lymph to coagulate is diminished (see Note XXIV). The same opinion is likewise supported by observing what happened to the blood in the first cup, which coagulated sooner than that in the third, owing to the vessels then acting more weakly, as was evident from the blood's trickling down the arm, and from the lowness of the pulse.[1]

[1] In like manner may be explained another variety in the appearance of the size namely, where it is found in the first and last cups, but not in the second or third: this, I suspect, seldom happens, but when it doe it may perhaps be found, on exami-

2dly. It may be observed, that the great difficulty in admitting the conclusion made in the former part of these sheets (viz. that the want of size in the last cup is occasioned by an alteration in the blood-vessels) was to conceive how these vessels could possibly alter the properties of the lymph so suddenly, as in the time between receiving the blood into the first cup and into the last. But this case confirms that inference, by showing the fact in a clearer point of view; for even here, where the appearance of the size was reversed, it was found that the blood which had a crust or size was latest in coagulating, and that it was this blood which was taken out of the vessels when they acted most strongly, as was proved by the rapidity with which it flowed into the cup.

3dly. Since the times in which the blood jellied in these cups were so very different (the first coagulating in twelve minutes, the second in about twenty-two, the third in thirty-five, and the fourth in less than three minutes, notwithstanding these cups were filled in less than two minutes after one another), it shows, I say, how soon that state of the blood-vessels on which the size depends, can be removed and assumed, and therefore leads us to conclude, that although this size is in general a sign of an inflammatory disorder, or a strong action of the vessels (see Notes XXIII and XXIV), yet there may be several circumstances to be taken into the account, before we can judge from its presence, or absence, whether or no venesection should be repeated: and it likewise shows clearly, that it would be improper to determine, from the presence of this alone, when bleeding is

nation, that the vessels were acting more weakly whilst the second or third cups were filled. For so easily does this size appear to be removed, or formed, that I suspect it may sometimes happen that when the blood is taken away in a full stream, from a large orifice, the patient may be so suddenly weakened, and the properties of the blood may in consequence be so changed by the time the second cup is filled, that the size shall be removed; and yet afterwards the vessels may recover their former tone, so that the third or fourth cup may acquire a size again. Nay, I suspect that this appearance may even be affected by the passions, particularly from observing that the patient above mentioned, as well as others whose blood at first trickled slowly down their arms, had been much afraid of the lancet (XXVIII).

(XXVIII.) See Note XXIV. Some interesting speculations on the probable modifications in the nature of the current of blood under different degrees of vital action, are given in Dr. Davy's Researches, vol. ii, p. 83.

necessary; and yet there have been not a few who have inclined to make such a conclusion, from their considering this crust or size as so very morbid an appearance.

4thly, As the blood in the third cup was so late as thirty-five minutes in coagulating, and was sizy, whilst that in the fourth was not so, and jellied in less than three minutes, although it had been taken from the vessels only two minutes after the other, but at the time the patient had become faint; it shows how much faintness and languor increase the viscidity of the blood, and likewise its disposition to coagulate, since in two minutes they produced such a change as to remove the size, and to reduce the time of coagulation from thirty-five to three minutes. It therefore shows clearly how much languor and faintness should be encouraged in hemorrhages, and how carefully we should avoid giving anything that can stimulate, or rouse the patient; that the medicines likely to be of service are nitre and the acids; or such as cool the body, or have the property of diminishing the force of the circulation, or of increasing that languor or faintness;[1] that all agitation of mind should, as much as possible, be prevented, lest it increase the circulation; that all muscular motion should be avoided for the same reason: for that an exertion of the patient's strength can lessen the disposition of the blood to coagulate, I am persuaded from some of the above-mentioned cases, and likewise from what I have observed in dying sheep, where the struggles of the expiring animal seemed in some instances, when violent, to alter the properties of the lymph (see Note XXIV).

We have endeavoured to explain the appearance of the inflammatory crust or size, from the red globules having subsided from the surface of such blood before it coagulated: this we observed was partly owing to the lymph's being later in coagu-

[1] It has been objected here, that nitre would seem improper for this purpose, because in experiments mentioned before (p. 12) it was found to prevent the coagulation of the blood, out of the body; but this objection is removed, by considering, that, in order to prevent coagulation, the nitre must be used at least in the proportion of two scruples to every two ounces of blood. But, when we exhibit it internally, we seldom give more than a scruple every two hours, which can have no effect in attenuating the whole mass of blood, nor in preventing coagulation, especially as we have reason to believe its properties are changed before it passes the digestive organs. Its good effects in hemorrhages, therefore, are probably owing to its action upon the stomach. For proofs of its utility, see 'Medical Observations and Inquiries,' vol. iv, art. xvi.

lating in those cases, but principally to its being thinned. But, we may now add, that although the attenuation of the lymph and its lessened tendency to coagulate are connected in most of those cases, yet they do not always go together; for the lymph may have its disposition to coagulate lessened without being thinned; which was evident in the preceding case, on comparing the blood in the second with that in the third cup; for the blood in the second cup had no size, notwithstanding it remained fluid at least ten minutes after the size had begun to appear in the third: this I attribute to the blood in the third being more attenuated, and thereby more readily allowing the globules to subside.

That the blood may have its disposition to coagulate lessened, without being attenuated (XXIX), is likewise probable from the following cases.

EXPERIMENT XXVIII.

In the month of January I bled a man, who complained of a pain in his head, attended with giddiness and shivering, a pain and sickness at his stomach, and with a full and quick pulse: the blood was found to remain fluid for ten minutes, and then jellied, but no size appeared.

EXPERIMENT XXIX.

In another person, who was bled merely for a drowsiness, and because he was accustomed to that evacuation in the spring, I found the blood remain seven minutes without coagulating, and yet it was without any size.

Now, since in these cases the blood remained so long fluid, and yet the red particles did not subside, or no size appeared, I should conclude, that only the disposition of the lymph to coagulate was lessened, without its being thinned. And from

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(XXIX.) Mr. Hewson infers that the blood is not attenuated because the corpuscles do not sink in it. But the rate with which the red corpuscles sink in a fluid may give an incorrect idea of its density or consistency. They may even not subside at all in blood artificially made thinner and lighter, and its coagulation somewhat retarded. If it be admitted that the sinking of the corpuscles, during the formation of the buffy coat, affords an accurate means of estimating the consistency of the liquor sanguinis, it must also be admitted that it becomes thinner some minutes after it is taken from the animal, which is not at all probable. See Note XXIII.

the last case we may likewise conclude, that although the times, at which the blood taken from persons in health begins to coagulate, be allowed to be about three minutes and a half, as I have found from repeated observations, yet there may be some variety in this respect; for a plethora and other circumstances may make it later in coagulating in some cases, even where the patient is otherwise in perfect health.[1]

We have observed before that the size is sometimes very firm, and at other times spongy and cellular; these differences in its density are, I suspect, in proportion to the degree of attenuation and lessened disposition of the blood to coagulate; for as the coagulation begins on the surface, and forms there a film which attracts the rest of the lymph (see Note XLIV), the more that lymph is attenuated, and the slower it coagulates, the more will the film be able to separate it from the red globules, and from the serum: thence perhaps it is, that when the blood, besides being very thin, likewise jellies slowly (see Note XXI), we sometimes see almost the whole coagulable lymph collected at the top, forming a firm crust, which being free from the serum, as well as from the globules, contracts the surface into a hollow form. But when the blood has its disposition to coagulate less diminished in proportion to the attenuation, then, although the globules subside from the surface, yet the whole of the lymph jellies so soon after the coagulation begins, that there is not time for its being separated from the serum, of which it therefore contains a considerable quantity, and is of course more spongy and cellular.

In proportion to the thickness and density of the size, the bottom of the cake is of a looser texture; but this looseness of texture is not owing to putrefaction, as has been suspected, but merely to the lymph's being collected at the top, and therefore leaving the bottom of the crassamentum.

Notwithstanding bleeding does in general weaken the action of the vessels, increase the disposition of the blood to coagulate, and even thicken the lymph, yet it may happen that in the ordinary quantity in which blood is taken away, none of these effects shall be produced; of this the following case seems to be an instance.

EXPERIMENT XXX.

A woman in the seventh month of her pregnancy was bled

[1] This inference is confirmed by a case mentioned below, Experiment XXXII.

for a violent pain in her side, attended with a cough; the quantity taken away was eight ounces, which was received into four cups; and as the orifice was small, about ten minutes were spent in the bleeding. On attending to the different cups, I could observe no difference in the periods at which the coagulation commenced and finished in each, allowance being made for the time the blood began to run into each. In every one of these cups the blood was completely jellied in about twenty minutes, and each had a crust or size nearly of the same thickness. So that the bleeding seemed not to have produced any change in the strength of the patient's vessels, nor was her pain sensibly abated by it. She was therefore desired to live low, to confine herself to a vegetable diet, and to take a scruple of nitre every three hours in a draught of the decoctum pectorale; and if her pain and cough were not abated in a day or two, she was directed to repeat the bleeding. As close attendance was not required, I did not visit her till four days after, and then she had got free of her complaints, notwithstanding her blood had been apparently so little changed in the time of the evacuation.

In this case the bleeding seemed neither to have thickened the lymph, nor increased its disposition to coagulate, nor weakened the action of the vessels; but that it generally produces these effects cannot, I think, be doubted, from our having observed it in so many instances. Perhaps the dread of the operation might here have made the coagulation of the blood in the first cup approach nearer to that in the last; or perhaps the smallness of the orifice prevented there being so manifest a change produced by the evacuation, from its giving time to the blood-vessels to adapt themselves more equally to the quantity they contained, by which means she was not weakened by the loss of blood.

It has been observed by Sydenham (xxx) and others, that it sometimes happens, even in inflammatory disorders, when the blood trickles down the arm, instead of running in a full stream,

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(xxx.) Sydenham, Opera Omnia, imp. Soc. Syden. 8vo, Lond. 1844, sect. 6, cap. 3, p. 247. The fact was noticed, too, by Dr. Richard Davies.[a]

[a] Essay on the Blood, p. 25, 8vo, Bath, 1760.

it does not acquire a crust or size.[1] May not this be explained from what is observed in the case related in Experiment XXVII? that is, in such instances the vessels, either from a febrile or from some other oppression, act more weakly than they do in the ordinary cases of inflammation; by which means the lymph is not sufficiently attenuated to allow the red globules to subside before the coagulation begins, and therefore the size does not appear, as in other cases of inflammation where there is no such oppression.

As air is found to coagulate the blood and cold to thicken it, an objection has thence been made to the conclusions from some of the preceding experiments; and it has been supposed that the changes in the properties of the blood, that happen during the time of bleeding (which I have attributed to a difference in the action of the vessels), might possibly be owing merely to a difference in the exposition to the air or to cold (see Notes XI and XII). For instance, since the blood that trickles down the arm seems to be more cooled than that which flows in a full stream, it has thence been supposed that its want of size, in those cases, might be owing to the exposition to the air, which made it coagulate sooner, and to the cooling which had thickened it, and thereby prevented its red particles from subsiding, so that the size should be formed. This objection is indeed plausible, and to those who have not seen these experiments might at first seem sufficient to explain the appearance; but upon further examination it will not be found to do it satisfactorily. Thus, for example, although it be true that air coagulates the blood, and likewise cools it, yet there are changes remarked in the preceding experiments that cannot be explained merely by a difference in the exposition to the air: thus, in Experiment XXVII, the blood

[1] It may be necessary to observe, that it is not in every case where the blood trickles down the arm that it is without a size; on the contrary, it sometimes happens, that even in such circumstances it has a very dense one; an instance of which may be seen below, in Experiment XXXI. In those cases the trickling down the arm may perhaps be owing to some circumstance in the orifice preventing its flowing in a full stream, or to a difference in the tightness of the ligature, rather than to a weak action of the vessels. Or, although the size be occasioned by a strong (or some particular mode of) action of the vessels, and therefore is removed by weakening them, yet it may not always be removed immediately on their being weakened. For it may happen, that in some cases the lymph may not be so susceptible of changes as in others; or when it has been very much attenuated it may not again be thickened immediately, on the vessels acting weakly.

in the third cup was thirty-five minutes in being completely coagulated, whilst that in the fourth, although taken from the arm only two minutes later, yet coagulated in three minutes. Now no exposition to the air nor to cold, from the blood's trickling down the arm, could produce such a change. Of this I am persuaded from what I have observed on comparing the blood received into a cup with that which dropped on the plate which held the cup; for I have repeatedly seen on those occasions that the blood on the plate, although it was so much more cooled and so much more exposed to the air than that in the cup, yet instead of coagulating proportionably sooner, was later in being coagulated. The following experiment shows this clearly.

EXPERIMENT XXXI.

A young woman with a violent inflammation in her eyes, was bled on the fifth of March, early in the morning, before she had breakfasted, and whilst she was complaining of a sickness at her stomach; the blood followed the lancet in a stream, but immediately after it only trickled down the arm, and continued to do so during the whole of the evacuation. About eight ounces of blood were taken away into four vessels, viz., into two cups and two saucers, in the following manner: A plate holding both a cup and a saucer was held under the arm, and the blood was first received into the saucer, to the quantity of a spoonful, then as much more was received into the cup that stood by it; then again the blood was suffered to run into the saucer, and afterwards into the cup, and so alternately till there was about two ounces in each, when they were carefully set down on a window where the thermometer stood at 57°; the plate was placed by them, and contained about a spoonful of blood, which had missed the saucer in the begining of the evacuation. Next, the second plate was brought, and some blood was received first into the cup and then into the saucer, in the same manner; and three portions of blood were suffered to drop at different times on the plate, each of them about the breadth of a shilling. Now, here, according to the reasoning in the objections made to some of the preceding experiments, the blood in the saucers having twice as much surface as that in the cups, ought to have coagulated in half the time; and that on the plates ought, from the largeness of the surface, to have coagulated in much less time; but just the contrary happened; for the blood in the cups was first completely coagulated, that in the saucers next, and that on the plates latest of all. But, as the experiment seems curious, it may be proper to give a more particular detail of what was observed.

On looking at the first plate, at the end of seven minutes after it was set down on the window, the surface of the blood in the cup was considerably transparent, and a pellicle (that is the surface beginning to coagulate) was formed upon it; but no transparency was distinguishable on that in the saucer, nor could any pellicle be observed upon drawing a pin through it, or through that which had dropped upon the plate. At the end of fifteen minutes the blood in the cup and in the saucer was pretty much coagulated, or had a thick pellicle, whilst none could yet be observed on that upon the plate. At the end of fifty-five minutes that in the cup was just beginning to part with its serum, whilst the blood in the saucer was not yet completely coagulated; for on inclining it to one side, a part of the blood appeared fluid under the pellicle. That on the plate was now coagulated. They were all three sizy; and the blood in the saucer had a size which seemed to be as firm and in as large a quantity nearly as that in the cup; and the size upon the blood in the plate was thick enough to be easily distinguished.

In the second plate, at the end of seven minutes after being set on the window, both the blood in the cup and in the saucer was beginning to coagulate; and had a pellicle of a considerable thickness, and were both sizy; but no pellicle appeared on any of the three portions that had dropped on the plate. At the end of fifteen minutes, that in the cup was firmly jellied, that in the saucer not quite so much, and one of the spots on the plate was but just beginning to coagulate at its edge. At the end of twenty-five minutes, the two last spots were still perfectly fluid, but in twenty-eight minutes they were beginning to coagulate; whilst the blood in the cup was now parting with its serum. At the end of fifty minutes a considerable quantity of serum had separated in the cup, and the separation was just beginning in the saucer.

This experiment was repeated on another person's blood two days after, in the presence of Mr. Field and Mr. Hendy, two studious gentlemen, at that time living at the Middlesex Hospital, and the appearances were exactly similar: and it was evident to them that the blood in the saucers was later in coagulating than that in the cups, and that on the plates (one of which was of pewter) was considerably later in jellying than that in the cups or in the saucers.

These experiments therefore show clearly, that the differences in the periods of coagulation, and in the appearance of size upon the blood received into the different cups in bleeding, cannot be accounted for from a difference in the exposition to air; for here blood more exposed to the air than that is which trickles down the arm is found equally sizy, and to be even later in coagulating than blood less exposed.

As we have here observed a new circumstance that appears remarkable, and which at first sight seems not reconcilable to some of our conclusions, it may therefore be necessary to examine it farther, or to inquire, if air be a coagulant of the blood (as we have endeavoured to prove in the beginning of this Essay) how comes it that in this experiment the blood was not coagulated proportionably to its exposition to the air? This, I think, may be explained from considering another fact that was mentioned in the preceding pages, viz. that cold lessens the blood's tendency to coagulation. The blood, therefore, in the saucer, although it was more exposed to the air, yet being more cooled than that in the cup, was for that reason later in coagulating; and that on the plate, which was most exposed to the air, being most cooled, was therefore latest of all.

But we may add, that although it be evident from this experiment, that the difference in the exposition to air, or to cold, is not sufficient to explain the changes which we see produced upon the blood, in so little time as in the filling of a small cup, especially when those changes are so great as what are mentioned in Experiment XXVII (where the blood in one cup was thirty-five minutes in coagulating, and had a very thick size, whilst that taken away soon after, coagulated in three minutes, and was without a size) yet, I think, that the effects of air and of cold are considerable enough to deserve to be taken into the account in some cases, where the changes on the blood are not so great. For as cold thickens the blood, it is probable that in some cases where the lymph is but little attenuated, and where therefore, in the ordinary manner of bleeding, there would have appeared but very little size, such blood, if more exposed to the air so as to be sooner cooled, may thereby have the small degree of attenuation counteracted, or removed, and the red particles may be prevented from subsiding. So that although in this experiment, where the size was thick, it appeared equally in the saucer as in the cup, and even appeared on the blood upon the plate; yet, if we repeat these experiments on a variety of subjects, it is probable that we may sometimes find the saucer without a size, whilst the cup has one; for when the lymph is but little attenuated a slight cause may thicken it again; and its being a little more cooled in the saucer and on the plate may in some cases be sufficient to prevent the size from appearing.

It has likewise been suggested, that possibly there might be some difference in the orifice from which the blood flowed, to which its different appearances in the several cups might be owing. But there does not seem to be any foundation for this objection, and it may, I think, be removed by a careful examination only of some of these experiments, particularly the 27th; for there the blood ran into a full stream both into the second cup and into the third, the orifice being apparently unaltered, and yet there was a great difference in the appearance of the blood: for that in the third cup had a thick size, but that in the second had none. So that there does not seem to be any circumstance attending these experiments that can explain the changes produced upon the blood in the time of bleeding, excepting that to which I have attributed it, viz. a change in the strength of the blood-vessels, or in their mode of action; and every observation I have yet made confirms me in that opinion.

A very eminent physician,[1] after reading the first edition of these sheets, informed me, that from a suggestion which he met with in Professor Simson's Dissertations, viz, "That a ligature on the arm would produce a size,"[2] he had been for many years cautious how he took any indications from this appearance of the blood, and desired I would endeavour to deter-

[1] Dr. Fothergill.

[2] Dr. Simson's observation is, that if a tight ligature be made on the limb, and the vein opened three hours after, a size will be produced. Vide 'De Re Medica,' Dissert. iii, § 38, p. 112.

mine, by experiment, whether the ligature being a longer or shorter time upon the arm, even in the ordinary way of bleeding, might not influence this appearance of the blood. And accordingly, in the presence of Dr. Drummond, Mr. Field, Mr. Hendy, and Mr. Cockson, I made the following experiment:

EXPERIMENT XXXII.

On the 9th of October, I tied up both the arms of a healthy young man with a degree of tightness sufficient to make the veins swell and become turgid, whilst the pulse remained free; a vein in one arm was opened immediately after making the ligature, and an ounce of blood was received into a cup. I chose to take away so small a quantity that there might be the less probability of producing any change upon the blood by weakening the body. The ligature was left upon the other arm for an hour, which made the veins very turgid, and likewise made the person complain of a stiffness in his fore-arm; the artery in his wrist being felt all the time, but less distinctly than in the other arm which was without a ligature. At the end of an hour this vein was opened, the orifice was large, and an ounce of blood was taken away. Upon attending carefully to each cup, it did appear that the ligature had produced some change; for in the first place, the blood which had been so long detained in the arm by the ligature was darker coloured (XXXI), or blackish, whilst that from the other arm was more florid, even at its first running from the vein. 2dly. The blood that had been so long in the arm was rather later in being coagulated; for it did not begin to part with its serum till at the end of thirty-seven minutes after puncturing the vein; whilst in the other the coagulation was completed, and the serum was beginning to ooze out in thirty minutes. 3dly. The blood which was first taken away was without a size, whilst that which had been so long in the other arm had a small spot about the breadth of a silver penny, and did not cover a twentieth part of the surface.

From this experiment, therefore, it would seem that a ligature long continued may produce a size, agreeably to Dr.

(XXXI.) The darkening of blood by its stagnation in the living body, is mentioned in Note V.

Simson's observation (XXXII), but then it will probably be only in small quantity.

The same learned gentleman, on being informed of the result of this experiment, ingeniously suggested that the quantity of blood which I had taken away might perhaps be too little to make the experiment decisive; for, as only one ounce of blood had been taken from each arm, all that blood might be supposed to have been contained in the veins themselves; and as it was more probable that the disposition to size took place in the arteries, a larger quantity should be taken away, in order to judge whether the blood in the arteries had not been changed by the veins of the arm being so long compressed. The experiment was, therefore, repeated upon the same person, on the 7th of March, Mr. Field and Mr. Hendy assisting me as before, and we observed as follows:

EXPERIMENT XXXIII.

The blood from the arm first opened was in quantity about ten drachms, had no size, but was late in being completely coagulated. The pellicle first appeared on its surface six minutes after opening the vein, and at the end of fifteen minutes a considerable quantity of the blood was still fluid; but in thirty-four minutes it was completely coagulated. The serum did not begin to ooze out till at the end of fifty minutes.

After the other arm had been tied up an hour, the vein was opened, and about ten drachms of blood were received into the first cup, as much more into a second, and about an ounce and a half upon a pewter plate, and about two ounces and a half into a third cup (in all six ounces and a half); and it was observed that the blood in the third cup and that in the second coagulated in about twenty minutes, and the serum began to

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(XXXII.) Thomas Simson[a] thought that the buffy coat is formed of crude chyle not yet converted into blood; and that the buff appears after putting a ligature for three or four hours on the thigh or arm, because the assimilation of the chyle with the blood is thus prevented. Mr. Hey[b] found that neither the tightness of the ligature, nor its longer continuance than usual on the arm, caused the blood to become buffy.

[a] De Re Medica, 8vo. Edin. 1726, Diss. iii, § 38, p. 112.

[b] Obs. on the Blood, 8vo, Lond. 1779, p. 65.

ooze out in twenty-five minutes after opening the vein; the blood on the plate was later in coagulating, and none of these portions of blood had the least appearance of size. But the blood in the first cup was considerably the latest in jellying; for at the end of twenty-five minutes a large quantity was still fluid under the pellicle, and even at the end of fifty minutes the coagulation was incomplete. This blood seemed to have rather more size than that in the former experiment, but it was not collected at one part, but was diffused over the surface, and appeared in spots not bigger than pins' heads.

The result, therefore, of this experiment was similar to the preceding. The ligature long continued produced a size, but in small quantity; and therefore it does not appear probable, that, when the ligature has been only a few minutes on the arm (as is the case in the ordinary manner of bleeding), it can deserve to be taken into the account, when we judge of the disease, or of the indications of cure from the appearance of the size; especially when it is considered that the blood on which these experiments were made was very favorably circumstanced for Dr. Simson's opinion; that is, it was from a patient who seemed to be plethoric, by the blood which was first taken away not jellying in less than thirty, and thirty-four minutes, which is later than ordinary, as appears from what is observed above, page the 35th; where we found, that the blood of people in health commonly jellies in seven or eight minutes.

As many of these experiments show how easily the disposition of the lymph to coagulate can be altered, even by slight changes, as it would seem, in the state of the blood-vessels, they help us to explain how it should happen, that the blood, in some diseases, is found without this property of jellying; an instance of which is mentioned by M. de Senac[1]; another

[1] Traité du Cœur, tom. ii, p. 129 (XXXIII).

(XXXIII.) Senac's case[a] is as follows: "Un homme de trente-cinq ans avoit une ancienne galle, je le fit saigner, son sang ne se figea point." In most of the instances observed by Dr. Davy,[b] in which the blood was liquid after death, and without the power of self-coagulation, the fatal event was referrible either to drowning, hanging, the fumes of charcoal fire, or effusion of blood into the bronchia or air-cells.

A man aged 35, and his three children, aged $1\frac{1}{2}$, $4\frac{1}{2}$, and $6\frac{1}{2}$ years,

[a] Ed. 1749, t. ii, p. 129. [b] Researches, Physiol. and Anat. ii, 192.

was observed by the learned Professor Cullen; and a third I saw lately by the favour of Dr. Huck and the physicians of the British Lying-in Hospital, who, having bled a woman in a fever that came on soon after delivery, found her blood did not coagulate on being exposed to the air, but appeared like a mixture of the red globules and serum only, the globules having subsided to the bottom in the form of a powder. She died three days after, and, upon opening her, we found the blood had coagulated in her vessels after death, and that a tough white polypus was formed in each auricle of the heart, one of which I have now by me. I examined the blood taken away before she died, and found, on exposing it properly to heat, that it did not coagulate sooner than serum commonly does, nor under 160°; so that it is probable that, at the time she was bled, her blood either was without the coagulable lymph, or its properties were changed.

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were suffocated in a burning house in Crawford street, March 23, 1846. One of them was scarcely burned at all, and none of them sufficiently so to cause death. Through the kindness of Dr. Boyd, I had an opportunity of examining the bodies at the St. Marylebone Infirmary, 48 hours after the fatal event; the temperature of the air was 50°. In the heart of every one of them the blood was fluid, without the smallest clot; nor did the blood ever coagulate after having been set aside in cups for observation; the fingers were contracted, and all the limbs rigid; the bodies fresh, and remarkably free from disease.

A coachman, aged about 55, hung himself; he was cut down, dead and stiff, from six to ten hours afterwards, and examined by Dr. Boyd and myself. The blood was fluid in the heart and veins, except two or three very soft clots, so small and nearly diffluent that they were only found after a search; there was no further coagulation of the blood when it was set apart in a cup. In the above instances, this blood was examined from time to time for about twenty hours. But the observations of Dr. Polli[c] induce him to believe that no blood becomes putrid before it has coagulated; and that in cases in which its property of self-coagulation has been supposed to be wanting, the blood has not been kept long enough for an unusually slow coagulation to take place. A mixture of salt and blood, which will keep fluid and fresh for months, readily putrefies after it has been made to coagulate by diluting it with water (see Note VII). In kittens killed by hanging and by drowning, I have seen coagulated blood in the heart, and the limbs rigid; in a dog killed by hanging, as mentioned in Note CXXXII, there were some soft clots in the splenic blood.

The causes commonly said to prevent coagulation are mentioned in Note XII.

[c] Mr. Paget's Reports, Br. and For. Med. Rev. xix, p 253, and xxi, p. 543.

After a blow or contusion, the blood now and then bursts from the vessels into the cellular membrane, sometimes forming an ecchymosis, and sometimes a tumour, and it is a question with some, whether such blood coagulates or not; but that it coagulates in most of these cases, is proved by opening such tumours. Yet it has likewise been observed, that now and then these tumours have been attended with a fluctuation, and that, after some time, their contents have been absorbed, and it has also been found that, upon opening some of them, even several weeks after the accident, the blood was fluid. In such cases the blood had probably undergone a change similar to what was observed to take place in some of the preceding experiments, that is the lymph had been deprived of its property of coagulating (XXXIV), in passing from the blood-vessels into the tumour: a circumstance, which, how remarkable soever it may appear, agrees with what we have above observed of the lymph, whose properties seemed to vary with the state of the blood-vessels (see Note XXIV).

(XXXIV.) Extravasated blood, which has long remained fluid in a bruised part, may yet coagulate when let out (see Note X).

CHAPTER V.

CONTAINING A RECAPITULATION OF THE PRINCIPAL FACTS AND CONCLUSIONS THAT ARE MENTIONED IN THE PRECEDING PAGES.

THE separation of the blood into crassamentum and serum, in a given time, appears to be in proportion as the heat in which it stands is nearer to that of the human body (XXXV). The heat in which the blood is kept should therefore be attended to, when we draw inferences from the proportions of these two parts.

The florid colour of the surface of the crassamentum seems to be owing to the air. The venous blood, in passing through the lungs, has a similar change produced upon it, or becomes more florid by the time it gets into the arteries; and this florid colour is again lost in passing from the small arteries into the veins, especially if the person be in health. But sometimes in diseases it does not undergo this change, but comes florid out of the veins (XXXVI).

Neutral salts, on being mixed with the blood out of the body, make it more florid: they likewise, if used in great quantity, prevent its coagulation when exposed to the air, and some of them allow the lymph to be precipitated, or to jelly on being diluted with water (XXXVII). But we are not to conclude from thence that they would produce the same effects when used as medicines; for then they are only given in small quantities (XXXVIII), and may have their properties changed

(XXXV.) See Notes II and III.

(XXXVI.) On the colour of the blood, see Notes IV and V.

(XXXVII.) The effects of neutral salts on the colour and coagulation of the blood are discussed in Notes IV, VI, and VII.

(XXXVIII.) A very weak saline solution will prevent that aggregation of the corpuscles, which is characteristic of the blood in inflammation. The effect may be easily tried on the regularly buffy blood of the horse (see Note XXIII). On the different effects of different quantities of salt on coagulation, see Note VII.

by the digestive organs, before they are mixed with blood. And as they are most of them cooling and sedative, they produce nausea and languor of the stomach, and lessen the force of the vascular system. And as the properties of the blood seem to depend upon the action of the vessels (see Note XXIV), these salts may thus, by affecting the vessels, produce changes on the blood very different from what might have been suspected, from observing what takes place when a large quantity of them is mixed with the blood out of the body.

The blood is not coagulated (I do not mean thickened, for it is indeed thickened) by cold, but on the contrary, has its disposition to coagulate lessened, and even entirely taken off, if the exposition to cold (XXXIX) be long continued. When therefore the blood in the basin jellies, it is not the cold that produces this effect, nor is it the want of motion; for although the blood by being at rest will jelly at last, yet it will not do it in the time the coagulation takes place in the basin; which in the blood of healthy persons is in seven or eight minutes after being received from the vein. The coagulation of the blood in the basin is therefore probably owing to the air (XL).

When the blood is at rest in the body, it will at last coagulate merely for want of motion; but this coagulation takes place with different appearances from that of the blood in a basin, for it begins in ten or fifteen minutes, and is not completed in three or four hours; whilst the blood of the same animal, taken out of the veins, and exposed to air, will begin to jelly in three or four minutes, and will be completely jellied in seven or eight (XLI).

The effect that air has upon the blood is not immediate on its application, but takes place sooner or later, in different circumstances of health; in some cases the blood is coagulated in a few seconds after it has been exposed to the air, in others not in less than an hour and a half, or perhaps more, as appears from Experiment 14th.

(XXXIX.) Concerning the effects of a low temperature and of freezing on coagulation, see Note XI.

(XL.) The effect of air on coagulation is mentioned in Note XII.

(XLI.) On the effect of living tissues on coagulation, see Note XIII; on the slow coagulation of the blood in the body after death, see Note X.

The inflammatory crust or size is not a new-formed substance (XLII), but is merely the coagulable lymph separated from the rest of the blood. This separation seems to be occasioned by the lymph being attenuated (XLIII), by which means the red particles soon settle to the bottom, and leave the surface of the blood transparent; and this transparent part being a mixture of the coagulable lymph and the serum, the former coagulates on its surface where, in contact with the air, and the disposition to coagulate being likewise diminished, the blood remains a long time fluid, and thereby gives time for the pellicle formed on its surface to attract the rest of the lymph, and to collect it at the top, leaving the bottom of the cake proportionably softer (XLIV). The size therefore is thicker and denser, in proportion as the lymph is thinned (XLV), and is less

(XLII.) Dr. Benjamin G. Babington[a] concludes that fibrin and serum do not exist as such in the circulating blood, but that this is composed only of corpuscles and of a homogeneous liquor sanguinis; that there is no such animal fluid as coagulable lymph, since the liquor sanguinis is essentially liquid, separating indeed under certain conditions into two parts, of which one only is spontaneously coagulable; and that there is no better reason for affirming that fibrin exists in a fluid state in the liquor sanguinis than for affirming that muriatic acid exists in a solid state in muriate of ammonia. But it is difficult to reconcile Dr. Babington's conclusion with the fact, admitted by him, that serum differing very little from that of the blood, and unaccompanied by fibrin, is not unfrequent in the serous cavities; which fact, as Dr. Davy remarks[b], agrees with the opinion of Hewson and Hunter, that the fluid part of the blood is an equable mixture of lymph and serum, one spontaneously coagulable and the other not. The specific gravity of some pellucid serum which I took from a hydrocele was as high as 1024.

(XLIII.) See Notes XXIII and XXIX.

(XLIV.) The influence here attributed to the attraction of the pellicle is doubtful, because the rising of the fibrin, and the difference of consistency in the upper and lower parts of the clot, may be accounted for by the sinking of the corpuscles. When the liquor sanguinis of the horse is removed from the corpuscles, and allowed to coagulate in a tube, I find that the upper and lower parts of the clot do not differ in consistency. The observations of Dr. Davies on the specific gravity of the different parts of the blood are noticed in the Introduction. Dr. Davy[c] found the specific gravity of a mixture of human red corpuscles and serum, obtained by straining the broken-up clot through linen, to be 1074; of the cruor drained of serum 1087; of the buffy coat, by weighing, 1060; of the corpuscles, by immersion in a saline solution,

[a] Med. Chir. Trans. vol. xvi, p. 301.
[b] Researches, Physiol. and Anat. ii, 21.
[c] Researches, Physiol. and Anat. ii, 17-20.

disposed to concrete (XLVI). But it is not a certain sign of inflammation, and does not appear to be the cause of that disease, but only its effect.

But the most remarkable conclusion that these experiments have led to is, that the properties of the blood depend on the state of the blood-vessels, or that they have a plastic power over it, so as to be able to change its properties in a very short time (XLVII). The novelty of this opinion, and the difficulty of conceiving how the vessels should have so remarkable a power, has made some object to the conclusion, who had not well considered all the facts from which it was deduced. I shall here therefore sum up these facts.

That the blood-vessels, by their stronger or weaker action, can change the properties of the lymph, even in the short time spent in filling the different cups in bleeding, is first inferred from Experiment 19th, where the blood in the first cup had a size, whilst that in the others had none. Now as this want of size in the last cups was owing to the lymph having by this time become thicker, and to its being more disposed to coagulate, and no other circumstance being observed that could account for this alteration (for the difference in the exposition to cold or to air even then appeared inadequate to the effect, and are now proved to be so by Experiment XXXI), I concluded, that it was entirely owing to an alteration in the

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1132. Dr. Benj. Babington[a] estimated the specific gravity of fibrin at 1079, by putting it into a saline solution. But this estimate is probably too high; for a strong solution of salt may increase the specific weight of the fibrin, by depriving it of some of its natural moisture. Thus I have ascertained that a slice of the buffy coat of the horse's blood will float at first and afterwards sink in a solution of nitre, sp. gr. 1132; in a solution of Glauber's salt, sp. gr. 1084; and in a solution of muriate of soda, sp. gr. 1176. After the fibrin had been some weeks in these solutions their specific gravity became diminished, that of the nitre, for instance, being 1108, no doubt in consequence of the moisture extracted from the fibrin. In every instance they were weighed at a temperature of 60°. The fibrin did not appear to be at all dissolved. The objection of Dr. Babington to calculating the weight of the corpuscles by immerging them in saline solutions, entirely agrees with my own experience: (see Notes XXIII and XXIX).

(XLV.) See Notes XXIII and XXIX.

(XLVI.) See Note XXI.

(XLVII.) See Note XXIV.

[a] Cyclopædia of Anat. and Physiol. i, 418.

strength with which the vessels acted upon the blood. And to this opinion I was led by the well-known fact, that bleeding weakens the body; and as bleeding which weakens the body had here removed the size, by thickening the lymph, and by disposing it more to coagulate, I thence inferred that it was by its weakening the body, or the action of the vessels, that it had produced those changes on the lymph. The consideration of what takes place in inflammations confirmed me in this opinion; for in them the blood-vessels are known to act more strongly, and it is proved above, that the lymph is proportionably thinned, and has its disposition to coagulate proportionably diminished (XLVIII).

And this opinion was further strengthened by observing what occurs when an animal is bled to death, or when the vessels are acting with the lowest degree of strength; for here it was found that, in proportion as the strength of the animal was reduced, the blood was more and more disposed to coagulate (XLIX).

And having thus observed the connexion between the alterations in the lymph and changes in the strength of the blood-vessels in these cases, I next inferred there might be the same correspondence, even in others where the changes in the properties of the lymph are more sudden, as in Experiment 27th, where there was no size in the first cup, but a thick one in the third; and even this case, when carefully examined, confirmed the inference; for the blood-vessels were found to be acting with different degrees of strength, at the very time that the lymph was found to have different properties. And the only difficulty that remained here was to explain how it should happen that the first cup, contrary to what in general takes place, should have its blood coagulated in less time than the second or third; and this I concluded was owing to some febrile cause affecting or oppressing the patient, in which conclusion I am confirmed by the fact admitted amongst physicians, that the pulse is frequently oppressed in inflammatory disorders, and rises in strength in proportion as blood is taken away.

(XLVIII.) See Notes XXI, XXIII, and XXIX.
(XLIX.) See Note XXIV.

From this conclusion I went further, and conjectured that even temporary exertions of strength in the blood-vessels might alter the properties of the lymph; to which opinion I was led by having observed the blood sizy in the case mentioned in Experiment 20th, where great weakness soon followed the evacuation; and likewise from having observed, that the struggles of dying sheep seemed to alter the lymph (L).

So that upon the whole the opinion agrees with all the appearances, and is supported by all the differences in strength that occur in the various deviations from the standard of health: for when the vessels act more strongly than they do in health, the lymph is proportionably more thinned, and is less disposed to concrete; and when the vessels act more weakly than in health, then the lymph is proportionably thickened, and is more ready to concrete. Is it not therefore probable that the differences we observe in the thickness or thinness of the lymph, and in its being more or less disposed to coagulate, are owing to these differences in the strength of the blood-vessels? For such alterations in the strength of the blood-vessels are always connected with those of the lymph, and we can observe no other circumstance connected with those changes of the lymph that can at all explain them.

And although it must be admitted that it is very difficult to conceive how the blood-vessels should do this, yet I should hope that ingenious men would not merely on that account reject my conclusion; but would consider, that as it is deduced from a number of experiments, as it agrees with all the appearances, and as it leads to an explanation of many of them which we cannot otherwise account for, it may be well founded, although it be difficult to be conceived; for there may be powers in the animal economy that are not yet dreamt of in our philosophy.

This observation of the properties of the blood depending on the state of the vessels, besides explaining many morbid appearances, leads to a further application; for we may thence be led to advance more rational and more certain rules for the treatment of hemorrhages. For as hemorrhages seem to be stopped, partly by a contraction of the bleeding orifices, and

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(L.) See Note XXIV.

partly by the coagulation of the blood, and as the disposition of the blood to coagulate is increased by weakening the body, and likewise the contraction of the bleeding orifices is promoted by the same means, it is therefore evident that the medicines to be used should be such as cool the body, and lessen the force of the circulation; and experience teaches us, that such are the most efficacious (LI).

It likewise shows that all agitation of mind and all bodily motion should as much as possible be prevented; because they increase the force of the circulation, and are thence unfavorable to the stopping of the hemorrhage. But that languor and faintness being favorable to the coagulation of the blood, and to the contraction of the bleeding orifices, should not be counteracted by stimulating medicines, but on the contrary, should be encouraged. And as evacuations weaken the body more when they are sudden, we see a reason why bloodletting should be advisable in hemorrhages, and why a large orifice should be preferable to a small one, when we want to produce that languor or faintness, or that weak action of the vessels, so useful for the stopping of the hemorrhage.

Before we conclude, it may be added that the practice here proposed is far from being new or uncommon; but as some have recommended opposite methods, and both parties have appealed to experience, where authority so equally balances authority, the young practitioner must be at a loss which to follow, and for want of principles to direct his choice may frequently adopt the worst practice: witness the use of port wine, and other stimulating astringents, which is so very common in most parts of England.

(LI.) See page 47.

CHAPTER VI.

OF THE SERUM OF THE BLOOD, AND PARTICULARLY OF THE MILK-LIKE SERUM.

The serum, when separated from the crassamentum, by letting the blood rest in the basin into which it is received, is a fluid, apparently homogeneous and transparent, of a yellowish colour, saltish to the taste, in consistence thicker than water, and its specific gravity, according to Dr. Jurin, is to water as 1030 to 1000 (LII).

When chemically examined the serum is found to consist of a mucilaginous substance, which is dissolved in a water that contains a small quantity of neutral salts. The mucilaginous substance of the serum agrees with the coagulable lymph of the blood in being fixed or coagulated by heat; but the degree necessary for the coagulation of the serum is greater than that which is necessary for fixing the lymph, for the lymph is coagulated by a heat between 114 and 120½ degrees of Fahrenheit's thermometer (see Experiment 9th); whilst the serum requires 160° to coagulate it (LIII): (see Experiment 10th).

(LII.) Dr. Jurin's estimate[a] is probably rather above the mean. In five trials, Dr. Marcet[b] found the specific gravity of serum of human blood from 1024·5 to 1032·5. Serum of the blood of different animals, Dr. Davy[c] found to vary between 1020 and 1031; and of man labouring under disease, from 1021 to 1033. In soldiers bled for slight ailments, I have seen the serum 1025, 1026, 1028, 1029, and 1031; in all these instances the blood had a healthy appearance. The specific gravity of the serum of buffy blood is mentioned in Note XXII. The following numbers show the mean of Dr. Davy's observations on the sp. gr. of the blood and of the serum of mammalia:

Arterial blood .	1050	Venous blood .	1053
Arterial serum .	1022	Venous serum .	1026

(LIII.) On the heat most favorable to the coagulation of fibrin, see Notes III and XVI; and on the coagulation by heat of serum, Note XVII.

[a] Phil. Trans. 1719, xxxvi, 1007.
[b] Med. Chir. Trans. 1813, ii, 366.
[c] Researches, Physiol. and Anat. ii, pp. 17-25, 34-39.

But the degree of heat necessary for their coagulation is not the only circumstance in which the lymph differs from the mucilage of the serum; they differ more remarkably in the former coagulating when exposed to air, whilst the serum undergoes no such change.

When the serum is coagulated by heat, a watery fluid can be pressed out of it; and this fluid the learned M. de Senac distinguishes by the name of serosity (LIV).

This serosity contains the neutral salts of the blood, and although it has been exposed to the heat of boiling water, yet it still contains a part of the mucilaginous substance, which is combined with the water in such a manner as not to be coagulated by the heat of boiling water, till a part of the water is evaporated by boiling; and then it coagulates, and appears not very much unlike the mucus spit up from the aspera arteria in a morning.

When the mucilaginous part of the serum has been coagulated by heat, it cannot again be dissolved in the serosity, except by putrefaction or by the addition of some chemical substance, and then it differs from what it was before, particularly in its not being coagulable by heat.

But if the serum be exposed to a less degree of heat than is required for its coagulation, for example, to that of 100°, it is gradually inspissated into a brownish solid mass, and this mass can readily be dissolved again merely by the addition of water;

(LIV.) Mr. Hunter[a] claimed the discovery of the serosity. But it was probably known before 1760; for in that year Dr. Butt distinctly mentioned the serosity, and referred inaccurately concerning it, like Mr. Hewson afterwards, to Senac, as I have more fully explained in the Introduction. Dr. Cullen[b] described the properties of the serosity more perfectly. Mr. Hunter subsequently made many experiments on it; he believed, but by no means proved, that it is identical with the gravy of cooked meat, and observed that more gravy is contained in the meat of old than of young animals. The serosity is probably only the serum wanting the albumen which has been coagulated by heat; so that, cæteris paribus, the weaker the serum in albumen the more serosity there would be.

Dr. Bostock[c] has given the best account of the serosity, and full references to the observations of others on the subject.

[a] On the Blood, pp. 17, 32-4. 4to, Lond. 1794.

[b] Institutions of Medicine, § 252, 2d ed. 8vo, Edin. 1777.

[c] An Elementary System of Physiology, i, 473, 2d ed. 8vo, Lond. 1828.

and the serum seems to possess the same properties that it did before, particularly it is capable of being coagulated by heat. But care must be taken not to add more water than it had lost, for if more be added than was evaporated, it alters its properties of coagulation.

In this circumstance of being inspissated, and again rendered dissolvable in water, the serum agrees with the white of an egg,[1] but differs from the coagulable lymph, which even when mixed with a neutral salt (viz. true Glauber's salt), cannot be inspissated and dissolved again without coagulation.

If fresh serum be diluted with an equal quantity of water, and then exposed to heat, it does not coagulate in that of 160°, as when undiluted (see Note XVII), nor even in the heat of boiling water, as I have lately observed, but it can now be boiled without immediately coagulating. And as the water evaporates, a pellicle is formed on the surface, which becomes thicker and thicker as the evaporation advances. This pellicle seems to be the mucilage coagulated, for it cannot again be dissolved in water like the inspissated serum.

Milk, when boiled, has its mucilage or coaguable part separated in like manner, in the form of a pellicle, in proportion as the evaporation takes place. And both milk and serum, whether diluted or not, agree in being coagulated by rennet when exposed to heat (LV). So that milk seems to be made

[1] See Newman's Chemistry, sect. ix.

(LV.) Dr. George Fordyce[a] says that the serum may be coagulated by a juice secreted in the stomach. Dr. Davy[b] found that serum is not coagulated by calf's rennet; and this observation is confirmed by the following note with which he has favoured me: "I have repeated the experiment with rennet, and with the same negative result, whether using the weak brine in which the stomach of the calf was infused, or a portion of the membrane itself; whilst both coagulated milk, they had no effect on serum, not even appearing to render it coagulable at a lower temperature. The serum used was from the blood of the sheep. If Hewson were deceived, as I am inclined to believe he was, it may have been from his employing too high a temperature, and attributing to rennet what was the effect of heat." I have tried calf's rennet on the serum of the horse, without producing any coagulation, at a temperature gradually raised from 65°-103°.

[a] Elements of the Practice of Physic, part i, p. 3, 8vo, Lond. 1770.

[b] Researches, Physiol. and Anat. ii, 97.

of the mucilaginous part of the serum, or is a diluted serum, with the addition of an expressed oil, or with a saccharine substance instead of the neutral salts.

But although there is an analogy between milk and diluted serum, in the circumstance of coagulation, yet they differ in another, namely, diluted serum can by a moderate heat be inspissated without coagulating, or forming any pellicle on its surface; but if milk be exposed to the same heat, it is not inspissated so completely; for a pellicle is formed on its surface in proportion as the evaporation takes place, and this pellicle seems to be as perfectly coagulated as if the milk had been exposed to a boiling heat; for it will not dissolve again merely by adding water, as inspissated serum does.

Serum, therefore, by being diluted, comes near to milk in the circumstance of its coagulation. But the coagulable lymph cannot, by any art yet discovered, be made exactly to resemble serum (LVI).

The mixture with neutral salts makes it indeed so far ap-

(LVI.) According to the views of M. Denis,[a] the liquid mixture of coagulable lymph and a neutral salt is but serum with an excess of fibrin, which he regards as identical with albumen, and ready to be precipitated or coagulated under certain circumstances. See Notes VII, XII, XVI, and XVIII. Scherer[b] regards fibrin of arterial blood as distinct from fibrin of venous blood, because he finds the first insoluble and the next soluble in a saline solution. He attributes the difference to the action of oxygen, stating that fibrin is deposited from its saline solution after exposure to the air; and that the fibrin of venous blood, after having been some time exposed to the air, is no longer soluble in a solution of salt. I have kept slices of the buffy coat of venous blood, without any obvious alteration, except a little swelling, for months in saturated solutions of nitre, common salt, and Glauber's salt; the specific gravity of the solutions always became rather diminished. As mentioned in Note VIII, fibrin certainly undergoes modifications in its chemical properties some time after its coagulation. Mülder,[c] in support of Scherer's view as to the action of oxygen on fibrin, says that the protein compound which, during the coagulation of the blood, is changed into fibrin, absorbs oxygen in the lungs, and circulates through the arteries in the state of a bi-oxyde and of a trit-oxyde of protein; and that both of them, besides being ordinary constituents of blood, exist in it to an increased extent during inflammation, and form the chief part of the buffy coat.

[a] Essai sur le Sang, pp. 81-4, 8vo, Paris, 1838.

[b] Dr. Turner's Elements of Chemistry, 8vo, Lond. 7th ed. 1842, p. 1188.

[c] Chem. of Vegetable and Animal Phys. tr. by Dr. Fromberg, pp. 307, 314, 316, 341, 8vo, part ii.

proach to the nature of serum as not to be coagulated by exposition to the air; but it does not alter so considerably its property of coagulating by heat, for the mixture with Glauber's salts (in Experiment VII) coagulated at 125°; and, I believe, would coagulate in a heat of 123°, if long exposed to it; whilst the pure coagulable lymph is fixed between 114° and $120\frac{1}{2}$° (see Note XVI), and the serum not under 160° of Fahrenheit's thermometer (see Note XVII).

Of the white serum.

Although the serum of human blood be naturally transparent, and a little yellowish, yet it is frequently found to have the appearance of whey, and sometimes to have white streaks swimming on its surface like a cream, and now and then to be as white as milk, whilst the coagulum is as red as usual. In all these three cases of whiteness I have examined it in a microscope with a pretty large magnifier, and have found it to contain a number of very small globules, although naturally, when transparent, no globules can be observed in it, notwithstanding what has been affirmed by some authors. These globules differ from the red particles (improperly called globules) in their size, which is much smaller; and likewise in their shape, which is spherical, whilst the red particles are flat. They agree more with the globules of milk. I have compared them with those of woman's milk, and have found that in the milk the globules are of different sizes, some being three or four times as large as others, and the smallest little more than just visible, when viewed with a lens of one-twenty-third of an inch focus, whilst those of the white serum are more regular, and are all of them about the size of the smallest globules of milk (LVII). Of this white serum I have met with the following

(LVII.) This is an accurate account of the microscopical characters of the milky matter. A few granules, about $\frac{1}{6000}$ of an inch in diameter, are commonly mixed with it; but its bulk is made up of particles scarcely $\frac{1}{30000}$ of an inch in diameter, and agreeing in all respects with those which compose the molecular base of the chyle,[a] and give to it the white colour. These are not to be confounded with the chyle-globules, of which the average diameter is about $\frac{1}{4600}$ of an inch.[b]

[a] Appendix to Gerber's Anat. pp. 21, 98, figs. 274-8; and Dr. Willis's tr. of Wagner's Physiol. i, 247, fig. cxlix.

[b] See the Tables of Measurements, Note cxviii*.

instances in books: in Tulpius one instance;[1] in Morgagni two;[2] in the Philosophical Transactions some instances;[3] in Skenckius's Observations two cases are related from other authors.[4] I have likewise heard of the same appearance having been observed by the learned Sir John Pringle, Dr. Pitcairn, Dr. Hunter, Dr. Watson, Dr. Bromfield, Dr. Gartshore, and Dr. Fothergil of Northampton. And other instances have lately occurred to persons of my acquaintance. who have favoured me with a short account of them.

Mr. French, apothecary in St. Alban street, having informed me that he had some blood by him, taken from a woman the day before, whose serum was as white as milk, he favoured me with a small quantity of it for examination, and with it the following particulars of the case: "Mary Rider, about twenty-five years of age, of a fresh complexion, and lusty, has not had her menses for these seven months. She discharges blood sometimes by vomiting and sometimes by stool; complains of a pain in her left side and in her stomach; she has an inclination to eat, but when she tries, she soon after loathes her food. She complains of great lassitude and sleepiness; her pulse is ninety-five in a minute. She has been bled twelve times within these six months, and every time the serum was as white as milk."

Mr. Robertson, apothecary in Earl street, acquainted me, that "Mr. Herbert, a publican, of about thirty-five years of age, and corpulent, had been subject to a bleeding at the nose, to the piles, and to such profuse sweats in the night, as to be frequently obliged to change his shirt in the morning before he got out of bed, but that, for some time past, his sweats had ceased. That, on September the 23d, he was seized with a bleeding at his nose, which had been preceded by a pain in his head for two or three days. That his bleeding continued till he had lost about two pounds of blood, and then stopped; and that the serum of his blood was as white as milk. That at ten o'clock the same night, the hemorrhage returned, and he lost a considerable quantity; nevertheless, it was thought proper to take sixteen ounces of blood from his arm, during which evacuation he fainted, but his bleeding at the nose stopped. That the serum of this last blood was likewise very

[1] Tulp. Ob. l. i, cap. 58.

[2] Morgagni, Ep. xlix, art. 22.

[3] Philosoph. Transact. Nos. 100, 442.

[4] Sckenckij Obs. lib. iii.

white. That on the 25th, in the morning, he again complained of a pain in his head, and about ten o'clock his nose began to bleed again; but the serum now appeared no whiter than whey. That he continued to lose blood during most part of the night, so that it was supposed he could not lose less than two or three pounds, the serum all this time being a little whitish, but so little, that the bottom of the vessel in which it stood could now be seen through it. That his bleeding returned repeatedly till the 3d of October, when it entirely stopped, the serum having become more transparent towards the last."

Mr. Eustace, apothecary in Jermyn street, sent me a phial of white serum from one of his patients, by trade a butcher. "This man," he told me, "was tall, of a strong make, a hard drinker, subject to puke every morning, took little food, sweated a good deal, but did not waste in his flesh. He was bled for a slight asthma to which he was subject, and of which he had always been relieved by bleeding. In other respects he was in a good state of health, so as to follow his business without much inconvenience."

Besides these cases, my friend Mr. Lambert, surgeon at Newcastle-upon-Tyne, told me, "that he had a patient some years ago with a violent rheumatic pain in his hip, whom he was obliged to bleed thrice, and every time his serum was as white as milk, but the coagulum of its natural colour. This gentleman," Mr. Lambert adds, "was a free liver, of a full make, but rather muscular than corpulent, and remarkable for being a great walker."

When I first saw this unusual colour of the serum, I was inclined to adopt the opinion of those who have attempted to explain it by the patient's being bled soon after a meal, or before the chyle was converted into blood. But afterwards, on considering the cases above related, I found this could by no means be the cause, as none of these patients had taken a sufficient quantity of food to occasion this appearance; on the contrary, most of them had a bad appetite, and had taken remarkably little food, and were subject to vomitings. I therefore concluded it was owing to something else, and what confirmed me in this opinion was an observation I had repeatedly made in dissecting geese, whose serum I had frequently seen white, whilst their chyle was transparent; although they had been killed only three or four hours after eating. And as the whiteness, in all the cases that I examined, was owing to a quantity of small globules like those of milk (which are known to be oily) I concluded that these in the human serum, when white, were oily likewise, and recollecting to have read somewhere of an experiment by which butter had been got from such human serum, I tried, by agitating some of it a little diluted, to separate its oil, or to churn it, but without success. I then inspissated some of it to dryness, and compared it with the natural serum of human blood prepared in the same way, and found it less tenacious, and much more inflammable; and when thus dried, its oil oozed out so much as to make the paper in which it was kept greasy (LVIII). Another portion

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(LVIII.) Mr. Hunter[a] observed that the milky serum is not the same in all cases, as the globules which form the wheyish appearance sometimes swim and sometimes sink in water; and he believed that the supernatant milky matter does not exist in the circulating blood, but is formed after the separation of the serum. Hewson's proof, that the milk-like matter is of a fatty nature, has been confirmed by Dr. Traill,[b] Dr. Christison,[c] Dr. Benjamin Babington,[d] and Heller.[e] I have several times seen the milk-like matter on the surface of the blood, both arterial and venous, before there was any separation of serum and before coagulation was complete.

The milky appearance on the blood or in the serum was ascribed by Boyle and Lower[f], Gibson,[g] St. Hilaire,[h] Lister,[i] Dr. Arbuthnot,[j] Quesnay,[k] Stuart,[l] Senac,[m] William Fordyce,[n] and Rush,[o] to an admixture of chyle with the blood. This opinion has been confirmed by the experiments of Hutchinson,[p] whose dissertation I have not seen, by Mr. Thackrah,[q] Dr. Buchanan,[r] and myself.[s] But there is a turbid and whitish serum neither owing to the presence of chyle, nor to oily matter; and probably connected with disease, as indeed a fatty serum sometimes is. In one specimen of serum resembling thin water-

[a] Works, ed. by Palmer, iii, 55-6.

[b] Edin. Med. and Surg. Journal, xvii, 235, 637; and xix, 319.

[c] Ibid. xxxii, 286.

[d] Med. Chir. Trans. xvi, 47-50.

[e] Dr. Simon's Animal Chemistry, tr. for Syd. Soc. p. 271, 8vo, Lond. 1845.

[f] Phil. Trans. 1665-6, i, 117.

[g] Anat. of Human Bodies Epitomized, p. 276, 3d ed. 8vo, Lond. 1688.

[h] Anatomie du Corps Humain, tom. ii, p. 124, 8vo, Paris, 1698.

[i] Dissertatio de Humoribus, 8vo, Lond. 1707, p. 236.

[j] Essay concerning Aliments, p. 33, 8vo, Lond. 1731.

[k] Principes de Chirurgie, 8vo, Paris, 1746, p. 34.

[l] Phil. Trans. 1735-6, xxxix, 289.

[m] Traité du Cœur, tom. ii, p. 77, ed. 1749.

[n] Inquiry into the Causes of Putrid and Inflammatory Fevers, 8vo, Lond. 1773, p. 24.

[o] Med. and Phys. Journ. 1806, xvi, 199.

[p] Inaug. Dissert. on the Conversion of Chyle into Blood, 1804.

[q] On the Blood, ed. 1834, p. 130, Exp. CLXVII.

[r] Trans. Glasgow Phil. Soc. March 1844.

[s] Appendix to Gerber's Anatomy, pp. 21-2, 90.

of this white serum being kept some days, putrefied, and when putrid, it jellied as milk does when become sour; but it differed from milk, in being extremely fetid.

Now, as the white globules appear from these experiments to be of an oily nature, and as it is improbable, from these patients having taken little food, and from the transparency of the chyle in birds, that this whiteness of the serum should be owing to unassimilated chyle, accumulated in the blood-vessels; we must therefore believe it to be owing to some other cause. And as we know there is a considerable quantity of oil laid up in the cellular substance of animals, which is occasionally reabsorbed is it not most probable that this curious appearance was, in the above-mentioned cases, owing to such a reabsorption? And as all these patients had symptoms of a plethora,

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gruel, Dr. Traill could detect no oil. Dr. Bostock[t] obtained similar negative results from an examination of turbid and cream-like serum in cases of dropsy with albuminous urine; it was in this disease that Dr. Christison referred the cloudiness of the serum to the presence of oily matter. Dr. R. D. Thomson found the white matter oily in one of Dr. Buchanan's cases, and in another not so. In a case of anasarca, Mr. Ancell[u] observed the serum in the highest degree turbid, but not milky. I have seen three specimens of turbid whitish serum in the human subject, in which the opake matter sunk in water, was not at all soluble in ether, and appeared irregularly granular under the microscope, resembling coagulated albumen, but quite unlike chylous or fatty matter.

In Dr. Benjamin Babington's observations,[v] the specific gravity (see Note XXII) of the milk-like serum appeared to be so regularly reduced, as to lead him to believe that the oil exists at the expense of the albumen. There are some facts in favour of the idea that albumen may be converted into oil. The rapid disappearance of the matter of the curd of perfectly fresh salmon, with the subsequent more oily state of this fish, may be owing to a conversion of this kind, according to the conjecture of Sir Humphrey Davy, as I have learnt from Dr. Davy. I have observed that the oil in the liver of several fishes increases after death, probably in connexion with incipient putrefaction; and Dr. Davy informs me that in the liver of the cod, after it had been kept in a damp place for twenty-five days, he found a small increase of oil, with a formation of ammonia and carbonic acid at the same time. Perhaps, as Dr. Babington suggests to me, there may be a conversion of vegetable albumen into oil, in olives, after they have been heaped up and subjected to fermentation; by which the quantity of oil is said to be increased, though its quality is not so good.

[t] Dr. Bright's Reports of Med. Cases, 4to, i, 83.

[u] Lectures on the Blood, Lancet, vol. i, 1839-40, p. 607.

[v] Cyclopædia of Anat. and Physiol. i, 423.

and were relieved either by spontaneous hemorrhages, or by bloodletting, is it not probable that, to whatever purpose (see Note LX), the oil is applied in the body after it is reabsorbed from the cellular membrane, in these patients it had been reabsorbed faster than it was applied, and by that means was accumulated in their blood-vessels? This conjecture seems to be confirmed, from considering that in most of these cases the people were inclined to corpulency, and that two of them laboured under a stoppage of a natural evacuation.[1]

Another conclusion which these observations lead us to is, that since the chyle of the birds (LIX) which I dissected was not white, but transparent, at whatever time after eating it was examined, it follows, that the fat (in these animals at least) is not merely the oily part of the chyle or of the food, but is a new substance, or a new combination of the principles or elements, which is made probably in the secretory organs of the adipose membrane; the form of oil being made use of by nature in preference to any other for the nutritious substance of the body, from its being the least liable to putrefaction, and from its containing the greatest quantity of nourishment in the least bulk. This circumstance was clearly proved by my valuable and ingenious friend the late Dr. Stark (LX), who, in a

[1] Although it appears probable that the whiteness of the serum in the above-mentioned cases was not owing to the chyle, yet I would not conclude that the chyle does not in the human subject occasionally colour the serum. We frequently observe the serum of such people as are bled a few hours after a meal, a little turbid, like whey, which I believe may be owing to the chyle. But if the milk-like serum was occasioned by a full meal, it is likely we should oftener see it than we do.

(LIX.) The chyme of birds differs very little from that of mammalia. In geese fed exclusively on barley, in many wild finches, and in pigeons fed on nothing but peas, I found a much greater proportion of fatty matter in the chyme than in the food. An observation, communicated to me by Dr. Davy, is still more conclusive: in the chyme of a pheasant he found oily matter, which he could not detect in the food, consisting of pilewort, in its crop. The subject is interesting in connexion with the contending views of Liebig, Boussingault, and Dumas,[a] on the question of the formation of fat in animals. The chyle of birds, as Dr. Davy suggests to me, may perhaps enter the circulation by the veins rather than by the colourless lymphatics.

(LX.) Dr. William Stark's works, consisting of clinical and anatomical

[a] Mr. Paget's Reports, Br. & For. Med. Rev. xvii, 260; xix, 564.

course of curious experiments, made by weighing himself after living for some time on different sorts of food, discovered that a less quantity of suet was sufficient to make up for the waste of his body, than of any other sort of ordinary food; and that when compared with the lean part of the meat, its nutritive power was, at least, as three to one.

I may here add another circumstance that occurred to me when I first thought on this subject, which is, that since we believe the oil, or animal fat, is reabsorbed from the adipose membrane to serve for nourishment to the body; and as some of the patients (whose cases have been related above) could not take food, the reabsorption therefore of this oil might not be so much the cause, as the effects of the disorder under which they then laboured; or, in other words, that upon some defect in the digestive organs, the powers of nature drew from their magazines of oil in the adipose membrane a supply of

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observations, with experiments dietetical and statical, were revised and published from his original MSS. by Dr. James Carmichael Smith, 4to, Lond. 1788. Dr. Stark died in 1770, seemingly from the effects of his experiments; he was encouraged in them by Dr. Franklin and by Sir John Pringle.

Liebig considers fat as chiefly useful, by its slow combustion during respiration, in maintaining animal heat. Nearly two centuries ago, an opinion was current, and opposed by Needham[a] that the body is kept warm by an internal combustion of the blood, chiefly in the lungs. Fatty matter, I believe,[b] is intimately concerned in growth and nutrition, healthy and diseased. The base of the chyle, referred to in Note LVII, is made up of minute and equal-sized fatty or oily particles; the molecules or seeds of cells, so abundant in the semen of oviparous vertebrate animals, just before the breeding season,[c] are composed chiefly of fatty matter; and such also is more or less the nature of elementary granules generally, and of the nucleoli of cells. The analyses of Dr. Davy support my observations as to the fatty nature of the molecules of cells in disease.[d] Mr. George Ross[e] is of opinion that the general lymphatic system converts oily matter into a compound of protein; and he states that Dr. Carpenter included in this idea the whole supposed waste reorganizable matter of the system.

[a] De Formato Fœtu, cap. vi, 12mo, Lond. 1667.

[b] Med. Chirurg. Trans. xxvi, 93-6; London Med. Gaz. June 21, 1844, p. 411.

[c] Proceed. of Zoological Society, 1842, pp. 99 et seq.

[d] Notes appended to Dr. Boyd's Vital Statistics, in Edin. Med. and Surg. Journ. lx, 159 et seq. Henle, Anat. Gen. tr. par Jourdan, p. 126; Ascherson, Mr. Paget's Report, Br. and For. Med. Rev. xiv, 263.

[e] Lancet, 1843-4, i, 629.

that fluid then perhaps necessary for the use of the body. In order to clear up this point, I thought it would be a satisfactory experiment to compare the serum of the blood of animals at different periods after feeding them. For if the reabsorption of the oil was merely to make up for the want of other food, or if the serum was white merely from a greater quantity of oil being taken up in order to supply the wants of the body, then the serum ought to be whitest in the animal kept longest without food, or whose body was most in want. And as I had found that geese had very commonly this white serum, though their chyle was transparent, I chose to make the experiment on them. I therefore took two of them that were very hungry, and feeding both of them with oats, one I killed four hours after, when I knew a part of the oats were undigested; and upon examining the blood, I found the serum whitish, and full of small globules; on its being suffered to stand a little time, the white part ascended to the surface like a cream. The other was killed forty-eight hours after eating, when its stomach was found empty, and the serum of its blood quite transparent, and without any cream rising to the surface, or any appearance of small globules, when examined with the microscope. Now this experiment seemed to me decisive, and to point out clearly that the whiteness of the serum was not occasioned merely by the body being in want of food, and therefore drawing the oil from its magazines; because here the animal most in want of food had its serum least white; but was occasioned by the fat being reabsorbed faster than it was used (from its place being supplied by the fresh chyle) and thence was accumulated in the blood-vessels, so as to give whiteness to the serum. And from the same observation it likewise appears probable, that the great reabsorption, and the accumulation of the fat in the vessels of the plethoric patients above mentioned, was the cause of their want of appetite, and of their other complaints, and not the effect of them.

May not therefore a too great reabsorption of the fat, and its accumulation in the blood-vessels, be now admitted as the cause of one species of a plethora?

And may it not likewise be useful in some complaints of the stomach to attend to the whiteness of the serum? For although fat be a substance little liable to disease, yet it may perhaps be sometimes so vitiated, or may so incommode nature, that she may be obliged to take it up from her magazines, and to use it, or to throw it out of the body. Whilst this is doing, a sickness of the stomach and want of appetite may be indications of fulness; and therefore, instead of wanting remedies to strengthen the stomach, may require bleeding and other evacuations.

APPENDIX,

RELATING TO

THE DISCOVERY OF THE LYMPHATIC SYSTEM IN BIRDS, FISH, AND THE ANIMALS CALLED AMPHIBIOUS;

BEING

A VINDICATION OF THE AUTHOR'S RIGHT TO THESE DISCOVERIES, IN OPPOSITION TO THE CLAIM OF DR. ALEXANDER MONRO, PROFESSOR OF ANATOMY IN THE UNIVERSITY OF EDINBURGH.

An account of the lymphatic system in birds, fish, and a turtle, was given to the public in the Philosophical Transactions, vols. lviii and lix, for which communications the Royal Society has since honoured me with their gold medal. These discoveries Professor Monro claimed, in a letter read before that most respectable body on the 19th of January, 1769, and has since persisted in that claim, in a pamphlet called 'A State of Facts,' &c., printed at Edinburgh, 1770. Now as both that letter and the pamphlet must of course have been seen by many who know not what can be urged against them, I think it but a duty I owe my own character to lay before the public the proofs I have collected of their insufficiency to procure Professor Monro the credit of having anticipated me in those discoveries; and I hope that, although in doing this I shall trespass on the time and patience of the reader, yet it will be some excuse for me that I had endeavoured, as much as could be expected on my part, to settle the dispute without troubling the public with it.

As Professor Monro has, in this pamphlet, not only endeavoured to vindicate his claim to these discoveries, but has likewise censured me on account of a paper on the emphysema, it is necessary, before I come to the controversy about the lymphatics, that I should relate what has passed between us on that occasion.

In the third volume of the 'Medical Observations and Inquiries' is published a paper on the emphysema, in which I proposed the operation of the paracentesis of the thorax, to let air out of the cavity of the chest, which air I endeavoured to show was the cause of the worst symptoms attending that disease. Not long after this, I was informed that Dr. Monro had declared publicly he had mentioned that observation in his lectures, both at the time and before I attended them, which was in the winter 1761, and complained that I had omitted doing him justice in this particular.

When I heard this I made inquiries of some of his pupils, who I found had taken notes at his lectures, and by two of these gentlemen I was favored with excerpts from their notes, which convinced me that he had anticipated me in proposing that improvement. I then determined to let him know that my omitting to mention his name on that occasion was entirely owing to my ignorance of his claim. This I was the more desirous of doing, from having heard that he had exclaimed against me with some acrimony, on the supposition that I had got the hint from him, and was conscious of it, which being far from the truth, I determined to show him in what manner I had really made the observation, and thereby stop his exclamations. I determined likewise to show him that I was desirous of giving him the credit of having had the idea before me, and thereby to prevent all dispute about the matter. The following is a copy of the letter which I sent him on that occasion:

"Sir,—Being informed that you have publicly complained of me 'for having, in a paper printed in the third volume of the Medical Observations and Inquiries, omitted doing you the justice of mentioning your having proposed the operation there recommended, in the same circumstances, long before;' and as I am confident I deserved not to be complained of on that account, I have taken this opportunity of stating the manner of my making the observation, and at the same time of letting you know that since I have learned that you likewise had made it, I am willing to do you justice. The thought first occurred to me in reading Mr. Cheston's Pathological Inquiries and Observations, in which he gives a case of the emphysema; this case is told in such a manner that I think it is hardly possible any unprejudiced person should read it and not be convinced, as I was, that the cause of the principal symptoms was air in the cavity of the chest. Mr. Cheston himself, in relating that case, came as near making the observation as possible.[1] From this hint I prosecuted the subject, as is mentioned in that paper; and before I published it, I consulted every author I could easily procure, who I thought was likely to treat of the subject. And I certainly should have done justice to any that I found had anticipated me, and should not have avoided the opportunity of doing you the same justice. But I knew not, at the time of that publication, that you had ever given the least hint on that subject. About the middle of last summer I was told by a gentleman from Edinburgh of your manner of treating me, at which I was not a little surprised, as I was not conscious of having given you the least cause of complaint. But having since learned, from other gentlemen who attended your lectures before the time of my publishing that paper (and who, at my request, consulted their notes) that you had really mentioned it, I cannot now doubt that you had made the observation before me. At the same time I must assure you, that to suppose I knew it at the time of publishing that paper was doing me injustice. Your accusation, I presume, is founded on the supposition of my having heard you deliver the observation at your lectures, when I had the pleasure of attending them. But I do assure you, that if I ever heard the least hint on that subject, either from you or from any other person, I had not any remembrance of it at the time I wrote that paper. You are not, indeed, the only person who, as I now find, has anticipated me: the author of the Monthly Review for last June[2] says he had long had the same idea, and that he mentioned it in his account of Mr. Cheston's book. But of this too I assure you I was ignorant when I wrote my paper. What must give farther conviction to any unprejudiced person of my ignorance of your having made the observation is this—I first mentioned it in a paper

[1] I have since been informed by Mr. Cheston that I misunderstood his meaning, when I concluded that he explained the worst symptoms of the emphysema on different principles from what I have done, and that he meant to attribute them to air in the chest, and, therefore, that the observation was sufficiently made out in his paper; which see in his 'Pathological Inquiries and Observations,' pp. 7, 8.

[2] See Monthly Review for June, 1768, p. 446.

which I read to a private society, in which were present many gentlemen that had attended your lectures, and yet all these gentlemen expressed themselves pleased with the observation, as new and interesting, and not one of them gave the least intimation of their having ever heard it before. And yet those gentlemen are as likely to remember any observation which tends to the improvement of physic or surgery as any I know. I shall mention their names, to justify my good opinion of them—Drs. Stark, Parsons,[1] Saunders,[2] Pepys,[3] and Ruston.[4] The observation was likewise mentioned in another society of young gentlemen, and also in a public hospital, where many who had been your pupils heard it; and yet no person ever told me before I published that paper (which was almost a twelvemonth after I had first mentioned the subject) that you or any other person had ever anticipated me. However, this I relate only to show I was ignorant at that time of your having made the observation. But now I know that you had, I have not the least unwillingness to acknowledge it, and to do you justice in any future publication. At the same time that I justify my own conduct, give me leave to say that your manner of treating me (if fairly represented) was not so civil as might have been hoped for. When you complained of me, 'tis a pity you had not likewise hinted there was a possibility of my being ignorant of your having had the idea. You might perhaps too, without impropriety, have hinted that should it come to my ears that you had anticipated me, I might possibly be capable of such an exertion of candour as to acknowledge it. But, to have done with suppositions, this at least I am sure of, that though I may be as covetous of fame as most people, yet I am incapable of taking any unjustifiable methods of acquiring it. "I am, Sir, &c.

"Dec. 31, 1768."

This letter, Professor Monro could not but acknowledge, "sufficiently satisfied him in having secured his title, as the

[1] Professor of Anatomy at Christ's Church, Oxford.

[2] Physician to Guy's Hospital.

[3] Physician to the Middlesex Hospital.

[4] Some of these gentlemen attended Dr. Monro's lectures about the same time with myself, the others since.

first who had proposed that improvement." Yet so unfair an account does he give of it in his 'State of Facts,' that he only says I acknowledged that "I could not doubt he had made the observation before; but the farther particulars of it (he adds) it is needless to trouble the reader with, since as much as is necessary of these will be sufficiently understood from his letter" in answer to me, which surely is not the case; for it nowhere appears in his letter that, besides mentioning my conviction of his having anticipated me, I had likewise promised to do him justice in a future publication. Nor does it appear in his letter that I had in mine shown how little probability there was of my having got the idea from him. These the reader may perhaps think Professor Monro ought to have declared, in justice to me. For what more could be expected of me, seeing I had by accident hit upon an observation which, as it happened, he had made before, than to acknowledge the priority of his title as soon as I knew it, and to put that letter into his hands by which he might always be sure of securing to himself what was his due. But Professor Monro says it was unnecessary to give a fuller account of my letter. But why was it so? Not surely in justice to me, nor for the satisfaction of the reader. Nay, so far is Dr. Monro from doing me justice on this occasion, that he even intimates I rejected tapping the chest with a trocar, because it happened to be his method, as if the same was not the method recommended by many of the writers on the subject of the paracentesis of the thorax, for the cases in which they advise that operation, to whose method I alluded, and not to his, which I then knew nothing about.

Next, as to the discovery of the lacteals and lymphatics in birds, fish, and the animals called amphibious; of these an account was laid before the Royal Society on December the 8th, 1768. I was present when it was read, and had afterwards some conversation on the subject with Dr. Donald Monro, who, as appears by the sequel, informed his brother, the Professor, of what I had done. Not long after this, I again saw Dr. Donald at St. George's Hospital, and he then told me, that the lymphatics and lacteals in those animals[1] had been discovered by his brother eight years ago, as he now learnt by a

[1] Meaning birds and fish.

letter from Edinburgh, a part of which letter he was to show to every body, and which was already given to be read before the Royal Society. When I was informed of this, I was astonished, as I remembered to have heard the Professor, since that time, declare that they were not discovered. Besides, I had a note taken from his lectures within two years of his making this claim, in which was a similar acknowledgment.[1] I was convinced therefore that he had no title to these discoveries. Upon which I laid before the Royal Society my reasons for that conviction, in a letter to one of their secretaries. Of this letter I shall give the reader an account, but shall first lay before him a literal copy of Dr. Monro's claim.

Copy of Dr. Alexander Monro's claim, &c. read before the Royal Society, Jan. 19, 1769.

"Above four years ago (says he) I injected the lacteal vessels of a turtle, or sea-tortoise, with quicksilver, after injecting the artery and vein with wax, and have shown this instance of the vessels in the oviparous animals every year in my college, and had a drawing made of it two years ago by Dr. Palmer, a copy of which I have sent inclosed, engraved by Donaldson.

"I likewise, eight years ago, mentioned those vessels in fowls and fishes, which I had seen, but not injected."

Here then is an assertion about the vessels which I had discovered, that is far from being equivocal. For here he affirms, that he really had seen them eight years ago, nay, that he had even mentioned them to others. This letter too was sent immediately after he heard that I had laid before the society an account of those vessels in birds and fish.[2] It could not therefore be meant merely to inform the society that, now seeing Mr. H. had discovered the lacteals and lymphatics in birds and fish, he likewise had the pleasure of shewing them that he (Dr. Monro) had discovered them in the turtle. This, I say, could not be his meaning; for if it was, why did he send his letter so precipitately? Why did he not send a description of those vessels in the turtle, in order to make his letter worthy of their notice? And why did he say he had seen them in birds and

[1] That note is now printed below, p. 98.

[2] As he acknowledges, 'State of Facts,' p. 4.

fish? The society, he knew, wanted not his testimony to prove that birds and fish had them. What, then, could he mean by it, but to claim the discovery?

As there could be no doubt that Professor Monro meant this letter as a claim to these discoveries, neither had I any doubt but I should, for the reasons above mentioned, prove that he had no right to them. In order therefore to prevent the prejudices that might arise against my papers, from his being believed to have anticipated me in these discoveries, I wrote a letter to one of the secretaries of the Royal Society, in which I first showed that I had seen the lacteals of the turtle about a year before Dr. Monro, and then, when I came to speak of those vessels in birds and fish against the probability of Dr. Monro's having anticipated me in these discoveries, I made use of the following arguments:

1st. His not having, by his own confession,[1] injected them, which he certainly would have done in order to complete the discovery. To which I observed he had the strongest motive, both from his knowing the importance of the subject, and from his having unfortunately declared, in the 57th page of his 'Anatomical and Physiological Observations,' printed at Edinburgh, 1758, "that, after a considerable number of experiments which he had made, he was convinced that neither birds, fishes, nor oviparous animals in general, had either lacteals or lymphatic vessels." After which declaration I conceived it improbable he should patiently wait eight years without injecting them, especially as I had found it an easy matter to inject them, when once they were discovered; and, I added, the probability was that if he had seen those vessels, he would have hastened to inject them, and to complete the discovery, were it only to prevent another person's doing it, and thereby acquiring the reputation of having done what he himself had in vain attempted by such a considerable number of experiments as were sufficient to convince him that such vessels existed not in those animals.

2dly. I said his claim to the discovery of those vessels, by affirming he had seen them eight years ago, was contradicted by public declararions made after that time; for he had since acknowledged in his lectures, that he had sought for them in

[1] In his claim; see above, p. 96.

vain by a variety of experiments. And even so late as within these two years,[1] he declared likewise in his public lectures, "that the lymphatic system was supposed to take place only in men and viviparous animals, and by analogy in those fishes placed by Linnæus amongst the mammalia, and how far was their just extent (he said) was not certain, but that he had found them in some amphibious animals, as in the turtle." These declarations, I observed, were inconsistent with his claim to the discovery.

Besides using these arguments, I promised the Society I would hereafter produce unquestionable proofs of the invalidity of his claim, having by this time found that the Doctor, fortunately for me, had expressly acknowledged in his lectures, that he had sought for them in vain, almost every year since the time that he now pretends to have seen them.

Dr. Monro being informed of these proceedings, sent me his letter, dated June the 8th, which he has since printed in his 'State of Facts.' But that letter appeared so confused, that I knew not what to make of it. Sometimes I thought it was meant to prove that he had discovered those vessels, agreeably to his assertion read before the Royal Society: but this I soon after suspected could not be the case; because, after relating all his facts and experiments, he concludes, not that he had discovered them, but only "that he had seen what he strongly suspected to be lacteals in those animals" (viz. birds.[2])—And, "that (from preceding experiments) he was persuaded that birds were provided with lacteal vessels, and confirmed in this opinion by having injected them in one of the same oviparous class, the turtle;"[3] or, in other words, Dr. Monro claimed a discovery by telling the Royal Society that he had seen those vessels in birds eight years ago, which he now proves by showing he had only suspicions about them, and an opinion that birds had them because turtles had them.

At other times I thought that possibly, after finding what his pupils testified, he might now be convinced he had imprudently claimed those discoveries, and might intend this as a sort of an acknowledgment (though an awkward one) of my being the first who had seen those vessels. But the vindictive

[1] My letter being dated Jan. 10, 1769. [2] State of Facts, p. 21. [3] Ibid. p. 23.

style of his letter convinced me it was not meant as an apology,[1] as likewise did the style of a short note written on the cover of that letter, of which note the following is a copy.

To Mr. Hewson, &c.

Sir,—When you have read the inclosed, you are very welcome to write such remarks on it as to you, or to your friend Dr. H—r, or to any of his friends, such as Dr. ——, &c. may seem proper; only when you have done, I think you ought to show it to all those societies, physicians, and students to whom you have made free with my name; or, if this talk should not suit your disposition, or be irksome to you, after the great fatigue you have taken about me already, please to let me know this, and I shall take that trouble on myself. I am, sir, &c.

(Signed) Alex. Monro.

Edinburgh, June 24, 1769.

This seemed clearly not to be the style of one who was sensible of his error, and was apologizing for it; and convinced me that Professor Monro intended that letter as a proof of his right to those discoveries. However, not to be positive that I had hit upon his meaning, I determined, before I laid anything before the public, to ask an explanation of that letter. For this purpose I wrote to him on the 15th of July, and desired him to tell me "whether he meant it as a proof of his right to those discoveries;" or, "whether he meant by it to give up to me the right to them." And as I had found him in that letter wandering from the subject, and instead of concluding that he really had seen those vessels, concluding only that he had seen what he suspected to be the lacteals in birds. And again, that he was persuaded birds were provided with those vessels, but nowhere saying that he had seen what he knew or could prove to be their lacteals, which alone could give him a right to the discovery; I therefore told him "that, to avoid for the future all wandering from the subject, I should state the dispute as it appeared to me;" and then I said that "it was he who began it, for, on hearing that I had discovered the lymphatic

[1] As for instance where he talks "of (his) making all the allowance I require for my natural, or," says he, "I should rather call it, unnatural imbecility of memory." This passage is altered in his 'State of Facts,' p. 22.

system in the three classes of oviparous animals, he had sent a letter, which was read before the Royal Society, and was to be shown to everybody. In this letter he asserted that he had discovered the lacteals in a turtle about four years ago, and in birds and fishes eight years ago, and that he even mentioned these discoveries to others." These assertions (I added) were construed a claim to the discoveries I had made. With this letter I likewise sent him a copy of mine, which had been read before the Royal Society. By these means I thought I should either keep him to the subject in question, or, if he should again wander, the reader would be convinced it was not my fault, but his own, that he now knew not what he had then asserted.

This letter, however, had no effect. I therefore wrote to him again, hoping he might now be convinced that his claim was ill-founded, and might therefore be induced to retract it, instead of obliging me to prove to the world its invalidity. The following is a copy of the letter which I sent him on that occasion:

Sir,—It is now above six weeks since I wrote to you, desiring an explanation of your letter of the 8th of June. As you have not given me that explanation, I have now taken up the pen to inform you, that agreeably to your own desire, and in order to justify my conduct towards you, I am commenting upon that letter which you sent me. My comment would be more to the purpose, were I always sure I understood you, but if that satisfaction should still be denied me, I must proceed as well as I can, and I must say, that if I should mistake your meaning, it will not be wilfully, since you might, by an answer, have cleared up all ambiguity. I cannot help regretting that this dispute should subsist between us, both on my own account, as I think it hard to have the trouble of proving my right to discoveries which are certainly my own, particularly as it takes up that time which I hoped to employ to a better purpose; and I likewise regret it on yours, since, in order to maintain my right, I shall be under the necessity of producing some facts and testimonies, which, in my opinion, cannot but lead to conclusions very unfavorable to your reputation. And as I should be sorry that one of my first attempts to lay the foundation of my own character, should be attended with circumstances which may hurt yours, and really wish to avoid it; I therefore still hope that this dispute may be settled in a more easy manner. You must, I think, be now convinced, that in claiming these discoveries you have injured me, and cannot be at a loss to know what might be expected from you on such an occasion. But if instead of doing me that justice which might be expected from a man of candour, you treat this letter likewise with silence, then justice to myself requires that I should no longer delay producing such proofs as I possess of your having no right to these discoveries, and showing them to the very respectable Society to which I have promised them; or to such physicians, surgeons, &c. as may have heard of your claim; without regretting much that those measures which I take to maintain my right, may perhaps affect sensibly the character of a man, who having first injured me and afterwards had his error pointed out to him, was incapable of candidly acknowledging it.—I am, sir, &c.

(Dated) *Sept.* 9, 1769.

In answer to this letter, he sent me one dated September 30, in which, instead of answering my questions, he evades them, concludes as vaguely as in his former; and when he speaks of his assertions read before the Royal Society, alters its sense, qualifying the alteration with "to the best of his recollection," denies he was mistaken in claiming these discoveries, and what is still more remarkable, accuses me of having made conclusions injurious to him, "by arguments, weak, inconclusive, not real, but feigned."

It was evident, from such a letter, that he would not embrace the opportunity I offered him, and avoid a dispute, by acknowledging his mistake, and retracting his claim. I therefore no longer hesitated to print the proofs I had collected of his not having anticipated me; and though I had once intended to make some remarks on his letter of the 8th of June, as is mentioned above, yet I afterwards determined to omit these, since the testimonies of his pupils alone sufficiently proved that he had no right to those discoveries. By these means I reduced the publication to half a sheet of paper: in which I first gave an account of his claim made, by saying "he had seen those vessels eight years ago;" then I mentioned, as arguments against its validity, that I had myself heard him since that time declare, "he could not find those vessels," and that, besides, I had a note taken at his lectures by a gentleman within two years of his making it, which contained a similar declaration,[1] and afterwards I said that I had written to such gentlemen as I knew had, as well as myself, attended his lectures within these eight years, desiring them to consult their notes, and to let me know what Dr. Monro had said as to the existence, or non-existence, of the lacteals and lymphatics in these animals; without mentioning the dispute between us, or any opinion I had formed, that they might be unprejudiced to either party. And that such of these gentlemen as had taken notes sent me excerpts from them, which, as I had suspected, agreed with what I had myself heard the professor say upon that subject.

That Dr. Haygarth, physician at Chester, had sent me the following passage from the notes which he had taken in 1764.

"Fowls, and some fish," says Dr. Monro, "have not lacteal vessels that we can see; they have no conglobate glands in the mesentery; perhaps they (the lacteals) do not run into each other, but into red veins, and hence never are so large as to become visible."

"This note," adds Dr. Haygarth, "was taken in 1764, and if Dr. Monro had changed his sentiments on this subject in the year 1765, I should certainly have taken notice of so remarkable a circumstance."

That, from Mr. Orred, surgeon at Chester, who attended Dr. Monro's lectures during the winter 1765-6, I had learned, that Dr. Monro, when he spoke of the anatomy of a cock, declared: "he never saw, or observed any glandulæ vagæ, or lacteals, but had seen lymphatics in the neck, ending in the jugular."[2]

[1] Dr. Monro has misinterpreted this passage. He supposes I meant that this note was taken two years before 1762, whereas I meant two years before 1768, the time when he claimed these discoveries, and when I wrote my letter to the Secretary of the Royal Society. The note itself is printed above, p. 98.

Dr. Monro has taken notice of another inaccuracy, that is, where I had said, "He asserted that he had anticipated me in these discoveries;" instead of saying, "that he claimed them by asserting he had injected," &c. The former Dr. Monro seems to allow to be what he meant, but not exactly what he said. It was therefore a small inaccuracy; but his claim is now printed verbatim. See above, p. 96.

[2] It may be necessary to mention here, that the dispute between Dr. Monro and me is, who first discovered the lacteals of birds? for as to the lymphatics in their necks, (mentioned in this gentleman's note,) these we both allow were discovered by Mr. John Hunter, about ten years ago.

That, from some notes, said to be copied from those taken by Dr. Taylor of Reading, in the winter 1765-6, I had procured the following excerpt; and Dr. Taylor, on being requested to consult his own copy, had acknowledged it was a just one.

"The lymphatic system" says Dr. Monro, "is said to take place only in men and viviparous animals, and from analogy in those fishes placed by Linnæus, under the class of mammalia:[1] how far is their just extent is not certain, but we have found them in some amphibious animals, as in the turtle.[2]

"It is said that this system is wanting in oviparous animals; but this is not universally true; for we mentioned that we found them in a turtle, and they would probably be found in other orders and genera, if properly examined. But admitting that they are not demonstrable there, it doth not follow that they are wanting; for perhaps they may run only a little way, and terminate in red veins."

That Dr. Maddocks, physician to the London Hospital, had favoured me with the following excerpt from notes which he took at Dr. Monro's lectures, in the winter 1765-6.

"Lymphatics are found in viviparous animals, and therefore, I presume, in the whale, which is of this kind. They are not to be found in oviparous ones, fishes, nor the amphibia: this is the common doctrine. *I will not say how far they may be found in some birds*, but I have found them in some of the amphibious animals, as in the turtle, running along the root of the mesentery."

That Mr. Hull, surgeon at Stevenage, had sent me the following excerpt from his notes, taken about four years ago.

"I never could, to this day" says Dr. Monro, "find a single branch of a lacteal in the abdomen of fowls, nor any lacteals, or glands of the conglobate kind in the mesentery, notwithstanding I have made experiments with that view very often. I kept fowls twenty-four hours without food, then fed them

[1] In the excerpt it is amphibia; but it is evident from the sense, and from comparing it with the other notes, that it should be mammalia.

[2] As to the lacteals of the turtle, there is no doubt but that Professor Monro and myself have both discovered them. He, in the summer 1765; I before that time, viz. in the autumn 1763; when I took a short description of those vessels, which is published with my paper on the lymphatic system in birds, in the 58th volume of the Philosophical Transactions.

with bread soaked in milk, and tinged it by turns with blue, madder, and saffron, and afterwards opened them at several different times, in order to discover the lacteals, but all without success. Yet, perhaps, the lacteals may be in fowls, though not demonstrable." This, adds Mr. Hull, I will answer for being verbatim, or nearly so, as Dr. Monro delivered it in the anatomical hall, at Edinburgh, on the 13th of February, 1765.[1]

These passages, I added, were sufficient to show how little right Dr. Monro had to these discoveries, Besides, I said it was a strong argument against him, that in the letter I had received (which he has printed in his 'State of Facts,' p. 8,) he could not, after relating all his experiments and observations, conclude he had really seen those vessels as he had told the Royal Society; but in one place he says, "that, from the preceding experiments, &c. it is evident that he had seen what he suspected to be the lacteals in birds." And in another, "that he was himself persuaded that birds were provided with lacteal vessels, and confirmed in this opinion, by having now injected them in one of the same oviparous class, the turtle."[2]

Such conclusions appear merely evasive, and never can be considered as proofs of his having discovered those vessels before I did, agreeably to his assertion read before the Royal Society, and since repeated in his 'State of Facts.'

The half-sheet of paper, containing these arguments and testimonies against Dr. Monro, was printed Dec. 1, 1769, and was given to such gentlemen of my acquaintance as had heard of his claim, and a copy of it was sent to the Doctor. Upon receiving which, he published his 'State of Facts;' but what is singular, he has attempted his justification without taking proper notice of these testimonies against him; as if he could be justified whilst they remain unanswered. And in this 'State of Facts', in spite of those testimonies, he repeats to us, "that long before 1762, he observed blueish vessels in the mesentery

[1] If the reader will take the trouble of comparing this note with Dr. Monro's own account of his experiments, (State of Facts, p. 12,) he will be convinced how accurately this gentleman must have taken notes.

[2] This argument was repeated in a note, to prevent Dr. Monro's writing; as if the dispute between us was, who first had suspicions about those vessels, instead of who first discovered them.

(of birds) which he judged to be the lacteals, and had mentioned as such in his lectures."[1] And again, "that about the years 1759-60, he had seen collapsed blueish vessels, which he concluded lacteals, &c."[2] What shall we say of this?

Nay, Dr. Monro has, upon this occasion, even ventured another assertion, viz. "that the notes of his pupils taken for three years before 1762, will be found to prove, that he then taught the direct contrary" of what I have brought these testimonies to prove he has since taught.[3] Now, surely this is very improbable; and Dr. Monro should have adduced some testimonies to prove it. But supposing it were true, it would lead to a conclusion unfavorable to him. It would show, that he must have misled either the one set of gentlemen or the other; for he says, he told the first he had seen the lacteals; the last prove he has since taught them that he never could see those vessels.

The reader, I fancy, by this time thinks with me that Professor Monro's claim deserves no more of our attention. But as he has printed some excerpts from his own book of notes, with the parade of having them authenticated, as if they contained the discovery, notwithstanding the above-mentioned proofs of his having acknowledged repeatedly since he wrote them, that he never could find those vessels, I shall next, therefore, make some remarks upon his notes.

To begin with those relating to the turtle. He discovered its lacteals in the summer 1765. I had seen them before that time, viz. in the autumn 1763. Besides, I have since injected and traced out the whole system;[4] he does not even pretend to have done so: it is, therefore, not difficult to determine who was the first discoverer, and who had carried the discovery farthest.

Next, as to the lacteals in fish. To prove that he had found those vessels eight years ago, he tells us, that in a note taken from the dissection of a skate on April 24, 1760, he has said, "He had discovered a whole system of lacteals and lymphatic vessels running towards the heart, on the left of and above the vena portarum, and from these the auricle of the heart was blown up. They are proportionally larger, but have fewer

[1] State of Facts, p. 4.
[2] Ibid, p. 27.
[3] Ibid. p. 26, in the note.
[4] See Philosophical Transactions, vol. lxix.

valves than in man."[1] Now I will take upon me to say, there is nothing in this note which proves whether he had inflated a lacteal or a vein. For what he says of the situation of the vessels, and of his blowing up the heart, is equivocal: the only part of the note which appears to characterize the lacteals, is in reality a mistake; that is, where he says they have valves. But the lacteals on the mesentery of a skate have no valves, and injections pass readily from the large to the smaller branches. And what is even more to the purpose, although it appears, from his calling what he saw lacteals and lymphatics, that he had at that time some suspicions about them. Yet I am persuaded he has since changed his opinion; and this I think is evident, even from the manner in which he speaks of his experiments made the year after. For, says he, "I have dissected this year (1761, in summer) eight skates, and about a like number of cods and codlings, but without being able to observe by dissection, or to inflate any like to lacteal or lymphatic glands—I find indeed (he adds), that blowing backwards in the meseraic veins, the intestines and the cellular substance between their coats are inflated; but this is no direct proof of branches of red veins absorbing, as these veins may be burst, or the air may have first entered the arteries."[2] Now this surely is not the language of a man who had seen the lacteals, but of one that was seeking for them. Had he found them, he certainly would have mentioned it in this note, but he avoids the subject entirely, and only says he could not find the glands, thereby leaving us to suppose that these dissections were made for the glands only, after having discovered the vessels; which is highly improbable, since, by his own confession,[3] he did not inject the vessels, which he knows well enough is the best way of determining whether the glands exist or not; and one experiment in this way would have been more satisfactory than his eight, or than eight hundred made by dissection only.[4] Add to this, he would not, I think, if he

[1] See State of Facts, p. 12. [2] Ibid. p. 12. [3] See his claim above, p. 96.

[4] If the reader should happen not to be well acquainted with this part of anatomy, he may not see all the force of this argument, which will be satisfactory to anatomists; for it is a fact admitted amongst them, that the mesenteric glands are placed only in the course of the lacteals, so that the lacteals must pass through these glands in their way to the heart. The readiest method therefore of discovering the glands, after having seen the vessels, is by injecting those vessels; for the injection, in its

had now seen the lacteals, have taken up his time with trying whether the red veins did the office of absorption for them, as he seems to have done by blowing into these veins. Nay, I will go farther, and will take upon me to say that it is probable he was in these last dissections convinced he had been mistaken in what he took the year before for lacteals and lymphatics. This I think evident, both from the notes above mentioned, and from his manner of treating the subject since that time. For if he thought he had seen those vessels, he would doubtless have used this discovery as an argument against absorption by the common veins, as he has since used that in the turtle. But it appears, from the notes of his pupils,[1] and even from his own account of those arguments,[2] that he has not done so. And again, had he thought he had discovered those vessels, he would not have acknowledged in his lectures since that time, that they were not yet known to exist.[3] He has therefore aggravated the impropriety of his conduct in claiming these discoveries, by the disingenuity of sending such notes as proofs of his claim.

And lastly, as to the lacteals in birds, he tells us, " that in 1758, he remarked a vessel making an arch on the mesentery of a cock, which at first he believed to be a trunk receiving the lacteals, but not being able to inject it on trial, he conjectured to be rather a nerve." And afterwards, in April 1759, " he observed in a cock what looked like lacteal vessels collapsed, of a blueish colour, which seemed to terminate at the back-bone, &c.—These he showed to the students." And, again, after relating the manner of making his experiments upon no less than twelve cocks,[4] he tells us, " that in 1761 he observed, in the interstices of the great arches of the red mesenteric vessels a pellucid network, some part of which seems to be composed

way to the heart, must distend the glands, and make even the smallest of them visible. The vessels seen by the Doctor seem to have been very large; can it be supposed then, if he had been convinced they were lacteals, that he would not have injected them, and thus have determined whether they were glands or not, by one experiment, instead of tediously dissecting sixteen fish ?

[1] See his pupils' notes above, p. 102, &c.

[2] See State of Facts, p. 16.

[3] See the notes of his pupils above, p. 102.

[4] The experiments were made by feeding these birds with oatmeal and madder, oatmeal and rhubarb, &c. See State of Facts, p. 12.

of branches sent from a large nerve, running parallel with the intestines, and nearer to them than where the trunk of the mesenteric artery sends off its large branches; but although (says he) I suspect strongly there are here too numerous lacteals, and I even observe very small knots, which I take to be analogous to our mesenteric glands, yet I have not observed the above-mentioned kinds of food to make any odds in their appearance,[1] &c." And again, after a variety of other experiments, he says, "he could not observe more than above described." Now what is there in these notes that can entitle him to the discovery of the lacteals in birds? Can his seeing a blue plexus on the mesentery, which at first indeed he suspected to be lymphatic, but afterwards to be nervous, and a part of which, he acknowledges, was in his last experiments found to be made of nerves, entitles him to it? or can his discovery of the small knots, which he takes to be analogous to our glands, entitle him to it? Certainly not. Birds have no lymphatic glands on their mesenteries, as I have shown.[2] Is it not therefore plain, from these notes themselves, that he had not discovered the lacteals in birds? Has he not repeatedly since that time acknowledged this in his lectures?[3] What shall we say then to his asserting that he had seen them eight years ago, and his laying before a respectable Society, and desiring his brother to propagate such an assertion? Or what shall we say to his persisting in it, or above all to his telling us, in his last publication (p. 26) "that in these notes the reader will find the appearance of these vessels after death really described?"[4]

After these notes follow some others, to prove that he had argued in favour of the probability of the existence of those vessels in birds and fish; and a conclusion that he had supposed frogs might have them—and his persuasions that they must have them (not because he had seen them, but) because turtles had them. Which are nothing to the purpose, and ought never to have induced him to claim the discovery, or to

[1] State of Facts, p. 12. [2] Philosoph. Transact. vol. lviii, art. 34.

[3] See the notes of his students above, p. 102.

[4] Let me beg of the reader again to examine these notes, and then judge of the propriety of Dr. Monro's affirming they really contain a description of those vessels, when he has himself put it in the power of the reader to observe the contrary.

say he (actually) had seen them. I cannot therefore think it worth while to take any farther notice of these conclusions.

It is indeed remarkable that Dr. Monro could persuade himself he had any original merit even in entertaining such suspicions and opinions. More than one writer had suspected those animals had them, and that they themselves had seen something like them; for a proof of which the reader need look no farther than Dr. Haller's 'Elem. Phys.'[1] But as those writers had given no proofs of their having discovered them, their suspicions and opinions passed for nothing.

Professor Monro, not satisfied with claiming these discoveries, has even gone farther; he has intimated in some parts of his book that I might have learnt them, or a part of them, from him. As in p. 4, where he speaks of "my giving an account of these vessels entirely as my own discovery;" this in p. 6 he calls "broaching another subject with him;" and complains of me "for passing in silence what I might have heard him observe concerning it when I attended his lectures." How Professor Monro could pretend that I had learnt anything from him on this subject, that ought not for his sake to be passed in silence, is astonishing. What could I learn from one who has repeatedly since that time acknowledged he never saw these vessels; that they might be too short to become visible; and who, at the time I attended his lectures, said he could not find them, as I have already declared. But as my testimony will have more weight with the reader, when corroborated with that of a gentleman unconcerned in the dispute, I shall next add a copy of some notes taken by Dr. Morgan, now professor of medicine in Philadelphia, who attended Dr. Monro's lectures at the same time with myself, and who, at my request, sent me the following excerpt, taken at his lecture upon the question, Whether the common veins absorb or not?

"Most authors (says Dr. Monro) concurring in opinion that fowls were destitute of lymphatics, and not being able to discover them myself, I was led to be of their opinion. I have already observed, that where conglobate glands are found, there are lymphatics, and the converse of this proposition, namely, where there is no conglobate gland, there are no lymphatics. And there being no conglobate glands to be seen

[1] Lib. ii, sect. iii; lib. xxiv, sect. 2.

in the mesentery of fowls, nor in fishes, I judged these animals to be destitute of lymphatics; but Mr. John Hunter having discovered conglobate glands in the neck of a swan, put me on further search, and I then found them plainly in common fowls, but never could find any lacteals in their mesentery, though experiments were tried by means of coloured tinctures of various sorts, as of rhubarb, &c."

From this excerpt it is evident that Professor Monro, when I attended his lectures, taught, as he has since done, that what he knew of the lymphatics he learned from Mr. Hunter, and as to the lacteals he could not find them; and this was in the spring 1762, the very year after the time when, according to his letter read before the Royal Society, he should have seen these vessels, and mentioned them in his lectures: and finally, to complete the whole, he now complains of me for passing in silence what I might then have heard him observe concerning them.

Thus have I endeavoured to obviate the arguments in Dr. Monro's publication, and the reader must now, I think, see clearly, not only the impropriety of the Doctor's asserting his right to these discoveries, but the still greater impropriety of his persisting in that assertion.

Besides claiming these discoveries, Dr. Monro has, in his letters on the subject, treated me in a manner which I cannot pass unnoticed. Thus, he first gives the name of misinterpretation to my concluding from the notes of his pupils, that he had not seen "what he believed" the lacteals, and then adds:

"Should we even suppose the above misinterpretation venial, what must the reader think, when he is told, you was informed that a gentleman, who had attended my lectures two years at least before I injected the lacteals of a turtle, that is, nearly about the time you did, declared he heard me then speak of having seen the lacteals in fowls; and yet that you continued to vent this injurious supposition? That is, you must have sunk this material information, since it overturned the whole purport of your story."[1]

Now here is an accusation, which, were it true, would fall heavy upon me. But the case is this: I had indeed heard that a gentleman, who attended Professor Monro's lectures about the

[1] See his State of Facts, p. 23.

time I did, had declared he then understood the Professor had seen the lymphatics in birds. And Dr. Donald Monro, when I saw him at St. George's Hospital, asked me if I had not heard that this gentleman (mentioning his name, viz. Dr. James Blair, then in London, now in Virginia) had said that the Professor had then mentioned his having seen the lacteals in birds. I answered Dr. Donald, that I had heard something of the matter, but could conclude nothing from it (or to that effect). The reason was this: I knew from the testimony of my own memory, that the Professor had then acknowledged the contrary of his ever having seen the lacteals.[1] I knew the same from the testimony of gentlemen who had attended his lectures since. I therefore concluded that this gentleman had confounded his saying he had seen the lymphatics, with his saying he had seen the lacteals, which I thought might easily happen, as I never knew him take any notes. And upon receiving the Professor's letter, I wrote to Dr. Blair, and in his answer he acknowledged, "that although he had, indeed, for several years, been under a general persuasion that Dr. Monro had seen the lacteals or lymphatics in fowls, yet he had no note on the subject, and a very confused remembrance of what he had heard."

Similar to this accusation is the greater part of a letter which I received from Dr. Monro, in answer to two of mine. This letter is dated Sept. 30, 1769. The Doctor has not printed it, but I beg leave to take a little notice of it.

He begins it by altering the sense of his assertion read before the Royal Society, by the introducing the word *believed*, making it rather a doubtful than a positive assertion. He has done the same in the beginning of his 'State of Facts,' qualifying the alteration, by adding, "to the best of his recollection, and that he had not kept a copy of his letter, not supposing it material to do so."[2] But surely this was not sufficiently qualifying it. If he did not know exactly what he had then asserted, why, before he defended it, did he not ask a copy from his brother, who, most probably, would keep it, in order to show it to everybody; or solicit that favour from the secretaries of the Royal Society where it was read, who, he might be sure, had preserved it, as they do every paper that is laid before

[1] Dr. Morgan's note proves he did so, see above, p. 109.

[2] See State of Facts, p. 5.

the Society. And again, if he was not sure of its contents, how could he now venture in his 'State of Facts,' positively to insist, in opposition to what I had declared, "that his first and last assertions were exactly the same."[1] This at least was inexcusable.

Next, he repeats his vague inferences as in his letter of the 8th of June, "that he had seen what he suspected to be those vessels, &c.," and afterwards, when he comes to speak of the conclusions concerning his claim, which I made in my letter read before the Royal Society, he says, "that he was almost ashamed, on my account, to add a plain corollary, that I must or might have been conscious, that the injurious conclusion with respect to him, which I was labouring to impress on the members of a respectable Society, was drawn from arguments that were weak, inconclusive, not real, but feigned."[2] Afterwards, he tells me, that he is glad, on my account as well as his own, that I am at last really ashamed of my letter." And he then finishes with the following passage: "Another unhappy mistake of yours," says he "is, that you should not have known, or rather perhaps misfortune of yours, since you do not seem to have known so much, that you should not have been told, that your presuming to draw the above conclusion concerning any person who had the smallest pretence to character, without producing proof and absolute certainty of its being true, was what you never could be able to justify to any gentleman."

Now, when it is considered that Dr. Monro obliged me to act in the manner I have done, in order to secure my right, do not these passages appear very extraordinary? But the reader, I believe, will excuse my not dwelling upon them. I shall therefore only add, that the proofs on which my conclusions were founded being now laid before the public, to their judgment I willingly submit them, and that, with respect to Dr. Monro, I have nothing more to say, than that I hope, for his sake as well as my own, to see no more of his claims, his assertions, and his conclusions.

[1] State of Facts, p. 26, in the note.

[2] The conclusions alluded to in these passages are printed above, pp. 97, 98.

EXPERIMENTAL INQUIRIES.

PART II.

A DESCRIPTION

OF

THE LYMPHATIC SYSTEM

IN THE

HUMAN SUBJECT, AND IN OTHER ANIMALS.

ILLUSTRATED WITH PLATES.

TOGETHER WITH OBSERVATIONS ON THE LYMPH, AND THE CHANGES
WHICH IT UNDERGOES IN SOME DISEASES.

BY WILLIAM HEWSON, F.R.S.
AND TEACHER OF ANATOMY.

LONDON:
MDCCLXXIV.

Atque in anatomia corporum organicorum (qualia sunt hominis et animalium) opera sane recte et utiliter insumitur; et videtur res subtilis et scrutinium naturæ bonum.—Lord Bacon.

TO

BENJAMIN FRANKLIN, ESQ., LL.D., F.R.S.

THIS ESSAY

IS RESPECTFULLY INSCRIBED,

BY HIS MUCH OBLIGED,

AND MOST OBEDIENT HUMBLE SERVANT,

WILLIAM HEWSON.

PREFACE.

THE science of anatomy has now been so long and so successfully cultivated, that most parts of the human body have been both carefully described and accurately delineated; but the vessels which are the subjects of this essay, having only of late been made known to anatomists, and not being easily traced by dissection, have not been completely described, nor have they ever been delineated. The following, therefore, is an attempt, in some measure, to supply these deficiencies; and the author flatters himself, that when it is considered how great a share those vessels have in the composition of our body, and how important the offices are which they perform, that this small addition to the stock of Anatomical Science will not be unacceptable, either to the practitioners of the healing art, or to the philosophical inquirers into the works of nature.

THE LYMPHATIC SYSTEM.

CHAPTER I.

A SHORT HISTORY OF THE DISCOVERIES MADE IN THE LYMPHATIC SYSTEM.

Since the days of Asellius, of Rudbeck and of Bartholin, who by their successful inquiries first proved the existence of those vessels in the human body which are now called the Lymphatic System, no part of anatomy has more engaged the attention of its professors; partly from its being the largest field that has lately been opened for their cultivation, and partly from their being so thoroughly persuaded of its great importance.

Asellius, in the year 1622, reaped the first laurels in this field, by his discoveries of those vessels on the mesentery, which, from their carrying a milk-like fluid, he denominated lacteals.[1] This discovery being made by opening a living dog, anatomists were thence encouraged to make experiments on living animals; and Pecquet, on opening a dog in the year 1651, found a white fluid mixed with the blood in the right auricle of the heart. Suspecting this fluid to be chyle, he endeavoured to determine how it got from the lacteals into the heart; this he found was by means of the ductus thoracicus, which he traced from the lacteals to the subclavian vein,[2] and thus he clearly proved the existence of that duct which we now consider as the trunk of the system. Just before his time the lacteals had been supposed to terminate in the liver, conformably to the idea which the physiologists of that period had

[1] Asellius de Lact.

[2] Pecquet Exp. Nova Anat. fig. 2, in Hemsterhuis Messe Aurea.

adopted about the use of this organ, which, from the authority of the older anatomists, they believed was the viscus hæmatopoieticum, or that received the chyle from the intestines to convert it into blood.

Next, Rudbeck, anno 1651,[1] Dr. Jolyffe,[2] and Thomas Bartholin, about the year 1652,[3] discovered the other parts of this system, which, from their carrying a transparent and colourless fluid, are called the lymphatic vessels. And thus there was proved to exist in an animal body a system of small vessels carrying fluids very different from the blood, and opening into the sanguiferous vessels at the left subclavian vein.

To Asellius, Pecquet, Rudbeck, Jolyffe and Bartholin, we are therefore indebted for the discovery of the different parts of this system; not but that some of these vessels had been seen and mentioned by their predecessors, but it was in too cursory a manner to give them any title to the discovery.[4]

After this period Nuck added to our knowledge of this system by his injections of the lymphatic glands,[5] and Ruysch by his description of the valves of the lymphatic vessels,[6] and Dr. Meckel by his accurate account of the whole system, and by tracing those vessels in many parts where they had not before been described.[7]

Besides these authors, Dr. Hunter and Dr. Monro have called the attention of the public to this part of anatomy, in their controversy concerning the discovery of the office of the lymphatics (LXI).

When the lymphatic vessels were first seen and traced into

[1] Ol. Rudb. Exercit. Anat. cap. i, in Hemsterhuis Messe Aurea.

[2] Glisson de Hepate, cap. xxxi.

[3] Barthol. de Lacteis Thoracis, in Hemsterhuis Messe Aurea.

[4] Thus the lacteals had been seen in kids by Erasistratus, who calls them arteries, as we are informed by Galen. See Galen Oper. tom. i, p. 61, edit. apud Junt. The thoracic duct had been seen by Eustachius, who speaks of it as a vein of a particular kind. See Eustachius de Vena sine Pari.

[5] See his Adenographia.

[6] In his Delucidatio Valvularum.

[7] Epistola ad Hallerum.

(LXI.) See the Introduction. The controversy alluded to by Mr. Hewson will be found in the 'Observations, Anatomical and Physiological, wherein Dr. Hunter's claim to some discoveries is examined, by Alexander Monro, junior, M.D.' 8vo, Edinb. 1758; and 'Medical Commentaries, Part I, containing a plain and direct answer to Professor Monro, junior, by William Hunter, M.D.' 4to, Lond. 1762.

the thoracic duct, it was natural for anatomists to suspect, that as the lacteals opened into the intestines to absorb, the lymphatic vessels (which are branches of the same system) might possibly do the same office with respect to other parts of the body; and accordingly Dr. Glisson, who wrote in 1654 (that is, the very year after Bartholin published on the lymphatic vessels), supposes these vessels arose from cavities, and that their use was to absorb; and Frederic Hoffman has very explicitly laid down the doctrine of the lymphatic vessels being a system of absorbents.[1] But anatomists in general have been of a contrary opinion; for, from experiments, particularly such as were made by injections, they have been persuaded that the lymphatic vessels did not arise from cavities, and did not absorb, but merely were continued from small arteries. The doctrine therefore that the lymphatics, like the lacteals, were absorbents, as had been suggested by Glisson and by Hoffman, has been revived by Dr. Hunter and Dr. Monro, who have controverted the experiments of their predecessors in anatomy, and have endeavoured to prove that the lymphatic vessels are not continued from arteries, but are absorbents.

To this doctrine, however, many and strong objections have been started, particularly by the learned M. de Haller,[2] and it has been found, that before the doctrine of the lymphatics being a system of absorbents can be established on a solid foundation, it must first be determined, whether other animals, besides man and quadrupeds, have or have not this system. I have been so fortunate as to prove the affirmative of this question, by discovering the lymphatic system in birds, fish, and amphibious animals,[3] and in consequence of these discoveries I have also arrived at the knowledge of considerable varieties in the composition of those vessels through the various classes of animals; by comparing this knowledge with some facts that I have lately observed concerning the blood, I have thence been led to ascertain the use of the lymphatic glands, the thymus, and the spleen; which have so long been considered as the opprobria of anatomists.

These last observations I propose making the subject of a

[1] See further on, chapter x.

[2] Elem. Phys. lib. xxiv, sect. 2, 3.

[3] Accounts of which have already been published in the Phil. Transactions, vols. lviii, lix.

future publication;[1] but in order to prepare the reader for it, I have thought it necessary to lay before him a description and comparison of the lymphatic vessels in different animals. And that this may be the more worthy his attention, I have not only traced those vessels in most parts of the human body as Dr. Meckel has done, but I have observed some circumstances which had escaped his notice; and I have illustrated the description with plates, the necessity of which has appeared so strongly to some who have preceded me in this subject, particularly to Messrs. Monro, Meckel, and Hunter, that they have each promised to supply the deficiency, but none of them having yet done it, I have undertaken the task myself, that I may be able to refer the reader to the facts from which my conclusions are made.

[1] An abstract of these discoveries, though an imperfect one, has been published in the Medical Commentaries of Edinburgh, No. 1, but without my concurrence (LXII).

(LXII.) See 'Medical and Philosophical Commentaries, by a Society in Edinburgh,' vol. i, p. 99, 8vo, Lond. 1773; and the Letter to Dr. Haygarth, republished in the present edition of Mr. Hewson's works. His views concerning the glands without regular ducts are given at length, by Mr. Falconar, in the Third Part of the Experimental Inquiries.

CHAPTER II.

A GENERAL ACCOUNT OF THE LYMPHATIC SYSTEM.

The lymphatic system consists of the lacteals, the lymphatic vessels, their common trunk the thoracic duct, and the glands called conglobate.

The lacteals begin from the intestinal tube, and can readily be seen in a dog or other quadruped that is killed two or three hours after eating, when they appear filled with a white chyle. The experiment succeeds best when the dog is fed with milk; but they do not always convey a white fluid, for, even in a dog, if opened long after a meal, they are found distended with a liquor that is transparent and colourless like the lymph; and in birds I have never found the chyle white, but always transparent and limpid (see Note LIX); these vessels, therefore, might with as much propriety be called the lymphatics of the intestines.

The lymphatic vessels are small pellucid tubes that have now been discovered in most parts of the human body; the fluid they contain is generally as colourless as water, a circumstance which procured them at first the name of ductus aquosi,[1] and afterwards that of vasa lymphatica.[2] The course of the lymph, like that of the chyle, is from the extreme parts of the body towards the centre, and the lymphatic vessels commonly lie close to the large blood-vessels. If, therefore, a ligature be made round the large blood-vessels of the extremity of a living animal, or of one just dead, that ligature, by embracing the lymphatics, will stop the course of the lymph, which by distending the vessels will make them visible below the ligature.

All the lacteals and most of the lymphatic vessels open into the thoracic duct, which lies upon the spine and runs up towards the neck of the animal, where it commonly opens into the angle between the jugular and subclavian veins of the left

[1] See Rudbeck, l. c. [2] See Bartholin, l. c.

side, and thus both the chyle and lymph are mixed with the blood. If, therefore, the thoracic duct be tied instantly after killing an animal, not only the lacteals, but also the lymphatics in the abdomen and lower extremities become distended with their natural fluids, the course of those fluids being stopped by the ligature.

The lacteals, the lymphatics, and the thoracic duct, all agree in having their coats more thin and more pellucid than those of the blood-vessels. But although their coats are so thin they are very strong, as we daily see on injecting them with mercury, since they resist a column of that fluid, whose weight would make it burst through blood-vessels, the coats of which are many times thicker than those of the lymphatic system.

The thinness of the coats prevents our dividing them from one another, and thereby ascertaining their number, as we do those of the blood-vessels. But as the blood-vessels have a dense internal coat to prevent transudation, we have reason to believe the lymphatics have the same. And as the blood-vessels have a muscular coat, which assists in the circulation,[1] so may

[1] The existence of the muscular coat of the blood-vessels has been rendered probable by Dr. Verschuir's experiments, wherein these vessels were found to be irritable, and also by the following circumstance which I observed in dissecting an ass. The arteries of this, like those of other animals, have a strong elastic coat, which coat after distension contracts them again to a certain degree; but this contraction never goes so far as to shut up the cavity of the artery, and as it acts equally in the dead as in the living body, large arteries are therefore always found with considerable cavities. But in this ass, which I bled to death, the arteries contracted more than their elastic coats were capable of doing; for those of the kidneys were without a cavity, and resembled a cord; and that this contraction was muscular appeared upon distending them again, in which case they stood open as they commonly do in dead bodies. This fact will help us to explain why the arteries appear empty in dead bodies; which may be owing to their muscular fibres having (before death) contracted to the degree seen in this animal, by which means all the blood was driven into the veins; but these muscular fibres ceasing to act after death, the elastic fibres overcome that contraction, and expand the arteries which therefore appear empty (LXIII). Since writing the above, I have dissected a still-born child which was defective in many parts of the body, and in particular in having no heart. In this child the circulation had been carried on merely by an artery and a vein, whose coats therefore probably were muscular.

(LXIII.) Hewson takes no account here of the pressure of the atmosphere. Dr. Davy[a] examined the carotid artery of man in nineteen cases after death, and always found it collapsed, and quite or nearly

[a] Researches, Physiological and Anatomical, ii, 188.

the lymphatics, as is rendered probable from what Dr. Haller says of his having found them irritable in his experiments,[1] and also from what is observed on seeing them distended with their lymph in living animals, in which case they appear of a considerable size; but upon emptying them of their contents they contract so much as not to be easily distinguished. This is an experiment which I have frequently made in the trunk of the lacteals in a goose and on the lymphatic vessel on its neck, both of which, when distended with their natural fluids, are as large as a crow-quill; but on emptying them in the living animal, I have seen them contract so much that it was with the greatest difficulty I could distinguish them from the fibres.[2]

[1] Sur le Mouv. du Sang, ex. 295, 298.

[2] See also Haller, El. Phys. lib. ii, sect. 3, § iii. The celebrated Nuck thought he could separate the coats of the lymphatics, Adenographia, cap. iii (LXIV).

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empty; but expanding and recovering its cylindrical form when opened either in the air or under water.

Mr. Hunter[a] describes the arteries as possessing both a muscular and an elastic power, the former chiefly acting in the tranverse direction of the tube; and the elasticity, "when the muscular action ceases, being exerted to dilate the vessel and restore it to a middle state again, becoming the elongator or antagonist of the middle coat, and by that means fitting it for a new action. . . . . There appears to be no muscular power capable of contracting an artery in its length, the whole of that contraction being produced by its elasticity." Mr. Hunter's experiments on the functions of the arteries are supported by the latest and best observations on their structure. Professor Henle[b] describes the proper contractile or middle coat, the muscular coat of Hunter, the middle or elastic of former writers, as composed of fibres encircling the vessel, and possessing much the same ultimate structure as the organic muscle of the stomach and intestines; while the yellow tissue, or elastic coat of Hunter, so remarkable in the larger arteries, is situated immediately external to the contractile coat, has all the microscopic characters of elastic tissue, and its fibres variously directed. The external or cellular coat consists of longitudinal filaments of common cellular tissue.

It is well known that the blood may be propelled by the action of the vessels alone. The heart is sometimes wanting even in a large human fœtus, of which a remarkable instance was described by the second Dr. Monro.[c]

(LXIV) The larger lymphatic and lacteal vessels[d] are similar in structure to

[a] Works, ed. by Palmer, iii, 164–169.

[b] Anatomie Générale, tr. par Jourdan, t. ii, pp. 28-34. 8vo, Paris, 1843.

[c] Essays, &c. page civ, ed. by his Son. 8vo, Edin. 1840.

[d] Henle, op. cit. tom. ii, p. 89.

The coats of lymphatic vessels have, in common with all other parts of the body, arteries and veins for their nourishment; this is rendered probable by their being susceptible of inflammation, for they are frequently found in the form of a cord, painful to the touch, and extending from an ulcer to the next lymphatic gland, instances of which are mentioned below.[1] These painful swellings of lymphatic vessels likewise show that their coats have sensibility, and therefore that they have nerves as well as arteries and veins.

The lymphatic system in most animals, but particularly in man and quadrupeds, is full of valves. These valves have been painted by the celebrated Nuck, Ruysch, and others, and are much more frequent than in the common veins, and thence these lymphatics have sometimes been distinguished by the name of valvular lymphatic vessels. Those valves are generally two in number, are of a semilunar shape, and the one is sometimes much larger than the other. In most parts of the body these valves are so numerous that there are three or four pair in an inch of space, but sometimes there is no more than one pair. They are less numerous in the thoracic duct than in the branches of the system; thence it might be supposed that, in proportion as we go from the trunk to the branches, we should find them thicker set; but this is not always true, for I have observed them more numerous in the lymphatic vessels of the thigh than in those of the leg. When the vessels are distended with lymph they appear larger where the valves are, which sometimes gives a lymphatic vessel an appearance of

[1] Chapter xiii.

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the veins. Mr. Sheldon[a] relates that he often saw the lymphatic vessels contract powerfully in the necks of living dogs. In horses and dogs killed during digestion, I have several times seen the lacteals well distended; and yet contracting so fast in the course of less than an hour, as to become nearly or completely empty. Müller's[b] discovery of pulsating sacs connected with the lymphatic cavities of frogs, is now well known; and similar lymphatic hearts, as they are called, have since been observed, containing lymph and connected with the lymphatic vessels, in reptiles generally and in birds. See Dr. Jos. J. Allison's paper, in the American Journ. Med. Sc. Aug. 1838; and Mr. Paget's Report in the Brit. and For. Med. Rev. vol. xix, p. 279.

[a] History of the Absorbent System, p. 27, 4to, Lond. 1784.

[b] Physiology, tr. by Dr. Baly, vol. i, p. 274.

being made of a chain vesicles; as such they are represented by some authors:[1] but it is an appearance that very seldom occurs; the reader will not observe it in any of the plates which are here laid before him.

Lastly, the lymphatic system, in different parts of its course, has the glands called conglobate or lymphatic. These glands are so placed that the vessels come in on one side and pass out on the other, in their way to the thoracic duct. Before the discovery of the lymphatic vessels in birds, fish, and turtle, some anatomists have considered these glands as so essentially necessary to the lymphatic system, that they have generally set about discovering the vessels by first looking for those glands; and wherever they found glands they pronounced that there must be vessels; and when no glands could be seen, they thought it as certain a proof of there being no vessels. But as we know the glands are wanting in some animals, I shall not take notice of their structure and use in this chapter; but shall speak of them in a future publication (LXIV*), where I shall treat of the spleen and thymus, with which they are connected in their office.

[1] See Nuck's Adenographia.

(LXIV*.) See the Third Part of these Experimental Inquiries.

CHAPTER III.

A PARTICULAR DESCRIPTION OF THE LYMPHATIC SYSTEM IN THE HUMAN BODY.

It has already been observed that the lymphatic system, besides the glands, is divided into three parts, viz. the lacteals, the lymphatic vessels, and the thoracic duct. That the lacteals belong to the intestinal tube, whilst the lymphatic vessels belong to all the other parts of the body, and that the thoracic duct is the common trunk which receives both the lacteals and the lymphatics. I shall next proceed to give a particular description of each of these vessels, and shall begin with the lymphatics of the lower extremities.

The lymphatic vessels of the lower extremities may be divided into two sets, viz. a superficial and a deep-seated.

The superficial set of lymphatics consists of a considerable number of vessels that lie between the skin and the muscles, and belong to the surface of the body or the skin, and to the cellular membrane which lies immediately under it. Of these there are two large branches that can be readily enough discovered in the limbs of dropsical subjects; one of which runs upon the top of the foot, as is represented Plate I, fig. 1, *a*; another can generally be found just under the inner ankle. I have introduced pipes into both of them, and have thereby filled them the whole length of the lower extremity, as is seen in Plate I.

The lymphatic *a*, which belongs to the toes, runs up on the outside of the tendon of the tibialis anticus I, till it has got above the ankle; and it divides at *b*, and again at *c*, *c*, *c*, forming a plexus, which runs over the shin-bone II, and ascends in the cellular membrane immediately under the skin, between that bone and the internal belly of the gastrocnemius G to the inside of the knee F, where in this figure it disappears, but may be seen in Plate I, fig. 2. This plexus, having passed the inside of the knee, appears upon the thigh immediately under the skin, and over all the muscles, as is seen in fig. 1 *e*, from which it passes to the groin, where these vessels enter the lymphatic glands.

The lymphatic glands of the groin are six, seven, or eight in number; of these some lie in the very angle between the thigh and the abdomen, and others lie a few inches down, on the fore part of the thigh. The lymphatic vessels above described enter the lowermost of these glands, which in the subject of Plate I, fig. 1, are four in number, viz. *f*, *f*, *g*, *g*: one branch, however, avoids these glands, as at *h*, which afterwards bends over at *i* to the gland *k*; from which go vessels to the other lymphatic glands *l*, *l* that lie in the angle between the thigh and the abdomen. It is into these upper glands alone that the lymphatic vessels of the genitals enter, so that the venereal bubo, which arises in consequence of an absorption of matter from these organs, is always seated in those upper glands, and the lower glands *f*, *f*, *g*, *g* are never affected, except by the regurgitation of the matter, or from their vicinity to the glands first diseased, which very seldom happens. And as it is the upper glands that are affected by the absorption of matter from the genitals, so it is the lower which are commonly first affected from the absorption of the acrid matter of an ulcer, diseased joint, carious bone (in the parts below these glands), a circumstance that may assist us in the diagnosis of those two kinds of buboes; remembering, however, that this rule may be liable to an exception, from one of the lymphatic vessels passing the lower glands, and only entering the upper, as is seen at *h* in the same plate.

The lymphatic vessels of the genitals having joined those of the thigh, a network is formed, which enters the abdomen under the edge of the tendon of the external oblique muscle, called Poupart's ligament; one of these vessels is seen in Plate II, *b*, *b*. This plexus on the inside of Poupart's ligament consists of many branches, some of which embrace the iliac artery, of which one is seen in Plate II, at *c*, *c*, but the greatest number of them pass up in the inside of the artery, as is seen at *m, n* Plate I, fig. 1, and at *c,c* Plate II.

These superficial lymphatics, small as they are,[1] probably are the trunks of those vessels which absorb from the skin and the cellular membrane immediately under it; and as no considerable branches can be distinguished on the outside of the leg or thigh, it is probable that all the lymphatic vessels of those parts bend towards the inside, and open into the trunks that are here represented.

Upon these vessels, from the foot to the groin, there is commonly not one lymphatic gland. But this rule has likewise some exceptions, for, even at the lower part of the leg, there is a very small one in the subject from which this plate was taken; it is represented at *d* Plate I, fig. 1; and in another subject I saw a small lymphatic gland near *e*, but these, I believe, seldom occur; however, they lead to this conclusion, that the lymphatic glands even in the human body, are in number and situation a little different in different subjects.

Besides these superficial lymphatic vessels which lie above all the muscles, or in the cellular membrane under the skin, there is a set deeper seated, that lie amongst the muscles and accompany the crural artery: of these the principal trunk can be discovered by cutting down to the posterior tibial artery, near the inner ankle; I have introduced a pipe into it at this part, and have injected it in several subjects, one of which is represented in Plate I, fig. 2.

From the inner ankle at *a*, Plate I, fig. 2, this vessel passes up along with the posterior tibial artery, being hid amongst the muscles on the back part of the tibia. About the middle of the leg it enters a small gland at *c*; and as I have seen this gland in several subjects, I suspect it will be commonly found. Having passed through this gland, the lymphatic runs up to the back part of the ham, still lying close to the artery, and in the ham it passes through three glands, viz. *f, g, h*. I have seen a subject in which I could find only two glands, so that I suspect

[1] With respect to the size of these lymphatics, it is necessary to observe that in this plate all of them appear larger than they ought to be in proportion to the size of the limb.

the number varies. Hitherto this lymphatic has been a single trunk; but after it has passed these glands it commonly divides into two or three branches, which still accompany the crural artery, and pass with it through the perforation in the triceps muscle. This muscle is divided in the preparation from which this figure was taken, in order to give a better view of the lymphatics; and the cut ends of the muscle appear at F, F, though not very distinctly, from their being shrunk by drying. The lymphatic vessels having perforated the triceps, pass up with the artery, (as is seen at *k, l*) and enter a gland *m* which is deeper-seated than those which appear in the groin in Plate I, fig. 1; from this gland they pass into the superficial glands, represented in Plate I, fig. 1 *f, f, g, g,* where the lymph of the deep-seated, and of the superficial lymphatics is mixed, and is conveyed into the body by the vessels seen just above in the same plate. At this part likewise the lymph from the genitals is mixed with that brought by the two sets of lymphatics from the lower extremities, and the whole enters the abdomen by the plexus of vessels represented Plate I, fig. 1, at *m*, and a part of it at Plate II *c*.

The lymphatics of the lower extremities having now reached the trunk of the body, and having passed under Poupart's ligament, appear upon the sides of the ossa pubis near the pelvis at *c, c* Plate II. A part of them passes up along with the iliac artery upon the brim of the pelvis, and another part dips down into the cavity of the pelvis, and joins the internal iliac artery near the sciatic notch. At this place they are joined by the lymphatics from the contents of the pelvis, particularly from the bladder and the vesiculæ seminales in the male, and from the uterus in the female, and there are likewise a few branches which pass through the sciatic notch from the neighbourhood of the glutei muscles. The lymphatic vessels of the uterus, like its blood-vessels, are much enlarged, and therefore easily distinguished, in the pregnant state of that organ. At this part, where so many lymphatic vessels join, there is commonly one or two glands.

Besides those lymphatic vessels which dip down into the cavity of the pelvis on the inside of the external iliac artery at *c, c* Plate II, there are others which keep on the outside of that artery upon the psoas muscle, some of which are seen on the left side in the same Plate at *d*; of these one part passes up to the loins at *h*, and goes under the aorta in different branches, getting from the left side to the right, and joining the thoracic duct. Another part passes under the iliac arteries and appears upon the os sacrum at *f*, making a beautiful network, joining the lymphatics of the right side, and passing under the right iliac artery, to form the network *g* upon the upper part of the right psoas muscle. In different parts of this course from Poupart's ligament to the loins, and also in the loins themselves, there are, in most subjects, many lymphatic glands; none of which were filled in the subject from which this plate was made.

The lymphatic vessels of the right side, joined by some from the left, having now reached the right lumbar region, appear there in the form of a plexus of large vessels, and pass through several glands, which in this subject occupied the spaces (*i*, *i*, *i*), but not being injected with mercury are not represented; at this part likewise they receive large branches, under the aorta, from the plexus on the left side of the loins, as is mentioned before, and having at last got up as high as the second lumbar vertebra, they all join, and form a single trunk called the thoracic duct, which is seen at *m* Plate II. At this part they are likewise joined by the lacteals, which I shall next describe.

The lacteal vessels, so called from their commonly conveying a fluid that is of the colour of milk, begin from the inner surface of the intestines, where they have patulous orifices (see Note LXXXIII) destined to imbibe the nutritious fluid or chyle; from the cavity of the intestines these vessels pass obliquely through their coats, uniting as they go so as to form larger branches. These branches run on the outside of the gut to get to that part which is next the mesentery; and, whilst they are yet upon the gut, they are sometimes of a size sufficient to admit a small pipe, so that I have injected them with mercury even in the human subject.

From the intestines they run along the mesentery and mesocolon, towards the spine, and in their way they pass through the conglobate or mesenteric glands, which in the human subject are very numerous. These glands divide the lacteals into two regions; for from the intestines to the glands these vessels are called lactea primi generis, and from the glands to the thoracic duct, lactea secundi generis.

The lacteals of the small intestines, as they run upon the mesentery, commonly accompany the superior mesenteric artery, and unite, as they go on, into larger branches, so that by the time they have reached the root of the mesentery, they are of a considerable size, as may be seen in Plate II at *k*; from the mesenteric artery they pass down by the sides of the aorta, and open into the thoracic duct, as is seen at *m*. Whilst the lacteals, or rather the lymphatics of the large intestines, accompany the inferior mesenteric artery, and open into the large lymphatic vessels near its root.[1]

Into the thoracic duct at *m*, likewise enters the lymph of the other viscera contained in the abdomen. This lymph is brought by a number of vessels; a plexus of which may be traced from each kidney, lying principally behind the emulgent artery, and opening into the large lymphatic vessels near the aorta: with these likewise go the lymphatics of the glandulæ renales, or renal capsulæ, as they are called.

The lymphatic vessels of the spleen pass from the concave side of that viscus, along with the splenic artery in the sinuosity of the pancreas, by the lymphatic vessels of which the splenic lymphatics probably are joined.

The stomach has two sets of lymphatic vessels, the one running upon its lesser, and the other upon its greater curvature; that which belongs to its lesser curvature accompanies the coronary artery, and passes through some lymphatic glands that lie by its sides. The other set of lymphatic vessels passes from the great curvature of the stomach, through some lymphatic glands that lie close to the arteria gastrica dextra; and descending by the pylorus, meets the plexus that accompanied the coronary artery; and near the lesser curvature of the duodenum, forms a considerable network: into which not only the lymphatics from the spleen enter, but likewise those from the gall-bladder; and those of the liver, which are very numerous, both in its convex and on its concave side.[2] From this network go some branches under the duodenum, and others over it, these branches open into the thoracic duct, near the termina-

[1] The lymphatic vessels arise even from the rectum, as can be seen in quadrupeds that are opened immediately after death; or in fish when a coloured injection is thrown into their lymphatic system.

[2] The lymphatic vessels of the liver are painted by Nuck in his Adenographia, p. 64.

tion of the large trunk of the lacteals, as seen at *m* Plate II. The thoracic duct therefore is the common trunk which receives the lymphatic vessels of the lower extremities, the lacteals, and the lymphatics of all the abdominal viscera.

With respect to the lymphatics of the larger viscera, (such as the liver, the spleen, and the kidneys,) they are generally in two sets: one which lies upon the surface of the organ, and the other which accompanies the large blood-vessels in its centre. In the liver I have found these two sets communicate with each other, so that by injecting mercury into the lymphatic vessels which lie upon its convex surface, I have filled those which accompany the pori bilarii and vena portarum in its centre. The greatest part of the lymphatic vessels which lie upon the convex surface of the liver, run towards its falciform ligament, and pass down by the side of the vena cava. But some of them run towards the right ligament of the liver, where they pass down upon the diaphragm to get to the thoracic duct. The lymphatics on the concave surface run towards the portæ where they join those which come from the centre of the liver along with its large blood-vessels; it is remarkable of those lymphatic vessels which run upon the surface of the liver, that their valves can readily be made to give way, so that I have injected them from their trunks to their branches, and to great minuteness; some preparations of which I have now by me.

It has been suggested by one of the best anatomists of this age,[1] that the lymphatics of the stomach do not open into the thoracic duct like those of the other viscera, but only open into the sanguiferous veins of the stomach; but from repeated dissections of the human subject I am convinced of the contrary; and likewise from the analogy with other animals, particularly fish, whose lymphatic vessels either have no valves, or the valves readily give way, so that I have repeatedly pushed injections from the thoracic duct into the lymphatics of their stomachs, as I have also done into the lymphatics of the other viscera contained in the cavity of their abdomen; as will be more particularly mentioned hereafter.

The thoracic duct, which receives all the vessels that we have yet described, differs in its size in different subjects, but

[1] Dr. Meckel.

is always smaller in its middle than at its beginning, as is seen in Plate II. Sometimes its lower part at *m* is still larger in proportion than is there represented, and that enlargement has been called the receptaculum chyli, and is considerable in some quadrupeds, in turtle, and in fish: but many anatomists have denied that there is any part of the thoracic duct in the human subject that deserves the name of receptaculum, and my experience makes me subscribe to their opinion, as I have never seen anything like a pyriform bag, as it has been described, but merely an enlargement not unlike a varix, and that only in few subjects: for commonly it appears, as in this plate, only a little larger than at its middle. This lower extremity of the thoracic duct is formed by the union of two, three, or four very large trunks of lymphatic vessels: these large vessels unite so as to form the duct about the lower part of the first, or the upper part of the second vertebra lumborum, reckoning downwards.

These large lymphatic trunks which form the thoracic duct are spread out upon the spine, those of the right side lying below the right crus diaphragmatis, and those of the left passing between the aorta and the spine; whilst the thoracic duct itself lies on the right side of the aorta, between that artery and the right crus diaphragmatis, and behind the emulgent artery of the right side, as is seen in Plate II at *n*. From this part it passes upwards, being at first covered by the crus diaphragmatis, and afterwards appears at *o* in the thorax, upon the spine between the aorta and the vena azygos. In the thorax it receives some lymphatics from the intercostal spaces; a few of which are seen at *p* and afterwards it receives vessels from the lungs.

The lymphatics of the lungs are in two sets, one of which passes on the posterior part of each lobe by its root, into the thoracic duct near the middle of the thorax; and another set passes from the fore part of each lobe up towards the jugular and subclavian veins. Some of the lymphatics on the posterior part of the left lobe pass under the aorta to get to the thoracic duct.

At the root of the lungs, where the large blood-vessels enter, are many glands called bronchial, they are generally of a blackish colour in the human subject, and have been suspected to secrete the mucus which is spit up from the trachea; but I have more than once distinctly filled them with mercury by injecting the lymphatic vessels of the lungs, and therefore it is evident that they are not mucous but lymphatic glands.

The lymphatic vessels from the anterior part of the left lobe of the lungs pass into the angle between the jugular and subclavian vein of the same side, joining the thoracic duct at its termination, whilst those from the fore part of the right lobe do not communicate with the thoracic duct, but pass into the angle between the right jugular and the right subclavian vein. These lymphatics from the anterior parts of the lungs are probably accompanied by those of the heart, which are represented by the accurate Nuck in his 'Adenographia,' fig. XLI.

The thoracic duct, after receiving the vessels before mentioned, passes behind the ascending aorta, see Plate III, and goes to the left side, terminating in the angle between the jugular and the subclavian vein. But, just before its termination, it generally goes higher up than the angle, and then bends down towards it, as is seen, Plate III, *b, c*. Sometimes there are two thoracic ducts instead of one, but this rarely occurs in the human subject, but it is not unfrequent for the duct to split near the upper part of the thorax, and the two branches, after spreading out from one another, commonly unite again at their termination in the angle between the left jugular and subclavian veins. I never saw any part of the thoracic duct terminate in the right subclavian of the human body, though such a circumstance has been observed by others.[1] But I have now by me a preparation where the duct splits at the upper part of the thorax into two branches, one of which enters the angle between the jugular and subclavian of the left side, and the other opens into the left subclavian vein, about half an inch on the outside of that angle.

In the description of the lymphatic vessels which lie near the spine, I have mentioned only a few glands, and in Plate II, *i, i, i* where those vessels are exhibited no glands are represented. This I at first considered as an imperfection in the preparation, and had intended to make a drawing from another, but there are two reasons which have induced me to lay the present one before the reader. First, the lymphatic glands are not con-

[1] Dr. Meckel, Epist. ad Hallerum p. 30.

stant, either in number or situation ; and therefore the describing them particularly in any one subject is less necessary, since we cannot be sure of finding them exactly the same in any other. Secondly, the injecting the lymphatic vessels from the groin to the neck, without filling one lymphatic gland, proves a fact which is contradictory to the received opinion concerning those vessels, viz. that they always pass through glands in their way to the blood-vessels, so that if these glands were obstructed, a dropsy must be an inevitable consequence, which is not strictly true when we speak of the lymphatic vessels in the abdomen, where, I find, besides the vessels which go into the glands, there are generally some which escape them. The same is true with respect to the lacteals, so that an obstruction of the mesenteric glands may not always produce a marasmus, as a part of the chyle may pass the glands and get into the thoracic duct.

But although no lymphatic glands are represented in Plate II, it may be necessary to mention where they are commonly seen.

The mesentery of the human subject is well known to contain a considerable number of them ; they are likewise found in the mesocolon where the lymphatics of the large intestines pass through them. The stomach has also several glands which belong to its lymphatic vessels, and lie near the arteria coronaria, and the arteria gastrica dextra. There are likewise a few upon the omentum in some subjects, and there are also many glands by the sides of the pancreas, particularly near the lesser lobe of that viscus, close to the duodenum.

Besides these glands which belong to the intestinal tube, there are many more in the cavity of the abdomen, and a few in the cavity of the pelvis, which belong to the lymphatic vessels of the other organs.

There is commonly a pretty considerable gland seen just on the inside of the edge of the tendon of the external oblique muscle, called Poupart's ligament, on the outside of the iliac artery ; and there are others near that artery, where it lies upon the psoas muscle. There are likewise commonly one or two near the internal iliac artery in the cavity of the pelvis, and there is a considerable number generally met with by the sides, and upon the lumbar vertebræ. In the subject from which I took Plate II, the spaces *i*, *i*, *i*, were occupied by such glands, which were not injected, and therefore are not represented in that plate.

Near the spleen, liver, kidneys, and renal capsules, there are also lymphatic glands which belong to the lymphatic vessels of these organs.

There are likewise lymphatic glands sometimes observed by the sides of the thoracic duct, particularly about the middle of the thorax, which glands belong principally to the vessels of the lungs.

There are also many lymphatic glands (called bronchial) near the root of the lungs; these glands are placed upon the lymphatic vessels, just where they quit the lungs. But no lymphatic glands have yet been observed in the substance of the lungs, and the tubercles (LXV) which some suspect to be obstructed lymphatic glands, seem to have a different origin. There are likewise some glands seen on the lymphatic vessels which lie near the subclavian veins at the upper part of the thorax, and which belong to the fore part of the lungs.

Besides these there are some lymphatic glands upon the aorta near the œsophagus, and there are also others occasionally met with in the intercostal spaces, and there are generally two or three contiguous to the thoracic duct at the lower part of the neck and upper part of the thorax, near the termination of that duct, in the angle between the left jugular and the left subclavian vein.

Having thus traced the lymphatic system in all parts of the body that are below the termination of the thoracic duct, I shall next examine that part of it which lies upon the head, neck, and upper extremities, where the tracing it is attended with greater difficulties.

By the side of each internal jugular vein is a large lymphatic vessel, which is the trunk of those of one side of the head and neck; that of the right side is seen Plate III *l*. There are likewise smaller lymphatics which are seen near the branches of the external carotid artery. There are also lymphatic glands

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(LXV.) In pulmonary consumption, the tubercle may be situated either within the air-cells or in the filamentous tissue between and on the outside of them. See my observations in Professor Wagner's Physiology, translated by Dr. Willis, p. 360, fig. CLXXV; and in the Edin. Med. and Surg. Journ. lx, 161.

by the sides of the parotid and maxillary glands, and by the sides of the large artery where it lies upon the chin, and by the side of the occipital artery; and I have seen one upon the root of the mastoid process of the temporal bone. Those glands, which accompany the lower part of the artery that runs upon the face, are sometimes swelled in consequence of absorption from the lips and the parts adjacent, and also from gum-boils; and those which accompany the occipital artery are frequently enlarged in consequence of the absorption of matter from wounds of the scalp; from which facts it is evident that the external parts of the head are supplied with lymphatic vessels. In quadrupeds I have distinctly seen those vessels, particularly in a dog and in an ass, by passing a ligature round the large blood-vessels of their necks immediately after killing those animals. These experiments I made with a view to determine whether the brain had lymphatic vessels, but I never yet have been able to see any on that organ; neither when I tied up the lymphatics on the necks of those animals, and thereby stopped the course of the lymph; nor when I dissected the human brain, which I have carefully done several times with the view to discover those vessels, and have particularly sought for them in the plexus choroides where they have been suspected to be seen, and near the glandula pituitaria which is supposed by some to be a lymphatic gland, but improperly, since neither that gland nor the glandula pinealis agrees with the lymphatic glands, as I shall show in the Third Part of these Inquiries.

But although lymphatic vessels have not yet been demonstrated in the brain, it is probable, from analogy, that this organ is not destitute of them; and the following case affords an argument in favour of absorption being carried on here by lymphatics, as well as on other parts of the body.

J. H., a young man of twenty-five years of age, by trade a silk dyer, and whose father at that time laboured under a third attack of madness, consulted me about a glandular tumour upon the left side of his neck, of which he gave the following account: that for some time he had been troubled with an eruption which had gone off and returned repeatedly; that a week after its last disappearance, he was seized with a fixed pain in his forehead, for which he was bled; that one day whilst at work, after this pain had continued a fortnight, he felt a weakness in his left arm, and the brush with which he was working fell out of his hand, but he had no weakness in his right arm, nor in his legs. That this weakness returned two or three times a day for nine days; and was rather relieved by putting his hands into warm water. About three days after he was first attacked with this weakness, a tumour appeared on the left side of his neck, just below the ear; when I first saw him this tumour had continued eleven days. It seemed to be an enlarged lymphatic gland, was then hard, but afterwards gradually came to suppuration, and at the end of six weeks it burst and discharged a yellow curdled matter. He adds at the same time that he was attacked with the weakness, he had a faltering in his speech, and slight convulsions in his lips.

Now as in this case there seemed to be a compression of the brain, which was removed when the glands swelled, is it not probable that the cause of the compression had been some extravasated fluid, which afterwards being absorbed, occasioned the tumour and suppuration of the lymphatic gland ? and therefore is it not a presumption that absorption is here likewise carried on by lymphatic vessels ?

The small lymphatics which accompany the branches of the external carotid artery unite upon the neck, and form a large trunk which accompanies the internal jugular vein, passing through some lymphatic glands, near the termination of this trunk in the angle between the jugular and subclavian veins; there are likewise some glands on the outside of this angle, which seem to belong to the lymphatics from the back of the neck, and of the shoulder.

The thyroid gland has many lymphatic vessels, which can sometimes be inflated by blowing air into the cells of the gland: these vessels pass on each side of the trachea, one part going into the angle of the right subclavian and jugular, and the other joining the thoracic duct upon the left side. They are seen in Plate III at *f*.

So much for the lymphatics of the head and neck; I shall next proceed to describe those which belong to the arms.

Each arm, like the leg, has two sets of lymphatic vessels, one which lies immediately under the integuments, and belongs to the skin and cellular membrane connecting it to the muscles; and the other which accompanies the large arteries, and belongs to the parts deeper seated.

The superficial set of lymphatic vessels may be discovered in emaciated dropsical subjects, by a careful dissection on the fore and back part of the arm, where I have fixed pipes into them and have injected them with mercury. In Plate IV, fig. 1, they are seen running on the back part of the fore arm as at *a,a,a*, most of them passing on its outside, and twisting to the fore part, near the head of the radius, as at *b*. But there is one vessel in this preparation which passes towards the inside, under the inner condyle of the os humeri at *c*, and sends a branch amongst the muscles, which branch perforates the interosseous ligament, getting between the radius and ulna to the fore part, where it joins a deep-seated one that had accompanied the radial artery.

In Plate IV, fig. 2, the lymphatic vessels are seen on the fore part of the upper extremity; those superficial branches which passed on the outside of the back to the fore arm appearing now on the fore part at *b* and ascending under the skin that covers the supinator longus and the biceps, they enter some glands in the axilla at *f, f*, whilst that vessel which passes on the inside of the back of the fore arm under the internal condyle, appears on the fore part at *c*, and just above the condyle enters a gland *d*, and then passes up on the inside of the arm, communicating with a lymphatic from the fore part of the wrist, and passing to the axillary glands.

A superficial lymphatic vessel is seen under the skin, on the fore part of this extremity just above the wrist; a pipe was introduced at *a*, and the lymphatic thereby injected with mercury. This vessel passes under the integuments over all the muscles, and joins the lymphatic from the back part of the fore arm at *e*, and there forms a plexus which passes under the integuments, on the inside of the arm to the axillary glands at *f*.

Besides these superficial lymphatic vessels upon the upper extremity, I have traced a deeper-seated one near the radial artery, and have injected it from a pipe fixed at *g*. This lymphatic vessel accompanies the radial artery, and passes, first under the interosseous, and then under the ulnar artery, which in this subject runs over the muscles. Near the part where the lymphatic passes under the interosseous artery, it receives the branch (as formerly mentioned) from the back of the fore arm. After passing under these arteries, this lymphatic appears on the inside of the brachial artery at *i*, where it is deep seated, ascending close to that artery, and near the middle of the arm, passes through the two glands *k k*, after which it appears considerably enlarged, and goes under one of the arteriæ anastomaticæ at *l*, *m*, and then ascends to the lymphatic glands in the axilla.

But these vessels, though filled more successfully in this extremity than in any other that I ever injected, are only a part of the larger lymphatic vessels of the arm, as there are probably some accompanying the ulnar and interosseous arteries, although not here injected: and they should moreover be considered as only trunks of the lymphatics, since it is probable that every (even the smallest) part of this, as well as all other parts of the body, has one of these vessels adapted to absorption (LXV*); that this is the case seems to be proved by the experiments made with the variolous matter, for at what part soever of the arm that matter is inserted, the lymphatic vessels take it up and carry it into the body, as can be traced by its inflaming the conglobate glands through which these vessels pass.

In Plate III the termination of all these lymphatic vessels is exhibited. Two of the trunks of those of the left arm are seen at *d*, *d*, which pass under the clavicle, whose cut end is seen at D, and under the subclavian vein S, where, having joined, they form the large trunk *e*, which appears just above the left subclavian vein, and joins the extremity of the thoracic duct at its entrance into the angle between that vein and the jugular. That these lymphatics commonly join the thoracic duct, as is here described, I am persuaded from having seen it distinctly in three subjects. But that they may, in some instances, open into the subclavian vein before they reach the angle, I think is likewise probable from having observed it in the case above mentioned; in which case, the thoracic duct having split into two trunks, one of these trunks, instead of entering into the angle between the veins, opened into the subclavian itself, about an inch from the angle; but this circumstance I should consider as only an exception from a general rule.

(LXV*.) It is now generally admitted that absorption may take place independently of lymphatic vessels; see Note LXXXII. In those parts into which blood-vessels cannot be traced, lymphatics have not been discovered.

The thoracic duct is not only joined by this trunk of the lymphatics of the left arm, but also by the lymphatic vessels of the left side of the thyroid gland, which appear at *f*, and by the trunk of the lymphatics of the left side of the head and neck, and also by those from the fore part of the lungs of the same side; but neither of these appear in this plate.

The lymphatic vessels of the right side I have repeatedly traced with great care, particularly from their having been suspected to terminate in the subclavian vein, without reaching the angle between it and the jugular; but I have always distinctly seen them go precisely into the angle, not only in the subject from which this plate was made, and which I now have by me, but also in three others. When therefore these lymphatics are seen to enter the subclavian vein at any other part, I should consider it as only an accidental variety, like the double termination of the thoracic duct formerly mentioned.

These lymphatic vessels of the right side form four considerable trunks, which join near their termination. These trunks are—first, one from the upper extremity, which appears at *k*, Plate III, lying above the clavicle between the subclavian artery and vein; this trunk is formed by the lymphatic *g*, *g*, which comes up with the brachial artery, and the plexus *h*, which likewise belongs to the arm, and passes under the subclavian vein. Secondly, the trunk of the lymphatic vessels of the right side of the head and neck which passes down on the outside of the jugular vein, as is seen at *l*. Thirdly, a lymphatic from the thyroid gland, which lymphatic is seen at *m*, passing under the right jugular vein to get to the others. Fourthly, the trunk of the lymphatics from the fore part of the lungs of the right side, which trunk I have distinctly traced under the subclavian vein to its termination, in common with the others, at the angle between the jugular vein and the subclavian.

To finish this description, I shall observe that it is the more necessary to understand the exact termination of these lymphatics of the right side, in order to explain how tumours about this place, by compressing those vessels, occasion œdematous swellings of the parts from which the vessels come, without affecting the other parts of the body.

CHAPTER IV.

A DESCRIPTION OF THE LYMPHATIC SYSTEM IN BIRDS.[1]

This system consists in birds, as it does in the human subject, of three parts, viz. the lacteals, the lymphatic vessels, and their common trunk the thoracic duct.

The lacteals indeed, in the strictest sense, are, in birds, the lymphatics of the intestines (see Note LIX), and like the other lymphatics carry only a transparent lymph. And instead of one thoracic duct there are two which go to the two jugular veins. In these circumstances it would seem, that birds differ from the human subject, so far at least as I may judge from the dissection of a goose, which was the bird I chose as most proper for this inquiry, and from which I took the following description, after previously injecting its lymphatic system with quicksilver.

The lacteals run from the intestines upon the mesenteric vessels. Those of the duodenum pass by the side of the pancreas, and probably receive its lymphatics: afterwards they get upon the cœliac artery, of which the superior mesenteric is a branch. Whilst they are upon this artery they are joined by the lymphatics from the liver; here they form a plexus, which surrounds the cœliac artery; at this part they receive a lymphatic from the gizzard; and a little farther, another from the lower or glandular part of the œsophagus. Having now got to the root of the cœliac artery, they are joined by the lymphatics from the renal glands, or renal capsules; and near the same part, by the lacteals from the other small intestines, which vessels accompany the lower mesenteric artery. These last-mentioned lacteals, before they join those from the duodenum, receive from the rectum a lymphatic which runs with

[1] This description has already been printed in the Philosophical Transactions, vol. lviii, where I have added a plate, which was thought unnecessary in this book.

the blood-vessels of that gut. Into this lymphatic some small branches from the kidneys seem to enter, which coming from those glands upon the mesentery of the rectum, at last open into its lymphatics. At the root of the cœliac artery, the lymphatics of the lower extremities probably join those from the intestines. The former I have not yet traced to their termination, though I have distinctly seen them on the blood-vessels of the thigh; and in one subject, which I injected, some vessels were filled, contrary to the course of the lymph, from the network of lymphatics near the root of the cœliac artery; these vessels ran behind the cava, down upon the aorta, near to the origin of the crural arteries, and I presume they were the trunks of those lymphatic branches which I had seen in the thigh. At the root of the cœliac artery, and upon the contiguous part of the aorta, a network is formed by the lacteals and lymphatics above described. This network consists of three or four transverse branches, which make a communication between those which are lateral. In the subject from which this description was taken there were four. From this network arise the two thoracic ducts; of which one lies on each side of the spine, and runs upon the lungs obliquely up towards the jugular vein, into which it opens, not indeed into the angle between the jugular and subclavian vein, as in the human subject, but into the inside of the jugular vein, nearly opposite to the angle. The thoracic duct of the left side is joined by a large lymphatic, which runs upon the œsophagus, and can be traced as far as the lower or glandular part of that canal; from which part, or from the gizzard, this lymphatic seems to issue. The thoracic ducts are joined by the lymphatics of the neck (and probably by those of the wings) just where they open into the jugular veins.

The lymphatics of the neck generally consist of two pretty large branches, on each side of the neck, accompanying the blood-vessels.[1] Those two branches join near the lower part of the neck; and the trunk is, in general, as small, if not smaller, than either of the branches. This trunk runs close to the jugular vein, gets on its inside, and then opens into a lymphatic gland. From the opposite side of this gland, a lymphatic comes out, which pours the lymph into the jugular

[1] These lymphatics in the necks of fowls were first discovered by Mr. John Hunter.

vein. On the left side, the whole of this lymphatic joins the thoracic duct of the same side, but, on the right, one part of it goes into the inside of the jugular vein a little below the angle which that vein makes with the subclavian.

This system in birds differs most from that in quadrupeds in the chyle being transparent and colourless (LXVI), and in there being no visible lymphatic glands, neither in the course of the lacteals, nor in that of the lymphatics of the abdomen, nor near the thoracic ducts.

For the sake of those who may incline to prosecute this inquiry farther, I shall relate the method by which these vessels may be demonstrated; and that is, having chosen a young and very lean goose, and fixed it upon a table, let the abdomen be opened whilst it is yet alive, and a ligature be passed round its mesenteric vessels, as near the root of the mesentery as possible. The lacteals will begin to appear near the ligature in a few minutes after it is made, especially if the bird has been well fed three or four hours before the experiment. The lymphatics in the neck may be shown in the same manner, that is, by making a ligature on the jugular vein at the lower part of the neck; and to be more certain of including the lymphatics which are near it, we must take care not to pass the needle too close to that vein.

(LXVI.) See Note LIX.

CHAPTER V.

A DESCRIPTION OF THE LYMPHATIC SYSTEM IN ONE OF THE ANIMALS CALLED AMPHIBIOUS, VIZ. THE TURTLE.

THIS system in a turtle, like that in birds, consists of the lacteals, the lymphatics, and their common trunks, the thoracic ducts. It agrees likewise with that in birds, in not having any lymphatic glands either on the mesentery or near the thoracic ducts; but differs from that in birds in not having any glands upon the larger lymphatics of the neck; at least I am inclined to believe so, from not having seen any of these glands in the dissection of one animal of this species in which I looked for them. It likewise differs from that in birds in another circumstance, to be taken notice of hereafter. Whether it agrees with the same system in birds, in the transparency and want of colour in the chyle, I cannot take upon me to determine, as I did not see any of that fluid in this subject.[1]

The following description I took from the animal, after I had injected the larger branches of this system with a coloured wax, and the smaller with quicksilver. To avoid my being misunderstood, when I speak of the situation of the different parts, I shall mention, once for all, that the description was taken from the subject as it lay upon its back; those parts being called highest which were nearest the head, those lowest which were nearest the tail, those posterior which were nearest the back, and those anterior nearest the belly.[2]

The lacteals accompany the blood-vessels upon the mesentery, running by their sides, and communicate frequently across those vessels. Near the root of the mesentery they anastomose, so as to form a network, from which several large

[1] In a crocodile, an animal of the same class, the chyle is white.

[2] The animal from which I took this description was pretty large, measuring from the lower to the upper part of the shell two feet seven inches, and two feet two inches from side to side.

branches go into some considerable lymphatics lying on the left side of the spine. These last can be traced downwards almost to the anus, and belong to the parts situated below the mesentery, and particularly to the kidneys. At the root of the mesentery, on the left side of the spine, the lymphatics of the spleen join the lacteals, and immediately above this union a sort of plexus or network is formed, which lies upon the right aorta (for there are two aortæ in this animal). From this plexus a large branch arises, which passes behind the right aorta to the left side, and gets before the left aorta, where it assists in forming a very large receptaculum which lies upon that artery. From this receptaculum arise the thoracic ducts. From its right side goes one trunk, which is joined by that large branch which came from the plexus to the left side of the right aorta, and then passes over the spine. This trunk is the thoracic duct of the right side; for, having got to the right side of the spine, it runs upwards, on the inside of the right aorta, towards the right subclavian vein. And when it has advanced a little above the lungs, or within three or four inches of the subclavian vein, it divides into branches, which, near the same place, are joined by a large branch that comes up on the outside of the aorta. From this part upwards those vessels divide and subdivide, and are afterwards joined by the lymphatics of the neck, which likewise form branches before they join those from below; so that between the thoracic duct and the lymphatics of the same side of the neck a very intricate network is made. From this network a branch goes into the angle between the jugular vein and the lower part or trunk of the subclavian: this branch, therefore, lies on the inside of the jugular, whilst another gets to the outside of that vein, and seems to open into it a little above the angle between that vein and the subclavian. I say seems to open, for the injection had not succeeded at this part so as to enable me to determine whether the last-mentioned branch did enter or not. Into the above-mentioned receptaculum the lymphatics of the stomach and duodenum likewise enter. Those of the duodenum run by the side of the pancreas, and probably receive its lymphatics, and a part of those of the liver. The lymphatics of the stomach and duodenum have very numerous anastomoses, and form a beautiful network on the artery which they accompany. From this receptaculum likewise (besides the trunk already mentioned which goes to the right side) arise two other trunks pretty equal in size, one of which runs upon the left side and the other upon the right side of the left aorta, till they come within two or three inches of the left subclavian vein, where they join behind the aorta, and form a number of branches, which are afterwards joined by the lymphatics of the left side of the neck, so that here a network or plexus is formed, as upon the right side. From this plexus a branch issues, which opens into the angle between the jugular and the lower part or trunk of the subclavian vein. In these networks, formed by the lymphatics near their terminations in the veins, this system in the turtle likewise differs remarkably from that in birds.

So much for the general description of the lymphatic system in this animal. I shall next add what I have remarked as to the more minute distribution of its lacteals. In the first place, it may be observed that what knowledge we have of the minute distribution of those vessels in quadrupeds has been acquired from examining them when filled with their natural fluid, the chyle; for the valves with which those vessels abound prevent our injecting their smaller branches, as we do those of the arteries and veins of the intestines. But in this animal I have been so fortunate as to force the valves, and to inject the lacteals from their trunks to their branches, so as to fill them all around with quicksilver, in several parts of the intestine. In these experiments I observed that the quicksilver was often stopped by the valves, where the lacteals run upon the mesentery, or where they are just leaving the intestine; but when those valves were forced, and the quicksilver had once got upon the surface of the gut, it generally ran forward without seeming to meet with any obstacle. The lacteals anastomose upon the intestines, so that the quicksilver, which has got upon them by one vessel, in general, returns by another, at some distance. The larger lacteals, which run upon the intestines, accompany the blood-vessels; but the smaller lacteals neither accompany those vessels nor pass in the same direction, but run longitudinally upon the gut, and dip down through the muscular coat into the cellular or nervous, as it has been called, which in this animal is very thin in comparison to what it is in the human subject. So far I have traced those vessels to my satisfaction; but what becomes of them after they have got to the cellular coat is not so easy to determine: in five or six different experiments which I have made, the mercury passed from the lacteals into the cells between the muscular coat and the internal, and spread from cell to cell very uniformly over a great part of the intestine, although but little force had been used, and although there was nothing like extravasation in any other part of the intestine. Upon inverting the intestine, after thus filling its lacteals, the mercury, on being pressed, was in many parts driven into small vessels upon the internal coat, or villous, as it is called. From whence it would seem that this cellular network was a part of the lymphatic system in this animal. It might indeed be supposed to be mere extravasation, but that it is rather a part of the lymphatic system appears from the following considerations: first, from the regularity in the size of the cells; secondly, from the little force used in the experiment, and from there being nothing like this appearance in the cellular membrane between the peritoneal and the muscular coat, where extravasations were as likely to happen; thirdly, from my having been able, after inverting the intestine, to press the quicksilver from the cells into the very small vessels upon the internal coat. But I must confess these facts would not be sufficient to determine whether these cells were or were not a part of the system, did not the analogy of the same part in fish clearly prove it. For in the cod, instead of the cellular network, as in the turtle, there is a network of vessels (of which a description shall be given in the next chapter), so that I have now no doubt but that those cells are parts of the lymphatic system, and that the small absorbent vessels of the internal coat pour their fluid into this network, from which it is conveyed by the larger lacteals.

CHAPTER VI.

THE METHOD OF DISCOVERING THE LYMPHATICS IN TURTLE AND IN FISH, TOGETHER WITH A DESCRIPTION OF THOSE VESSELS IN A HADDOCK.

In the foregoing chapter on the lymphatic system in a turtle, I have made no mention of the manner of discovering those vessels, because there is no difficulty in doing it; for in that animal the mesentery being very thin and transparent, and the lacteals pretty large, they are more readily observed than in any other animal; thence it happened that I saw those vessels in a turtle so long ago as 1763, which was before I discovered them in birds and fish.

But although it was an easy matter to see those vessels in the turtle, yet it was far from being so in birds and fish; as the reader will readily believe, from their having been so often sought for in vain by so many eminent anatomists (LXVI*), particularly of this age. I may add, that the discovery in birds did not give me so much trouble as that in fish, though now, since I have once seen them, I can more readily find them in fish than in birds or quadrupeds. After seeing them in birds, and in one of the amphibia, I was very desirous of determining whether fish were or were not provided with those vessels. This I endeavoured to do in the same way that I had found them in birds, that is, by tying up the mesenteries of live fish; and for this purpose I went frequently to the markets, and examined several small ones. I likewise dissected some larger when dead, but in vain. I next went to Brighthelmstone, where I found kingston, or monk-fish, a species of skate. These being very large, and having a lean mesentery, seemed well fitted to my purpose. I opened two of them alive, tied up their mesenteric vessels, and put them again into the salt water; and

(LXVI*.) Thomas Bartholin, referred to in the Introduction, saw lacteal vessels in a fish.

though one of them lived an hour, I could not observe any lacteals either upon its intestine or its mesentery. After this I repeatedly examined the intestines and mesenteries of common skate and cod, and at last was so fortunate as to discover the lacteals, and get a pipe into one of those vessels on the mesentery of each of these fish; and, injecting by this pipe, I found where the larger vessels lay; after which there was but little difficulty in tracing the whole system. I have now seen those vessels in a variety of fish, and shall give a description of them from a haddock. I shall proceed exactly in the order which I have found most convenient for tracing out the whole system for demonstration, beginning with one of its branches, which, as lying nearest the surface, must, of course, be divided before the other parts can be exposed to view. The account being taken from the fish as it lay on its back, those parts are called superior which are nearest the head; those inferior which are towards the tail; those posterior which are towards the back; and those anterior which are towards the belly.

On the belly of the fish, exactly in the middle line, is a lymphatic, which runs from the anus upwards; this lymphatic belongs not only to the parietes of the belly, but to the fin below the anus. It runs up towards the head, passes between the two pectoral fins, and, having got above them, it receives their lymphatics. It then goes under the symphysis of the two bones which form the thorax, where it opens into a network of very large lymphatics, which lies close to the pericardium, and almost entirely surrounds the heart. This network, besides that part of it behind the heart, has a large lymphatic on each side, which runs upon the bone of the thorax backwards, and when it has got as far as the middle of that bone, it sends off a large branch from its inside to join the thoracic duct. After detaching this branch it is joined by the lymphatics of the thoracic fins and soon after by a lymphatic which runs upon the side of the fish. This last-mentioned vessel consists of a trunk running on the side just opposite to the ribs, and from this trunk proceed branches on each side immediately under the skin; so that it has a beautiful penniform appearance. Besides these branches, there is another set, deeper seated, which accompanies the ribs. After the large lymphatic has been joined by the above-mentioned vessels, it receives the lymphatics from the posterior extremities of the gills, and having now got as far back as the orbit, it next receives lymphatic vessels from that cavity; but these vessels do not belong merely to the orbit; for one of them comes from the nose, and another from the upper part of the mouth. A little below the orbit another network appears, consisting, in part, of the vessels above described and of the thoracic duct. This network is very complex; some of its vessels lie on each side of the muscles belonging to the gills, and from its internal part a vessel goes into the jugular vein, by which vessel the whole system is terminated. The large lymphatic above mentioned, which lies upon the bone of the thorax, has likewise a process running towards the upper part of the kidney, and receives some of the lymphatics of that organ.

The lacteals run on each side of the mesenteric arteries, anastomosing frequently across those vessels. The receptaculum, into which they enter, is very large in proportion to them, and consists, at its lower part, of two branches, of which one lies between the duodenum and stomach, and runs a little way upon the pancreas, receiving the lymphatics of the liver, pancreas, those of the lower part of the stomach, and the lacteals from the greatest part of the small intestines. The other branch of the receptaculum receives the lymphatics from the rectum, and the lacteals from the greatest part of the small intestines. The receptaculum formed by these two branches lies on the right side of the upper part of the stomach (or the lower part of the œsophagus), and is joined by some lymphatics from that part, and also by some small vessels from the sound and from the gall-bladder, which in this fish adheres to the receptaculum. The thoracic duct takes its rise from the receptaculum, and lies on the right side of the œsophagus, receiving lymphatics from that part; and running up a little way (viz. about half an inch in this fish) it divides into two branches or ducts, one of which passes under the œsophagus to the left side, and the other goes straight up on the right side, passes by the upper part of the kidney, from which it receives some small branches, and soon after is joined by a branch from the large lymphatic that lies above the bone of the thorax, as formerly mentioned. It likewise, near this part, sends a branch to join the duct of the opposite side, and then a little higher is joined by those large lymphatics which make a network behind the heart, as is above described. These last mentioned vessels receive the lymphatics from the anterior or superior part of the gills, and from the fauces. The thoracic duct, after being joined by these vessels, communicates with that network near the orbit; where its lymph is mixed with that of the lymphatics from the posterior part of the gills, from the superior fins, belly, &c., and then from this network a vessel goes into the jugular vein, just below the orbit. This last vessel, which I call the termination of the whole system, is very small in proportion to the network from which it rises; and indeed the lymphatics at this part are so large as to exceed by far the size of the sanguiferous vessels.

The thoracic duct of the left side, having passed under the œsophagus from the right, runs on the inside of the vena cava of the left side, receives a branch from its fellow of the opposite side, and joins the large lymphatics which lie on the left of the pericardium, and a part of those which lie behind the heart, and afterwards makes, together with the lymphatics from the gills, upper fins, and side of the fish, a network, from which a vessel passes into the jugular vein of this side. In a word, the lymphatics of the left side agree exactly with those of the right, as above described.

Besides these vessels, there is yet another part of the system which is deeper seated, lying between the roots of the spinal processes of the back-bone; this part consists of a large trunk, that begins from the lower part of the fish near the tail, and as it ascends receives branches from the dorsal fins, and from the adjacent parts of the body. It goes up near to the head, and sends a branch to each thoracic duct, near the part where these ducts come off from their common trunk.

This description, though taken from a haddock, agrees, I believe, pretty exactly with the distribution of those vessels in the cod, whiting, and perhaps all other fish of the same shape.

To this general description I shall add what I have observed of the more striking peculiarities of this system in fish.

In the first place, those vessels are remarkable in not having any lymphatic glands, that I can discover, in any part of their course. In this they agree with the turtle, but differ from birds, which have lymphatic glands on the vessels of their necks.

Secondly, these vessels in fish either have no valves, or the valves readily give way, for it is an easy matter to fill them contrary to the course of the lymph. When I first observed this circumstance, I imagined that by injecting minutely those vessels, I might discover their very beginnings, and that I might also be enabled to determine whether such parts as the brain, eye, &c., of which the lymphatics have not been yet seen in any animal, have or have not such vessels. What success I have had in these experiments will be related in a future publication (LXVII).

Thirdly, the lacteals in the cod (and I presume in most other fish) are remarkable for having a beautiful network of vessels between the muscular and villous coat of the intestines.[1] This network may be filled from the lacteals on the mesentery with the least force imaginable. If mercury be injected into this network at one part, it spreads over the intestine, the communications in the network being very numerous; if the intestine be inverted, and the mercury squeezed, it is easily driven into the small vessels of the villi of the internal coat.[2] From these vessels the mercury can be forced into the cavity of the intestine; but not so easily as to make it clear whether they have or have not a valve at their beginning. In these circumstances there is a strong analogy between fish and the turtle; but in fish it is more evident that there can be no deception as to the network between the muscular and the internal coat; for in them it is made up of cylindrical vessels, and is not cellular as in the turtle, and therefore not in the least like an extravasation; and in fish the vessels on the internal coat are larger than in the turtle.

Fourthly, this system agrees with that of the turtle, in having a very large receptaculum, and in having the network of large vessels near its termination in the sanguiferous system;

[1] I have seen this network in the turbot, plaice, and cod.

[2] If instead of mercury a thin size be used, as an injection it will run with the same facility into the lacteals upon the villi, as it would do into their arteries and veins, when thrown in by a pipe fixed in those vessels.

(LXVII.) Mr. Hewson died May 1, 1774, the same year that this second part of his Experimental Inquiries was published. Dr. James Hendy[a] mentions Mr. Falconar's supposition, that he had injected lymphatic vessels in the brain of the cod-fish.

[a] On Glandular Secretion, p. 7, 8vo, Lond. 1775.

and likewise in having the vessel, which goes from the network into the vein small in proportion to the size of that network; so that the lymph must be lodged some time in those parts before it is poured into the mass of blood. In birds I also observed something like this, their lymphatic system being enlarged, or varicose at different parts; but these enlargements are small in proportion to those above mentioned in fish and in the turtle.

As to the manner of discovering those vessels in a fish, one might naturally suppose, that when it is known where the receptaculum, or any of the larger parts of this system lie, it could not be difficult to find them; but the coats of these vessels are so thin and transparent, that it is by no means easy. The readiest way of finding the whole system is, to look for one of the vessels which lie close to the skin; as, for instance, that which runs up exactly on the middle of the belly of the haddock, cod, and other fish of the same shape. This vessel is easily seen, as it grows pretty large at its upper part, near the head, and if a pipe be introduced, the whole system may be filled by its means.

It is partly owing to the ease with which those vessels may be seen, after discovering where their larger branches lie, that I have not added a figure of this system in a fish. Indeed it would be almost impossible to express all its parts in one figure, from the numerous and intricate communications of those vessels near their termination in the common veins. But I formerly laid before the Royal Society a haddock with its lymphatics and blood-vessels filled with coloured injections, to be compared with the description which was printed in the Philosophical Transactions, vol. 59. And those that are desirous of prosecuting this subject further, will, I flatter myself, find it an easy matter to fill the whole system by attending to what I have said above.

CHAPTER VII.

ON THE PROPERTIES OF THE LYMPH CONTAINED IN THE LYMPHATIC VESSELS, AND OF THAT WHICH LUBRICATES THE DIFFERENT CAVITIES OF THE BODY.

As the fluid contained in the lymphatic vessels resembles water in the circumstances of transparency and want of colour, thence their first discoverers denominated these vessels ductus aquosi, and seem to have concluded that the lymph was nothing but water.

This opinion some of the succeeding physiologists, particularly the learned Boerhaave, rendered more probable, by supposing that there were three series of arteries, the sanguiferous, the seriferous, and the lymphatic, and that those lymphatic vessels we are now describing were only veins corresponding to the lymphatic arteries, to restore their lymph to the heart. Thence the lymph seems to have been concluded the thinnest part of our fluids; in which opinion physiologists were confirmed by Leeuwenhoeck's theory, that the globules of lymph were smaller than those of the serum, or of the red part of the blood.

The fluids that moisten the different cavities of the body, viz. that of the peritoneum, pleura, pericardium, &c., being suspected to be formed solely from the condensation of that steam which appears on opening an animal just killed, have thence been also considered as mere water by some anatomists and physiologists, who were confirmed in this opinion by observing that in dropsies, where a great quantity of fluid is let out from such cavities, it is commonly a mere water, seldom coagulating either when exposed to the air or to heat.[1]

But, notwithstanding the plausibility of all the arguments

[1] Agreeably to this opinion, these dropsies are said to be occasioned by an increased secretion or an impeded absorption, which supposes that the fluids naturally moistening these cavities are the same as those let out from them in dropsical cases.

from which such conclusions were made, with respect to these fluids, it will appear in the sequel, that although they be so transparent in living animals, and so watery in dropsies, yet in animals in health they differ so much from water, that they not only coagulate when exposed to heat, but also when merely exposed to the air; in which circumstance they agree most with that part of the blood called the coagulable lymph, as is evident from the following experiments.[1]

EXPERIMENT I.

If, soon after killing an animal in health, the abdomen, thorax, or pericardium be opened, and if a little of the fluid that moistens these cavities be collected, which (even in cases where the quantity is very small) may be done by gently scraping the surface of these cavities with a wet teaspoon, and if the fluid thus collected be suffered to rest, exposed to the air, it will jelly as the coagulable lymph of the blood does. This is an experiment which I have made on a considerable number of animals, viz. on bullocks, dogs, geese, and rabbits, and the result of all the experiments was the same (LXVIII).

EXPERIMENT II.

If, immediately after killing an animal in health, a lymphatic vessel be tied up properly, and then cut out of the body and opened, so as to let out the lymph into a cup and expose it to the air, it will jelly as the coagulable lymph of the blood does in the same circumstances; this experiment I have likewise made several times on dogs, asses, and geese.

[1] From amongst those who concluded these fluids a mere water should be excepted M. de Haller. (See his El. Phys.) And Professor Monro says they coagulate by cold and rest. (Ob. An. and Ph. p. 68.)

(LXVIII.) The fluid from the pericardium of horses, shot on account of lameness, does not always coagulate spontaneously, as I have ascertained; yet it did in two trials, as mentioned in Note XVIII, p. 31, when mixed with serum from the same animal's blood. I carefully looked into the pericardium of each of the four suffocated human subjects noticed in Note XXXIII, p. 69, and found no coagulated lymph whatever, nothing but a little straw-coloured and transparent fluid, not more than from a quarter to half a drachm in each pericardium. Some colourless fluid which coagulated spontaneously from the belly of a tortoise, is mentioned in Note XXVII, p. 53.

With respect to that fluid which moistens the cellular substance, or cellular membrane, as it is called, I cannot speak with so much precision, since it cannot be collected in animals in health; but when we consider how great a probability there is of the lymphatic vessels absorbing that fluid, we may suspect that it is similar to what moistens the pericardium, thorax, abdomen, &c., especially as I have repeatedly observed, that the lymph returning from the extremities by their lymphatic vessels coagulates when exposed to the air equally as the lymph nearer the centre of the body.

Since, then, those fluids in healthy animals coagulate spontaneously on being exposed to the air, may we not conclude that they resemble the coagulable lymph of the blood at least more than they do the water, or even than they do the serum, which does not jelly on being exposed to the air? And is it not an argument in favour of this inference, that such a fluid appears fitter for the office of lubrication than mere water, and more similar to the synovia, which of all fluids is the best adapted to that purpose? (LXIX.)

(LXIX.) Professor Müller,[a] like Hewson, regards the self-coagulating property as essential to lymph. Dr. Davy has favoured me with the following note on the fluid in the lymphatic vessels of the spermatic cord of the ram: "The lymphatic vessels were large, and distended with a colourless fluid; it did not coagulate spontaneously, but afforded a considerable and rather dense coagulum when treated with nitric acid." In the careful observation of Mr. Lane,[b] fluid from the lymphatic vessels of the horse coagulated in about ten minutes. This corresponds with my own observations on the contents of the lymphatics of the dog and horse; but in some trials the colourless fluid never coagulated spontaneously. It is therefore probable that self-coagulation is not an essential property of lymph. In reference to the statement in the text, that serum does not coagulate on exposure, it will be recollected that certain varieties of serum will coagulate spontaneously when mixed together: see Note XVIII, p. 31. Hewson is not just to his own observations, when he compares coagulable lymph, or the fluid of the lymphatics, to synovia.

For an account of the properties of lymph, see Müller's Physiology, tr. by Dr. Baly, i, 258 et seq.; Professor Wagner's Physiology, tr. by Dr. Willis, vol. i, pp. 250 et seq.; Mr. Lane's article, Lymphatic System, in the Cyclopædia of Anatomy and Physiology; and the Appendix to the English version of Gerber's Anatomy, pp. 95 et seq., Plates xxxii and xxxiii.

[a] Physiology, tr. by Dr. Baly, i, 259. [b] Cyclop. Anat. and Physiol. iii, 219-20.

But although from these experiments I am convinced that the lymph in these cavities and vessels of a healthy animal will always jelly on being exposed to the air (LXX), yet I have likewise observed that the strength of that jelly is different in different animals. In geese these fluids jelly sooner than in dogs, and in the same animals the jelly differs in the different circumstances of health; in most of the dogs which I examined, the contents of the lymphatics formed a strong jelly, but in a dog which I had fed eight days with bread and water, and that rather sparingly, the lymph formed a very weak jelly; and in young geese, these fluids are later in jellying than in such as are full grown. I have observed the same of the fluid contained in the pericardium and abdomen of other animals, which fluid, when in a small quantity, always formed a strong jelly, but when more copious, and the animal more feeble, the jelly was thinner; and in dropsical cases, it is well known that the fluid let out of these cavities is not observed to jelly on being exposed to the air, as it does in animals in health; but in some cases it is found to coagulate by heat, like the serum of the blood, and in others it only becomes a little turbid when boiled, owing to the coagulable matter being in very small proportion to the water.

Although this lymph becomes more watery in a weak state of the animal, it is less watery and more coagulable in some diseases.

But, what is a more curious fact, in those cases where I have compared the fluid contained in the abdomen and pericardium with that contained in their lymphatic vessels, of animals in different states of health, I have always found them agree with one another in the degree of coherence of the jelly which they formed. For, when the animal was in perfect health, the lymph from the cavity of the pericardium, abdomen, and pleura formed a strong jelly, and that in the lymphatics of the neck and extremities was equally firm. When the animal was reduced, as in the dog fed eight days on bread and water, or when the goose was very young, then the jelly, formed by the fluid collected in these cavities, was weak, and that formed by the lymph in the lymphatic vessels was likewise weak in

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(LXX.) See Note LXIX.

the same proportion. So that although these fluids vary in the different circumstances of health, yet they always agree with each other (LXXI).

These fluids, likewise, as we have before observed, besides agreeing with one another, approach to the nature of the coagulable lymph of the blood in the circumstance of coagulating when exposed to the air, but they differ from it in the time necessary for that coagulation. In dogs that were seemingly in perfect health, whose blood and whose lymph were let out of their vessels at the same time, the lymph was found to be much later in coagulating than the blood. The time which the blood requires for its coagulation is about seven minutes after exposition to the air, but the lymph let out from the lymphatic vessels of the same animals was found to require half an hour, or more, for its coagulation. And although the blood coagulates soonest in the weak animals, yet the contents of the lymphatic vessels, or the fluids in these cavities, do not, but seem later in jellying in proportion as the animal is reduced, or as they become more watery.

Moreover, the coagulable lymph of the blood and the lymph of the lymphatic vessels not only differ from one another in the time which they require for their coagulation when exposed to the air, but also they differ more evidently in the time required for their coagulation in the body when merely at rest, without being exposed to air. As, for instance, in a dog killed whilst in health, and whose veins and lymphatic vessels were tied up immediately after his death, the blood in the veins was completely jellied in six hours, but the lymph in the lymphatic vessels of his neck was perfectly fluid twenty hours after his death, and, being let out at this time, jellied after being for some time exposed to the air.

There is another change of the lymph very evident besides those already mentioned, for it not only is varied from the natural state to the more watery, but also from the natural to

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(LXXI.) Professor Müller[a] says, that in frogs kept long without food, the blood frequently loses its property of coagulation, and that in such cases the lymph also does not coagulate; though it usually coagulates quickly, like the blood, when these animals are healthy and active.

[a] Physiology, tr. by Dr. Baly, i, 145.

the more viscid or coagulable; instances of which occur in those inflammatory crusts that are found, in some diseases, to cover the different parts of the body. Thus the outside of the heart and the inside of the pericardium are sometimes covered with a crust as tough as the size in pleuritic blood, and the surface underneath has marks of inflammation, but is not ulcerated (LXXII). Probably, therefore, it is the inflammation which produces that change, or which makes the exhalant arteries secrete a lymph with such an increased disposition to coagulate. Add to this, that the change which inflammation thus seems to produce is just the opposite to that produced by the dropsy, for in the dropsy the fluid is secreted with an extraordinary quantity of water and too little coagulable matter; but in inflammations the fluid is secreted with a greater proportion of coagulable matter, and with less water; and in some instances it seems to be a pure coagulable lymph, either unchanged by the exhalants, and then coagulating gradually on being at rest, as the coagulable lymph is found to do in the veins that are tied;[1] or else the exhalant vessels have the power of changing its properties, so as to make it coagulate in an instant after being secreted. And this supposition of the exhalants having a power of changing the properties of the lymph is rendered probable from the following consideration, viz. that it is sometimes found coagulated in the inner surface of the heart, forming a crust, similar to what we so often see on the outside. Now as there is a constant current of blood through the heart, unless the lymph forming that crust had coagulated instantly on being secreted, it must have been washed off by the blood. One of the clearest instances of this was observed by Sir John Pringle, who has given me

[1] See Exp. Inquiries, part i, p. 22.

(LXXII.) Dr. William Hunter, as early as the year 1757, observed that pus may be formed without any breach in the solids; and John Morgan,[a] of Pennsylvania, soon afterwards described pus as a secretion. The coagulated lymph on inflamed serous membranes, here and at page 164, so accurately characterized by Hewson, Dr. Hunter called "a glutinous concretion, or slough." See the Introduction.

[a] De Puopoiesi, sive Tentamen Medicum Inaugurale de Puris Confectione, p. 5, 8vo, Edin. 1763.

leave to transcribe from his notes the following circumstances of the case:

"Mr. J—, who had for some time been subject to palpitations of the heart, and who (perhaps from another cause) happened to die apoplectic, was carefully opened after death, and two ounces of water were found in the ventricles of the brain.

"The heart was large, and adhered in some places to the pericardium in such a manner as parts adhere from recent inflammation, that is, by an exudation of coagulable lymph. In the pericardium was found a small quantity of bloody serum, of a dark brownish cast. There were marks of inflammation on the surface of the heart, and some part of the coronary artery was ossified. In the right ventricle and in the pulmonary artery was a large concretion of the kind erroneously supposed a polypus, the upper part being white and sizy, whilst the lower had only the appearance of a dark coloured congealed blood. It was obvious that this concretion was formed after death (LXXIII). On the external surface of the left ventricle, near the septum, was a tumour, which on being cut into was found to contain above half a spoonful of a dark, reddish-coloured matter, of the consistence of pus, nor was there any doubt of its being produced by suppuration. This abscess would have broken into the left ventricle, had not the opening through the septum been covered and shut up on the side next that ventricle by a crust or polypus of the shape and about the size of half a large pigeon's egg, divided longwise, so that the flat side lay towards the mouth of the abscess, whilst the convex side was turned towards the ventricle, and occupied a space in it. This," adds Sir John Pringle, "was a true polypus; it cut tough, or like the coagulated blood of aneurisms of old standing, nor was there any doubt, from its appearance, of its having existed there for a considerable time."

Now this crust or polypus, lying over an inflamed surface, had probably been formed by a secretion of the lymph from the inflamed vessels, and being formed in the cavity of the heart where there was a constant current of blood, the lymph

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(LXXIII.) On the relative position of the red and white parts in these clots, see Note XIII, p. 24.

of which it was composed must, I think, have coagulated instantly on being secreted from the vessels, otherwise it would have been washed off with the current; and as the coagulable lymph is not naturally disposed to coagulate so instantaneously, it is probable that the diseased vessels here possessed the power of producing that change; and therefore, that as in dropsical habits, where the vessels act weakly, the fluids exhaled are of a watery tenuity; so in inflammatory cases, where the vessels act strongly, those secreted fluids, in consequence of that strong action, acquire a more viscid and a more coagulable nature (LXXIV).

And moreover, as it appears that the properties of the lymph exhaled upon surfaces and into cavities differ so widely in different circumstances, and as we find that pus is often met with in such cavities without ulcerations (see Note LXXII), is it not probable that pus itself is merely that lymph changed in its properties by passing through inflamed vessels? The cavities of the pleura, pericardium, &c., are sometimes observed to contain considerable quantities of pus without the least mark of ulceration, instances of which I have seen. In one patient I found three pints of pure pus in the pericardium, without any ulcer either on that membrane or on the heart. In another, the cavity of the pleura of the right side was distended with a pus that smelled more like whey than a putrid fluid, and the lungs were compressed into a very small compass; but there was no appearance of ulcer or erosion either on these organs or on the pleura, but only under the pus was a thin crust of coagulable lymph. In such cases it is manifest the pus must have been formed from the fluids; and as the exhalant vessels at one time appear to secrete a mere water, at another a coagulable lymph, and in a third (when a little inflamed) they secrete that lymph so viscid, and change its properties so much as to make it coagulate instantly on being secreted; so in like manner they may sometimes, when more inflamed, have the power of converting the lymph into pus; and, according to the kind

(LXXIV.) As to the consistency of the liquor sanguinis, and state of the red corpuscles in inflammation, see Note XXIII; detailed experiments and historical notices on the subject are given in my paper on Buffy Blood.[a]

[a] Edin. Med. and Surg. Journ. lxiv, 360 et seq.

and degree of the inflammation, the pus may vary from the bland, viscid, and inodorous nature, to that of the most thin and fetid sanies found in phagedenic and cancerous ulcers. And if pus in these cases is produced merely by a secretion (see Note LXXII), so likewise, it would seem probable, that even in abscesses, where there is a loss of substance, it is not the melting down of the solids that gives rise to the pus, but the pus being secreted into the cellular membrane, from its pressure, and from other causes, deadens the solids and then dissolves them; which is confirmed by observing that even a piece of fresh meat, if put into an ulcer and covered up, is soon destroyed or melted down by the pus, which is thereby rendered more fetid (LXXV). And this opinion that pus is made by a secretion is strengthened by observing that in its pure state it is full of globules; in which circumstance it agrees with milk, which is produced by a secretion, and not by a fermentation.[1]

Upon the whole, then, it appears that the lymph contained in the lymphatic vessels, and the fluids which moisten the different cavities of the body, as the pleura, peritoneum, &c., instead of being a mere water in healthy animals, are coagulable fluids (see Notes LXVIII and LXIX), approaching to the nature of the coagulable lymph of the blood, of which probably they are a species, or are composed of a mixture of that lymph with water, that the proportions of that mixture vary from the dropsical habit, where the coagulable lymph is in a small, and the water in a great proportion, up to the rheumatic or inflammatory habit, where the lymph abounds, and the water is in less proportion; and that in some cases the lymph, in passing through inflamed vessels, is even converted into pus.

[1] See Professor Morgan's ingenious Dissertation, De Puopoiesi.

(LXXV.) According to the experiments of Sir E. Home[a] pus has no particular effect in dissolving muscular fibre, which agrees with the result of several trials that I made. The effect on the muscular fibre mentioned in the text, probably arose from putrefaction, favoured by the heat of the ulcer. Pus itself is less prone to putrefaction than blood and most animal juices.

[a] On the Properties of Pus, pp. 32-36, 4to, Lond. 1788.

CHAPTER VIII.

OF THE MANNER IN WHICH THE LYMPH IS SECRETED INTO THE CAVITIES, FOR THEIR LUBRICATION.

Having in the preceding chapter spoken of the properties of the lymph moistening the different cavities of the body, I shall in this consider the manner in which that lymph is formed, or secreted from the mass of blood.

The most generally received opinions concerning this secretion have been, that it was performed either by small exhalant arteries, or else by pores on the sides of the vessels, which pores were believed to be organized.

But these opinions have been controverted by a celebrated anatomist,[1] who has endeavoured to prove that this secretion was not performed by exhalant arteries, or an effect of what is properly called organization, but merely by the thinner or more watery parts of the blood filtrating or transuding through the inorganized interstices between the fibres of our vessels and membranes; so that, according to this idea, the fibres of our vessels were close enough to retain the serum, or the red globules, but not close enough to prevent the water oozing out as through a sieve, and the arguments with which this doctrine is supported are as follow:

First. The ready transudation of water and other injections after death.

Secondly. The transudation of blood after death, but not during life; for during life he supposes the blood to be thickened by the coagulable lymph; but when that lymph is jellied, he concludes the rest of the blood is thereby made thinner, and therefore more capable of oozing through the inorganized interstices by which it could not pass before.

Thirdly. The transudation of bile, which he thinks takes

[1] Dr. Hunter.

place in the living body, because on opening a dead one we see all the neighbourhood of the gall-bladder tinged with this fluid.[1]

Such are the arguments brought in favour of transudation; but, on a careful examination, they are not so satisfactory as those which may be produced in defence of the opinion that these secretions are by organized passages, as I think will appear from the following observations:

First. Although fluids do transude on being injected into the vessels of the dead body, yet we must not thence conclude that a similar effect would certainly take place in the living; for it is probable that "our fibres and vessels have a degree of tension which they may lose with life." Besides, if transudation took place in the living body, it would seem to defeat the principal purpose for which the blood-vessels were made, that is, the containing and conveying the fluids; and upon drinking a greater quantity than ordinary of watery liquors, instead of the liquors being carried to the kidneys or other emunctories, and thereby thrown out of the body as a redundancy, they would escape into the cellular membrane, and occasion an anasarca. That this would be the case will appear the more probable, when it is considered how small the fibres of our blood-vessels must be, and therefore what millions of pores (did they exist) the water would be exposed to from its entrance into the stomach, and its passage through the lacteals, the thoracic duct, the veins, the heart, the lungs, and the arteries, before it reached the kidneys. So that were we in imagination to follow a drop of these liquors, according to the idea of transudation, we should find it first leaking through the stomach or through a lacteal, then being absorbed, then escaping a second time, and being again absorbed, &c., an idea by no means consistent with what we know of the works of nature, who, as a learned and ingenious author says of her, "Operam suam non ludit, neque quod actum est agit denuo."[2] It is more probable, therefore, that as the blood-vessels are made to contain and convey the fluids, nature has taken care to construct them properly to prevent this purpose being defeated.

Secondly. To suppose that the fluids which moisten the

[1] See Dr. Hunter's Medical Commentaries, part i, p. 40.

[2] Dr. Glisson.

different cavities of the body, as the pericardium, pleura, peritoneum, tunica vaginalis, &c., get into these cavities merely by transudation, is to suppose, not only that the small vessels in contact with these membranes have inorganized pores, but also that these membranes themselves have the same just opposite to those of the vessels. Now if we admit inorganized pores at one part of those membranes, we must admit them in all parts, and in the same degree; but as the blood-vessels are circular, and touch those membranes only by a small part of the circle, the parts touched by the vessels must be smaller than the interstices between the vessels, and the lymph must have fewer chances in favour of its leaking from the vessels into the cavities, than of its oozing again from these cavities into the interstices between the vessels or into the cellular membrane; so that, if these membranes admitted of transudation, there would be no such thing as a partial dropsy, for the water would run out at one part of the pleura, pericardium, peritoneum, &c., as fast as it ran in by the other, and an anasarca would always accompany an ascites; which not being a fact leads us to believe, that those membranes do not admit of transudation in living bodies, and that the fluids get into them not by inorganical, but by organized passages.

Thirdly. To prove more satisfactorily that these fluids are not filtrated from the blood merely by inorganical transudation, let us recollect the experiments related in the last chapter concerning the properties of those fluids, which we found varied in different circumstances of health. For, in inflammatory affections of the parts from which they were secreted, they assumed the appearance of the coagulable lymph of the blood, and formed a tough jelly; in animals in health they formed a jelly of a weaker nature; and in dropsical cases they were almost a mere water, without the property of coagulation. Now if these fluids be so variable in their properties, it is manifest that the passages secreting them cannot be always unalterably the same, or inorganized; since at one time we find them secreting one fluid, and at another time secreting another; especially as we sometimes find them secreting a fluid very different from the blood, viz. pus. Which pus being found in cavities without any ulcer or erosion (see Note LXXII), we must conclude it formed by something more than a mere filtration; for we cannot suppose there should be filtrated from the blood a fluid that was not in it. And if pus, which passes from the same pores, can only be accounted for by supposing these pores to be organical, in like manner is it not probable, that the secretion of the natural lymph is not a straining through inorganical, but through organized passages?

Lastly. It has been brought as an argument in favour of transudation in the living body, that blood transudes after death, and this has been explained on the supposition that the blood was thicker before the coagulation of the lymph. Which supposition appears ill-founded, when we speak of the living body; for in former experiments[1] we have observed that this lymph, frequently at least, rather thins than thickens the blood (LXXVI). If therefore the blood transudes in the dead and not in the living body, we should rather attribute it to a change in the vessels than in the blood, as is probable from a careful examination of that very fact which has been brought as the principal argument in favour of transudation, viz. the parts adjacent to the gall-bladder being tinged with bile; for any one who will take the trouble of standing by a butcher, whilst he kills a sheep, will find the contrary to that gentleman's conclusion, that upon opening the animal immediately there is no appearance of the gall having transuded, for none of the parts surrounding the gall-bladder are tinged. But let the animal continue a day or two unopened, and then the gall will be found to have transuded, and to have tinged the neighbouring parts; as is the case in the human body by the time that we inspect it.

Since then the gall-bladder so readily allows of transudation after death, and not during life, is it not probable that there is in our membranes and our blood-vessels a degree of tension, or a power of preventing the fluids oozing out of them, which power is lost with life? (LXXVII.)

[1] See Exp. Inquiries, part i.

(LXXVI.) See Note XXIII.

(LXXVII.) Hewson's view is supported by the experiments of Dr. Davy,[a] showing that the fresh urinary bladder is perfectly retentive of water, though it passes slowly through after a few hours; and still later, when the bladder becomes putrid, the water passes as through coarse filtering paper.

[a] Researches, Physiol. and Anat. vol. ii, pp. 250-51.

Upon the whole then it appears, that the interstitial lymph, or the fluid which moistens the different cavities of the body, being different from mere water, cannot be produced simply by transudation through inorganical interstices; but that there are small exhalant arteries, or organized passages, which not only transmit the lymph from the blood, but change its properties, and adapt it to the office of lubrication, and likewise make it assume very different appearances in different circumstances of health (LXXVIII).

(LXXVIII.) The accuracy of this opinion is supported by the recent observations on the agency of cells in secretion, of which an account will be found, with reference to the researches of Purkinje, Henle, Goodsir, and Bowman, in Dr. Carpenter's Report on the Origin and Functions of Cells, Brit. and For. Med. Rev. xv, 279. Some of Mr. Goodsir's works are referred to in Notes LXXXII and LXXXIII. The fact that the fluid of the ventricles of the brain rarely contains coagulable lymph, and commonly but a very little albumen, is also in favour of Hewson's view. Were this fluid merely an exudation, as the vessels of the brain seem to have very thin coats, might it not be expected that the fluid from them would be rich in albumen and coagulable lymph?

CHAPTER IX.

AN EXAMINATION OF THE OPINION WHETHER THE COMMON VEINS DO THE OFFICE OF ABSORPTION.

As there is a secretion upon the different surfaces, and into the different cavities of the body for the purposes of the constitution, so there is likewise an inhalation or an absorption. For example, if food be taken into the stomach and intestines, it is there digested, and being converted into chyle, it is in that form taken into the blood-vessels. If garlic be applied to the skin it gets into the body, and is smelt in the breath with as much certainty as when taken into the stomach, where its juices are absorbed by the lacteals. So likewise terebinthinate medicines applied to the skin are soon smelt in the urine, and cantharides in a blister affect the urinary passages.

In the same manner fluids are taken from different cavities of the body into the vascular system. Thus the water of an ascites and an anasarca are occasionally taken up and carried by the blood-vessels to the intestines and kidneys, and evacuated by stool or by urine. And the pus of an abscess is sometimes absorbed, and carried to distant parts of the body and there deposited, or is evacuated by the intestines or urinary passages. So also fluids injected into cavities, as that of the chest or the belly of living animals, soon find their way into the blood-vessels (LXXIX). These circumstances are admitted by anatomists amongst the unquestionable facts of physiology.

(LXXIX.) It is curious with what rapidity water is absorbed when injected either into serous sacs or into the subcutaneous filamentous tissue. In February 1839, I opened the tunica vaginalis testis in three dogs, and through it injected one pint of water into the peritoneum of the first dog; two pints into the second; and three pints into the third. The water in each case was well secured in the belly by a ligature on the neck of the tunica vaginalis. The temperature of the air was 56° and of the water 54°. In each trial, all the water had disappeared forty-nine hours after the injection; and the peritoneum had no marks of inflamma-

Nor do anatomists differ in their opinions about the mode in which these fluids are taken up, for it is universally allowed to be by absorption, or that there are small orifices (see Note LXXXIII) adapted to imbibe them; the only question is, what the vessels are to which these orifices belong, whether to the lymphatic system, or to the common veins (see Notes LXV* and LXXXII).

That the common veins did the office of absorbing both the chyle and the lymph was the opinion of anatomists before Asellius discovered the lacteals; but after his time few doubts were entertained of the lacteals absorbing, at least, a part of that fluid. But most anatomists have been so tenacious of the old opinion as still to believe, that the veins partly performed that office, or absorbed some of the chyle and carried it to the liver.

As to the absorption of the lymph, they have been still more positive of its being performed by the common veins; nay, even after the discovery of the lymphatic vessels it occurred but to few that these vessels contributed in the least to this absorption. And no wonder, since, besides the respect for the contrary opinion, because it was transmitted from antiquity, anatomists thought themselves possessed of many strong arguments in favour of the common veins performing absorption; and as these arguments still continue to have weight with some modern physiologists, we shall make a particular examination of them in this chapter.

First. That the common veins arise from cavities, especially in the intestines, and do the office of absorption, is thought probable from injections into these veins in dead bodies having sometimes passed into those cavities, even in cases where but little force was used. This is a circumstance which has occurred in the experiments of the most eminent anatomists, both of the past and of the present age, so that there is no fact in anatomy in favour of which more respectable authorities might be produced. And yet whoever has made numerous experiments with injections, must be convinced how easy it is to be

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tion. In other trials, water seemed to be absorbed still more quickly from beneath the skin. Blood injected, immediately after it was drawn, into the pleura and into the peritoneum, was readily absorbed, but not so rapidly as the water.

deceived by them in this matter. For the veins in dead bodies being easily ruptured, whenever we see injections get from them into cavities, we have reason to doubt whether these injections had passed by natural passages or by laceration of the small vessels; and whoever will examine the authorities that have been quoted in defence of this fact, will find that an equal degree of credit has been given to experiments made with such coarse materials as no experienced injector will now believe could pass through such small orifices,[1] as to those injections which from their subtilty leave the point more doubtful. Besides, as we found in the former chapter, such changes are produced upon animal bodies by death, that membranes, which during life had been so tense as to prevent transudation, after death were so much altered (see Note LXXVII), that in the gall-bladder, for example, they allowed the viscid bile to pass. Does it not therefore become doubtful, when an anatomist injects a cavity from a vein, whether (although he cause no rupture) he may not separate the fibres already relaxed by death, in such a manner as to imitate this transudation? And if one anatomist has been misled when he concluded transudation took place in the living body, because he found it in the dead body, so may they likewise, who have concluded veins arose from cavities in the living because they had been able to push injections into such cavities in the dead body. It must therefore, I think, be allowed that such experiments are at the best equivocal.

Another argument used in favour of veins arising from cavities, particularly from the intestines, is that some anatomists have affirmed that they have seen white chyle in the blood taken from the mesenteric veins. But this argument will appear very inconclusive, when the reader recollects, that the serum of the blood let out from the veins of the arm is sometimes white, which must arise from some other cause than these veins absorbing chyle (LXXX). And, therefore, if that appearance in the brachial veins can be otherwise accounted

[1] For example, Dr. Hales's injection of tallow, resin, and turpentine varnish; which being injected, a part of the vermilion got into the bowels, although no greater force was used than that with which the blood circulates in the living body; but then it is probable that the vessels are weaker in the dead than in the living animal.

(LXXX.) See Note LVIII.

for than by absorption, we are left in doubt whether in those instances, where anatomists observed such a fluid in the veins of the mesentery, it had been owing, not to those veins absorbing it, but to their receiving it from the arteries. All the serum of the body being now and then as white as milk.[1]

A third argument produced in support of absorption by the common veins is taken from the structure of the penis, whose veins arise from its cells; which cells, however, are now allowed to be particular organizations, and very different from those of the cellular membrane, and the blood is believed not to be absorbed, but to be impelled from these cells into those veins; and the argument is now given up even by some of those who were once the most strenuous in its favour.[2] It need not therefore be here dwelt upon.

Ligatures, or compression on the large veins, have been considered as furnishing a fourth argument in favour of these veins arising from cavities, and doing the office of absorption. Thus the swelling of the legs in pregnant women, and in cases where tumours have been seen near the veins, has been explained from the uterus in the one case, and the tumours in the other, occasioning such compression, as to prevent the return of the venous blood. But there are two circumstances which make this argument far from being satisfactory. First, the lymphatic vessels run near such veins, and it is doubtful whether the lymph may not be retained in the limbs more by the compression of these vessels than by that of the veins. Secondly, the compression of a vein may, by stopping the return of the blood, not only distend the small veins, but the small arteries, and the exhalants may be so dilated, or so stimulated, as to secrete more fluid than they did naturally. In this way, perhaps, the ligature which Dr. Lower made on the cava inferior of a dog occasioned the ascites.[3] An experiment which I have repeated, but my subject did not live so long as his, for mine died in half an hour, and had only a very little water in the abdomen (LXXXI).

[1] Instances of which may be seen in the first part of these Exp. Inquiries.

[2] See Professor Monro's State of Facts. [3] De Corde, cap. ii, p. 112.

(LXXXI.) Œdema of the lower limbs is much more frequently connected than is commonly supposed with obstruction of the veins by

Lower has related another experiment which has frequently been quoted by writers on the dropsy; that is, where he tied the jugular veins of a dog, and the dog's head became dropsical. Now were this an experiment which always succeeded it would be more decisive, for when the whole cava was tied, no part of the blood being able to return, all the vessels below, not only the small veins, but the small arteries, must have been extremely distended. Whereas in this experiment no such thing would take place, because the jugular veins so frequently communicate with other vessels, that there would still be a regress allowed to the blood; if the neck therefore became œdematous, it would appear more likely to have been occasioned by the ligature on the veins. But what shows that there must have been some fallacy in Lower's experiment is, that these veins have since been frequently tied without an œdema being produced, or any signs of extravasated lymph. Thus, in not one of the experiments which I made on these veins in living dogs (as related in the first part of these Experimental Inquiries) was this effect ever produced; and Baron Van Swieten tied up both the jugular veins, and though he kept the dog four days afterwards, did not observe him any way incommoded.[1] In one dog I even cut out both the external jugulars, and kept him near a twelvemonth without observing the least symptom of dropsy. I should therefore suppose, that in Lower's experiment, not only the veins, but the lymphatic vessels which lie near them, had been tied; in which case the lymphatics would burst and occasion these symptoms. But in my experiment I took care to separate the vein from the lymphatics.

These arguments, therefore, in favour of absorption being performed by the common veins, which are brought from experiments where ligatures were made on large vessels, seem likewise to be liable to fallacy.

A fifth argument is taken from the structure of the placenta, where it has been concluded there are no lymphatics; and yet there must be absorption, and not a communication of the

[1] Comment in Boer. Aph. § 170, p. 266.

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coagulated blood; see Note XIII, p. 24. I long ago deposited preparations illustrating this fact in the museum of the Army Medical Department at Chatham.

vessels; neither of which arguments are decisive. For there may be lymphatics in the placenta, though not yet discovered; or there may be small vessels passing from the mother to the fœtus, though not yet injected.

A sixth argument is furnished by the experiments of some authors, in which experiments it is affirmed, that fluids injected into the intestines were soon afterwards discovered in their mesenteric veins. The experiment related by the ingenious Kauw Boerhaave, has been the most depended upon in this matter.[1] In which experiment water was injected into the intestines, and those intestines being compressed, the water was afterwards observed to run from the veins; but that some fallacy had crept into this experiment is now probable, from its having been repeated several times in a very satisfactory manner,[2] without being attended with the like success. The learned M. de Haller, indeed, in comparing these arguments, says, that in such cases where authority seems to balance authority, he chooses rather to adopt the opinions of those who affirm than of those who deny the fact. For, as he observes, this experiment may easily fail of success; but if it has ever succeeded, we shall not easily find another way of accounting for it, except by allowing that these veins open into the intestines.[3] But, with due deference to the opinion of this excellent author, Kauw Boerhaave's experiment is not so conclusive as those alluded to above; for in his, the dog was opened immediately after death, and water being injected into his stomach, that water was seen first to dilute the blood, then to wash it from the vena portarum, and the experiment was continued a considerable time by means of pressing the stomach, which pressure furnishes a strong presumption that the water did not get into the veins by absorption but by a laceration, especially as the experiment continued to succeed for some hours after death; whereas absorption always ceases long within the first hour.[4] This argument, therefore, which

[1] See De Perspir. § 469-71.

[2] Dr. Hunter's Medical Comm. chap. v.

[3] Elem. Phys. lib. xxiv, sect. 2, § vi.

[4] K. Boerhaave's words (after mentioning that the blood was washed out by the wound made in the auricle of the heart) are as follow: "Tandem pura aqua tædioso labore per horas, lenissime immittere aquam et premere ventriculum continuavi donec pallerent omnia vasa sanguine orbata per resorptam aquam."—See De Perspir. § 470.

has been considered as so strong in favour of absorption by the common veins, is liable to objections. And lastly, a seventh argument used in favour of common veins absorbing, was, that many animals were destitute of any other vessels which could do that office. This was supposed to be the case with birds, fish, and amphibious animals; all of which some anatomists did not hesitate to affirm must want every part of the lymphatic system, and with great appearance of reason; since in the smallest quadruped they could easily find either lacteals or lymphatic glands upon the mesentery, but in the largest bird or fish neither lacteal vessel nor conglobate gland could be seen. And if these animals (said they) be without the lymphatic system, absorption in them must be performed by other vessels, viz. the common veins; and if in them the common veins can do the office of absorption, why should not they likewise perform it in the human body, where such veins equally exist.[1] But this argument is overthrown by the lymphatic system being now discovered in all these animals.

Such are the arguments produced in favour of the common veins doing the office of absorption, a doctrine which has lately been espoused by that excellent anatomist, Dr. Meckel, to whose observations, though agreeing with some already mentioned, it may be necessary to pay a particular attention.

Dr. Meckel's conclusions in favour of this doctrine are made entirely from injections in dead bodies. For, having filled the common veins by injecting mercury into the lymphatic glands, into the excretory ducts of the breasts, into the vesicula seminalis, into the hepatic ducts, and into the urinary bladder, he concludes that the veins open into these parts in the living body to absorb from them.[2] A conclusion which is already proved to be liable to considerable objections, as we never can be sure whether our injection, in getting from these cavities into such veins, had gone by a natural or by a forced passage. Dr. Meckel does indeed mention that there were no marks of an extravasation in his experiments. Perhaps it might have been too small for observation. Nay, we have even reason to believe that as the small vessels of the

[1] See Prof. Monro's Obs. Anat. Phys. p. 57; Dr. Haller's Elem. Phys. lib. xxiv, sect. 2, § 3, pp. 66-7.

[2] See his Nova Experimenta et Observationes, Berol. 1772.

human body are very close to one another, our injection may sometimes burst from one into another lying in contact with it, without distending the cellular membrane which lies between them. A circumstance which I have seen happen even on the mesentery of a turtle, where upon injecting the lacteals I have more than once made the mercury pass into the common veins; but in all these cases, on a careful examination, I found it was by rupture, as could readily be distinguished in this animal, whose mesentery is extremely thin and transparent. And that it actually was so, and not by a natural passage, must be evident to every anatomist, who considers that this is an experiment which does not always succeed on the mesentery of the turtle, where, if there were natural passages, or if the lacteals opened into the veins, the mercury would probably run with great facility.

And the very same circumstance which Dr. Meckel has observed of a lymphatic gland has happened to me sometimes on injecting these glands in diseased cases; that is, I have filled the common veins, and in some instances, where I looked for it, I could distinguish the extravasation very readily, and therefore concluded that in the other cases, where the veins were filled, that it was also by an extravasation, though a more obscure one. I should thence suspect that in Dr. Meckel's experiment, where he filled the common veins by injecting into the lymphatic vessels of a diseased gland, a similar deception had taken place; especially as the force applied was considerable, he having used a column of mercury eighteen inches high.

And the supposition of the red veins opening into a lymphatic gland appears improbable from an observation concerning the structure of the glands, for which we are indebted to Dr. Meckel himself, namely, that they are made of a convoluted lymphatic vessel.[1] (See Note CXIX*). Now to suppose a lymphatic, which is a vessel given to absorb, should itself, even when convoluted, have a common vein opening into it for absorption from its cavity, is not, I think, consistent with what we know of nature's operations, who, to repeat the words of Glisson, "Operam suam non ludit, neque quod actum est, agit denuo."

Similar objections might be made to the other experiments

[1] Epist. ad Hallerum.

related by this very ingenious author; but enough, I think, has been said to show how cautious we should be in making conclusions with respect to the passages of the living body, from experiments made on the dead, where, from the weakness of the vessels and other circumstances, we are so liable to be deceived.[1]

Thus, on taking a review of the doctrine, that the common veins are the instruments of absorption, that doctrine appears to have no other support than respect for the authority of our predecessors; for all the arguments in its favour are liable to considerable objections (LXXXII). Let us next, therefore, in-

[1] Dr. Hunter and Dr. Monro found in their experiments that injections readily burst from the arteries into the lymphatic vessels, by the intervention of the cellular membrane; these experiments they at first considered (as Dr. Meckel does his) as proofs of their having filled the natural passages; but more careful observations seem to have now convinced the former of these gentlemen that such conclusions are fallacious, and he now thinks that the injection may have burst into the sides of the lymphatic vessel. See his Medical Commentaries, p. 5.

(LXXXII.) It is now generally admitted that imbibition takes place through the coats of veins, lymphatics, and other tissues, and that the veins convey various matters into the system. Indeed, absorption or wasting of a tissue may take place where, in the healthy state, neither blood-vessels nor lymphatics can be found, as mentioned in Note LXV*; the same tissue may also become thickened; among other examples, those of hypertrophy and atrophy of the articular cartilages may be mentioned: see 'Description of the diseased Joints in the Fort Pitt Museum,' sect. ii, iii, 'Edinb. Med. and Surg. Journ.' vol. xlviii, pp. 67-70. Mr. Toynbee[a] concludes that certain tissues are capable of being nourished without the presence within them of blood-vessels. And Mr. John Goodsir[b] has pointed out some organic processes in which absorption and ulceration appear to be functions independent of the vessels, which he considers as mere passive ducts in carrying away the absorbed products. He concludes that, "the primitive cell is primarily an organ of specific absorption, and secondarily of nutrition, growth, and secretion." The old mechanical notion, entertained in Hewson's time, of regular pores or orifices in the vessels for absorption and secretion is now exploded. With a good microscope, it is most easy to see fresh cells, as those of pus or of the semen, swell from imbibing water added to them. From the observations of Tiedemann and Fohmann, it was supposed that the chyle could be absorbed from the mesenteric glands of the seal by the veins only; but Dr. Knox[c]

[a] Phil. Trans. 1841, p. 188.

[b] Anat. and Pathol. Obs. pp. 8, 14, 19, 34, 8vo, Edin. 1845.

[c] Edinb. Med. and Surg. Journal, xxii, 23.

quire whether some other part of the human body may not do that important office.

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clearly proved that in this animal the chyle takes the usual course through the lacteals and receptacle to the thoracic duct. MM. Flandrin and Danger[d] confirm the fact of the absorption of poison from the intestines by branches of the portal vein; and Dr. Handyside[e] made some experiments, showing that the veins generally absorb various foreign matters. A reference to his opinions, and to those of Dr. Carson,[f] will show that there is still a doubt whether the colourless lymphatic vessels absorb merely the effete or the nutritious elements of the body. The lymphatics are not merely absorbents, for they alter the properties of the fluid which they convey. Some interesting observations on the functions of these vessels are given in the second Dr. Monro's 46th Lecture.[g]

[d] Mr. Paget's Report, Br. and For. Med. Rev. xix, 280.

[e] Notices of Communications to British Association, August, 1835, p. 92.

[f] Notices of Communications to British Association, August, 1836, p. 119.

[g] Essays, &c. pp. 3 et seq., ed. by his Son and Successor. 8vo, Edin. 1840.

CHAPTER X.

ON THE USE OF THE LYMPHATIC SYSTEM.

This system in all animals, we have found, consists of a trunk or thoracic duct, and of two extremities, namely, the lacteals and the lymphatic vessels. The lacteals can be traced from the thoracic duct to the intestines, through the coats of which they pass, and open into their cavities by patulous orifices (LXXXIII), in order to absorb the chyle and to transmit it through the thoracic duct to the blood-vessels. That this is their use has never been questioned since the first discovery of those vessels, from its always admitting of easy demonstration, that is, by giving an animal milk, and then opening him a few hours after, in which case the same fluid that is seen in his intestines can likewise be seen to have got into his lacteals—a satisfactory proof of the lacteals beginning from the intestines.

(LXXXIII.) Before Hewson's time, it was a popular opinion[a] that the lacteals begin by open mouths in the villi of the intestines, and this view continued to be generally entertained until lately. Mr. Sheldon,[b] after stating that these mouths were discovered by Liberkühn, declared that "the ampullulæ, with their orifices, are to be considered as the beginnings of the lacteal vessels." Mr. Cruikshank[c] figured what he conceived to be these orifices. But Professor Müller[d] found that mercury injected into the lacteals does not escape from the surface of the mucous membrane of the gut, and therefore he concluded, with Rudolphi, that the villi are not perforated at their extremity. In short, it is now well known that the radical extremities of the lacteals form loops, or closed passages in the villi. See Mr. John Goodsir's paper on the Structure of the Intestinal Villi, 'Edin. New Phil. Journal,' xxxiii, 165; Henle, 'Anat. Gen.' tr. par Jourdan, t. i, 455; Mr. Paget's Report, 'Brit. and For. Med. Rev.' xiv, 290; and Dr. Carpenter's 'Human Physiology,' pp. 372-3.

[a] Arbuthnot on Aliments, pp. 17, 20, 8vo, Lond. 1731.

[b] History of the Absorbent System, pp. 32-8, 4to, Lond. 1784.

[c] Anatomy of the Absorbing Vessels of the Human Body, plate 2, fig. 3, 4to, Lond. 1786.

[d] Physiology, tr. by Dr. Baly, i, 266, 8vo, Lond. 1838, 1st edition.

After thus being convinced that the use of one branch of the system is to absorb, we cannot at first sight but wonder that any anatomist should have hesitated to attribute a similar office to the other. Nevertheless, some anatomists have been led to ascribe to the lymphatics a very different use to what they found the lacteals perform; particularly since the time that Nuck first made his experiments, in which he thought he injected these lymphatic vessels from the arteries, and therefore concluded that they had no other use than as correspondent veins to return the lymph from such arteries as were too small to admit the red blood or the serum. And in this opinion anatomists were confirmed by the theories of Leeuwenhoek and of Boerhaave, concerning the gradation in the series of the globules of our fluids, and of the sizes of the vessels destined to convey them; thence the idea of the lymphatic vessels being small veins continued from arteries became so general amongst physiologists.

But although this idea was so commonly received, yet there were some physiologists who reasoned better on the subject; and amongst the first of these was Glisson, who, in a book published the very year after that in which Bartholin wrote upon the lymphatics, attributes to those vessels the office of carrying back to the blood-vessels the lymph which had lubricated the cavities of the body.[1]

M. Noguez (LXXXIII*) likewise, in a chapter where he mentions the name of Dr. Glisson, speaks of the use of the lymphatics as follows: "Ils reportent la lymphe dans les vaisseaux

[1] De Hepate, cap. xlv, edit. Lond. 1654.

(LXXXIII*.) Dr. Simmons[a] stated that the main points at issue in the controversy between Dr. Hunter and Dr. Monro, alluded to by Mr. Hewson at page 120, were known long before to M. Noguez; whereupon Mr. John Hunter[b] insisted on his brother's claim to the discovery of the office of the lymphatic vessels, observing that Noguez entertained on the subject the erroneous doctrines of his day, and that he did not in the least comprehend the absorbent system as Dr. Hunter understood it. But, as mentioned in Notes LXV* and LXXXII, it is now well known that absorption may take place in parts of the animal body which have no lymphatic vessels.

[a] Life of Dr. William Hunter, p. 30, 8vo, London, 1783.

[b] Extract from his MSS. in the Catalogue of the Museum of the Royal College of Surgeons, vol. ii, pp. 10-13, 4to, London, 1834.

sanguins ou dans les veines, il y en a dans toutes les parties du corps, ils repompent la matière lymphatique qui s'evacue par les artères, on peut les nommer conduits absorbans;" and again, in another place, he says, "ils reçoivent la lymphe subtile qui se repand sur la surface de toutes les parties, et dans les différentes cavités du corps, ils la reportent au sang."[1]

Hambergerus also seems to have had this idea of their office, for he says, "ex omnis generis cavo, humidum liquidum vehente, sive sit arteria, sive vas secernens, vel excretorium, vel aliis usibus destinatum, vasa lymphatica oriuntur."[2]

Frederic Hoffman has been still more explicit on this subject, and has expressed the doctrine of the lymphatics being absorbents very completely, in his Medic. Ration. System. lib. 1, sect. 2, cap. 3, where he says—

"§ 2. Duplex est origo vasorum lymphaticorum, quaedam ex ipsis arteriis prodeunt, alia ex porosa et cellulosa partium substantia nascuntur.

"§ 4. Lymphatica, quæ ex partium substantia oriuntur, aquosi succi nutritii partem resorbent, ac revehunt ad cor.

"§ 7. Revehunt vero omnia lymphatica ex universo corpore lympham suam ad capsulam lumbarem et chyliferum ductum, in quam se exonerant.

"§ 11. Ad facilitandum lymphæ regressum vasa hæc valvulis instructa sunt, et quidem sigmoideis, numerosioribus et angustioribus, quæ quidem lympham libere transmittunt, impediant tamen quo minus regurgitet."

This opinion of the lymphatics being a system of absorbents has been adopted and supported with additional arguments, first by Dr. Hunter (LXXXIV), and afterwards by Dr. Monro,

[1] Anatomie de l'Homme, 2d edit. cap. viii. [2] Physiol. Med. § 469.

(LXXXIV.) The following passage is interesting, as showing the state even of Dr. Hunter's knowledge concerning the office of the absorbents and some of the properties of the blood, in the year 1757. Speaking of the erosion of the sternum and vertebræ in a case of aneurism of the aorta, he says: "The appearance was rather as if the blood had insensibly dissolved away the substance of the bone, making the greatest havoc in the softest parts of the bone, as we see in stones of unequal texture that have been long washed by a dropping, or stream of water. Has the blood that property which some have ascribed to it of dissolving bony matter? A surgeon of my acquaintance, whose who, besides showing the fallacy of the experiments brought in favour of the common veins doing the office of absorption, have advanced the following to prove that the lymphatics perform it.

First. Their great analogy with the lacteals, with which they agree in their coats, in their valves, in their manner of ramifying, in their passage through the lymphatic or conglobate glands, and in their termination in the thoracic duct, and, in short, in every circumstance with regard to their structure; and thence it is probable that they also agree with them in their use. And as the lacteals are known to begin from the surface of the intestines, and to be the absorbents of these parts, the lymphatics may begin from the other cavities of the body, and may absorb the fluids which had lubricated those cavities.

Secondly. The passage of the venereal, variolous, and other poisons into the constitution; these poisons first making an ulcer, and then being absorbed along with the matter of the ulcer, and infecting the whole body. That in such cases they are not absorbed by the common veins, but by the lymphatics, appears from their inflaming these lymphatics in their course, and by their generally inflaming a conglobate gland before they enter the system; a strong argument in favour of their being taken up by the lymphatic vessels, which pass through these glands in their way to the thoracic duct.[1]

These two are the principal arguments by which the doctrine of the lymphatics being a system of absorbents has been supported. Experiments made by injections in the dead body, where such injections have been forced from the arteries into the cellular membrane, and from the cellular membrane into the lymphatics, have been likewise brought in favour of this

[1] See Dr. Hunter's Medical Commentaries. See also Dr. Monro, De Vasis Lymph. Valv.

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experience, abilities, and veracity are unquestionable, told me, upon my asking this question, that he had once opened a little tumour upon the temple, that contained only pure blood, and that there was no bone under it. He supposed that the blood had dissolved that part of the skull upon which it lay."[a]

[a] Med. Obs. and Inq. by a Society of Phys. in London, i, 344-5, 8vo, 1757.

doctrine, but improperly, and being now given up by those who advanced them,[1] they need not be dwelt upon here.

But our experiments, related above, furnish another argument in favour of the lymphatics being a system of absorbents; for, in chapter the seventh, we have mentioned that in these experiments we have always found the fluids contained in the different cavities of the body and that contained in the lymphatics exactly agreeing with one another, in their transparency, in their consistence, &c. And in animals in health we likewise found, when the one jellied on being exposed to the air, the other did so too; and in the animal reduced by low diet, where the properties of the one were altered, those of the other were so likewise, and exactly in the same manner (LXXXV). So that we now seem to have obtained as decisive an argument in favour of absorption by lymphatics as we before had of that by the lacteals; for the lacteals were concluded absorbents from their being found to run from the intestines filled with a fluid similar to what was in the cavity of the gut; so we seem here to have the same reason for believing that the lymphatics absorb from cavities, because they are found to contain a fluid exactly similar to what is observed in these cavities; a strong argument that the fluid had passed from such cavities into these lymphatics by absorption.

Such then seems to be the purpose for which the lymphatic vessels were provided, that is, to do the office of absorption, an office of the greatest importance to the animal: no wonder, therefore, that there should be a system set apart for performing it, and not only in man and quadrupeds, but also in birds, fish, amphibious animals, and perhaps even in insects of the most perfect kind.

[1] See Dr. Hunter's Medical Commentaries, p. 57.

(LXXXV.) See Notes LXVIII, LXIX and LXXI.

CHAPTER XI.

AN EXAMINATION OF THE OPINION WHETHER SOME OF THE LYMPHATIC VESSELS MAY NOT BE CONTINUATIONS OF THE SMALL ARTERIES.

I HAVE already observed that, soon after the discovery of the lymphatic vessels, Glisson and others suspected that they arose from the cavities of the body to take up the fluids exhaled from the blood-vessels; but, at the same time, another opinion was entertained by some anatomists, namely, that a part of the lymphatics were reflected from the small arteries to which they corresponded, in the same manner as the common veins belong to the arteries carrying red blood.

In this opinion, that the lymphatics were only veins, anatomists were confirmed from experiments made by injections, particularly the blowing air into the arteries of the kidney, spleen, &c., and seeing it return by the lymphatics;[1] a fact that has since been proved to be owing to the air having burst from the arteries into the cellular membrane, and so having got into these vessels, and therefore by no means proving a direct communication between those arteries and the lymphatics.[2]

Other injections, likewise, such as mercury, water, &c., having been thrown into arteries, and afterwards having got into the lymphatics, have been mentioned as so many proofs of a direct communication; but greater experience with injections has convinced some of the more accurate amongst later anatomists of there likewise being a fallacy in these experiments; or of the fluids having got from the arteries into the lymphatics, not by passages which were natural to the living body, but by such as were the effects of laceration in the dead one.

[1] Nuck, Adenog. cap. iv, vi.

[2] See Professor Monro, De Venis Lymph. Valvul.

The present Professor Monro has distinguished himself in this subject; from his observations[1] and those of Dr. Hunter,[2] the notion of the lymphatics being continued from arteries seems to be very fairly exploded. And it is made probable that the injections in dead bodies had misled their predecessors, who had not been sufficiently aware that these injections might possibly have passed, not by natural, but by forced passages.

CHAPTER XII.

ON THE STRUCTURE OF THE VILLI OF THE INTESTINES, AND THE MANNER IN WHICH ABSORPTION IS PERFORMED.

The term villi, applied to the very small processes of the internal coat of the intestines, conveys an improper idea of their figure in the human body. In many quadrupeds indeed they are cylindrical, or like hairs or wool;[3] but in the human subject they are broad and flat; and when viewed with a microscope

[1] De Ven. Lymph. Valv. [2] Medical Commentaries.

[3] I have seen them of that shape in the dog, cat, lion, and the ass (LXXXV*).

(LXXXV*.) Of the intestinal villi, Professor Müller[a] observes, "It is true that they are flattened in most mammalia, as in the rabbit, dog, and hog; but in the calf, ox, and sheep, many of the villi are cylindrical; and sometimes, as in the sheep and ox, the flattened villi are more numerous in one part of the intestines, in another part the cylindrical; and in the two last-named animals, particularly in the sheep, the villi in many parts of the intestines are flattened and broad, with cylindrical tips. By the villi becoming broader at their base, and being connected with each other so as to form folds, a gradual transition is established from the villi of mammalia to the rugæ or folds by which they are replaced in many birds and in reptiles. This transition is sometimes perceptible in the intestines of one and the same animal." He adds, that the villi, especially those of the cylindrical form, contain a simple cavity, which he has observed in the calf and ox; and, with the assistance of a microscopic examination by Henle, in the human subject. Professor Wagner[b] made the interesting observation that the villi in the human small intestine differ in shape at different periods of life.

[a] Physiology, tr. by Dr. Baly, 1st edit. pp. 267-8. [b] Physiology, tr. by Dr. Willis, pp. 323-4.

they look like the valvulæ conniventes in miniature, or are small folds of the internal coat: so the accurate Lieberkühn has painted them.[1]

The whole surface of the alimentary canal is covered with these processes; but in the large intestines they are so very short, that to the naked eye the surface of these intestines appears smooth; thence the learned Albinus has considered them as having no villi,[2] which is true in one sense only, viz. that their inner coat does not appear shaggy, but spongy or cellular; yet the partitions between these cells are similar in structure to the villi of the small intestines.

The appearance of the villous coat is very different in different parts of the alimentary canal.

In the œsophagus the villi are small and not so full of vessels, and are of the cylindrical or conical shape.

At the upper part of the stomach the villous coat appears in a microscope like a honeycomb, or like the reticulum, or second stomach of a ruminant quadruped in miniature; that is, full of small cells, which have thin membranous partitions. Towards the pylorus these partitions are lengthened so as to approach to the shape of the villi of the jejunum.

The villi of the jejunum are thin folds considerably broader than they are long, and when not injected they are very flat, so as to resemble valvulæ conniventes in miniature, but are so small that they can but just be distinguished by the naked eye.

In the ileum the villi become rather longer in proportion to their breadth. In the colon and rectum the villous coat is like the upper part of the stomach, honeycombed or cellular. These facts are only evident after a minute injection; for in the uninjected state the villi collapse, so that their figure cannot be distinguished. The partitions between the cells of the internal coat of the colon and of the stomach being each very vascular, and agreeing with the villi of the jejunum in every circumstance except magnitude, are to be considered as having the same use, namely, to absorb, as will appear probable hereafter.

Upon each of the villi is an artery and a vein which make a network of branches, as is well expressed in the ingenious Lieberkühn's plate.[3]

[1] De Villis Intestin. tab. i.

[2] Annotat. Academic. lib. vi, cap. viii.

[3] De Villis Intestinorum, tab. i, icon. 2.

Besides arteries and veins, it is probable that the villi have nerves distributed to them.

They likewise have lacteals, which, according to Lieberkühn, open on the extremities of the villi, sometimes by one and sometimes by more orifices (see Note LXXXIII).

Each villus, the same author thinks, has an ampullula, into which these orifices lead, and from the other side of the ampullula the lacteal passes through the coats of the intestines. This is the only circumstance concerning these parts in which I should differ from this very accurate observer, whose experiments in support of his opinion about this ampullula seem to be liable to fallacy. Of this I was first persuaded from observations made on fish, birds, and amphibious animals, in all of which I can demonstrate that the villi have a network of lacteals as well as a network of arteries and veins.[1]

That the villi in some fish have a network of lacteals, I have distinctly seen in the turbot, where I have injected the lacteals with mercury, which readily runs from those vessels into the villi, and makes them turgid and erected. In the same way, I have likewise seen a network of lacteals on the villi of a turtle, where these villi are of a different shape, and in some parts of the gut are cellular, or honeycombed, something like the lower part of the human stomach, only the partitions of the cells are here much larger.

In birds the experiment is more difficult, because their lacteals are full of valves, and their villi are small compared to those of the turbot; nevertheless, I have succeeded in getting

[1] An account of some preparations exhibiting these facts was printed in the Phil. Trans. vol. lix. (LXXXVI.)

(LXXXVI.) At the College of Surgeons, in Lincoln's inn-Fields, there is a small box, marked on the lid, "Mr. Hewson's Microscopical Preparations of the Intestinal Villi," but in which there is now only one portion of small intestine left.

The circumstantial observations in the text on the intestinal villi of the turbot and turtle seem to have been completely overlooked. Professor Müller,[a] after stating that something similar to villi may be seen in a few fishes, remarks that Retzius has described in a serpent "processes of the mucous membrane of the intestine, which resemble villi, and can scarcely be anything else, although Rudolphi has said that fishes and reptiles have no true villi."

[a] Physiology, tr. by Baly, vol. i, p. 267, 1st edition.

the valves to give way, so as to fill a few of the lacteals distinctly enough to be seen to divide into branches upon the villi, and therefore to prove that they do not form a bag or ampullula.[1]

Since therefore a network of lacteals is found upon the villi of all these animals, from analogy we should suspect the same in the human subject, whose villi are of the same shape, that is, broad and flat, which figure would appear not a proper one for an ampullula.

The experiments from which the ingenious Lieberkühn was persuaded there was an ampullula, were first, the villi appearing turgid with milk which had curdled in them, in such subjects as had taken milk just before their death.[2] But whoever has made experiments with injections must be convinced of its being difficult to distinguish clusters of small vessels from bags, when these vessels are not filled with fluids of a brighter colour than milk or chyle; and even in those cases where such vessels were filled with vermilion (which is so much more vivid and distinguishable) some anatomists have been misled; particularly concerning those corpora globosa in the kidney, which have been considered as bags or cryptæ. But I have repeatedly observed, and have now by me some preparations which prove that these corpora globosa are not uniform bags, but convoluted arteries, which comes near to the idea that Ruysch had of them (LXXXVII). Some ingenious anatomists have warmly espoused a contrary opinion, and have not only supposed the kidney to have follicles, but most other glands of the body, particularly the breast or mamma, and the salivary glands. But that they likewise have been deceived by a cluster of small vessels will appear probable when we consider that the corpora globosa in the kidney, which have by so many

[1] See Philos. Transactions, vol. lix, p. 213.
[2] De Villis Intestinorum, § 2, 3.

(LXXXVII.) Mr. Bowman has given an excellent account of the structure and use of the Malpighian bodies of the kidney, with observations on the circulation through that gland. See 'Philosophical Transactions,' 1842, Part I, pp. 57 et seq. Among some of Hewson's preparations at the College of Surgeons, are two good injections of these glomeruli in the kidney of the lion and of the ass.

been pronounced bags or follicles, are only small vessels clustered together or convoluted. And on making a variety of experiments on these other glands, I think it evident in what manner the deception has happened to those ingenious anatomists; namely, when the excretory ducts of the breast, for example, are injected with vermilion and painter's size, the small acini of which that gland consists are made extremely red, and such a preparation being dried, the acini appear as large as pins' heads, so that the breast has been suspected to have follicles of that magnitude; but on injecting the breast with mercury, which is a brighter substance, and better contrasted to the dried fibres, I have distinguished what in the other preparation might be mistaken for a bag, was here evidently no more than the extremity of the excretory duct, terminating in one of these acini, and dividing into a number of branches so suddenly as to come near to Ruysch's description of the penicilli of arteries; but the small branches, into which this extremity of the duct divided, were so close to one another, that in the preparation where they were filled with size and vermilion, they could not be distinguished, but in that where they contained mercury, it evidently appeared that in each acinus of the magnitude of a pin's head, there was a considerable number of branches, but so small as not easily to be seen with the naked eye (LXXXVIII).

The ingenious Lieberkühn has mentioned another experiment, from which he was not only persuaded that the lacteals formed an ampullula upon the villi, but that this ampullula was filled with a spongy substance. This experiment he made by inflating the villi by the arteries and the veins, and upon dry-

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(LXXXVIII.) That the roots of the milk-ducts are little cells or vesicles, typifying a like structure in a whole series of glandular organs, is now a well-known fact. Professor Müller[a] has noticed the observations made on this subject in the hedgehog, as early as 1751, by Duvernoi. Mr. Cruikshank[b] proved the existence of this structure in the human breast; and Mascagni, according to Müller, observed, in 1819, the absence of any direct communication between the vesicular ends of the milk-tubes and the blood-vessels. Sir Astley Cooper has given an admirable description of the Anatomy of the Human Breast, 4to, London, 1840.

[a] On the Structure of the Glands, tr. by Mr. Solly, p. 9, 8vo, Lond. 1839.

[b] Anatomy of the Absorbing Vessels, p. 209*, 2d edit. 4to, Lond. 1790.

ing the intestine and cutting the villi across he observed them spongy.[1] But this is an appearance which may be as well explained, from knowing that each villus contains a network of small arteries and veins, which being inflated might occasion the villi to assume a spongy appearance.

Since then the experiments from which the villi of the human subject were supposed to contain an ampullula are so equivocal; and since the villi can be proved in the other classes of animals, viz. in birds, fish, and the amphibia, to have networks of lacteals as well as of arteries and veins, the probability is in favour of their having the same structure in the human subject. But the difference between us is inconsiderable; for it may be nearly the same thing whether there is a bag filled with sponge, or a plexus of vessels.

I have some preparations by me adapted to the microscope in Lieberkühn's manner, in which I think I can clearly show the orifices of the lacteals on the extremities of the villi, where there appears, as he has described, sometimes to be one, and sometimes more orifices.[2] My preparations were made by injecting into both arteries and veins a thin size or glue, coloured with vermilion; when this was not pushed to great minuteness the villi appeared exactly as Lieberkühn has painted them, with a network of arteries and veins on each, and when examined with a microscope no orifices could be distinguished, but each villus appeared to have a smooth edge. Yet in some part of the ileum where the injection had run more minutely, the villi appeared erected, and instead of being broad and thin were more round and cylindrical, and the extremity seemed spongy and porous, whilst all the sides of the villus were perfectly smooth and uniform. And moreover as in these preparations the orifices only appeared when the villi were completely erected, I think this circumstance points out the use of the villi.

It might be here objected that these were only lacerations of the villi, but I am persuaded they were not, from having, on repeatedly examining them, observed the pores or orifices very distinct and empty; whereas, were they lacerations, I think I should have seen the injection in them, as the villi were so much distended by it.

[1] De Villis Intest. § 8.

[2] Ibid. § 3 (See Notes LXXXIII and LXXXVI).

It has long been observed by physiologists that absorption takes place only in living animals, and not in the dead; for if an intestine in a dead animal be filled with milk, none of that milk will get into the lacteals; but in the living animal, the milk will readily be absorbed. This I think may be explained from what is above observed of the orifices of the lacteals appearing to open when the villi are erected, something like which may take place in the living animal; that is, whenever absorption is to be performed the blood-vessels of the villi may become turgid, and the orifices of the lacteals may then stand rigidly open, and be capable of attracting, like capillary tubes made of hard substances. But in the dead body the villi being emptied of blood, the coats of the lacteals being soft collapse, by which means their orifices are closed, and they are thence made incapable of attracting the chyle, or of absorbing.

It is observable that those parts of the skin which are intended to have more sensibility than the rest, have those processes called villi most remarkably; this is evident, when after a minute injection we compare the tips of the fingers, the lips, the glans penis, with other parts of the skin; and this is still more observable in the tongue, whose papillæ are the instruments of taste. In these instances some physiologists have suspected that the blood-vessels were some way subservient to the nerves for sensation, an opinion which I think is very probable; and the use of the villi of the skin, agreeably to their opinions, seems to be as organizations of vessels to become more turgid at particular times, by which turgescency the extremities of the nerves are made more capable of doing their functions; and agreeably to this idea it is observable that when we attempt to taste anything extremely grateful, the papillæ of the tongue can be seen to become erected.[1]

And as those villi of the skin seem to be organizations

[1] The papillæ of the tongue in the human subject appear to the naked eye, when they are not minutely injected, quite smooth, but on a minute injection each of these papillæ appears covered with small vascular processes or villi; so that in such a tongue every one of the papillæ seems in the microscope like a bunch of fibres, or rather like a sheaf of corn; some preparations of which, adapted to the microscope, I have now by me. The learned Albinus seems not only to have observed this, but to have had the same idea of the use of these processes, which he calls tubercles, and has painted them like those little eminences that appear upon a nipple, but I find them much longer. See his Annot. Acad. lib. i, cap. xv.

capable of that turgescency which is necessary to adapt the sentient extremities of the nerves to receive impressions, so I suppose the villi of the intestinal tube are able to exert a similar erection or turgescency, in order to make the small absorbents stand rigidly open, and thereby act like capillary tubes of glass or other hard substances: and perhaps such membranes as the pleura, peritoneum, &c. may be without villi, because such processes would be less proper for affording the smoothness of surface required for the motion of one viscus upon another; but to answer the same purpose they may have a network of blood-vessels surrounding the absorbing pores, which reticulation, by its turgescency, may make the pores stand rigidly open as those we have observed upon the villi (LXXXVIII*). But this being a less perfect organization, and used here, merely because the more perfect, or villous, would be incompatible with the motion of the viscera upon those membranes, may, for that reason, be more liable to fail in doing its office, and thereby occasion dropsies of those parts; nothing like which seems to happen to the villi, which do not, as far as we know, ever fail in absorbing chyle so as to occasion a disease.

The orifices of the lacteals and lymphatics, therefore, by acting as capillary tubes, in consequence of a particular organization near their beginnings, are capable of absorbing the chyle and lymph; and as a capillary tube of glass being put into a basin of water will attract the water to a considerable height above the surface of that fluid, so the animal tubes or absorbents, merely by their attractive power, may not only imbibe but convey the fluid at least as far as the first pair of valves, whose distance from the orifice of the absorbents is probably very small. But whether the force of attraction extends much farther than the first pair of valves may be a question. Some have suspected that these fluids were propelled forwards principally by this attraction, but it is not necessary to admit such a supposition, in order to explain the motion of the chyle, or of the lymph, because the vessels which convey these fluids are believed to have muscular fibres, which being stimulated by

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(LXXXVIII.*) As mentioned in Notes LXXXII and LXXXIII, this doctrine of absorption is now exploded. The serous membranes are covered with smooth pavement-epithelium.

the fluid may contract peristaltically, and press the fluid forwards from one pair of valves to another.

The lymphatic system is very full of these valves, much more so than the venous, and the reason of this difference seems to be, that the blood in the venous system is strongly pressed forwards by the vis a tergo from the action of the heart and arteries, and therefore its course is less liable to be interrupted by any accidental pressure. But the motion of the absorbed fluid in these vessels having no such force, but only that of the attraction at the orifice, and the peristaltic contraction of the coats, might easily be overcome by any lateral compression, were it not for the valves, which seem to be given to prevent the retrocession of the lymph being considerable, and to make any lateral pressure, instead of preventing, rather promote its passage to the heart; and as the lymphatic vessels in the human subject not only accompany the arteries, but in many places pass under them, when the course over them is as direct, it would seem probable, as some physiologists have suggested, that this was done in order that they may have the pulsation of such arteries communicated to them, which pulsation, inconsiderable as it is, may rather promote the passage of the lymph.

CHAPTER XIII.

CONTAINING SOME PATHOLOGICAL OBSERVATIONS RELATING TO THE LYMPHATIC SYSTEM.

The fluids which lubricate the different cavities of the body are sometimes collected in these cavities in an extraordinary quantity, and form dropsies, such as the ascites, anasarca, dropsy of the pericardium, thorax, tunica vaginalis, &c.

In a former chapter we observed, when speaking of the lymph that moistens such cavities, that its properties vary in different circumstances; that in these dropsies the fluid that is let out by tapping is generally as thin as water, and instead of coagulating, when exposed to heat, only becomes a little turbid. Sometimes indeed it agrees with the serum of the blood in coagulating by heat, and sometimes it flows from these cavities in a viscid or ropy state. In all these cases the fluid occasioning the dropsy is different from the fluid that naturally moistens those cavities, which, in experiments related above, was found to agree with the coagulable lymph of the blood, in the circumstance of jellying merely by exposition to the air.[1]

These circumstances being considered, I think we may

[1] The water in the ventricles of the brain should, I believe, be excepted, as I never saw it jelly, even when exposed to heat (LXXXIX).

(LXXXIX.) The fluid of a blister, when let out and set aside, frequently coagulates spontaneously: see the careful observations by Dr. A. Buchanan, in the 'Proceedings of the Philosophical Society of Glasgow,' No. 7; and note to the English edition of Gerber's 'Anatomy,' p. 105. As observed in Notes LXVIII and LXIX, the fluid of the lymphatic vessels and of the pericardium does not always coagulate spontaneously when taken from healthy animals. The effect of heat on some fluid from the vaginal tunic of the testicle is mentioned in Note XVII; and the coagulation of that fluid, at the temperature of the air, when mixed with serum of blood or with some other substances, in Note XVIII, p. 31.

thence be led to a more correct notion about the causes of those dropsies, which causes have been supposed to be either an increased secretion, or an impeded absorption, or a rupture of the lymphatic vessel; none of which probably, strictly speaking, gave rise to such morbid collections of water. For if merely an increased secretion or an impeded absorption was the cause of an ascites or an anasarca, then the fluid let out should resemble that contained in these cavities in living animals. The same reasoning holds good against these dropsies being occasioned by the rupture of a lymphatic vessel; that is, the fluid evacuated is not similar to what we found contained in those vessels in our experiments, where the lymph jellied on exposition to the air.

And as we observed in those experiments, that these fluids approached nearer to the nature of those found in dropsies, in proportion as the animal was weakened, or reduced, as particularly in the dog fed eight days on bread and water, is it not therefore more probable, that in these kinds of dropsies there is something more than an increased secretion, or an impeded absorption? that is, there is a perversion of the secretion, or the vessels throw out a fluid different from the natural one; which may happen, either from the exhalant arteries being themselves altered by disease, so as to change the properties of the fluid which passes through them, or from the mass of blood being vitiated or abounding so much with water as to affect this secretion; thence these dropsies are not primary diseases, but the consequences of others, and a diseased liver, spleen, or lungs, which so often accompany these dropsies, are not so properly to be considered as giving rise to them by causing a rupture of a lymphatic vessel, or obstructing the course of the lymph, as by affecting chylification and sanguification; for when the liver, for example, is diseased, and the bile deficient in quality or in quantity, the food not being properly assimilated, may make a bad blood, which may affect the vessels, and may let go its water into these cavities.

But although from these considerations it seems probable that an obstruction or rupture of the lymphatics is not the cause of these dropsies, where a mere water is found in those cavities, yet they may occasionally be the causes of others. If a lymphatic should burst in a person in health, a dropsy may ensue; but the fluid would possess the properties which that lymph does naturally, and be either found coagulated, or would be capable of coagulation, when let out; the same may be observed of an increased secretion, or of an obstruction of a lymphatic vessel; the fluid would differ from mere water, and would either coagulate or be viscid. Instances of such fluids sometimes occur in dropsies.

Thus a viscid ropy fluid has been let out of the abdomen, not only in the dropsy of the ovarium, in which such a fluid is commonly met with, but likewise sometimes in the ascites in men.

In like manner, the cellular membrane is sometimes filled with a gelatinous fluid, which does not ooze out when the integuments are scarified, nor does it retain the impression on being pressed with the finger, as in the common anasarca; this was remarkable in a woman that was in St. George's Hospital a few years ago, and who at the same time had an obstruction of her menses, but no other symptom of ill health. The legs in this woman were swelled to twice their ordinary size, but did not pit on being pressed with the finger. A case of the same sort may now be seen in one of the nurses at St. Bartholomew's Hospital.

A similar gelatinous fluid is sometimes seen upon cutting into tumours of the rheumatic kind near the ligaments of the joints; and Dr. Lower observed, that it was common to find the pericardium filled with a jelly in dead bullocks.

As we have remarked of a rupture of the lymphatic vessels in an animal in health, that the fluid which escapes will coagulate, so we may observe of a wound of such a vessel, the lymph which oozes from it, if the person be in health, will not be a mere water, but will be like the coagulable lymph of the blood in jellying on exposition to the air, only a little later than the blood itself does, if we may judge from what is observed in Chap. vii, pp. 157 et seq. A case of this sort I saw in a butcher, who, by letting his knife fall upon his shin, cut some of the large lymphatic vessels which pass over the tibia, as represented at *c c*, Plate I, fig. 1. From this wound there flowed a considerable quantity of a clear lymph, which, being confined by the dressings, jellied, and then, at first sight, appeared like a whitish fungus, but being loose could be removed with a spatula.

Some cases of wounds of lymphatics are related by the late Professor Monro,[1] who describes a white fungus as apt to arise from them; but since seeing the case above mentioned, I cannot help suspecting that, notwithstanding the accuracy of that gentleman, he had met with a deception of this sort. My patient, like his, was cured by tight pressure, and lint dipped in a solution of vitriol.

When a blister is applied to a person not much weakened by disease, as for example, behind the ear for the toothache, or to one who labours under a violent fever or an inflammation, we find, on removing the cuticle, a serous fluid discharged. This fluid, I have found, coagulates by heat, exactly like the serum of the blood, or the white of an egg. After this serous fluid is let out from the blistered part, we sometimes see over the inflamed skin a white crust or jelly, which is easily removed, and seems to be the coagulable lymph of the blood, which has been thrown out by the inflamed exhalant arteries, and had jellied amidst the serum. When this jelly is more condensed, it appears not much unlike a second cuticle.[2]

It is a fact universally admitted by physiologists, that all parts of the human body are bibulous, or imbibe fluids applied to them; thus, garlic applied to the skin is soon smelt in the breath, and turpentine rubbed upon any part of the body soon gives to the urine a violet-like smell. In like manner, poisonous substances are sometimes taken into the constitution. The variolous matter, inserted under the skin by the point of a lancet, produces the smallpox, and the venereal matter, introduced by a chancre, occasions the lues venerea. These facts have been long known, but it is still a question by what channels these substances enter the body, whether by the common veins, which have most generally been suspected to absorb, or by the lymphatic vessels (see Note LXXXII).

How little probability there is of the common veins doing this office has been observed above; but there are many circumstances which prove that the lymphatic vessels perform it; and there are some appearances in diseases which cannot otherwise be well accounted for.

For example; after the insertion of the variolous matter

[1] Med. Essays, vol. v, art. xxvii.

[2] In dropsical cases the fluids discharged by blisters are more watery.

under the skin of the arm, in inoculation, before that matter enters the constitution so far as to produce any fever or eruption, the lymphatic glands in the axilla most frequently swell or inflame ; a strong presumption that it is through the channel of the lymphatic system that this poison enters the constitution.

After the application of the venereal matter to the genitals, where the skin is abraded, before the lues venerea is occasioned, there is commonly an inflammation of the inguinal glands, which circumstance renders it probable, that in this case, too, the poison enters by the lymphatic vessels.

On the application of blisters, we sometimes find lymphatic glands swelling between the part inflamed by the blister and the heart.[1] Thus the axillary glands sometimes become painful from a blister between the shoulders; and I once from this cause saw glands swell where they are not commonly met with on dissection. It was in the case of my ingenious friend, Mr. H. Apothecary, who, having applied a blister to his back, observed some small swellings opposite to the inferior costa scapulæ; he showed them to me, and I told him they were glands inflamed in consequence of an absorption of a part of the cantharides, and would subside on drying up the blister, which accordingly happened.

It may be worth remarking, that these cases of glands swelling in consequence of the application of blisters, furnish the strongest arguments in favour of the lymphatics being the instruments of absorption ; because where a blister is applied, the skin is only inflamed and not eroded, so that if the acrid matter gets into the lymphatic system, it can only be by absorption. Whereas, when the variolous matter is inserted with a lancet, or the venereal matter enters from a chancre, we might question whether it got into the lymphatics by absorption or by an erosion of the side of those vessels. But when the lymphatic glands swell in consequence of a blister, it seems decisive in favour of the poison entering the lymphatic system, merely in consequence of that system being endowed with a power of imbibing whatever is applied to the surfaces of the body.

Poisons which enter the constitution, besides being discovered by their affecting the lymphatic glands, can sometimes

[1] See Professor Monro, De Ven. Lym. Valv. p. 93.

be traced by their effects on the lymphatic vessels. A case of this sort, in consequence of the bite of a gnat, was lately observed by my ingenious friend, Dr. Maddocks, physician to the London Hospital. This patient, as Dr. Maddocks informs me, had been weeding in a garden, and had been bit near the root of her thumb, where a painful tumour appeared. Soon after which one of the axillary glands inflamed and swelled, and from the tumour of her thumb to the axilla, the ascent of the matter could be traced by a painful ridge or cord, which went on the fore part of the cubitus, and inside of the arm, exactly in the situation of the lymphatic vessels shown in Plate IV, fig. 2, *g*, *h*, one of which seemed to be inflamed in consequence of the absorption of the poisonous matter.

I have likewise lately seen the gland just above the inner condyle of the os humeri, as is represented Plate IV, fig. 2, *d*, swelled in consequence of a wound and suppuration on the back of the middle finger.

In ulcers of the legs, the matter is sometimes absorbed and carried up the lymphatics, till it arrives at the glands in the groin, where it occasions a bubo; which bubo, as has already been observed at page 129, differs from the venereal one in being at the lower part of the groin, viz. in the glands, *f*, *f*, Plate I, fig. 1.

I have even seen the matter of an issue in the leg produce such a bubo by absorption, and in this case too the matter could be traced by a painful line in the inside of the thigh in the course of the lymphatic vessels, represented in the same plate.

Matter formed in the joints, on being absorbed, likewise produces such buboes.[1]

And milk which has stagnated in the breast creates a painful swelling in the axillary glands.

The axillary glands are likewise frequently observed to swell in consequence of cancers in the breast;[2] and it is found to be of no use to extirpate the breast itself, unless the infected glands can likewise be removed; for otherwise the cancerous humour left in the glands may renew the disease; and indeed when these glands are affected in consequence of a cancer, the operation of extirpation must be very precarious, as we can never

[1] See Dr. Hunter's Medical Commentaries. [2] Monro, De Ven. Lymph. Valv. p. 92.

be certain that the matter which has got so far as these glands may not also have got a little further, and have entered the constitution.

In cancers of the lips, the lymphatic glands under the angle of the lower jaw, and on the side of the neck, are apt to swell from the same cause, viz. the absorption of the cancerous matter. And the like swellings may be produced by the absorption of the venereal virus from sores in the lips. I have seen these glands swell in consequence of gum-boils, which frequently appear on the upper jaw, from the fang of a rotten tooth making its way through the jaw, and producing a suppuration that sometimes bursts outwards, sometimes into the socket of the tooth, and sometimes disappears without rupture; in which case I have several times seen the glands under the angle of the lower jaw swell and become painful, during the few days that the boil was diminishing.

In short, wherever there is an erosion or ulceration of the body attended with acrid matter, that matter is apt to be absorbed, and in passing into the constitution commonly inflames the lymphatic glands which lie between the part eroded and the thoracic duct; a fact well deserving the attention of the surgeon, who might otherwise take these glandular tumours for primary diseases, and might expect to cure them without attending to the ulcers themselves, but in vain; for being occasioned by the absorption of acrid matter, they will remain so long as the matter continues to be absorbed; but that matter being once removed, these glandular tumours will generally subside of course.

And moreover, as it frequently happens that these poisons are not immediately absorbed, but remain for some time in the wound before they enter the vascular system, it gives us an opportunity of preventing the disease by cutting out the morbid flesh, and thereby extirpating the poison before absorption has taken place; a practice that has been used successfully for the bite of a mad dog, and cannot be too strongly recommended, as it seems to be the only certain way of preventing the ill consequences of such an injury. In those cases where the knife cannot be used, the application of the actual or potential cauteries has been recommended; for these cauteries, by destroying the poisonous matter, and the parts which it has already tainted, may preserve the constitution from the infection. It is also probable that the venereal poison might be prevented from entering the constitution, if immediately upon the appearance of a chancre the patient would submit to the excision of that chancre, or to have a caustic applied to it. In like manner, since it is known that when the cancerous matter is once generated, whether in the mamma, testis, or any other gland, that such matter, on being absorbed, will infect the other parts of the body; is it not therefore a strong argument in favour of the early extirpation of cancers, that the longer they are suffered to remain, the more probability there will be of the cancerous humour being taken up by the absorbents, and spreading the infection?

As the lymphatic vessels pass through the lymphatic glands in their way to the thoracic duct, when these glands are obstructed, the lymph, not being able to get into that duct, is retained in the extremities; thence we so often see dropsies the consequences of diseased lymphatic glands; which dropsies cannot be cured till the obstruction of the gland be removed. But, as I have already observed, the lymphatic vessels do not constantly pass through glands, but some of them pass by their sides; thence it is possible that a gland may be perfectly obstructed, and yet the lymph may get by the collateral branch of a lymphatic into the thoracic duct, and not be retained in the extremity so as to occasion a dropsy or œdema. In like manner, a gland may be perfectly eroded, or may be cut out, sometimes without the lymph being thereby prevented getting into the thoracic duct. And it may also happen, that the venereal or other poison may, upon being absorbed, pass into the constitution by one of these lateral branches without entering a lymphatic gland or inflaming it. That this is probable the reader may believe, upon looking over the plates, particularly Plate II, where the mercury appears to have passed from the groin the whole length of the trunk without entering a lymphatic gland. From which fact may be understood how the venereal poison sometimes enters the constitution, and produces the lues without occasioning a bubo, an instance of which I saw lately. And the variolous matter introduced by inoculation, although frequently producing inflammation and swelling of the lymphatic glands, yet is not always attended with those symptoms: to which may be added, that another reason why these poisons do not constantly affect the lymphatic glands in their way into the body, may be the lessened sensibility of these glands in some particular cases; whence the same poison which at one time would have produced the worst effects, may at another pass through these glands without inflaming them.

And lastly, not only obstructed lymphatic glands sometimes occasion dropsies, but also whatever impedes the passage of the lymph into the veins; whether it be a thickening of the coats of the jugular or subclavian veins near the termination of the thoracic duct, or a tumour of the aneurismal, scirrhous, or encysted kind, contiguous to any part of the lymphatic system; for such tumour, by compressing the lymphatic vessels, may prevent the return of the lymph, and may thereby occasion a dropsy or œdema of the parts from which these vessels originated.

Upon the whole, whoever carefully views the lymphatic system must be convinced that, as it explains and points out the cure of many diseases, it deserves the attention of the practitioners of the healing art. And as it is so generally diffused through the animal kingdom, it strongly claims the regard of those who wish to inquire philosophically into the animal economy, especially as, by the knowledge of this system, we are now flattered with the hopes of ascertaining the use of the lymphatic glands, the thymus, and the spleen; which discoveries are to be the subjects of the Third Part of these Inquiries.

EXPERIMENTAL INQUIRIES.

PART III.

A DESCRIPTION

OF THE

RED PARTICLES OF THE BLOOD

IN THE

HUMAN SUBJECT AND IN OTHER ANIMALS;

WITH

AN ACCOUNT OF THE STRUCTURE AND OFFICES OF THE LYMPHATIC GLANDS, OF THE THYMUS GLAND, AND OF THE SPLEEN:

BEING

THE REMAINING PART OF THE OBSERVATIONS AND EXPERIMENTS

OF THE LATE

MR. WILLIAM HEWSON, F.R.S.

AND TEACHER OF ANATOMY.

BY MAGNUS FALCONAR,

SURGEON, AND TEACHER OF ANATOMY.

LONDON:

MDCCLXXVII.

Nullius addictus jurare in verba magistri,
Et verum et veri cupio cognoscere causas.

HOR.

TO

WILLIAM BROMFIELD, ESQ.

SURGEON TO HER MAJESTY, AND TO ST. GEORGE'S HOSPITAL;

AND TO

PERCIVAL POTT, ESQ., F.R.S.

AND SURGEON TO ST. BARTHOLOMEW'S HOSPITAL,

THE FOLLOWING SHEETS ARE MOST HUMBLY INSCRIBED,

AS A TRIBUTE OF GRATITUDE AND RESPECT,

BY THEIR MUCH OBLIGED

AND VERY HUMBLE SERVANT,

THE EDITOR.

PREFACE.

THE following sheets comprise the remaining part of the discoveries and experiments of my late friend Mr. William Hewson, in whose death the public sustained an almost irreparable loss; the loss of a genius, whose superior abilities in his profession rendered him eminently conspicuous among his cotemporaries, and I have no doubt will transmit his fame to posterity, enrolled among the highest names of antiquity. But to the men of science of this age, his talents require no commendation from my pen.

Unfortunately for the world, his death prevented him from completing the work he had begun: the first chapter of this treatise only was written by him, and read in the Royal Society, June 17th and 24th, 1773; which was afterwards published in the second part of the sixty-third volume of the Philosophical Transactions; and it is much to be lamented, that among his papers we have not found the smallest note upon the subject of the other four chapters. But a three years' acquaintance, during the greatest part of which the strictest intimacy and friendship subsisted between us, afforded me numberless opportunities of discoursing with him upon this subject, and of making myself perfectly acquainted with his ideas; besides which, as I had the advantage of assisting him in other anatomical pursuits, it was frequently my good fortune to make and repeat many of the experiments; by which means I became not only better acquainted with the doctrine, but also perfectly confirmed in my knowledge of its truth.

As far as I can recollect, I have recited the experiments in the order they were made by Mr. Hewson; but lest I might err, or not represent facts in their true state, I have repeated all the experiments frequently since his death, and have written them circumstantially as they appeared to me. But to make these experiments with the attention and circumspection necessary upon so curious a subject, requires more time than my other employments would permit, on which account I have been obliged to defer the publication of them till this time, when I hope their greater accuracy will in some measure compensate for my delay.

I have chosen to publish them under the form of chapters and sections, for the sake of more precision for those who are not much conversant in anatomy; at the same time I flatter myself this method will not be wholly unacceptable to those who have made that branch of natural philosophy their particular study: for as the facts are numerous, by this method they will be referred to with much greater facility, and the propriety of the inferences will be judged of the more readily.

To such gentlemen as are not well acquainted with anatomy, the subject, from the quantity of new matter it contains, may appear obscure; but as this is a doctrine in which not only the medical part of the world, but philosophers in general, are much interested, we most earnestly entreat them to suspend their judgment till they have deliberately considered all the facts. The inductions will then, I presume, be thought just.

As it is the ultimate end of philosophy to investigate truth, so it must afford the greatest pleasure to generous minds to embrace it, in whatsoever form they find it. The remarks and criticisms of such I shall esteem the greatest obligations. Some there may be who, having early imbibed prejudices, will find it very difficult to shake them off; others may be very unwilling to believe that they have erred both in opinion and practice all the preceding part of their lives. I shall at all times esteem it a happiness and an honour to remove the doubts of these gentlemen, not so much from a wish to persuade by argument, as to convince by demonstration.

And as many gentlemen who are not possessed of glasses may be desirous of seeing the experiments upon the blood, I shall at all times be happy in showing them to such gentlemen as will do me the favour of calling upon me.

RED PARTICLES OF THE BLOOD.

CHAPTER I.

ON THE FIGURE AND COMPOSITION OF THE RED PARTICLES OF THE BLOOD, COMMONLY CALLED THE RED GLOBULES.

THE red particles of blood in the human subject have, since the time of Leeuwenhoek, been so generally allowed to be spherical, that in almost all books of physiology they are denominated red globules (XC). A few authors, however, have, at different times, doubted whether they were spheres, and amongst the rest Father de la Torré, whose curious observations, together with his glasses, were presented to the Royal Society, anno 1766.

It is a curious and important fact, that these particles are found so generally through the animal kingdom, that is, they are found in the human species, in all quadrupeds, in all birds, in all amphibious animals, and in all fish, in which animals they are red, and colour the blood.

The blood even of insects contains particles similar in shape to those of the blood of more perfect animals, but differing in colour (XCI).

(XC.) Senac[a] first mentions them as spherical, and subsequently states that, far from being little spheres, their shape is lenticular, comparing them to lentils. But the corpuscles were commonly considered to be spherical in Hewson's time, and even long afterwards. The error is hardly now completely discarded.

(XCI.) An account of Mr. Newport's observations on "The Development of the Blood-Corpuscle in Insects and other Invertebrata, and its Comparison with that of Man and the Vertebrata," will be found in the 'Proceedings of the Royal Society,' Feb. 6, 1845. A paper was read

[a] Traité de la Structure du Cœur, tom. ii, pp. 81, 656, ed. 1749.

In water insects, as the lobster and shrimp, these particles are white; in some land insects, as the caterpillar and grasshopper, they appear of a faint green when in the vessels, as I am persuaded from experiments. I have seen them in an insect no bigger than a pin's head, and suspect they exist almost universally through the animal kingdom.

What is so generally extended through the creation, must be of great importance in animal economy, and highly deserving the attention of every inquirer into the works of nature.

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before the same Society, June 19, 1845, by Mr. Wharton Jones, "On the Blood-Corpuscle considered in its different phases of development in the Animal Series."

Mr. Newport states that the matured blood-corpuscles in the invertebrata are always oat-shaped, or oval, but change their form when removed from the body. They commence as minute rounded molecules, which are gradually developed into nucleated oat-shaped corpuscles, within which nucleoli are formed. These corpuscles he regards as analogous to the white corpuscles of vertebrata. When matured they become diffluent, liberate their nucleoli, and supply plastic fluid to the blood. Nearly the whole of these corpuscles disappear in this way in lepidopterous insects, during the first few days of the pupa state, when the organic changes are most active, and very few remain in the perfect insect. From these observations, Mr. Newport, like the physiologists cited in Note CXVIII, regards the blood-corpuscle as analogous in function to the secreting cells of glands. The further development of the nucleoli into discs, as stated in his paper, he is now satisfied is incorrect.

Mr. Jones recognizes three phases in the development of the blood-corpuscle, which he respectively names the phase of granule-cell, the phase of nucleated-cell, and the phase of free cellæform nucleus. In the invertebrata and oviparous vertebrata, the blood-corpuscle exists only in the first two phases. It is found in all the three phases only in man and the mammalia. In the phase of free cellæform nucleus it is the well-known red corpuscle of man and the mammalia. The red corpuscle of oviparous vertebrata belongs to the phase of nucleated-cell. In the invertebrata the blood-corpuscle in the phase of nucleated-cell does not attain a perfect coloured stage, as in the oviparous vertebrata, but it may be met with slightly tinged red.

In the lymph of oviparous vertebrata there are granule-cells and nucleated cells; in the lymph of man and the mammalia there are, in addition to these, free cellæform nuclei. In fact, as regards corpuscles, the lymph of the vertebrata is identical with their blood,—the principal difference being that, as regards the lymph of the oviparous vertebrata, the nucleated cells have not attained to the perfectly red stage, whilst, as regards the lymph of man and the mammalia, the free cellæform nuclei have not attained to their perfectly red stage. A more complete account of Mr. Jones's views will soon be published in the 'Philosophical Transactions.'

This subject becomes the more interesting, so much reasoning in the theory of medicine being built on the properties of those particles.

It is by the microscope alone that we can discover these particles; and as some dexterity and practice are required in the use of that instrument, there have not been wanting men of character and ingenuity, who, having been unsuccessful in their own experiments, have questioned the validity of those made more fortunately by others. Some have gone so far as to assert that no credit can be given to microscopes; that they deceive us, by representing objects different from what they really are (XCII).

These assertions, though not entirely without foundation, when we speak of one sort of microscopes, are very unjustly applied to them all. In compound microscopes, when the object is viewed through two or more glasses, if these glasses be not well adapted to the focus of each other, the figure of the object may be distorted; but no such circumstance takes place when we view an object through a single lens. All who use spectacles agree that the figures of objects appear the same through them, as they do to the naked eye. And as the single microscope has, like the spectacles, but one lens between the

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(XCII.) The differences in the results obtained by the use of the microscope have often been cited, especially by Dr. Bostock, to prove the inutility of this instrument in anatomical and physiological inquiries. But, on the same principle, the use of the naked eye might be objected to; for there are observations made by it nearly or quite as discordant as those which have resulted from the use of the microscope. Let any one, for example, attend to the descriptions given by various eminent men, who have used the naked eye only, of the intimate structure of the bones, or of the filamentous and adipose tissues. Yet these very subjects have been made so clear by the aid of the microscope, that no anatomist is now likely to fall into the old error of confounding these two last-named tissues, or, in distinguishing them, to find it necessary to have recourse to the evidence of remote physiological and pathological phenomena. With the microscope, too, Harvey could have shown a direct proof of his great discovery of the circulation of the blood, which took so much time to establish by inference from observations with the naked eye. A curious collection might be formed of the errors of anatomical observations made by the naked eye compared with the errors which have arisen from the use of the microscope.

eye and the object, there is no reason to suppose the one can deceive us more than the other. The compound having a larger field, is more pleasant than the single microscope for many purposes; but the single should be always preferred by those who wish to ascertain the figures of minute bodies. It was this instrument, supported on a scroll, as delineated by Mr. Baker (Microscope made Easy, plate 2, chap. ix), that has been used in these experiments, and almost all the observations were made with lenses, as they are prepared by some of our more skilful workmen in London. One observation only was made by means of those globules made of glass, which the ingenious Father de la Torré presented to the Royal Society, and which they were so obliging as to lend to me. Of these globules, two only were fit for use when they came to my hands; viz. that which, according to Father de la Torré, magnifies the diameter of the object 640 times, and that which magnifies 1280 times. The lenses of the greatest magnifying power made in London, are those of $\frac{1}{50}$ of an inch focus; which, even allowing eight inches to be the focal distance of the naked eye, magnifies the diameter of the object only 400 times; a power much inferior to what may be obtained by globules, and particularly by that globule which, according to Father de la Torré, magnifies the diameter 1280 times; and this globule I have used in some of these experiments. But our lenses, though inferior in magnifying power to these globules, are much superior in distinctness. The globules are full of clouds made by the smoke of the lamp used in preparing them, and the object can be seen only through the transparent parts of the globules, which makes it difficult to get a satisfactory view of it; this, with the trouble of adapting the object to the focus of the glass, made me prefer our own lenses for all the experiments mentioned in these sheets, except one; and it is but doing justice to the ingenious gentleman above mentioned, to acknowledge that the greater power of his glasses was found, in that experiment, more than to compensate for their want of distinctness.

These particles of the blood, improperly called globules, are in reality flat bodies; Leeuwenhoek and others have allowed that in fish, and in the amphibia, they are flat and elliptical, but in the human subject and in quadrupeds almost all microscopical observers have agreed in their being spherical (XCIII). When we consider how many ingenious persons have been employed in examining the blood with the best microscopes, it will appear wonderful that the figure of these particles should have been mistaken; but our wonder will be lessened when we consider how many obvious things are overlooked, till our attention is very particularly directed to them; and, besides, the blood in the human subject and in quadrupeds is so full of these particles, that it is with great difficulty we can see them separate, unless we find out a method of diluting the blood. It is to such a discovery that I attribute my success in this inquiry; for having examined the blood as it flows from the vessels of the human body, it appeared a confused mass, notwithstanding I spread it thin on a glass, or a piece of talc. It then occurred to me to dilute it, but not with water, for this I knew dissolved the particles, but with serum, in which they remained undissolved (XCIV). By the serum I could dilute it to any degree, and therefore could view the particles distinct from each other; and in these experiments I found that these particles of the blood were as flat as a guinea (XCV). I like-

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(XCIII.) See Note XC.

(XCIV.) Muys[a] mentions the difference between the effect of serum and water on the corpuscles. That they are not dissolved by water was observed by Dr. Young.[b] It merely deprives them of their colouring matter, leaving undissolved the pale membranous base of the disc, and the nucleus also, when there is one; see Note CII, pp. 222-3: and for an account of the action of water, and fluids of different densities, on the blood-corpuscles, with a description of their power of endosmose and exosmose, see the observations by Dr. Rees and Mr. Lane.[c] The corpuscles should be examined in their own serum; for it may be difficult to distinguish their outline in the serum of another animal, which is apt to cause them to aggregate into clumps. The serum of the horse has this effect remarkably on the corpuscles of many mammalia; the addition of a small quantity of salt to the serum will diminish or destroy the clumping of the corpuscles: see Notes XXIII and CXI.

(XCV.) They are not nearly so thin, in relation to their breadth, as a guinea, the thickness of which is about a nineteenth of its diameter. Dr. Hodgkin and Mr. Lister,[d] indeed, state the thickness of the blood-corpuscle to its diameter as 1 to 45; but this must be a misprint. From measurements of the thickness, at the circumference, of the

[a] Musculorum Artificiosa Fabrica. 4to, Lud. Batav. 1751, p. 100.

[b] Medical Literature, 8vo, Lond. 1813, p. 547.

[c] Guy's Hospital Reports, 1841, vol. vi; and 1843, vol. i.

[d] Philosophical Magazine, or Annals, &c. vol. ii, 1827, p. 133.

wise observed, that they had a dark spot (XCVI) in the middle, which Father de la Torré took for a hole; but upon a careful examination I found it was not a perforation, and therefore that they were not annular. I next made experiments by mixing these particles with a variety of other fluids, and ex-

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corpuscles of several mammalia,[a] I find that it is generally between one third and one fourth of the diameter; and the average thickness of the human blood-corpuscle I estimate at $\frac{1}{12400}$th of an English inch, and the diameter at $\frac{1}{3200}$th. These proportions are shown by the figures in the 'London and Edinburgh Philosophical Magazine' for Aug. and Sept. 1842, pp. 109, 170. More than a century ago, Jurin[b] estimated the diameter of the blood-corpuscle of man at $\frac{1}{3240}$th of an English inch; and Senac,[c] afterwards, at $\frac{1}{3500}$th and $\frac{1}{3300}$th of a French inch; all, I have no doubt, most accurate measurements of a great number, though not of the majority, of the corpuscles in any drop of human blood. According to Senac,[d] the English physicians regarded the corpuscles as hollow on both sides; which he disbelieved, concluding, I think, like many of the old observers, only from what he saw in the lower vertebrate animals. But in man and the mammalia generally, it is certain that many of the corpuscles have, like a biconcave disc, a depression on each side of the broad surface; as inferred by Dr. Young,[e] and proved by Dr. Hodgkin and Mr. Lister.[f] Some of them are without the depression, and quite flat; a few may even be slightly swoln on the broad surfaces, and still more so at the edges, so that their relative thickness is variable. This doubly convex form is very remarkable and general in the corpuscles of young embryos. Varieties in the form of the corpuscles are mentioned in Notes CVI and CX; the size and form of the corpuscles in different animals, in Notes XCVIII and CXVII; and the central spot of the corpuscles of mammals, in Note XCVI.

(XCVI.) The central spot of the blood-corpuscle of mammalia should not be confounded with the nucleus of the blood-corpuscle of oviparous vertebrata, described in Note CII. The central spot is not visible in the best focus and light; then, if the object-glass of the microscope be so slightly removed from the corpuscles as not to destroy the distinctness of their contour, a dark spot will appear in their centre; if the glass be next so far moved towards the corpuscles as to place them slightly within the focus, the dark spot will become bright; and when in a clear light the spot is altogether invisible, it may be instantly brought into view by diminishing the light, without altering the focus. Senac[g] described the centre of the frog's blood-corpuscle as sometimes white and sometimes black; and Della Torre[h] depicted a like variation in the centre of the corpuscles of mammalia, though he mistook it for a hole.

[a] See the Tables at the end of this chapter, Note CXVIII*.

[b] Phil. Trans. 1718, vol. XXX, p. 762.

[c] Traité du Cœur, tom. ii, p. 655, ed. 1749; and 2d ed. tom. ii, p. 276.

[d] Op. cit. tom. ii, p. 80, ed. 1749.

[e] Med. Liter. p. 546, 8vo, Lond. 1813.

[f] Phil. Magazine, 1827, vol. 2, p. 132.

[g] Traité du Cœur, ed. 1749, tom. ii, pp. 656-7; and 2d ed. tom. ii, p. 277.

[h] Nouve Osservazioni Microscopiche, tab. xiv, figs. 3, 4, 4to, Napoli, 1776.

amined them in many different animals; and the result of these experiments was, that their size is different in different animals (XCVII), as is seen in Plate V, where they are represented of the size they appeared to my eye, when viewed through a lens of $\frac{1}{23}$ of an inch focus; which, allowing eight inches to be the focal distance of the naked eye, magnifies the diameter 184 times.

It may not be improper to observe here, that the accurate Leeuwenhoek, not having diluted the human blood or that of quadrupeds so as to see these particles separate from each other, was thence not qualified to describe them from his own observation, as he has done those of fish and of frogs, and suspecting a round figure was more fit for circulating in our vessels, was thence led to suppose these particles spherical in the human subject. But I shall hereafter be able to show from his own words, that it is not his observations, but his speculative opinions, or his theory, that differ from what I have discovered by these experiments.

In Plate V, it appears that of all the animals which I have examined, the particles are larger in the fish called a skate; next to a skate they are larger in a frog and a viper, and other animals of this class; they are somewhat smaller in the common fish, as the salmon, cod, and eel. In birds they are smaller than in fish; in the human subject smaller than in birds; and in some quadrupeds still smaller than in the human subject. Leeuwenhoek, speaking of their size, says, he is confident the red particles of the blood are no larger in a whale than in the smallest fish.[1] And others have since his time said, they are of the same size in all animals; but it is evident from comparing their size as delineated in the above-mentioned plate, that it differs considerably, and that they are not larger in the largest animals; for we find that in an ox they are not so large as in a man, and so far are they from being larger in the whale than in the small fish, it appears probable, from comparing their size, as delineated Plate V, fig. 2, from a por-

[1] Cons. Arcan. Nat. p. 220.

(XCVII.) Concerning the shape and size of the corpuscles of man and different animals, see Notes XCV and XCVIII, and the Tables of Measurements, Note CXVIII*, pp. 236 et seq., at the end of this chapter.

poise, which belongs to the same genus as the whale, that they are smaller in those animals than in fish; neither is their size inversely as in the size of the animal, for they are as large in an ox as in a mouse (XCVIII). The difference in their size,

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(XCVIII.) Among the mammalia, the harvest mouse, the smallest British species, has corpuscles quite as large as those of the horse; and in the common mouse they are larger than in the ox or horse. The corpuscles of the elephant are larger than those of any other mammal; the sloth has them next in size, and then comes the whale, in which they are somewhat larger than in man, the capybara, and the porpoise. The Napu musk-deer has the smallest corpuscles yet discovered; those of the Stanley musk-deer are about the same size; in the ibex (Capra Caucasica) they are rather larger, and in the goat slightly larger still. The corpuscles of the goat were the smallest known before my observations, referred to in Note CXVIII*, p. 236.

Although in mammals there is not much relation between the size of the corpuscles and that of the animal in different orders, there is such a connexion in any one natural family. The corpuscles of the large ruminants or rodents, for example, are larger than the corpuscles of the smallest species. In the intermediate series there are several slight exceptions, but no considerable one. Yet in different subdivisions of one order the size of the corpuscles may differ so remarkably, that the fact will probably be available as a help to classification. Thus the feræ, if set down according to the size of their blood-corpuscles, would stand as follows,—seals, dogs, bears, weasels, cats, viverras; and one of the last may generally be distinguished, simply by the smaller size of its corpuscles, from any species of the three first-named families. Among genera of doubtful affinities, if regard were paid only to the size of the blood-corpuscles, the hyæna would be arranged with the canidæ, basaris with the ursidæ, and cercoleptes with the viverridæ. The fox has slightly smaller corpuscles than the dog. Though the smallest corpuscles are found in the ruminants, some of the largest species of this order have larger corpuscles than many carnivora, or than the horse. The camel tribe are the only mammals with oval blood-discs, like those of lower vertebrata in shape, but entirely agreeing in size and structure[a] with the corpuscles of mammalia. The blood-corpuscles of marsupial animals agree generally in form, size, and structure, with the corpuscles of the corresponding placental animals. In the monotremata, according to the observations of Dr. Davy,[b] Dr. Hobson and Dr. Bedford,[c] the corpuscles are similar, in all respects, to those of man; my measurements of the corpuscles of the echidna are to the same effect.

In birds and reptiles, with a few exceptions, the long diameter of the oval blood-corpuscle is rather less than twice the short diameter. These proportions used to be considered as universal; but the long

[a] See Note CII, pp. 222-3.
[b] Proc. Royal Society, May 28, 1840.
[c] Tasmanian Journal, vol. i, p. 94, Van-Diemen's Land, 1841.

therefore, depends on some other circumstance than a difference in the size of the animal.

As to their shape, I have already mentioned that they are flat in all animals, even in the human subject; of which any one may be convinced by repeating the following experiments.

EXPERIMENT I.

Take a small quantity of the serum of human blood, and shake a piece of the crassamentum in it till it is coloured a

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diameter may be either thrice, or scarcely one and a half of the short diameter; and it is remarkable that these differences of shape may occur in the corpuscles of nearly allied genera, as explained in the 'Proceedings of the Zoological Society,' 1840, pp. 43, 132; and 1842, p. 110. The short diameter of birds' corpuscles is commonly about the same as the diameter of the circular corpuscles of mammals. The size of the blood-corpuscles of mammalia in connexion with that of the animal, is described in a former part of this note. Throughout the entire class of birds, the law for the size of the corpuscles is very nearly the same as in a single family of mammals. At least my measurements, given at the end of this chapter, afford but few if any examples of comparatively large corpuscles in the smallest species, and of more minute corpuscles in the largest species of birds. In a humming bird, Dr. Davy[a] found the average length of the corpuscles $\frac{1}{2000}$th, and the breadth $\frac{1}{4000}$th of an inch. These corpuscles are therefore shorter than those, yet examined, of any other bird. In my Tables of Measurements in this class, it will be observed that the blood-corpuscles of that great bird, the cassowary, are the largest, while the smallest corpuscles occur in some of the little insectivorous and granivorous birds, and in a few of the smallest species of the gallinaceous order.

In the naked amphibia the blood-corpuscles are generally larger than in any other animals; and largest of all in the amphibia with permanent gills, as discovered by Professor Wagner,[b] and confirmed by my measurements of the corpuscles of the siren.

The blood-corpuscles of fishes are noticed in Note CXVII; the thickness of the corpuscles of mammals, and their comparative size and shape in the embryo, in Notes XCV and CXVI; the structure of the corpuscles of mammalia and lower vertebrata, in Note CII; the central spot, in Note XCVI; the shape of the nucleus in oviparous vertebrata, in Notes CV and CXVII; irregularities in the figure of the corpuscles of mammals, in Notes CVI, CX and CXI; and the blood-corpuscles of invertebrata, with the corpuscle in its different phases of development in the animal series, in Note XCI. For the copious Tables of Measurements, see Note CXVIII*, at the end of this chapter, pp. 237 et seq.

[a] Proceedings of the Zoological Society, March 24, 1846.

[b] Physiology, tr. by Dr. Willis, p. 236; and Proc. Zool. Soc. 1837, p. 107.

little with the red particles; then with a soft hair pencil spread a little of it on a piece of thin glass, and place this glass in the microscope, in such a manner as not to be quite horizontal, but higher at one end than the other, by which means the serum will flow from the higher extremity to the lower; and as it flows, some of the particles will be found to swim on their flat sides, and will appear to have a dark spot in their middle (see Note XCVI); others will turn over from one side to the other, as they roll down the glass. No person who sees them turn over can doubt of their being flat; he will see them, in turning, have all the phases that a flat body has; first, he will see them on one side, then rise gradually on the edge, and turn over to the other side. I have in this way shown their figure to a number of curious persons, and particularly to many students of anatomy, who have attended lectures in London within the last six years. If instead of serum the particles should be diluted with water, containing rather more salt than serum does, or if instead of human blood that of an animal with larger particles be used, then they will sometimes be seen not only flat, but a little bended, like a crooked piece of money.

These experiments not only prove that the particles of the blood are flat, and not globular, but likewise, by proving that they are flat, they show that they are not fluid, as they are commonly believed to be (XCIX); but, on the contrary, are solid, because every fluid swimming in another which is in larger quantity, if it be not soluble in that other fluid, becomes globular. This is the case when a small quantity of oil is mixed with a larger quantity of water, or if a small quantity of water be mixed with a large one of oil, then the water appears globular; and as these particles are not globular but flat, they must be solid, a circumstance that will appear still more evident from future experiments.

It is necessary to remark, that in a few minutes after the

(XCIX.) Haller[a] says of them, "Solidi, perpetui, circumscripti, certa magnitudine et figura, certa ferri portione prægnantes, in fluido invisibili moventur." Senac,[b] too, considered the corpuscles as solid: see Note CVIII.

[a] Elementa Physiologiæ, tom. ii, p. 52. 4to, Lausannæ, 1760.

[b] Traité du Cœur, tom. ii, p. 665, ed. 1749; and ed. 1774, t. ii, pp. 278, 283.

particles are spread out on a glass, they run in clusters, and stick to each other, and then they appear confused (c).

When one of these particles is attentively examined, while separate from the rest, it appears, as it lies on its flat side, to have a dark spot (ci) in the middle, and round that dark spot it is more transparent. This dark spot was believed to be a perforation, or the particle was supposed to be a hollow ring, by the ingenious Father de la Torré. But I find from a great number of experiments, that the dark spot is a solid particle contained in a flat vesicle, whose middle only it fills, and whose edges are hollow, and either empty or filled with a subtile fluid. This will be evident to every one who will carefully make the following experiments.

EXPERIMENT II.

Take a drop of the blood of an animal that has large particles, as a frog, a fish, or what is still better, of a toad; put this blood on a thin piece of glass, as used in the former experiment, and add to it some water, first one drop, then a second, and a third,

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(c.) Further on,[a] Hewson says he has seen them laid one against another like a number of coins; and the way in which the corpuscles of mammalia run together by their broad surfaces, forming piles like rolls of money, has become well known since the clear description of the fact by Dr. Hodgkin and Mr. Lister.[b] The earliest representation which I have seen of these rolls of the corpuscles is that by Della Torre,[c] three years after the publication of Hewson's observations. Mr. Wharton Jones[d] has figured the rolls; and I have depicted[e] parts of them on a larger scale, from the blood of man and of the horse. These rolls further coalesce into clumps in buffy human blood, and still more remarkably in the naturally buffy blood of the horse: see Note XXIII. The aggregation commences as soon as the blood is drawn, goes on after two or three minutes at a greatly accelerated rate, and subsequently some of the corpuscles separate again from the piles. Dr. Davy[f] has particularly described the viscid quality of the corpuscles as distinct from that by which they form into columns; they stick together at their rims, so that the corpuscles, when separating from each other, become elongated almost into a fibre, and recover their circular form when completely detached. I have frequently observed these appearances in corpuscles which have been kept a day or two in the serum.

(ci.) See Note XCVI, p. 216.

[a] See Note CIX, p. 228.

[b] Phil. Magazine, 1827, vol. ii, p. 135.

[c] Nuove Osservazioni Microscopiche, tab. xiv, fig. 4, 4to, Napoli, 1776.

[d] Edin. Med. and Surg. Journ. lx, 311.

[e] Lond. and Edin. Phil. Mag., Aug. and Sept. 1842, pp. 109, 170.

[f] Trans. Royal Soc. Edin., xvi, 54-5.

and so on, gradually increasing the quantity; and in proportion as water is added, the figure of the particle will be changed from a flat to a spherical shape. When much water is added, the vesicle will by degrees become thinner and more transparent, and will at last be dissolved. When the vesicle has thus assumed a spherical shape, it will roll down the glass stage smoothly, without those phases which it had when turning over whilst it was flat; and as it now rolls in its spherical shape, the solid middle particle can be distinctly seen to fall from side to side in the hollow vesicle, like a pea in a bladder. Sometimes, indeed, instead of falling from side to side, the solid middle particle is seen to stick to one part of the vesicle; and in proportion as the vesicle, instead of being flat, assumes a spherical shape, its longest diameter is shortened, as might be expected on the supposition of its being hollow and flat (CII).

(CII.) The figures of Leeuwenhoek[a] indicate the nucleus in the corpuscles of the salmon and flounder. Bidloo[b] called the corpuscles globosæ vesiculæ. Emanuel Weis[c] inferred that the circumference both of the oval and round corpuscles must be bounded by a membrane; and he speaks of the nucleus as a distinct part. Hewson's demonstration of the nucleus and envelope in the corpuscle of the lower vertebrata is good; but I believe he erred in stating that a similar nucleus exists in the human blood-corpuscle.

The majority of the red corpuscles in man, and other mammalia, after an early stage of intra-uterine life, have no nucleus corresponding to that so obvious in the corpuscles of the lower vertebrate animals;[d] and even the oval corpuscles of the camelidæ agree with the corpuscles of other mammals in this respect. But in the very young embryo of mammals the blood-corpuscle has a distinct nucleus, the true representative of the permanent nucleus in the corpuscle of lower animals; and the blood of such embryos at the earliest period is pale, from an abundance of free globules like the nuclei, as noticed in Dr. Carpenter's Report, 'British and Foreign Medical Review,' vol. xv, p. 273.

Professor Owen[e] is surprised that Messrs. Delpech and Coste were ignorant of Mr. Hunter's claim to the discovery that it is a pale or colourless blood that first circulates in the embryo of a red-blooded animal. Yet this discovery was certainly made as early as 1654, for Glisson[f] knew of it; so did Needham,[g] and some of his successors in the first half of the last century.

When the blood-corpuscle of mammalia is deprived of its colouring

[a] Select Works. tr. by Sam. Hoole, vol. ii, pl. 17, fig. 26-8. 4to, Lond. 1807.

[b] Anatomia Humani Corporis, tab. xxiii, f. 16, Fol. Ams. 1685.

[c] Acta Helvetica, tom. iv, pp. 355-56, 4to, Basiliæ, 1760; tom. v, 1762. p. 348.

[d] See Notes CXXXVII and CXXXIX.

[e] Hunter's Works, vol. iv, p. xiii, 8vo, Lond. 1837.

[f] Anat. Hepat. p. 411, 8vo, Lond. 1654.

[g] De Formato Fœtu, pp. 72-3, 12mo, Lond. 1667.

After this experiment has been made on the blood of such animals as have large vesicles, it may be made on human blood, where the water will be found to have the same effect; the vesicles will become spherical, the diameters of these spheres will be less than the largest diameter of the vesicle in its flat state.

It is remarkable that more water is in general required to produce this change on the vesicles of the human blood than on those of frogs, or other amphibious animals; and those of

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matter by repeated additions of water, the membranous base of the disc is left pale, pellucid, thin, flat, about two thirds as large as the unchanged red corpuscle, and often not visible without the aid of sublimate or iodine. This colourless base has been described by Sir Everard Home,[a] and by Dr. Rees and Mr. Lane,[b] as the nucleus of the red corpuscle of man. When the corpuscle of the oviparous vertebrata is washed with water, the envelope and nucleus both remain, perfectly distinct from each other, the nucleus being the thickest, much the smallest, and very plain. If a layer of the corpuscles of mammalia be quickly dried on glass, preserving their form, and then moistened by the breath, the envelopes will become pale and pellucid; but no nuclei appear. Treat some corpuscles, no matter how minute they may be, of an oviparous vertebrate animal in the same way, and the nuclei will be seen through the envelopes as distinctly as any microscopic object can well be. The shape of the nucleus is described in Note CV; and more particular details on all these points, with illustrative figures, will be found in Wagner's 'Physiology,' translated by Dr. Willis, pp. 239-41; and in the 'Lond. and Edin. Phil. Mag.' Aug. 1842, pp. 107 et seq.

Granular spots, either circular or variable in form and size, often occur in the blood-disc of a mammal in consequence of a puckering of the envelope, shrinking in one part and swelling in another, from a variation of the contents of the corpuscle. Similar appearances, some of which look very like a nucleus, may also be observed by adding dilute muriatic acid or saline solutions to the blood under the microscope: see Note CXI. Some of the deviations from the regular appearance of the corpuscles are noticed in Notes CVI, CX, and CXV; and in the Appendix to the English edition of Gerber's 'Anatomy,' pp. 9-11. Of the contents of the corpuscles, De Blainville[c] observes, "Les globules seraient formés d'une petite vessie pleine d'une fluide analogue au véhicule, et dans lequel se trouverait la matière colorante; et chacun de ces globules en renfermerait d'autres plus petits, quoique de même structure; c'est l'opinion de M. Raspail, qui compare alors les globules du sang à ceux de la fécule de pomme de terre." See the interesting observations of Mr. John Queckett on the blood-discs and their contents.[d]

[a] Phil. Trans. 1818, pp. 173-74.

[b] Guy's Hospital Reports, 1841, vol. vi, p. 383.

[c] Cours de Physiologie gén. et comparée, tom. i, p. 209, 8vo, Paris, 1829.

[d] Microsc. Journ. p. 67, 8vo, Lond. 1841.

the amphibia require still more than those of fish, for the substance of these vesicles being thicker, and more coloured in man and in quadrupeds, than in the amphibia, is therefore later in being dissolved in water; and being thinnest in fish (CIII), it thence most readily dissolves. Those who are desirous of repeating these experiments, had best begin with the blood of toads and frogs, whose vesicles are large, and remain some time without dissolving in the water (when that is used with the above-mentioned precaution); by which means any one accustomed to microscopical experiments may readily be satisfied of these curious circumstances.

From the greater thickness of the vesicles in the human subject, and from their being less transparent when made spherical by the addition of water, and likewise from their being so much smaller than those of fish or frogs, it is more difficult to get a sight of the middle particle, rolling from side to side in the vesicle,[1] which is become round; but with a strong light, and a deep magnifier, I have distinctly seen it in the human subject (CIV), as well as in the frog, toad, and skate.

Since water makes these particles round, and makes the dark spot in their middle disappear, it is evident the red particles of the human blood are not perforated; but that dark spot is owing to something else than a hole; and this is likewise confirmed, by observing that although the particle does, in an obscure glass, appear only to have a dark spot, which might be supposed to have a hole, yet with a very transparent lens, and a good light, after diluting the blood with serum, that middle part can be distinctly seen to be only of a deeper red than the rest of the vesicle, and thence appears darker.

In these experiments, made by adding water to the blood, the middle particles appear to be less easily soluble in water than the flat vesicle which contains them: so that a little time

[1] These experiments were all made with daylight, in clear weather.

(CIII.) The vesicle is certainly much more tender and evanescent in fish than in birds or reptiles, as described in my paper on the blood-corpuscles of British ophidian reptiles, 'Proceedings of the Zoological Society,' 1842, p. 108; but in the two last-named classes the vesicle has generally appeared to me to be stronger and thicker than in mammalia.

(CIV.) See Notes XCVI and CII.

after the proper quantity of water has been added, the flat vesicles disappear, leaving their middle particles, which seem to be globular and very small (CV).

That these red vesicles of the blood, although flat, are not perforated, is evident, likewise, from a curious appearance which I have repeatedly observed in blood that has been kept three days in the summer season, until it was beginning to putrefy; the vesicles of this blood being diluted with serum, and examined with a lens $\frac{1}{50}$ of an inch focus (but more particularly when examined with M. de la Torre's glass, which by his computation magnifies the diameter 1280 times), were found to have become spherical, the diameter of these spheres were less than their largest diameter when flat, and their external surface was corrugated in such a manner as to make them appear like small mulberries (CVI).

I have seen the same appearance on mixing serum (that had been kept three days in a warm place, and smelt putrid) with fresh-drawn human blood; the vesicle assumed this globular and mulberry-like appearance. In these experiments on human

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(CV.) In birds, with some exceptions, the nucleus of the blood-corpuscle, when exposed by acetic acid, is a longer oval than the unchanged envelope. The nucleus of the corpuscle of oviparous vertebrata is insoluble in water or in acetic acid; but when soaked in water the nucleus swells into a globular form, so as closely to resemble a lymph globule,[a] and retains that appearance for many days, as shown in the 'London and Edinburgh Philosophical Magazine' for August, 1842, p. 109, fig. 2. Hence the globular appearance which Hewson notices in the nucleus, was probably caused by the action of water.

(CVI.) Granulated, angular, and jagged forms are very common in the perfectly fresh blood of mammalia, especially in young animals. I have commonly seen such altered corpuscles in the blood of puppies and kittens just killed. These corpuscles are rather smaller than the unchanged discs, and either irregular in shape, granulated throughout and nearly globular, or slightly flattened and indented, often with minute molecules attached. See Appendix to Gerber's 'Anatomy,' pp. 9, 10, 23, 24, 93, 99, fig. 268; where references are given to the observations of Dr. Barry on the spontaneous division of the blood-discs; of Mr. John Queckett on the parent discs throwing from their interior small globular particles; and of Professor Valentin on the minute granules so arranging themselves as to form the envelope of the blood-corpuscle.

[a] See Note CXXII.

blood beginning to putrefy I have likewise observed some of these vesicles break into pieces, without becoming spherical, and I have distinctly perceived the black spot in the centre fissured through its middle, another proof that it is not a perforation.

In the blood of an eel, which was beginning to putrefy, I have seen the vesicle split, and open, and the particle in its centre come out of the fissure (CVII). As the putrefaction advances, these vesicles which had become round spheres, or like mulberries, and those which had been merely fissured, each break down into smaller pieces: M. de la Torre seems to think they have joints, and break regularly into seven parts; and Leeuwenhoek suspected these globules (as he called them) were constantly made of six lesser globules.

But from observations I am convinced there is nothing regular or constant in the number of pieces into which they break. I have seen them fall into six, seven, eight, or more pieces, by putrefaction; for putrefaction breaks them down in the manner it destroys other animal solids. I need hardly take notice, that the small pieces into which the vesicles break are equally red as the vesicle itself. The theory of the red globules being composed of six serous ones, compacted together, and the serous globules of six of lymph, has not the least foundation, and is entirely overthrown by the simple experiment of mixing the blood with six, or thirty-six, times its quantity of water; for the water dissolving the globules, ought to reduce them to

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(CVII.) When the blood-discs of the lower vertebrate animals are drying or dried on glass, and quite fresh, fissures often extend from the circumference of the envelope to the nucleus. In the larger corpuscles, as of the toad and siren, I have counted many such fissures, which, as well as the granular appearance which the corpuscles of mammalia are prone to assume,[a] probably led to Leeuwenhoek's[b] noted fancy, that the corpuscles are formed of six globules, their union constituting the red, and their separation the white part of the blood: this error was well disputed by Senac.[c] De la Torre[d] depicted blood-corpuscles as if cleaving to pieces by fissures.

[a] See Note CVI.

[b] Opera Omnia, tom. iii, pp. 220-21, figs. 4, 6, 4to, Ludg. Bat. 1719; and Select Works, tr. by Samuel Hoole, vol. ii, Part III, p. 239, Plate xvii, figs. 29, 31, 4to, Lond. 1807.

[c] Traité du Cœur, tom. ii, pp. 91, 659, ed. 1749.

[d] Nouve Osservazioni intorno la storia naturale, tab. 4, figs. 3, 4, and tab. 5, fig. 16, 8vo, Napoli, 1763.

yellow serum or colourless lymph;[1] but it does not; on the contrary, it is coloured red by these particles, even when used in much greater proportion than thirty-six parts of water to one of blood.

These red vesicles of the blood have not only been commonly supposed globular and fluid, but they have with equal injustice been imagined to be oily, and more inflammable than the rest of the blood (CVIII). That they are not oily, is evident from their so readily dissolving in water, and that they are not more inflammable than the rest of the blood is manifest, by burning them after they are separated from the rest of the blood; which separation may be effected by shaking the crassamentum in the serum, so as to diffuse the particles through it, and then by pouring off the serum when they have subsided in it. I have separated them in this manner, and compared their inflammability with that of inspissated serum, and of dried coagulable lymph, and have not observed them more inflammable than the serum or the lymph; nor do they melt like oil, as some have suspected, but burn like a piece of horn. Some authors who have written on the figure of these vesicles in quadrupeds and in the human subject, have expatiated on the great advantage of their (supposed) spherical shape, in order for their more easy circulation; as it is probable that no form is preferable to a spherical one for easy motion; but as these vesicles are evidently not spherical, but flat in all animals, we must believe that nature has some good purpose to answer by making them of that form.

It has been objected that, notwithstanding they appear flat out of the body, they may possibly be globular in the body while circulating; and it has been said, that it is almost inconceivable that so many ingenious men should at different

[1] See Gaubii Pathologia.

(CVIII.) Senac and Haller, referred to in Note XCIX, regarded them as solid; and Haller refuted the opinion of their discoverer, Malpighi, that they are particles of oil; but Senac[a] described them as formed of sulphurous matter, burning and putrefying more quickly, and giving out more volatile oil, than the rest of the blood. Gaubius[b] considered them as oily globules, like those of milk.

[a] Traité du Cœur, tom. ii, p. 81, ed. 1749; and tom. ii, p. 275, 2d ed.

[b] Institutiones Pathologiæ Medicinalis, §§ 280, 347, 8vo, Leid. Batav. 1758.

times have viewed them through a microscope, and have concluded them spherical, if they be really flat. But however that may have happened, it is a fact that they are as flat in the body as out of it. Of this I am convinced, by having repeatedly observed them, whilst circulating in the small vessels, between the toes of a frog, both in the solar microscope and the more simple one above mentioned. I have seen them with their sides parallel, like a number of coins laid one against another (CIX).

I have likewise, in that animal where they are elliptical, seen them move with one end foremost, and sometimes with an edge turned towards the eye. I have moreover seen them, when entering a small vessel, strike upon the angle between it and the larger trunk, and turn over with the same variety of phases that they have when turning over upon a piece of glass.

Upon this occasion I may remark that it has been said by some microscopical observers, that in passing through very small vessels they seem to alter their shape, and to be lengthened.

This conclusion I suspect has taken its rise from the observer having seen them with their edge turned towards his eye, in which case they would appear long and small, as if lengthened by compression, especially to one who sets out with the notion of their being globular. I have seen them, in blood-vessels which would admit only single vesicles, move with difficulty, as if straitened for room, but never saw them altered in their shape by the action of the vessels (CX).

(CIX.) When the blood of a mammal is drawn, most of the corpuscles soon run together so as to resemble a roll of coins; only a few of the corpuscles are irregularly connected like a shapeless heap of money. It is in this latter way that the oval corpuscles of the lower vertebrata and of the camelidæ connect themselves together, and not with the curious regularity of the columns of the circular corpuscles of mammalia, mentioned in Note C. Hewson appears to be describing the human blood-corpuscles laid one against another like coins, because he is arguing for the flatness of these corpuscles, which, as he previously states (p. 214), had been long before known of the larger corpuscles of fishes and amphibia.

(CX.) The older observers, as Leeuwenhoek,[a] William Cowper,[b] and Senac,[c] were certainly right in describing the corpuscles as pliant or

[a] Phil. Trans. 1675, x, 380.
[b] Phil. Trans. 1702, xxiii, 1184.
[c] Traité du Cœur, ed. 1749, t. ii, pp. 81, 657; and 2d ed. t. ii, p. 277.

If, then, they really be not globular, but flat, and if water so readily alters their shape, whence is it that the serum has the property of preserving them in that form, which seems so necessary? because it is so general through the animal creation.

It is principally by the salts of the serum that this effect is produced, as is proved by adding a small quantity of any neutral salt to water, when the water is no longer capable of dissolving those particles, nor does it alter their shape when the salt is used in a certain proportion.

EXPERIMENT III.

If a saturated solution of any of the common neutral salts be mixed with fresh blood, and the globules (as they have been called, but which, for the future, I shall call flat vesicles) be then examined in a microscope, the salt will be found to have contracted or shrivelled the vesicles, so that they appear quite solid, the vesicular substance being closely applied all round the central piece. In proportion as the solution of salt is diluted with water, it has less effect, and when diluted with six, eight, ten, or twelve times its quantity of water, it produces no change in the figure of the vesicles, whose flat shape can then be seen, even more distinctly than when mixed with serum itself (CXI).

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flexible, changing their form in passing a narrow channel, and re-assuming their usual contour when they come into a larger space. In mammalia, indeed, the corpuscles are very soft and elastic; when passing a narrow capillary vessel, or impinging on a grosser particle, they may be seen to become suddenly elongated, twisted, bent, or indented; deeply concavo-convex; comma-shaped or spindle-shaped, a part of the corpuscle being drawn into a thread; and after all these rapid changes, the corpuscles as quickly recover their original form. The cup-shaped variety is often seen in blood on the object-plate of the microscope. See the English edition of Gerber's 'Anatomy,' Note, p. 79, and Appendix, pp. 11, 12; and 'London and Edinburgh Philosophical Magazine,' November, 1840, p. 329, for a figure of more permanent forms, chiefly spear-shaped, lunated, and sigmoidal, which the majority of the corpuscles of some mammalia, from an unknown cause, occasionally assume.

(CXI.) When a solution of salt is added to the blood, the corpuscles are made very distinct, for they are separated from each other, and their adhesiveness is quite destroyed: see Notes XXIII and C. They become smaller; some preserve their form, but the majority are less regularly circular, being bent, indented, or cup-shaped; many exhibit oblong, The neutral salts which, when diluted with water, have been observed to have the effects above described, are Glauber's salt, Epsom salt, a salt formed of the volatile alkali and the vitriolic acid, common nitre, cubic nitre, a salt made with the volatile alkali and the nitrous acid, as well as the salts made with the nitrous acid and magnesia, or with the nitrous acid and chalk, and also common salt, digestive salt of Sylvius, and a salt made with vinegar and the fossil alkali. These experiments were sufficient to convince me, that this property was very general among those salts which consist of acid and alkali, and therefore it seemed unnecessary to prosecute this inquiry further.[1] But acids and alkalies have different effects on these vesicles from what neutral salts have.

The fixed vegetable alkali and the volatile alkali were tried in a pretty strong solution, and found to corrugate the vesicles, and in proportion as they were diluted, their effects became similar to water alone; but it is not easy to find the point of strength where the vesicles would remain unaltered in the solution. And here we may observe, that since these vesicles are found to dissolve so readily in water, and not to be dissolved in these solutions of alkali, it is a strong argument against their being oily or saponaceous, as they have been suspected (CXII).

[1] These experiments were made by putting one drop of the saturated solution of the salt into a teacup, and then adding distilled water by a few drops at a time, and to this mixture the serum of the blood highly tinged with the red vesicles was added.

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triangular, or irregular depressions on the surface; in the centre of others, an appearance is presented as of a granular nucleus; many become mulberry-shaped. All these changes may be occasionally seen in pure blood, as noticed in Note CII, p. 223, and in the Appendix to Gerber's 'Anatomy,' p. 9. Urine, when rather strong, produces the same changes; though in certain states it has so little effect on the corpuscles, that it may be used for diluting the blood to observe their shape.

(CXII.) That the corpuscles are not oily or saponaceous globules, requires no proof: see Note CVIII. But I have always found that they disappear, as if dissolved, when treated with liquid ammonia or potash. Senac[a] remarks that the corpuscles, according to Leeuwenhoek, are dissolved by volatile salts. The effect of water and of salts on the cor-

[a] Traité du Cœur, tom. ii. p. 135, ed. 1749.

The effects of acids are very different. I have tried the vitriolic, nitrous, muriatic, distilled vinegar, and the acid of phosphorus; these, when much diluted, have the same effect as water in making the vesicle spherical, and in proportion as they are less diluted, they dissolve the vesicles, without making them spherical as water does. I never could find any point of dilution where the acids, like the neutral salts, produced no change on the figure of the vesicles. This experiment is the more to be attended to, as these vesicles have been supposed to be oily and saponaceous, which is improbable, since they dissolve more readily in acids than in alkalies.

Salts made with earth of alum, and any of the acids, always corrugate those vesicles unless they be much diluted, when their effects are similar to water alone; that is, they make the vesicle assume a spherical shape. I could not discover any point of strength in these solutions, where the particles would remain in them without being changed in their shape.

The same was observed of spirits of wine; some of the metalline salts, as copperas, sublimate and Roman vitriol, were tried, and when much diluted their effects were not different from those of water; but in proportion as the solution was stronger, they corrugated the vesicles more and more.

Urine, when containing much of its salts, has effects similar to the serum, but in proportion as it is weaker, its effects are more like those of water.

The use therefore of those salts which enter into the composition of the blood is probably to preserve the red vesicles in their flat form (CXIII), for we must suppose some advantages attend that shape, since nature has made use of it so generally

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puscles is mentioned in Notes XCIV and CXI; and the different action of alkalies and neutral salts on the red blood-corpuscles and pale primary cells, in Note CXXII, p. 254. The modern names of some of the salts mentioned in the preceding page of the text, are given in the Note at page 12. Mr. Ancell[a] has given a copious table of the effects of various substances on the red corpuscles.

(CXIII.) The effect of the salt of the serum is doubtless, as Hewson discovered, to preserve the flat shape of the blood-corpuscles. The salt also tends to keep them asunder, preventing that increased aggregation of them which takes place in buffy blood; and this may be a cause of the well-known efficacy of salts in inflammation: see Note XXIII.

[a] Lectures on the Blood, Lancet, 1839-40, i, 522.

in the blood of different animals. And as both a very strong solution of neutral salts and a very diluted one alters the shape of the vesicles, it is probable nature has limited the proportions of the water and the salts in our blood.

A degree of latitude in these proportions, however, seems to be admitted, for I observed the vesicles equally unchanged when mixed with a solution of salts, consisting of eight drops of water to one of the saturated solution. And when added to a mixture of fifteen drops of water to one of the same solution, not only the neutral salts in the blood are capable of preventing the serum from dissolving the vesicles, but the mucilage (CXIV), or lymph, with which the serum is so much impregnated, seems to contribute to the same effect.

When the vesicles have been made spherical by being mixed with water, if a small quantity of a pretty strong solution of a neutral salt be added, they are immediately shrivelled; a few of them recover their former flat shape, but the greatest part are contracted irregularly into small spheres. When these vesicles thus recover their shape, after having been a short time mixed with water, they are generally more transparent, and appear thinner, a part of their substance having been dissolved in water, and thence it is more easy to distinguish the little solid particle which is contained in them. By this experiment I have had the pleasure of convincing many curious persons of the composition of this part of the blood, who were not quite satisfied from some of the other experiments.

I have mentioned above, and shown in Plate V, that these vesicles are of different sizes in different animals. I have likewise observed, that they are not all of the same size in the

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(CXIV.) Coagulable lymph was by many of the older writers called mucilage—the *mucago* of Harvey[a], and in that sense the word may have been familiar in Hewson's time. In the First Part of these Experimental Inquiries, pp. 78, 79, and 80, he employs the term mucilage for the coagulable matter of the serum. Synovia was said to be an example of animal mucilage.[b] Verduc[c] states that the coagulation of the blood is owing to a mucilage mixed with the red part.

[a] De Generatione Animalium, pp. 160-1, Exer. 51, 4to, Lond. 1651.

[b] Dictionnaire Raisonné d'Anatomie et de Physiologie, tom. ii, p. 565, and tom. i, p. 724, 8vo, Paris, 1766.

[c] Traité de l'Usage des Parties, 12mo, Paris, 1696, t. i, pp. 184-5.

same animal, some being a little larger than others, and some dissolve in water more readily than others (CXV).

In the same species of animals, they even differ in size in the different periods of life. In a chicken, on the sixth day of incubation, I found them larger than in a full-grown hen, as it is represented in Plate V, fig. 4, and I have found them larger in the blood of a very young viper than in that of its mother, out of whose belly it was taken (CXVI). I have not, however, been convinced from experiments that there is any difference in size between those of a child at its birth and those of an adult person.

In the blood of some insects the vesicles are not red, but white, as may easily be observed in a lobster (which Linnæus calls an insect), one of whose legs being cut off, a quantity of a clear sanies flows from it; this after being some time exposed to the air jellies, but less firmly than the blood of more perfect animals. When it is jellied it is found to have several white filaments; these are principally the vesicles concreted, as I am persuaded from the following experiment.

EXPERIMENT IV.

If one of the legs of a lobster be cut off, and a little of the blood be catched upon a flat glass, and instantly applied to the microscope, it is seen to contain flat vesicles, that are cir-

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(CXV.) Hewson here justly notices the difference in corpuscles of the same blood. They are not all affected in the same manner by acetic acid; neither are they all exactly alike in form; nor as to the presence or absence of the nucleus. If considered as floating glandular cells,[a] they must be constantly changing from the time when they are first formed to that when they finally melt in the plasma.

(CXVI.) Hewson has also correctly depicted the round form of the corpuscles of the embryo of oviparous vertebrata, Plate V, figs. 4 and 7. In the very young embryo of mammals, the corpuscles are much larger than in the adult. At a later period, when the nucleus[b] has generally disappeared, the corpuscles of the fœtus are still larger and more unequal in size than those of the mother; and from the last fact it is difficult to ascertain, at a yet later term of intra-uterine life, whether they be larger or smaller in the fœtus than in the adult. In a kid, twelve days old, bred between an ibex and a goat, I found the corpuscles larger and more variously sized than those of either of its parents. See the Tables of Measurements at the end of this chapter, Note CXVIII*.

[a] See Note CXVIII, p. 235. [b] See Notes CII and CXXXVII.

cular, like those of the common fish (CXVII), and have each of them a lesser particle in their centre, as those of other animals. But there is a curious change produced in their shape by being exposed to the air, for soon after they are received on the glass they are corrugated, or from a flat shape are changed into irregular spheres, as is represented in Plate V, fig. 12. This change takes place so rapidly, that it requires great expedition to apply them to the microscope soon enough to observe it.

I have observed the sanies or blood of a shrimp, by cutting off its tail, and found vesicles in it similar to those of the lobster, which have been a short time exposed to the air. But I never could apply the blood so as to see them in their flat form; but since they change by exposition to the air, I conjecture that like the lobster's they are flat in the blood-vessels, but being more susceptible of changes from the contact of air, they were corrugated before I could get them applied to the microscope.

The ingenious Leeuwenhoek has observed, that in the blood of a grasshopper, its vesicles or globules, as he calls them, are green; I have seen the same circumstance in the white caterpillar, whose serum appeared green when in its vessels, but when let out from this animal, or from a grasshopper, the colour cannot be distinguished.

The smallest animal in which I have discerned these vesicles is an insect no bigger than a pin's head, that is seen almost constantly in the river water which we have in London. This insect, which is a species of the monoculus, being put into a

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(CXVII.) On the blood-corpuscles of fishes, see the observations of Professor Wagner.[a] The majority of the corpuscles in osseous fishes are of a rounded oval; in some species, considering the breadth of the corpuscle as 1, the length would be from $1\frac{1}{2}$ to $1\frac{3}{4}$: the nuclei are, commonly, nearly round, and the envelopes very delicate and evanescent, as stated in Note CIII. Sometimes, as in the pike, the corpuscles are rather angular or pointed at the ends. In the cyclostomes the corpuscles are of the same figure as those of man, and only slightly larger; while in the sharks and skates the corpuscles are much larger, oval, and resembling those of the frog. In Mr. Yarrell's lancelet (amphioxus), mentioned in Note CXXXIII, the blood is said to be colourless, and its corpuscles like those of lymph.

The corpuscles of mammals, birds, and reptiles, are described in Note XCVIII, and measurements of the whole are given in the Tables at the end of this chapter, Note CXVIII*, pp. 237 et seq.

[a] Physiology, tr. by Dr. Willis, i, 238.

concave glass with a little water, and the rays of the sun being made to pass through it, the heart may be seen to beat, and the transparent blood or sanies found to have a few vesicles, which appear to move one after the other; being made visible, though transparent, by the light passing in such a manner as to be refracted by them.

Since so small an animal as this has these curious vesicles equally as the larger and more perfect animals, is is not probable that they are diffused through the whole animal creation? And what is found so generally amongst animals must be of great use in their economy (CXVIII).

As these curious vesicles therefore are so universally found

(CXVIII.) The use of the corpuscles is now generally included in that of primary isolated cells. In Note CXXII, p. 254, I have shown how the red corpuscles differ in chemical properties from perfect and immature pale cells. Dr. Barry[a] concludes that the tissues generally are formed out of the blood-corpuscles, I think without sufficient proof. They have been regarded as the carriers of oxygen; and if, as Dr. Simon[b] observes, the greater part of the carbon exhaled from the lungs in the form of carbonic acid arises from the red corpuscles, the production of animal heat may be connected with this consumption by them of oxygen. Henle,[c] Wagner,[d] Mr. Wharton Jones,[e] and Mr. Newport, consider the red corpuscles as floating glandular cells, their especial office being, according to Mr. Jones, to convert albumen into fibrin: see Notes I and XVIII. Mr. Newport's views are mentioned in Note XCI. Dr. Simon's observations[f] support Mr. Jones's conclusion. Dr. Carpenter[g] objects to it, and believes that the pale corpuscles elaborate the fibrin. Certain it is, that fibrin is formed without the immediate agency of the red corpuscles, and in connexion with a great number of colourless ones, in the lymphatic and lacteal vessels of Mammalia and in the blood of Invertebrata. But to both the opinions of Mr. Jones and Dr. Carpenter a simple and heavy objection is the fact, mentioned in Note XVIII, that fibrin and its fibrils may be produced, merely by mixing together certain varieties of serum, in which, before mixing, no fibrinous particles can be discerned with the aid of the microscope. As to the opinion that fibrin is formed of the central part of the red corpuscles, Dr. Copland[h] adopts the hypothesis of Sir Everard Home, with the refinement that the supposed separation of the envelope proceeds from the

[a] Phil. Trans. 1840, p. 601; and 1841, p. 217.

[b] Animal Chemistry, tr. by Dr. Day, for the Sydenham Society, i, 220.

[c] Anatomie Générale, tr. par Jourdan, tom. i, p. 493.

[d] Physiol. tr. by Dr. Willis, pp. 448-49.

[e] Brit. and For. Med. Rev. xiv, 597; and xviii, 260-1, 263.

[f] Animal Chemistry, tr. for the Sydenham Society, i, 157, 219.

[g] Brit. and For. Med. Rev. xv, 272-73; and xviii, 569.

[h] App. to tr. of Richerand's Physiology, p. 638, 4th ed. 8vo, Lond. 1824; and Dictionary of Practical Medicine, i, 167, 8vo, Lond. 1844.

amongst all animals, and as their structure is in all animals similar, we are naturally led to conclude that parts so complicated cannot be made by a mere mechanical agitation of the chyle in the blood-vessels or lungs, but that nature has set apart certain organs in the body for their formation; and which we shall next inquire into.

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loss of its vitality on leaving the blood-vessels. Dr. Simon's hypothesis,[a] that such substances as urea, uric acid, and bilin, may be products of active changes in the blood-corpuscles, has not been verified.

(CXVIII*.) TABLES *of the Sizes of the Corpuscles of Blood, Lymph, Chyle, and Pus, in Vertebrate Animals.*

All the following measurements were made by myself. They are compiled from papers published elsewhere,[b] containing more details as to the names of the animals, the various sizes of the corpuscles, and other particulars, than can be given here. Such additions and corrections as further examinations have enabled me to make are now introduced. The corpuscles of adult animals only are measured, unless when mention is made to the contrary. The average diameter of the corpuscles of each species is exclusively given; in the blood of any one of them, there are red corpuscles about a third smaller and a third larger than the mean size; and among the pale globules of the blood, chyle, lymph, and fluid of the thymus, still wider variations sometimes occur. If the red corpuscles be obtained with difficulty from a minute prick, they may be smaller, and more irregular in shape, than in blood flowing freely from a larger wound. When the red corpuscles of mammals are rapidly spread very thin and instantly dried on glass, they

[a] Animal Chemistry, tr. for the Syd. Society, i, 160, 163, 219, 8vo, Lond. 1845.

[b] On the Blood-Corpuscles of Marsupiata, of the *Camelidæ*, and of *Moschus Javanicus*, Annals Natural Hist. Dec. 1839, Phil. Mag. same date, and Dublin Med. Press, Nov. 27, 1839; on the Blood-Corpuscles of Mammalia, Lond. and Edin. Phil. Mag. for Jan. Feb. March, and Aug. 1840; on the Blood-Corpuscles of Mammalia and Aves, in an Appendix to the English version of Gerber's Anatomy, 8vo, Lond. 1842; on the Nuclei of the Blood-Corpuscles of Vertebrata, Note to Dr. Willis's tr. of Wagner's Physiology, pp. 240 et seq. 8vo, Lond. 1844, and Lond. and Edin. Phil. Mag. Aug. 1842; on the Pus-like Globules of the Blood, Phil. Mag. Sept. 1842, Note to Wagner's Physiology (on the pale corpuscles of blood, lymph, and pus), pp. 250-52, and Lond. Med. Gazette, Nov. 1, 1839; on the Blood-Corpuscles of the *Camelidæ* and of the Musk Deer, Med. Chir. Trans. vol. xxiii, and Lancet, vol. ii, 1840-41, p. 101; on the Blood-Corpuscles of the Snowy Owl and Passenger Pigeon, Proc. Zool. Soc. June 9, 1840; Corpuscles of *Crocodilidæ*, ibid. Nov. 10, 1840; Corpuscles of Paradoxures, ibid. Nov. 24, 1840; Corpuscles of *Feræ*, ibid. May 25, 1841; Corpuscles of Marsupials, ibid. June 8, 1841; Corpuscles of the Ibex and of Ophidian Reptiles, ibid. Aug. 9, 1842; Corpuscles of *Struthionidæ*, ibid. Oct. 11, 1842; Additional Measurements of Blood-Corpuscles, ibid. Dec. 13, 1842, Feb. 13, and Sept. 10, 1844; Corpuscles of the Stanley Musk Deer, ibid. May 9, 1843; Corpuscles of the Sloth, ibid. June 11, 1844; on the Lymph-Globules of Birds, Phil. Mag. June 1842; on the Structure of Fibrinous Exudations, ibid. Oct. 1842; on the Lymph-Globules of Birds and Mammalia, Note to Wagner's Physiology, pp. 251-2; on the Size of the Blood-Corpuscles of Vertebrata, Proc. Zool. Soc. Oct. 14, 1845; on the Size of the Blood-Corpuscles of Birds, ibid. March 24, 1846.

are commonly slightly larger than when they are slowly dried, or than when they have been swimming for a few minutes either in the liquor sanguinis, in the serum, or in dilute watery solutions of neutral salts. On the contrary, the red corpuscles of birds and reptiles are usually rather smaller in the dried state, however quickly it may have been done, than in the fluid blood. The chyle-corpuscles were obtained from the thoracic duct while digestion was active; and the lymph-corpuscles from the lymphatic glands, except when stated to the contrary.

The measurements are all made in vulgar fractions of an English inch; but, for the sake of convenience, the numerator, being invariably 1, is omitted throughout, and the denominators only are printed. The corpuscles were all measured as they appeared flat on the object-plate, unless when a T indicates a measurement of their thickness. By L. D. their long diameter, and by S. D. their short diameter is denoted.

Remarks on the shape or size of the blood-corpuscles are contained in Notes XCV, XCVIII, CV, CXVI, CXVII; and on the corpuscles of chyle and lymph, in Note CXXII.

Measurements of the Red Corpuscles of the Blood.

MAMMALIA.

Homo	3200	Cebus Apella	3467
T.	12400	capucinus	3454
		Callithrix sciureus	3713
SIMIÆ CATARRHINI.		Jacchus vulgaris	3624
Simia Troglodytes	3412	Midas Rosalia	3510
Pithecus Satyrus	3383		
Hylobates Hoolock	3368	LEMURIDÆ.	
leucogenys	3425	Lemur albifrons	3976
Rafflesii	3539	Catta	3892
Semnopithecus Mona	3515	Anjuanensis	4003
Cercopithecus Maurus	3468	nigrifrons	4440
sabæus	3342	Loris tardigradus	3691
T.	12000	gracilis	3461
fuliginosus	3530		
ruber	3395	CHEIROPTERA.	
pileatus	3578	Vespertilio murinus	4175
pygerythrus	3401	noctula	4404
Petaurista	3478	Pipistrellus	4324
griseo-viridis	3429	Plecotus auritus	4465
Æthiops	3454		
Macacus radiatus	3563	INSECTIVORA.	
Rhesus	3429	Talpa Europæa	4747
niger	3583	Erinaceus Europæus	4085
cynomolgus	3429	Sorex tetragonurus	4571
Silenus	3430		
nemestrinus	3493	PLANTIGRADA.	
sylvanus	3338	Meles vulgaris	3940
melanotus	3389	Arctonyx collaris	3609
Cynocephalus Anubis	3461	Ursus maritimus	3870
leucophæus	3555	Arctos	3723
		Americanus	3693
SIMIÆ PLATYRRHINI.		Americanus, var.	3782
Ateles subpentadactylus	3620	ferox	3530
ater	3602	labiatus	3728
Belzebuth	3589	Helarctos Malayanus	3562

MAMMALIA—(*continued*).

Mellivora Capensis		3824
Procyon lotor		3950
Nasua fusca		3789
rufa		3878
Basaris astuta		4033
Cercoleptes caudivolvulus		4573
CARNIVORA.		
Paradoxurus leucomystax		4236
Bondar		5693
binotatus		4660
Pallasii		5485
Canis familiaris		3542
Dingo		3395
Vulpes		4117
fulvus		3920
argentatus		3888
cinereo-argenteus		3761
lagopus		3888
aureus		3860
	T.	14000
mesomelas		3645
Lupus		3600
Lycaon tricolor		3801
Hyæna vulgaris		3735
crocuta		3820
Herpestes griseus		4662
Javanicus?		4790
Smithii		4466
Viverra Civetta		4274
tigrina		5365
Felis Leo		4322
concolor		4465
unicolor		4481
Tigris		4206
Leopardus		4319
jubata		4220
pardalis		4616
domestica		4404
	T.	16000
Bengalensis		4419
Caracal		4684
cervaria		4220
Serval		4129
Galictis vittata		4175
Mustela Zorilla		4270
Furo		4134
vulgaris		4205
Putorius		4167
Lutra vulgaris		3502
Phoca vitulina		3281
CETACEA.		
Delphinus Phocæna		3829
Balæna Boops		3099
PACHYDERMATA.		
Sus Scrofa		4230
Sus Babyroussa		4316
Dicotyles torquatus		4490
Tapirus Indicus		4000
Elephas Indicus		2745
Rhinoceros Indicus		3765
Equus Caballus		4600
	T.	13422
Asinus		4000
Burchellii		4360
Hemionus		4421
RUMINANTIA.		
Camelus Dromedarius	L.D.	3254
	S.D.	5921
	T.	15337
Bactrianus	L.D.	3123
	S.D.	5876
	T.	15210
Auchenia Vicugna	L.D.	3555
	S.D.	6444
Paco	L.D.	3361
	S.D.	6294
Glama		ibid.
Moschus Javanicus		12325
Stanleyanus		10825
Cervus Wapiti		4138
Hippelaphus		3777
Axis		5088
Dama		4515
Alces		3938
Barbarus		4800
Elaphus		4324
macrourus?		5074
Mexicanus		5175
Marhal		4978
porcinus		5391
Reevesii		6330
Capreolus		5184
Virginianus		5036
Camelopardalis Giraffa		4571
Antilope Cervicapra		5108
Dorcas		4922
	T.	16000
Gnu		4800
Sing-Sing		5150
Philantomba		5116
picta		4875
Bubalis		5600
Capra Caucasica		7045
Hircus		6366
Hircus, var.		6430
Ovis Musmon		5045
Aries		5300
Tragelaphus		6355
Bos Taurus		4267
Taurus, var.		4571
Bison		4062

(MAMMALIA—*continued*).

Bos Bubalus	4586
T.	14000
Caffre	4703
frontalis	4299
Sylhetanus	4222
RODENTIA.	
Pteromys nitidus	3777
volucella	3892
Sciurus vulgaris	4000
niger?	3841
maximus	3633
cinereus	4000
capistratus	3930
Palmarum	3847
Listeri	3948
Arctomys? pruinosus	3484
Empetra	3503
Dipus Ægyptius	4172
Mus giganteus	3892
decumanus	3911
Rattus	3754
musculus	3814
sylvaticus	3839
messorius	4268
Alexandrinus	3900
Arvicola amphibia	3790
riparia	4199
Ondatra Zibethica	3550
Hystrix cristata	3369
Erithizon dorsatum	3380
Synetheris prehensilis	3444
Capromys Fournieri	3483
Myopotamus Coypus	3355
T.	10667
Castor Fiber	3325
Cavia Cobaya	3538
Dasyprocta aurata	3857
Acouchi	3777
Cœlogenys subniger	3481
Hydrochærus Capybara	3190
Lepus cuniculus	3607
Lepus timidus	3560
EDENTATA.	
Bradypus didactylus	2865
Dasypus sex-cinctus	3457
villosus	3315
MARSUPIATA.	
Didelphis Virginiana	3557
T.	12000
Dasyurus viverrinus	4056
Maugei	4034
ursinus	3534
T.	10910
Perameles Lagotis	3902
Hypsiprymnus setosus	4000
Macropus Bennettii	3535
ocydromus	3442
Derbyanus?	3405
T.	10910
Halmaturus Billardieri	3623
Phalangista vulpina	3617
nana	3856
fuliginosa	3688
Petaurista sciureus	3661
Phascolomys Wombat	3456
MONOTREMATA.	
Echidna Histrix	3300

AVES.

RAPACES.	L.D.	S.D.
Gypaëtus barbatus	1913	3425
Cathartes Iota	1880	3691
Sarcorhamphus Gryphus	1761	3892
Papa	1825	3600
Vultur auricularis	1835	3461
Nuclei	4000	10666
fulvus	1829	3399
T. 9600		
Kolbii	1794	3337
leuconotus	1806	3425
Angolensis	1684	3166
Polyborus vulgaris	1829	3572
Buteo vulgaris	1852	3691
Lagopus	1852	3691
Aquila chrysaëtos	1812	3832
Bonelli	1866	3598
Aquila fucosa	1852	3485
choka	1830	3691
Helotarsus typicus	1891	3461
Haliaëtus albicilla	1829	3390
leucocephalus	1909	3390
Aguia	1806	3585
Falco Peregrinus	1916	3862
Tinnunculus	1891	3490
subbuteo	1827	3507
rufipes	2000	3790
Milvus vulgaris	1931	3677
Gypogeranus serpentarius	1722	3301
Surnia Nyctea	1555	4042
Nuclei	3200	10666
Otus brachyotus	1763	4076
vulgaris	1830	3400

AVES—(*continued*).

	L.D.	S.D.
Bubo maximus	1720	3566
Virginianus	1837	4000
Syrnium Aluco	1930	3801
Strix flammea	1882	3740
Nuclei	4000	10666
passerina	1885	3555
Omnivoræ.		
Cracticus hypoleucus	2116	4000
Barita Tibicen	2118	3892
Garrulus pileatus	2041	4167
glandarius	2064	3878
Nuclei	4000	10666
cristatus	2041	3512
Nucifraga Caryocatactes	1875	4172
Corvus corax	1961	4000
frugilegus	1894	3196
Nuclei	4572	9140
monedula	2243	4167
Nuclei	4000	10665
Pica	1953	3365
T. . 11600		
Nuclei	4245	11138
Gracula religiosa	2075	4167
Fregilus graculus	2106	4505
Pastor roseus	2106	4630
cristatellus	2133	4050
tristis	1993	4167
Sturnus vulgaris	2115	3892
Nuclei	3764	11333
predatorius	2133	4175
Coracias garrula	2000	3478
Molothrus sericeus	2133	4567
Buceros Rhinoceros?	1690	3230
Insectivoræ.		
Troglodytes Europæus	2359	4133
Regulus cristatus	2284	4133
Motacilla alba	2182	3600
Nuclei	4000	10666
Sylvia Phragmites	2003	3550
Philomela luscinia	1895	4400
Nuclei	4000	12000
Curruca atricapilla	2359	4133
Erythaca rubecula	2305	4133
Accentor modularis	2342	4000
Turdus viscivorus	2247	4000
musicus	2203	4133
migratorius	2348	4133
canorus	2305	3892
Merula vulgaris	2097	4256
Orpheus polyglottis	2223	3732
rufus	2231	3646
Muscicapa grisola	2179	4173
Lanius excubitor	1989	5325
Vanga destructor	2019	3892

Granivoræ.	L.D.	S.D.
Dolichonyx oryzivorus	2400	4167
Ploceus textor	2213	4575
Cardinalis Dominicana	2140	3643
cucullata	ibid.	ibid.
Amadina fasciata	2001	4364
punctularia	2133	4133
Pyrgita domestica	2140	3500
Nuclei	4364	9200
simplex	2273	4000
Fringilla Cœlebs	2253	4133
Chloris	2232	3600
amandava	2243	4800
cyanea	2144	3741
Linaria minor	2416	4848
Parus cæruleus	2313	4128
caudatus	2136	4570
Nuclei	4800	10666
major	2133	3892
Alauda arvensis	2125	4128
Nuclei	4000	12000
Emberiza citrinella	2286	4000
Nuclei	4000	12000
cristata	2310	4167
Plectrophanes nivalis	2133	4740
Loxia coccothraustes	2042	3790
T. . 9141		
Nuclei	4570	10666
curvirostra	2365	4000
enucleator	2247	4083
Javensis	2286	3677
Astrild	2273	4740
cærulea	2290	3740
Malacca	2359	4167
Vidua paradisæa	1998	3740
Nuclei	3555	10666
Zygodactyli.		
Corythaix Buffonii	1902	3764
Cuculus canorus	2028	3600
Plyctolophus Eos	1981	3728
sulphureus	2203	3399
rosaceus	1842	3547
Nuclei	4000	12000
galeritus	1880	3600
Philippinorum	1974	4041
Macrocercus Aracanga	1902	4041
Illigeri	1924	4335
Ararauna	1961	4128
Macao	1902	4762
severus	2165	3801
Platycercus Pennantii	2106	3931
Pacificus	2118	4174
eximius	2193	3892
flaviventris	2118	3892
Vasa	2045	3892
scapulatus	2000	4042

AVES—(*continued*).

	L.D.	S.D.
Plactycercus niger	2133	3892
Nymphicus Novæ-Hollandiæ	2160	4174
Psittacara leptorhyncha	2067	3931
murina	2133	4031
Patachonica	2115	3977
viridissima	2029	4190
solstitialis	2133	4000
virescens	2097	4175
Trichoglossus capistratus	2203	3892
Palæornis Alexandri	2115	3892
torquatus	2174	3892
Bengalensis	2278	4000
Lorius domicellus	2093	4133
Ceramensis	2115	4000
Amboinensis	2045	4133
coccineus	2165	4000
Sinensis	2115	3692
Tanygnathus macrorhynchus	2106	3829
Psittacus erythacus	1898	4000
albifrons	1931	3692
Augustus	2085	3600
Americanus	2115	3600
Regulus	2037	3764
Dufresnii	2278	3374
Amazonicus	1800	3832
leucocephalus	2050	3727
badiceps	2165	3617
menstruus	2115	3708
melanocephalus	2005	3892
mitratus	2029	3892
Psittacula cana	2101	4174
pullaria	2097	4174
Picus minor	2170	3892
ANISODACTYLI.		
Sitta Europæa	2213	4188
Nuclei	4572	11000
Certhia familiaris	2305	4000
ALCYONES.		
Dacelo gigantea	2110	3555
Alcedo Ispida	2124	3693
CHELIONES.		
Hirundo rustica	2133	4000
urbica	2170	4000
Cypselus Apus	1982	3850
Nuclei	4000	10666
COLUMBÆ.		
Columba Palumbus	1973	3643
risoria	2133	3523
Turtur	2005	3369
tigrina	2088	3615
rufina	2314	3429

	L.D.	S.D.
Columba chalcoptera	2208	4062
Nicobarica	2133	3692
Guinea	2165	3839
Corensis	2193	3643
aurita	2322	3519
montana	2239	3692
Nuclei	5333	12000
Zenaida	2203	3571
migratoria	1909	4626
coronata	1954	3491
leucocephala	2132	3646
mysticea	2100	3512
GALLINÆ.		
Penelope leucolophos	1902	3607
Nuclei	3555	9166
cristata	1902	3607
Crax globicera	1985	3425
rubra	1993	3664
Yarrellii	2000	3456
Ourax Mitu	2005	3490
Pavo cristatus	1835	3589
muticus	ibid.	ibid.
Javanicus	1884	3491
Phasianus pictus	2213	3615
nycthemerus	1887	3470
Nuclei	4000	8000
superbus	2128	3587
lineatus	1855	3348
Nuclei	4570	9166
Colchicus	2168	3646
Nuclei	5647	7111
Gallus domesticus	2102	3466
Nuclei	6000	9140
Meleagris gallopavo	2045	3598
Numida Rendallii	2054	4415
Francolinus vulgaris	2106	4041
Perdix longirostris	2054	3801
Bonhami	1933	3282
Nuclei	4570	10666
Coturnix Argoondah	2347	3470
Ortyx Virginianus	2213	4000
neoxyenus	2305	3836
Tetrao urogallus	2248	3836
Tetrix	2376	3728
Caucasica	1923	3456
Nuclei	4570	9166
Tinamus rufescens	1752	3338
ALECTORIDES.		
Dicholophus cristatus	1884	3364
CURSORES.		
Struthio Camelus	1649	3000
T. 9166		
Nuclei	3200	9166

AVES—(*continued*).

	L.D.	S.D.		L.D.	S.D.
Casuarius Javanicus	1455	2800	Rallus Philippinensis	2097	3389
Dromaius Novæ-Hollandiæ	1690	3031	Gallinula chloropus	2055	3839
Rhea Americana	1898	3273	PINNATIPEDES.		
			Podiceps minor	2001	3200
GRALLATORES.					
			PALMIPEDES.		
Œdicnemus crepitans	2157	4000	Plectropterus Gambensis	1866	3728
Vanellus cristatus	1964	3310	Chenalopex Ægyptiaca	1866	3839
Hæmatopus Ostralegus	1895	4000	Cereopsis Novæ-Hollandiæ	1722	3692
Nuclei	3200	9000			
Psophia crepitans	1883	3488			
Anthropoides Virgo	1884	3740	Bernicla Sandvicensis	1866	3839
T. 11230			Magellanica	ibid.	ibid.
Stanleyanus	1909	3529	Cygnus atratus	1806	3692
Balearica pavonina	1859	3777	Dendrocygna viduata	1789	3555
T. 9597			autumnalis	1916	3764
Nuclei	4000	9750	arborea	1931	3724
Regulorum	1858	3478	Dendronessa sponsa	2001	4079
Ardea cinerea	1913	3491	Tadorna vulpanser	1925	3839
Nycticorax	1780	3555	Mareca Penelope	1873	4385
minuta	1993	3827	Querquedula crecca	2062	4592
Platalea leucorodia	1859	3600	acuta	1993	3839
Ciconia alba	1755	3439	circia	2088	3839
nigra	1806	3403	Anas galericulata	1937	3424
Argala	1728	3555	Larus ridibundus	2097	4000
Marabou	1859	3460	canus	1973	3839
Ibis ruber	1948	3153	Nuclei	3555	10666
Numenius Phæopus	1846	4465	Pelecanus Onocrotalus	1777	3369
Limosa melanura	1973	3754	Nuclei	3200	9600
Scolopax Gallinago	2170	3622	Phalacrocorax Carbo	2005	3765

REPTILIA.

	L.D.	S.D.		L.D.	S.D.
Chelonia Mydas	1231	1882	Lacerta viridis	1555	2743
Nuclei	4000	6000	Anguis fragilis	1178	2666
Testudo Græca	1252	2216	Natrix torquata	1371	2157
radiata	1241	2197	T. 8341		
Alligator ——?	1324	2122	Nuclei	3835	6817
Crocodilus acutus	1231	2286	Coluber Berus	1274	1800
T. 8000			Nuclei	3227	4986
Lucius?	1124	2215	Python Tigris	1440	2400
Champsa fissipes	1259	2315	Nuclei	3555	7468
Iguana Cyclura	1230	2285			
Nuclei	5333	6400			

AMPHIBIA.

	L.D.	S.D.		L.D.	S.D.
Rana temporaria	1108	1821	Triton Bibronii	848	1311
T. 7112			Nuclei	1901	3000
Nuclei	3114	6297	cristatus	848	1311
Bufo vulgaris	1043	2000	Lissotriton punctatus	814	1246
T. 5625			Nuclei	1778	2667
Nuclei	2802	5261	Siren lacertina	420	760
			Nuclei	1142	2007

PISCES.

	L.D.	S.D.		L.D.	S.D.
Perca fluviatilis	2099	2824	Cyprinus auratus . . .	1777	2824
T. 8000			T. 10666		
Nuclei . .	7482	8830	Nuclei . .	4570	8000
Cernua	2461	3000	Erythrophthalmus .	2000	3200
Nuclei . .	6000	8000	Phoxinus	2000	2900
Cottus Gobio	2000	2900	Esox Lucius	2000	3555
T. 8000			Nuclei . .	5333	8000
Cyprinus Carpio . . .	2142	3429	Anguilla vulgaris . . .	1745	2842
T. 8000			T. 8000		
Nuclei . .	6400	8000	Nuclei . .	7500	10000
Tinca	2286	2722	Gymnotus electricus . .	1745	2599
T. 8830					
Nuclei . .	8500	9600			

Measurements of the Red Blood-Corpuscles of Fœtal or Immature Animals.

Human, still-born, at fifth month of utero-gestation	2800	Fallow Deer, 5½ inches long . .	3478
Ditto, at the same period . . .	3472	Kid, 12 days old, bred between an ibex and goat	5918
Kitten, ½ inch long	2233	Tadpole, ½ inch long . . L.D.	1098
Nuclei .	4600	S.D.	1650
Fallow Deer, 4½ inches long . .	3478		

End of the Measurements of the Red Corpuscles of the Blood.

Measurements of Pale Corpuscles of the Blood.

Homo	3000	Vultur auricularis	3555
Pithecus Satyrus	2800	Corvus Pica	3555
Cercopithecus Sabæus	2900	Pyrgita domestica	3800
Helarctos Malayanus	3000	Plyctolophus rosaceus	3000
Nasua rufa	2700	Columba montana	3200
Herpestes griseus	3878	Gallus domesticus	3800
Felis Caracal	3100	Struthio Camelus	3329
Serval	3200	Casuarius Javanicus	2600
Equus Caballus	3200	Ciconia Argala	2666
Camelus Bactrianus	3348	Anguis fragilis	2628
Moschus Javanicus	3347	Natrix torquata	2322
Capra Hircus	3232	Coluber Berus	2666
Caucasica	3200	Rana temporaria	2664
Bos Taurus	3000	Triton Bibronii	2000
Perameles Lagotis	3000	palustris	ibid.

Measurements of the Corpuscles of the Chyle, of the Lymph, and of the Thymus-Fluid.

Chyle.	
Man	4600
Lynx	ibid.
Dog	ibid.
Horse	ibid.
Lymph.	
Man, from a lymphatic vessel of the groin	ibid.
Man, from a gland in the groin	ibid.
Moschus Javanicus	4725
Strix flammea	5227
Garrulus glandarius	4414
Corvus frugilegus	5053
Corvus monedula	5238
Pica	5001
Sturnus vulgaris	5152
Turdus musicus	5090
Pyrgita domestica	4682
Fringilla Chloris	4924
Emberiza Citrinilla	4572
Columba Livia	5274
Gallus domesticus	5261
Ardea cinerea	5150
Ditto, from a lymphatic vessel of the neck	ibid.
Rana temporaria, from the subcutaneous lymph	2725
Thymus-Fluid.	
Child	4572
Cat	ibid.
Calf	ibid.
Dromedary	ibid.

Measurements of the Globules of Pus.

Man, from a boil	2664
Pus globules in the human blood	2666
Ditto, in the blood of the horse	ibid.
From a subcutaneous abscess in the horse	2900
From a sero-purulent fluid in the pleura of the horse	2962
From a seton in a cat's neck	3200
From an abscess in a rabbit's leg	3000
From a seton in the neck of the Vicugna	3200
Ditto, in the Paco	ibid.

CHAPTER II.

ON THE STRUCTURE OF THE LYMPHATIC GLANDS.

These glands, which by anatomists are called the lymphatic glands, and vulgarly known by the name of waxen kernels, since the discovery of the lymphatic vessels by Rudbeck and Bartholine (anno 1651-2), have been properly considered as appendages to the lymphatic system, and though many excellent anatomists since that period have employed themselves with great assiduity, and made many curious experiments with a view to complete the discovery of this important subject, yet the lymphatic glands seem to have derived fewer advantages from this spirit of inquiry than the other parts of the system; their structure and office being unknown, except as far as relates to their gross anatomy, and even in that respect the best anatomists are not well agreed. Some suppose that each lymphatic gland is composed of large cells, and others that they are formed of convoluted lymphatic vessels (see Note cxix*), and some, that the red veins communicate with the lymphatic vessels in the substance of the gland. But in order to avoid perplexing a subject of itself too intricate, we shall pass unnoticed the various opinions advanced by different authors who have written on this subject, and endeavour to describe the structure and uses of these glands, from the ideas and experiments of the ingenious author of the subsequent discoveries; and as the anatomy of many parts is to be maturely considered, before we can reason on the functions of any one of them (from the mutual dependence they have on each other), we shall first describe their structure, and afterwards inquire into their use.

Sect. 1. A lymphatic gland, in size and shape, is commonly compared to a small acorn; though I think it more generally resembles the figure of a kidney-bean, being oblong, rounded at the extremities, and a little flattened on the upper and under sides, particularly in the uninjected state. But independent of any change produced by injection, we observe great variety in the size and figure of the lymphatic glands in different bodies, and in different parts of the same body. In some they are large and round, in others small and flat, and in every cluster of these glands this difference in shape and size is observable. Vide Plate VI.

In females they are generally smaller than in males, and are always considerably less in proportion before the time of puberty than after it (CXIX).

SECT. 2 These glands are dispersed in the course of the lymphatic vessels, in different parts of the body, and through them the lymphatic vessels pass. Their various situations have been already so accurately delineated by Mr. Hewson,[1] that a repetition would be tedious and unnecessary.

[1] Hewson on the Lymphatic System, Plates I-IV.

(CXIX.) Morgagni, Ruysch, and Haller, according to Mr. Cruikshank,[a] observed that the lymphatic glands are enlarged in the young animal in proportion to the growth of the body, that they diminish after middle life, and at last entirely vanish. Bichat[b] states that they are only very remarkable in infancy, becoming less numerous in the adult, and mostly disappearing in old age. I have generally found them proportionably larger in children than in adults, and in the necks of young birds than in their parents; in old age the lymphatic glands are more or less wasted, and deficient in juice, but they never entirely disappear, and are larger in healthy aged people than in younger subjects wasted by disease. I have seen the glands very distinctly in the neck and groin of a woman aged 95, and of a man aged 87; and Dr. Boyd, who undertook, at my request, to make a few observations on the subject, has kindly furnished me with the following results, obtained by weighing the glands of twenty human bodies, dead from various diseases, from birth to 90 years of age:—"In adults the lymphatic glands were largest, increasing up to 40 years of age, smaller at 46, and tough and fibrous in the very old. The inguinal glands were larger and more numerous in males than in females. The inguinal and femoral glands were smaller in females aged 50 years and upwards, and emaciated by chronic diseases, than in a female aged 90, whose body was in better condition. In one male, still-born, weighing six pounds, the glands were larger in proportion to the body, than in any other case; and in a male infant weighing ten pounds, aged eight months, and emaciated by disease, the glands were smaller than in the still-born infant." The thymus gland, as mentioned in Note CXXVII, increases in size for some time after birth, but soon becomes smaller in ill-nourished young animals.

[a] Anatomy of the Absorbing Vessels, p. 71, 4to, Lond. 1786.

[b] Anatomie Descriptive, tom. iv, p. 431, 8vo, Paris, 1803.

Sect. 3. Each gland is a congeries of tubes, consisting of arteries, veins, lymphatic vessels, and nerves, connected by the cellular substance, the whole forming a circumscribed apparatus for the purpose of secretion (see Note cxix*). The cellular substance surrounding the gland is a little condensed, and forms what has been called, but improperly, its capsula (see Note cxix*). For though we consider the cellular membrane to be the common connecting medium of all the parts of the body, yet we cannot allow of its forming a peculiar coat to any part, since it cannot be separated from those lymphatic glands, without destroying the texture of them.

Sect. 4. The branches from the aorta supply the lymphatic glands with blood, in common with all other parts of the body. In general two or three small branches enter each of these glands at different parts, and these branches ramify to exceeding minuteness (see Note cxix*) throughout the whole gland, and their corresponding veins return the blood into the adjacent venous trunks.

Sect. 5. The nerves in their course give off extremely small twigs to the lymphatic glands, but not modified to convey an acute sensation; for these glands, unless in a state of inflammation, are very little sensible.

Sect. 6. Besides arteries, veins, and nerves, every one of these glands has a number of small lymphatic vessels. The glands are nowhere to be found but in the course of the larger lymphatic vessels, which, in their passage from the extreme parts of the body towards the thoracic duct, enter and pass through the lymphatic glands in the following manner.

Sect. 7. About a quarter of an inch before a lymphatic enters a gland, it divides into two, three, or four smaller branches, sometimes into a greater number. These enter the gland at the part farthest from the thoracic duct, and are then subdivided into branches, as small as the ramifications of the arteries and veins before described, and which they accompany to every part of the gland. After being thus minutely divided, they reunite and gradually become larger, as they approach the opposite side of the gland, forming three or four branches, which are joined by other lymphatics, that arise from the cells of the gland. All these branches unite together, about a quarter of an inch from that part where they come out of the gland, and form a common trunk, but larger by the additional lymphatic vessels it receives from the cells of the gland. See Plate VI, fig. 3.

Sect. 8. Sometimes only one lymphatic vessel, and sometimes three or four of them, pass through the same gland, in the manner described above, and these either pass through other glands in the same way, or continue their course on to the thoracic duct. Vide Plate VI, fig. 1. Sometimes we observe a single lymphatic pass by all the glands, without entering any one of them, and continue on to the thoracic duct. This observation may perhaps account for the venereal, variolous, or other poisons, being sometimes taken into the habit, without inflaming a lymphatic gland in its passage. See page 129.

Sect. 9. In Sect. 3 we found that each lymphatic gland is a congeries of vessels, and in Sect. 7 we observed that some lymphatic vessels arose from the cells of the gland; but here we do not mean those appearances which have been called cellular[1] (and that are in reality only little eminences, formed by the bending of one vessel round another), but other cells which really do exist in the substance of the gland, and are so very small as to become visible only by the assistance of the microscope.

If we inject[2] a lymphatic gland with mercury, or inflate it with air, an irregular appearance is produced, very much resembling cells; and if a gland prepared in this manner is dried and cut through, at first sight it looks like a honeycomb, but if we examine it more attentively, we shall find this cellular appearance evidently made of convoluted vessels; and in by far the greater part of lymphatic glands, that we prepare, the subdivision of the lymphatic vessels into smaller and smaller branches, and not into cells, is apparent to the naked eye. Vide Plate VI, figs. 3, and 4.

Sect. 10. The cellular appearance in the lymphatic gland is, I think, probably a deception, which may happen in the following way. The very small lymphatic vessels are very much convoluted, and running in a serpentine direction one over the other, a part of one vessel is covered by that part of

[1] M. de Haller, Elem. Physiolog. tom. i, p. 183.
[2] Vide Plate VI, fig. 3.

another vessel which lies over it. This being general, an irregular surface is produced, so that it looks like a number of small globules or very small pins' heads (vide Plate VI, fig. 1), similar to what we observe in the epididymis when injected with mercury; and that this is really the case I am convinced, from what I have observed on the examination of other lymphatic glands, when the vessels have been less convoluted, and where the vascular texture has been more evident; for in some we can distinctly trace the continuity of vessels through the glands, especially in the more simple ones.

Sect. 11. Plate VI, fig. 2, exhibits a lymphatic vessel A, forming three lymphatic glands: the first at B, is a subdivision of the trunk A into about nine or ten branches, which are convoluted, and form a gland. These unite again and form the trunk at C, which ascending about three inches, divides into about six branches running parallel to each other, and then form a lymphatic gland at D, not larger than the trunk of the vessel, which is the most simple I have ever seen; the branches are again united to form the trunk E; this vessel next ascends about three inches further, and divides into two branches F and G; the branch G is continued on where it is joined with other lymphatic vessels. The branch F is again divided into four vessels, that are subdivided to form the third gland at H, evidently composed of small convoluted lymphatic vessels; these vessels are again united and form the large lymphatic vessel at I, which passes on to communicate with some other lymphatic vessels not expressed in this Plate. All the objects in the Plate are smaller than the natural size.

Sect. 12. This circumstance of the lymphatic glands being formed of convoluted lymphatic vessels is demonstrable not only in this preparation, but also in many of the larger and complete glands, particularly in Plate VI, figs. 3 and 4; and though we have not so clear a demonstration that every lymphatic gland is a convolution of lymphatic vessels, or that some glands may not have large cells, yet we have this useful fact, that a lymphatic gland is not always or necessarily composed of cells, and as we can prove that some, nay, many glands are only convoluted lymphatic vessels, and as the cellular structure in any is rather doubtful, it is probable that all lymphatic glands are formed of convoluted lymphatic vessels, and that the appearance of large cells (CXIX*) may be a deception, from the circumstance I have already mentioned; and I am the more confirmed in my opinion that this is the case, on taking a more general survey of this subject in the animal kingdom, as in the turtle, where the lymphatic glands are wanting, or at least that circumscribed form of a lymphatic gland, so general in the more perfect animals, is not to be found in those of the amphibious class.

SECT. 13. In the mesentery of a turtle no lymphatic glands are observable; yet in this animal nature does her business as well, though the apparatus is differently constructed. The small blood-vessels in the mesentery of this animal are transparent in an unprepared state, and the large vessels form a network, making seemingly pretty considerable meshes, but if we inject them (i. e. the veins and arteries) we find this part exceedingly vascular, and from its transparency we can here prove the artery terminating in the vein by continuity of canal (a fact not easy to demonstrate in the human body); and if we inject the lymphatic vessels, we find them very numerous, forming the most beautiful network imaginable.

The lacteals come from the edge of the intestines upon the mesentery; part of them ascend, surrounding the blood-vessels, but do not communicate with them. These send off lateral branches to the transparent part of the mesentery, whilst others come immediately from the intestines to it, where they divide to exceeding minuteness, making frequent anastomoses, and gradually becoming larger as they approach the upper angle, where they communicate with the larger branches, and pass on to the thoracic duct. Vide Plate VII.

SECT. 14. The arteries and veins are principally spread on

(CXIX*.) Mr. Abernethy[a] found the mesenteric glands of a whale hollow in the centre, like a bag. Mr. John Goodsir[b] concludes that the lymphatic glands are merely networks of lymphatic vessels, deprived of all their tunics but the internal, the epithelium of which is highly developed for the performance of particular functions; and that these peculiar lymphatics have a fine network of capillary blood-vessels to supply matter for the continual renovation of the epithelium. He states that the outer tunic of the extra-glandular lymphatics leaves them at their entrance to or exit from the gland, and passes on its surface to form its capsule.

[a] Philosophical Transactions, 1796, pp. 27 et seq.
[b] Anatomical and Pathol. Observations, pp. 45, 49, 8vo, Edin. 1845.

the coats of the lymphatic vessels, so that we here find the requisites to form a lymphatic gland; for as we prove that many of the lymphatic glands in the human body are no more than a congeries of arteries, veins, nerves, and lymphatic vessels convoluted, it is probable that all lymphatic glands may be formed in the same manner; so, perhaps, it may be the same thing in nature, or the same purposes of the animal economy may be equally well answered, whether the parts composing a gland (viz. arteries, veins, nerves, and lymphatic vessels) be circumscribed in a proper membrane, or spread over a larger surface. This, perhaps, will be more fully proved by some experiments and observations which I shall hereafter publish on the minute structure of glands (cxx).

Sect. 15. On cutting into a fresh lymphatic gland we find it contains a thickish, white, milky fluid. Then if we carefully wipe or wash this fluid from any part of the cut surface, and examine it attentively in the microscope, we observe an almost infinite number of small cells, not such as have been before described, or that have been supposed to exist in the lymphatic glands, but others too small to become visible to the naked eye, and expressed in Plate VIII, fig. 4.

Sect. 16. If the arteries and veins of a lymphatic gland have been previously injected with a coloured fluid, and a part of the gland be then viewed through the microscope, we observe these cells are extremely vascular, and it is into these cells that the white fluid found in the gland is secreted. This fluid is absorbed by the lymphatic vessels, which we observed (Sect. 7) arose from the cells of the gland, and is by them, in common with the other fluids, carried into the course of the circulation.

Sect. 17. The lymphatic vessels, therefore, which originate from the cells of the gland, are in the lymphatic glands analogous to the excretory ducts of other glands; and we have the

(cxx.) I apprehend Mr. Falconar did not live to fulfil this promise, although he devoted some space to the subject in the 8vo Synopsis of his Lectures, dated from Craven street, Strand, London, Sept. 26, 1777. He must have died within a month afterwards; for the sale catalogue of "The Museum of the late Magnus Falconar, Surgeon and Professor of Anatomy . . . . formed by the joint labour of these two young anatomists,"—Hewson and Falconar—is dated on the following 12th of October. Of this catalogue there is a copy in the library at the Royal College of Surgeons, in Lincoln's Inn Fields.

same proofs that the lymphatic glands secrete this white fluid, and that it is carried from the glands by the lymphatic vessels, that we have of glands in other parts of the body separating different fluids, and having excretory ducts; for if we cut into a lymphatic gland we find a white fluid, and if a ligature be made on the lymphatic vessel coming from that gland we find a fluid of the same kind contained in those lymphatic vessels (CXXI). This is as convincing a proof of the lymphatic vessels being excretory ducts to the lymphatic glands, and as satisfactory as that the hepatic duct is the excretory duct of the liver. We know the liver secretes bile because we find it in that viscus, and we know the ductus hepaticus is its excretory duct, because we find bile contained in it. The proofs are similar and equally conclusive.

SECT. 18. The existence of a white, thick, mucus-like fluid in the lymphatic glands has been long generally known to anatomists, and is particularly remarked by M. de Haller;[1] but the properties of this fluid seem to have been entirely overlooked and neglected.

This may perhaps have been owing to the same cause that the shape of the particles of the blood, till lately, has been so little known, viz. the want of diluting this liquor; for if we examine this fluid of the lymphatic glands in the natural state, we find it a homogeneous mass, discovering nothing of its composition or properties. But if we dilute it with a solution of Glauber's salts in water, or with the serum of the blood, and view it with a lens of the $\frac{1}{23}$ of an inch focus, as formerly mentioned in the experiments on the blood, we then observe the following appearance.

[1] Succum glandulis conglobatis inesse, album, serosum lacte tenuiorem, in juniori potissimum animali conspicuum, id quidem certum est. Eum, cremori similem dixit Thomas Wharton, cinereum Malpighius, diaphanum Nuckius, album Morgagnus, recte et ad naturam, ut puto, omnes.—Haller, tom. i, p. 184.

(CXXI.) I have never seen the fluid of the lymphatic vessels white or opake, except in the lacteals and in the absorbent ducts of the thymus. The juice of the lymphatic glands may be slightly so, in consequence of containing a greater proportion of corpuscles than the fluid either of the afferent or efferent lymphatics,[a] and perhaps when a ligature is placed on one of these last, its contents may become more opake from an accumulation of globules.

[a] Note CXLII.

Sect. 19. Numberless small, white, solid particles, resembling in size and shape those central particles (see Notes cii, cv, and cxxii,) found in the vesicles of the blood, are to be seen distinctly gliding down on the stage of the microscope, and if we dilute it sufficiently, we can examine them separately, and view them as distinctly as we can the particles of the blood.

Sect. 20. These particles found in the lymphatic glands likewise agree remarkably in their properties with the central particles found in the vesicles of the blood, not only as to size and shape, but also in being insoluble in serum, or a solution of any of the neutral salts in water, (except putrefaction takes place,) and are, like the blood, soluble in water, and in the same order (cxxii). These particles are by the lymphatic

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(cxxii.) As to the solubility in water of the nucleus of the blood corpuscle, Falconar speaks more decidedly than Hewson, who states[a] that it is less soluble than the envelope. The truth is, that the nucleus, like the globules of chyle and lymph, is insoluble in water. The partial effect of water on the envelope is described in Notes xciv and cii.

The globules of lymph and chyle, as well as those in the fluid of the thymus, appear delicately granulated on the surface; they are generally globular or lenticular, never following the wide peculiarities in shape and size of the blood-discs of different classes of animals, nor in birds at all approaching the long oval figure of the nucleus of the red blood-corpuscle, described in Note cv.

In the Napu musk-deer the lymph-globules are about as large as those of mammals with blood-discs of the ordinary size; and in the Camelidæ, as noticed more fully in the Appendix to Gerber's 'Anatomy,' p. 99, figs. 286, 287, and in the 23d vol. of the 'Med. Chir. Transactions,' pp. 25 et seq., the corpuscles of the fluid of the thymus and of the lymphatic glands, as well as the pale globules in the blood and pus, have all the same round shape as in mammals with circular blood-discs; and so have the globules of the lymphatic glands of birds, as well as the pale globules of the blood of birds and reptiles. Figures of all these globules are given in my Note to Dr. Willis's translation of Wagner's 'Physiology,' pp. 250-52, and to the English edit. of Gerber's 'Anatomy,' figs. 277-87, and 292; and in the 'London and Edinburgh Philosophical Magazine,' June, 1842, pp. 480-84. Measurements of the globules will be found in the Tables at the end of chap. i, pp. 243-4.

The globules of the chyle, of the thymus-fluid, and of lymph, are smaller and differ in structure from the pale globules of the blood. In these last there are two, three, or four nuclei, easily seen when the envelope is made more or less transparent or invisible by acetic, sulphurous, citric, or tartaric acid. But the globules of chyle, of lymph, and of the thymus fluid, like the nuclei[b] of the red corpuscles of the

[a] Chapter I, pp. 224-5. [b] See Notes cv, cii.

vessels taken into the course of the circulation, and mixed with the blood, where they are for a time retained, to be again separated from it, as we shall see afterwards in our inquiry into the anatomy of some other parts.

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blood, are only rendered more distinct and slightly smaller by any of these acids, so that the central part presents no regular nuclei, or divided nucleus, such as are contained in the pale globules of the blood. In short, these last-named globules have the characters of perfect elementary cells, while the former globules, as shown in the Note to Gerber's 'Anatomy,' p. 83, resemble, and probably are, nuclei or immature cells.

The most recent observers, Dr. Oesterlen,[a] and Mr. Simon,[b] also conclude that the corpuscles of the thymus have the structure and relations of nuclei; and it has been noticed in the Introduction how sagaciously Hewson always insisted that they are to be considered as central particles.

All these observations apply to the majority of the globules mentioned. In the blood, besides the common pale cells, there are a few smaller corpuscles, like those of lymph; while in the larger lymphatics and thoracic duct there are corpuscles identical in size and structure with the common pale globules of the blood: see Note CXLVI.

The microscopical and chemical characters of the globules of the chyle, of the thymus, and of the lymphatic glands, are nearly, if not exactly, the same. When quite fresh, they swell on being mingled with pure water, as does the nucleus of the blood-corpuscle.[c] When well mixed with a strong solution of an alkali, or of a neutral salt, the globules become partially dissolved, misshapen or fainter, forming a very ropy and tenacious compound with the fluid. This property, I believe, belongs generally to fresh primary cells, though not to the red corpuscles of the blood. Further details concerning the chyle, lymph, and fluid of the thymus, are given in the Appendix to Gerber's 'Anatomy,' pp. 88-100.

In reptiles, I have seen the envelopes forming around the pale globules of the blood, as described by Wagner and Nasse.[d] But in birds and mammalia there are certain facts not agreeing with the opinion that the red corpuscles are formed simply by a transformation of the globules of lymph or chyle. In birds, it has already been noticed[e] that the lymph-globule differs remarkably in figure from the nucleus of the red blood-corpuscle; while there is nothing like a lymph-globule within this blood-corpuscle of mammals. Besides, in some of the smaller ruminants, as the musk-deer and ibex, the red corpuscles are much smaller than the lymph-globules.

[a] Mr. Paget's Report, British and Foreign Medical Review, vol. xxi, p. 566.

[b] A Physiological Essay on the Thymus Gland, p. 80, 4to, Lond. 1845.

[c] See Note CV, p. 225.

[d] See the English edition of Gerber's Anatomy, fig. 294.

[e] In Note CV, p. 225.

CHAPTER III.

ON THE SITUATION AND STRUCTURE OF THE THYMUS GLAND.

SECT. 21. THE term gland has been given to certain parts of an animal body, that are by nature destined to separate fluids of different properties from the general mass of blood, which are to be applied to the various purposes of the animal economy, or to be excreted, as being either useless or hurtful to the constitution.

SECT. 22. But though the term gland is properly given only to such parts as are known to perform this office, yet it has also been applied to some parts whose uses are unknown; because their structure being apparently the same with that of glands, (properly so called,) it has thence been conjectured that their uses might likewise be similar. Thus the thymus has acquired the name of gland. In like manner we use the terms thyroid glands and glandulæ renales (CXXIII). For whenever any part receives more blood than is necessary for the immediate growth or nourishment of that part, it is concluded *a priori* that this blood is to undergo some change, or that some secretion is to be made from it; and for these reasons the appellation of gland has been given to the thymus, the thyroid, and glandulæ renales.

SECT. 23. The thymus is situated in the superior part of the chest, in that space called the anterior mediastinum; which in the fœtus-state, and for a few years after birth, is large. The shape and size of this gland is various, differing in almost every subject. It is triangular, adapted to the space between

(CXXIII.) As to the probable use of the supra-renal glands, and the nature of their secretion, see 'London Medical Gazette,' June 21, 1844, p. 411. A description, with figures, of the elementary molecules so abundantly produced in these glands, is given in the Appendix to the English edition of Gerber's 'Anatomy,' p. 103; and a notice of the constitution of similar molecules in Note LX, p. 88.

the right and left lobe of the lungs. The superior part of the gland seldom rises higher than the upper edge of the first bone of the sternum. Sometimes one and sometimes two processes of the same glandular structure arise from the upper part of the thymus, and ascend on the fore-part of the neck almost as high as the glandula thyroidea, and lie between the trachea and carotid arteries;[1] but this is a circumstance that rarely occurs. The two sides of the gland are placed next the lungs, and the inferior part or basis of the triangle extends downwards, (sometimes much lower than is expressed in the Plate,) lying on the upper and outer part of the pericardium, to which it is attached by the reticular substance. The superior and posterior part of the thymus lies on that part of the aorta called sinus aortæ, which arises from the left ventricle of the heart to form the curvature, the fore-part of which and the common trunk of the right carotid and subclavian arteries it generally embraces. The inferior posterior part is always connected with the upper part of the pericardium.

Sect. 24. The thymus receives two small arteries, called arteriæ thymicæ, which most commonly originate from the sinus aortæ; sometimes they arise from the curvature of the aorta, and I have seen some instances of one small artery coming from the common trunk of the right carotid and subclavian arteries to the upper part of this gland. Besides these, the thymus receives several small arterial branches on its fore-part, from the mammary and other arteries, that supply the mediastinal space with blood.

Sect. 25. The veins are always more regular in their termination, and are generally two in number. They open into the trunk of the jugular and subclavian veins on the left side, of which a part is covered by the superior portion of the thymus.

Sect. 26. The thymus is a gland of the conglomerate kind, and, like others of that class, has no particular centre of ramification for its blood-vessels, but they enter promiscuously at different parts of it. It is formed of a great number of small lobules, or acini, united to each other by blood-vessels and the tela cellulosa; and over the whole, giving it greater compactness, is a condensed cellular substance, which forms a kind of

[1] Hall. tom. i, p. 115.

capsule to the gland, and gives it a pretty regular smooth external surface, except where it is fissured into larger lobes. But this capsule is not of that kind we find on some other glands, as on the kidneys, &c., which can be readily separated from them. It is nothing more than a coarser condensed reticular substance adhering firmly to all parts of it; from which it cannot be separated, without doing injury to the glandular substance (CXXIV).

SECT. 27. Many attempts have been made, by dissection and other means, to discover an excretory duct from this part. For the organization being apparently the same in it as in some other known glands, it was but natural to conclude that, similar to them, it also should have an outlet. Accordingly, many fruitless experiments have been made, and much time employed to discover it, but with so little success, that all attempts of that kind seem long since to have been given up. Nay, some have been led into very unphilosophical conjectures, viz. that perhaps it was useless, or that if it did perform any office, it was so obscurely as to escape investigation.

SECT. 28. But the ingenious author, whose experiments we are about to relate, entertained too exalted an idea of Nature to suppose that any part of the animal frame was useless, though the structure of some parts might be so intricate, and their uses so obscure, as to elude the researches of the most assiduous and ingenious inquirers into the operations of nature.

SECT. 29. Mr. Hewson, after having made many attempts by dissection and injections to discover the use of this gland, with almost as little success as his predecessors, began to employ the microscope; but microscopical experiments, in the manner they were then conducted, afforded no other satisfaction here than that the blood-vessels were distributed in a similar manner to those of the lymphatic glands; but the external appearance, as well as the minute structure of the thymus (so exactly cor-

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(CXXIV.) The structure of the thymus has been particularly described by Sir Astley Cooper,[a] Dr. Haugsted,[b] and Mr. Simon.[c] The two authors last named have given full notices of the labours of the earlier writers on the subject.

[a] Anatomy of the Thymus Gland, 4to, Lond. 1832.

[b] Thymi in homine ac per Seriem Animalium descriptio Anatomica, Pathologica, et Physiologica. 8vo, Hafniæ, 1832.

[c] A Physiological Essay on the Thymus Gland, 4to, Lond. 1845.

responding with the structure of other glands), convinced Mr. Hewson it must have an excretory duct; yet possibly it might be so small, or the coats so transparent, that when collapsed in the dead body it might become almost invisible; though during life, while distended with the natural fluid, it might be more readily perceived. Therefore the following experiments were made in order to detect it.

EXPERIMENT I.

Sect. 30. The sternum of a half-grown dog being removed, a ligature was passed round the thymus, including at the same time all the neighbouring parts. The animal soon died. On examining the parts contained within the ligature, no excretory duct could be found; but a great number of lymphatic vessels made their appearance, filled with a darker coloured fluid than ordinary.[1]

As the gland in this dog was small, it was suggested that the experiment would probably succeed better in a larger animal, which gave rise to the following experiments.

EXPERIMENT II.

Sect. 31. The chest of a calf being opened, a ligature was passed round the lower part of the thymus, as had been done in the former experiment, and the parts contained within the ligature were taken out. On examining these very attentively, a great number of lymphatic vessels, containing a fluid almost similar to chyle, of a white colour, but not quite so opake, were seen coming from every part of the gland; one of which was so large, that at first sight it had the appearance of an excretory duct; but on a more attentive examination, it was discovered to be no other than a large lymphatic vessel. The remaining parts were dissected with all possible care, but no excretory duct could be seen.

[1] Ubinam is succus habitet, nondum consentitur. Ex thymi tamen exemplo, maximæ glandulæ, crediderim, cum Nuckio, omnino in areolis cellularum residere. Nam in tota thymi glandula, ubicunque læseris, exiguo etiam vulnere, in eam violationem exprimi potest, neque tamen aut manifesta cavitas reperitur, qua contineatur, neque ex vase aliquo effluere videtur, cum et copia eam guttulam superet, quæ ex vase non magno inciso speretur, neque ex remotis glandulæ partibus per vascula adeo facile in vulnus urgeri posset, et denique manifesto cum spuma ex cellulis cavernulisque exprimatur.—Hall. Elem. Phy. tom. i, p. 184.

SECT. 32. The fluid found in the lymphatic vessels, coming from the thymus, differing so much in colour (CXXV) from what is contained in the lymphatic vessels of the other parts of the body, was an inviting circumstance to examine the properties of it, with intent to determine upon what cause this remarkable difference in colour depended.

EXPERIMENT III.

SECT. 33. A drop or two of the fluid found in the lymphatic vessels coming from the thymus gland, being received upon a thin piece of glass, and examined in a microscope, with a lens of $\frac{1}{23}$ of an inch focus, it appeared opake, and like a drop of milk. But on diluting it with a few drops of the serum of human blood, the same appearance was exhibited as was observed on examining the fluid found in the lymphatic glands, a great number of small, white, solid particles, exactly resembling in size and shape the central particles in the vesicles of the blood, or such as are found in the fluid of the lymphatic glands.

EXPERIMENT IV.

SECT. 34. A few drops of the same kind of fluid as in the former experiment were diluted with a small quantity of a solution of Glauber's salt in water (as mentioned in Experiments on the Blood), and on viewing this with the microscope, the same particles were more distinctly seen than in the preceding experiment, on account of the fluid being more diluted (CXXVI).

SECT. 35. Particles of this shape being found in large quantities in the lymphatic vessels coming immediately from the thymus, through the substance of which lymphatic vessels

(CXXV.) See page 252 and Note CXXI.

(CXXVI.) The globules of the juice of the lymphatic glands and of the thymus have the same microscopical and chemical characters: see Note CXXII, pp. 253-4. The experiments in the text are most important. They should be carefully repeated with some modifications and the improved aids that science now affords. No experimental inquiry concerning the office of the thymus can be satisfactory without exact researches as to the nature of the matter carried by those vessels which, on such high authority, have been considered as the excretory ducts of the gland. See Notes CXXVII and CXLIV.

ramify to every part, gave reason for suspecting that these lymphatic vessels were possibly the excretory ducts of the thymus, and the following experiment proved the conjecture to be well founded.

EXPERIMENT V.

Sect. 36. On cutting into the substance of the gland, it was found to contain a white thick fluid,[1] in most respects resembling the fluid found in lymphatic glands, only in larger quantity. A small portion of this fluid being received on a thin piece of glass, was diluted first with serum, then with a solution of Glauber's salt in water, and examined with a microscope. In both these experiments the appearances were exactly the same, as we have related in the Experiments third and fourth; namely, numberless small particles precisely corresponding with those found in the lymphatic vessels passing from the thymus, and with those found in the fluid of the lymphatic glands.

EXPERIMENT VI.

Sect. 37. A small portion of the thymus gland having remained in water a few minutes, in order to wash the white fluid from its surface, was examined with the microscope, and the cellular appearance was seen here as evidently as in the lymphatic gland, which it in every respect resembles.

Sect. 38. From these experiments we are led to make the following conclusions. That one use of the thymus is to secrete from the blood a fluid, containing numberless small solid particles, similar to those found in the lymphatic glands; and that the lymphatic vessels arising from the thymus convey this secreted fluid through the thoracic duct into the blood-vessels, and thus become the excretory ducts to this gland. That the structure and uses of this gland are similar to those of the lymphatic glands, to which it may be considered as an appendage. And that this is the fact, is more probable from

[1] Interiorem si rimeris fabricam, in omnibus, quos unquam vidi, fetubus reperies, incisione facta, quocumque loco visum fuerit, ut tamen omnino caro glandulæ lædatur, succum lacteolum, frequenter etiam sanguine tinctum, ejusque non minimam copiam exprimi posse. Pressa quacunque glandulæ parte succus in vulnus confluit. Eum succum stillatitius vini liquor in grumos cogit.—Hall. Elem. Phy. tom. iii, p. 116.

observing, that the thymus exists during the early periods of life only, when those particles seem to be most wanted (CXXVII).

SECT. 39. Probably the thymus is formed in the human embryo, in the same proportion with all the other parts of the animal. It appears distinctly about the end of the third or beginning of the fourth month from conception. From this

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(CXXVII.) In the calf, Sir Astley Cooper[a] describes two large vessels, which he declares to be the absorbent ducts of the thymus, running upon its posterior surface, carrying the fluid of the gland, and terminating in the jugular veins at their junction with the superior cava by one or more orifices on each side. He dissents from the opinion that the structure of the thymus and of the absorbent glands is similar. "One is conglobate and the other conglomerate; one is firm and compact, and the other loose and pulpy; the one contains cells of considerable magnitude when in the distended state, whilst in the absorbent glands the cavities are small, and with so much difficulty traced, that there is still a doubt if they be cellular or vascular." The structure of the lymphatic glands is noticed in Sect. 12 and Note CXIX*. Sir Astley concludes, like some of the old anatomists, as Glisson[b] and Dionis,[c] that the use of the thymus[d] is to prepare a fluid well fitted for the growth and nourishment of the fœtus, before its birth, and consequently before chyle is formed from the food. But the thymus is not merely an organ to supply the wants of fœtal life; for, as I have noticed in the Appendix to the English edition of Gerber's 'Anatomy,' pp. 97-8, the functions of the gland are very active some time after birth, at which period Dr. Haugsted[e] has proved, in brutes, that the development of the thymus, both proportional and absolute, is greatest.

According to my observations, just cited, the thymus and its juice soon become diminished in ill-fed, over-fatigued, and diseased young brutes, and in badly-nourished and diseased infants. On this subject my friend Dr. Boyd, who has long been employed at the parochial infirmary of St. Marylebone, in researches concerning the weight of the human organs, has given me the following note:

"The weight of the thymus, in 170 cases, was found to vary more than any other organ, in early life. In a female fœtus, 10½ inches long and 10¼ ounces weight, the thymus weighed 6 grains; in a still-born female, 22 inches long and 9 lb. 10 oz. weight, the thymus weighed 340 grains. In 7 male still-born children, out of 24 examined, the thymus weighed half an ounce and upwards, and its average weight was 154 grains. In 3 female still-born children, out of 25 examined, the thymus also weighed half an ounce and upwards, and its average weight was 148 grains.

"The weight of the thymus was greater in males than in females;

[a] Anatomy of the Thymus Gland, pp. 14, 15, 38, 44, 8vo, Lond. 1832.

[b] Anatomia Hepatis, cap. xlv, p. 443, 8vo, Lond. 1654.

[c] Anatomie de l'Homme, 5th ed. p. 497, 8vo, Paris, 1716.

[d] See Note CXLIV.

[e] Thymi Descriptio Anatomica, cap. xi, pp. 89 et seq., 8vo, Hafniæ, 1832.

period to the time of birth, its size is considerably increased, when it is commonly about the size of a small walnut, though not of that figure. In some it is much bigger, but in others it does not exceed the size of a large filbert-nut.

Sect. 40. From the time of birth to the end of the first year, the gland continues to grow larger, and keeps pace with the general growth of the other parts of the body. From the end of the first to the third year, it is neither perceptibly increased nor diminished, but preserves nearly the same size it had acquired at the end of the first year. From the third to the eighth or tenth, it decreases in size, and, gradually wasting, becomes less and less till the child has reached to between its tenth and twelfth year, when ordinarily it is perfectly effaced, leaving only a ligamentous remains, that degenerates into a kind of reticular substance. As the gland becomes less, the vessels that supplied it with blood for secretion diminish in proportion, and at length when the gland totally disappears, these, like the umbilical vessels, being no longer wanted, degenerate into mere ligaments. Sometimes, though very rarely, they continue pervious, (but their diameters are exceedingly contracted,) and carry blood to the remains of the thymus and the mediastinum.

Sect. 41. This curious circumstance of the thymus being largest in the earlier periods of life, and becoming gradually less as the animal advances towards maturity, constantly takes place in the human subject, though the periods, when these

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in 86 children, from birth to 2 years of age, who died from various diseases, the average weight of the thymus was 90 grains in 44 males, and 80 grains in 42 females. In 14 males from 2 to 6 years of age, the thymus weighed 104 grains, and 71 grains in 21 females from 2 to 18 years of age.

"In emaciating diseases the thymus wastes rapidly, almost to a mere membrane devoid of juice. In a female aged 13 days, weighing at birth 6 lb., and after death but 3 lb. 11 oz., the weight of the thymus was only 10 grains; in another emaciated female, aged 18 days, the thymus weighed 15 grains. Of children not emaciated, in one male aged 7 months and in another aged 11 months, who both died of acute arachnitis, the thymus in the first was 330 grains, and in the second 220; and in a female, aged 1 month, who died of convulsions, the thymus weighed 330 grains."

The size of the lymphatic glands at different periods, is mentioned in Note CXIX, page 246.

changes happen, may vary occasionally, and it is probable that they do so in some degree in almost every individual. But I have never seen an instance of the thymus continuing till the time of puberty (CXXVIII). These changes in the thymus are not confined to the human body. The same generally take place in all quadrupeds. The thymus of a calf, called by butchers the neck sweetbread, is not found in the bullock of eight years old; at that age it is entirely wasted, and the same change obtains in every other quadruped that I have had an opportunity of examining.

SECT. 42. The inference naturally drawn from these experiments is, that the thymus is necessary to perform an office requisite in the fœtus-state, and in the early part of life depending upon respiration.

What this office is, we shall hereafter endeavour more fully to explain (CXXIX).

(CXXVIII.) It is now well known that the thymus is occasionally found in adults, examples of which are quoted from various authors by Mr. Simon.[a] I examined it from a woman aged 25, and found that it contained only a trace of the fluid so abundant in the gland when its functions are active. The thymus, as mentioned in Note CXXVII, is larger after than before birth in healthy animals; and probably also in the human subject, though this point requires further observations on children who have died suddenly. On the development of the thymus Mr. Simon has given copious details.

(CXXIX.) See Sections 91-94, and Notes CXXVII, CXLIV.

[a] Physiol. Essay on the Thymus Gland, pp. 28, 31, 32, 4to, Lond. 1845.

CHAPTER IV.

ON THE SITUATION AND STRUCTURE OF THE SPLEEN.

Sect. 43. It hath at all times been matter of surprise among the learned, that a viscus so large, and so advantageously situated as the spleen is, added to the frequent opportunities of inspecting it in different states of health, should, notwithstanding, have its uses so involved in obscurity, as to elude the researches of so many ingenious and industrious inquirers.

Sect. 44. Not that the spleen has at any time been considered as useless, for at different periods a variety of different offices have been assigned to it. Among the ancients, the most celebrated opinion was, that it made the atra bilis or succus melancholicus, which they supposed was carried by the vasa brevia into the stomach; but later observations have entirely exploded that idea, insomuch that the very term is almost extinct. And we shall endeavour to prove, that the more modern opinion, of its producing some change on the blood preparatory to the secretion of bile, hath no better foundation in nature.

Sect. 45. But it will be unnecessary to repeat the various opinions that have been entertained at different times respecting the use of this viscus. Our present endeavour will be to describe its situation and structure, and afterwards to inquire into some particulars respecting its use.

Sect. 46. The spleen then forms the superior part of the abdominal viscera on the left side; its figure is rather oblong, a little convex on its outer or upper, and a little concave on its inner or lower side; it is placed obliquely in the left hypochondrium, with its convex surface exactly corresponding with the concave or under surface of the diaphragm, to which it sometimes adheres, but is always in contact with it, unless when the left lobe of the liver extends very far over into the left side, and, covering the upper surface of the spleen, is interposed between it and the diaphragm; which is sometimes, though very rarely, the case. The inner or concave side of the spleen, from its oblique situation, is turned a little downwards, looking at the same time towards the spine, and to this part a portion of the omentum is attached. The edges of the spleen are not thin like those of the liver, but thick and round, giving a spheroidal figure to the whole.

Sect. 47. But the spleen is not uniformly of the figure above described; sometimes we find it fissured into two or three lobes, almost dividing it into so many distinct spleens, and frequently we find the edge of it serrated.

Sect. 48. It is generally a solitary viscus, yet two[1] distinct spleens have been found in the same body, sometimes three, and sometimes a cluster, as it were, of little spleens; but these are extraordinary deviations from the general conformation of the body, and when found, may properly enough be considered as so many freaks of nature.

Sect. 49. The ordinary weight of the spleen is from six to ten ounces; in some subjects it has been found very large, exceeding the weight of five pounds; but as this preternatural enlargement is ever found to be the effect of disease, so from disease in other cases, or from some cause existing in the body, it is found considerably diminished. One instance I have also seen of a spleen not more than one ounce in weight, yet it had the appearance of being perfectly sound (cxxx).

[1] Hall. Elem. Phy. tom. vi, p. 387. Est tamen unus tantum quamvis præter naturam duplex quoque nonnunquam observatus sit.—Adriani Spigelius de Humani Corporis Fabrica, cap. xiv, p. 309.

(cxxx.) Dr. Boyd finds that, in adults, the spleen is more variable in size and weight than any of the human organs, excepting the womb and ovaries. I am indebted to him for the following notes of his observations. The bodies died of various diseases.

"In 346 males, aged from 20 to 60 inclusive, the average weight of the spleen was 6·78 ounces. The largest spleen was 35·5 ounces, the smallest ·75 of an ounce. The spleen was largest between the ages of 30 and 50, when its average weight in 182 males was 7·23 ounces.

"In 314 females, aged from 20 to 60 inclusive, the average weight of the spleen was 5·42 ounces. The largest spleen was 20 ounces, the smallest ·75 of an ounce. The spleen was largest between the ages of 20 and 40, when its average weight in 123 females was 6·47 ounces."

SECT. 50. From the spleen being in contact with the diaphragm, and not fixed to the sides of the abdomen, its situation will be continually varying in the act of respiration, which will occasion much difficulty to determine at all times the exact situation of it.

SECT. 51. A spleen weighing nine ounces most commonly measures about six inches long and four inches broad.

SECT. 52. In a well-formed chest, in a state of the deepest expiration, from the cartilaginous margin of the thorax to the highest lateral part of the diaphragm, generally measures about six inches; therefore, supposing this to be the standard in the utmost expirations, the upper end of the spleen will ascend so high up as the lower edge of the eighth rib, and in this state the inferior part will be opposite to the lower edge of the tenth rib, or the whole of the spleen will be contained between the eighth and tenth ribs. In the deepest inspirations it never descends below the cartilaginous margin of the chest, unless it be preternaturally enlarged. Thus, by attending to the state of respiration, we may be able to form a good judgment of the situation and extent of this viscus; but if we wish to determine it with greater exactness, let the arm be raised as high as possible, and a line drawn from the inferior angle of the scapula, parallel to the spinous processes of the vertebræ dorsi; the whole of the spleen will be contained within the line drawn, and be found to occupy the space between the eighth and tenth ribs in a state of expiration; but in a deformed chest, or the chest of a woman whose ribs are pressed in by stays, it may differ considerably. In a woman whose ribs had been pressed in by stays, but not more than is ordinarily found, from the cartilaginous margin of the eleventh rib to the centre of the diaphragm, measured six inches and a half. The spleen weighed nine ounces two drachms and a half, and measured five inches and three quarters in length, three inches and seven eighths in breadth, and one inch seven eighths in thickness; the upper edge was opposite to the upper edge of the eighth rib, and the lower part was opposite to the upper edge of the eleventh rib; thus the whole spleen in this subject was placed between the eighth and eleventh ribs. And this will in females, I fancy, be found to be the general standard.

SECT. 53. The spleen has generally been described of a dark, blueish, leaden, or livid colour, and this is the aspect it commonly wears when we examine it in a body a few days after death, or when putrefaction has taken place; but if we examine it in a human body a few hours after death, or in an animal soon after it has been killed, we find it of a deep red or blood colour, which gradually changes as putrefaction advances. We shall therefore conclude that the colour, which has been generally considered as characteristical of the spleen, is no more than the effect of that change which takes place in all animal substances after life is extinct.

Sect. 54. The spleen, in common with all other viscera contained in the cavity of the abdomen, hath an external covering from the peritoneum; under the peritoneal coat is a proper capsule surrounding the whole gland, and to which its tender substance closely adheres.

Sect. 55. The substance of the spleen, particularly if putrefaction hath taken place, is extremely soft and tender, readily breaking down under the touch, and exhibiting that appearance called by the Greeks parenchyma;[1] and at the first sight it hath much the appearance of effused blood; but many experiments prove that this tender substance is no other than very small vessels broken down by putrefaction (cxxxi), and not parenchyma.

Sect. 56. On cutting into the spleen, many small ligaments are seen passing from side to side of it, and those in quadrupeds being large and intersecting each other, gave rise to the opinion, that the spleen was full of large cells into which the blood was thought to have been poured, and these cells were supposed to be demonstrated by a spleen prepared in the following manner. An injection pipe being fixed into the artery or vein of the spleen of an ox, warm water is injected, and the substance of the spleen is kneaded (by which the small

[1] Quarta denique, parenchyma in qua sanguis effusus circa venas nulla seri dispositas.—Adriani Spigelii de Humani Corporis Fabrica, p. 108.

(cxxxi.) The effect is too rapid for the cause assigned by Mr. Falconar. The change is probably owing to a softening of the fibrin of the blood coagulated in the spleen after death, and not to an alteration in the tissue of the organ. See Andral's Hématologie Pathologique, 8vo, Paris, 1843, pp. 70-71.

vessels are broken down) with the hand; the bloody water being pressed out, and with it the small vessels, fresh water is injected, and this process repeated until the water returns colourless; it is then inflated and dried. On cutting into a dried spleen thus prepared, it exhibits a cellular appearance, which has been called the cells of the spleen; but although these cells are artificial, and the structure of the gland is entirely destroyed by that mode of preparation, yet we shall presently endeavour to prove that there are cells, but of a different sort, existing in the substance of the spleen.

Sect. 57. The spleen is composed of arteries, veins, nerves, and lymphatic vessels, which are distributed to every point of it, so that it seems a mere congeries of vessels, and consequently receives a very large quantity of blood, and for that cause it has been very properly supposed to be a gland; and according to that idea, anatomists have made every attempt that their invention could devise to discover its excretory duct, but without success.

Sect. 58. The aorta, whilst in the cavity of the abdomen, gives off from its fore-part three branches: the first of these, called the cœliac artery, springs from the main trunk of the aorta as soon as it enters the cavity between the two crura of the diaphragm, and is immediately divided into three distinct branches, the first of which is called the coronary artery of the stomach, and carries blood to the lesser curvature of the stomach. The second branch, which carries blood to the liver for its nourishment, is called arteria hepatica. And the third branch, which is that we are now about to trace, is called arteria splenica, and carries blood to the spleen.

Sect. 59. The splenic artery, in its passage to the spleen, runs in a furrow through the whole length of the pancreas, and by several small branches supplies that gland with blood for the secretion of the pancreatic juice; besides which, in its course nearer toward the spleen, this vessel also gives off four other arterial branches, called vasa brevia, which are distributed to the greater curvature of the stomach, and the last of these going near the left extremity of the greater curvature, is called gastrica sinistra. The trunk of the artery then passes on to the spleen, and is divided into five or six branches, which enter the concave side at the hollow or sinuosity of the spleen, and as soon as they have passed through the capsule, are divided into exceedingly small twigs, which are distributed to every point of the gland. The arteries, thus minutely divided, transmit the redundant blood to the veins, which becoming larger as they approach nearer to the sinuosity of the spleen, at length pass out in branches, which everywhere accompany the arteries that entered it; these branches uniting form the splenic vein, the blood of which will not coagulate by exposition to the air, like other venous blood (CXXXII). The trunk of the vein then attends that of the artery, receiving veins from the stomach and pancreas, which correspond with the branches given off by the artery; the vein then passing on joins other veins from the intestines, &c., which transmit their blood by the vena portarum to the liver, for the purpose of secretion.

SECT. 60. Lymphatic vessels in great numbers may be distinctly seen running everywhere on the external surface of this gland; insomuch that the spleen of a calf has ordinarily been

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(CXXXII.) According to Mr. Thackrah,[a] the blood of the vena portæ coagulates much quicker, though the clot is softer, than the blood of other veins. Tiedemann and Gmelin[b] declare that the blood of the splenic vein coagulates like the blood of other organs. Schultz[c] found a much larger proportion of fatty matter in the dried constituents of the blood of the vena portæ, especially in the fibrin, than in those of arterial and venous blood of other parts. Dr. Simon,[d] from an examination of the portal blood of two horses, concludes that it coagulates more slowly, and contains more hæmatin and less fibrin than either arterial or common venous blood. In horses shot on account of lameness, in horses dead from disease, in drowned kittens, and in a dog killed by hanging, Mr. Siddall and I found the blood in the splenic vein less perfectly coagulated than in the other veins, but generally somewhat grumous, and containing a few soft though distinct clots. In one horse, shot in consequence of paralysis, the blood was fluid in the splenic vein, but there were some small red clots in the splenic artery. The blood in the splenic vein was always dark-coloured, not of a brilliant red, as stated by Senac;[e] though it was readily brightened by neutral salts, contrary to the experience of Schultz.[f] In the splenic vein of man, after death from disease, I have frequently seen a clot; and so has Mr. Evans.[g] Is not the ordinary firmness of the spleen, some time after death, owing to coagulated blood?

[a] Inquiry into the Nature, &c., of the Blood, ed. 1834, pp. 97-101.

[b] Müller's Elements of Physiology, tr. by Dr. Baly, vol. i, p. 572, 1st edit.

[c] Lancet, 1834-35, vol. ii, p. 573.

[d] Animal Chemistry, tr. for the Syd. Soc. vol. i, pp. 201-4, 8vo, Lond. 1845.

[e] Traité du Cœur, 2d ed. tom. ii, p. 282.

[f] Lancet, 1834-5, vol. ii, p. 572.

[g] Lancet, 1844, vol. i, p. 65.

chosen on which to demonstrate the lymphatic vessels; and they are also as numerous in the internal substance as on the external surface, which we can prove in fish, whose lymphatic vessels are without valves, so that we can inject them from trunk to branch. Now if we fill the lymphatic vessels coming from the spleen of a fish with a red injection, we can colour the gland as highly as if it had been injected by the artery or vein; hence it is evident that the lymphatics are coextended with the blood-vessels to all parts of this gland.

Sect. 61. In the human body the lymphatic vessels pass through some lymphatic glands which are situated near the sinuosity of the spleen, from whence they pass on towards the thoracic duct, into which they empty themselves.

Sect. 62. The nerves inservient to the spleen are branches from the par vagum and intercostal nerves, which form a plexus called the splenic; these enter the sinuosity with the blood-vessels, and attend them through their minutest ramifications.

Sect. 63. Thus we have shown that the spleen is extremely vascular; insomuch that when injected it appears a mere congeries of vessels, and that the quantity of blood circulating through it is very considerable.

Sect. 64. In section 56 we said, that there are cells in the spleen, but not of the kind commonly supposed to have been demonstrated in spleens prepared as there described; but although we deny the existence of such large cells, (which are no other than what may be called the skeleton of a spleen, made by destroying its minute structure,) yet we assert that there are innumerable cells dispersed throughout the whole substance of the spleen, but they are so very small as not to be discovered without the aid of a microscope: these may be seen in the following manner.

Sect. 65. Take a small thin piece of a spleen that has been minutely injected, (i. e. the arteries and veins completely filled with a coloured injection,) steep it a day in clean water, changing the water frequently; upon examination of this with a lens $\frac{1}{15}$ of an inch focus, an almost infinite number of cells may be distinctly seen, the round figure of which, as well as their great regularity, sufficiently distinguishes them from the irregular interstices of the reticular substance. The size and shape of these cells so nearly resemble those we have before described in the lymphatic glands, that a tolerably accurate idea of them may be obtained by referring to Plate VIII, fig. 4. The ultimate branches of the arteries and veins are so distributed as to form a most beautiful network of blood-vessels upon each particular cell, and we shall render it probable that the extreme branches of the arteries form a secretion into these cells.

Sect. 66. Mr. Hewson, in the beginning of his inquiries after the excretory duct of the spleen, was not more successful than his predecessors had been; but having observed that the lymphatic vessels were the excretory ducts of the lymphatic glands, and also that the lymphatic vessels of the thymus performed the office of an excretory duct to that gland, he was led, from this circumstance, to conjecture that the great number of lymphatic vessels found in the spleen might possibly be intended to return the secreted fluid from it, and thereby become in effect its excretory duct. In order to discover if his conjectures were well founded, the following experiments were made.

EXPERIMENT I.

Sect. 67. A dog was opened alive, and after a ligature had been passed round the splenic vessels, the whole spleen was cut out.

Sect. 68. On examination of the lymphatic vessels upon the surface of the spleen, they were found very turgid, and the fluid contained in them appeared of a much darker colour than he had observed in any other lymphatic vessels; on puncturing one of the largest of these vessels, a small quantity of the fluid it contained was received into a teacup, which was red, and coagulated soon after being exposed to the air.

Sect. 69. Mr. Hewson soon discovered that a dog was an unfavorable subject on which to make this experiment completely; because, in that animal, the splenic blood-vessels, both arteries and veins, are divided into many branches, which enter the spleen at some considerable distance from each other; therefore the following experiment was made.

EXPERIMENT II.

Sect. 70. As soon as a bullock was killed, and the abdomen opened, a ligature was passed round the splenic vessels (which in this animal enter the spleen at its upper part) and tied, the lymphatic vessels which accompany the artery and vein were also included in the ligature, and as absorption continues so long as the animal remains irritable, the lymphatic vessels, over the whole surface of the spleen, soon became turgid, and were distinctly seen filled with a red fluid, so highly coloured as equal parts of claret and water; the larger lymphatic vessel was then opened, and a quantity of the fluid it contained was received into a teacup, which, on being exposed to the air, soon coagulated.

EXPERIMENT III.

Sect. 71. A portion of this fluid was diluted with pure serum, when the red colour seemed evidently owing to a quantity of red particles, which were distinctly seen in very considerable numbers.

Sect. 72. But lest it should be objected, that the red particles were contained in the serum, this experiment was carefully repeated.

EXPERIMENT IV.

Sect. 73. Another portion of the fluid received from the lymphatic vessel was diluted with a weak solution of the Glauber's salt, when exactly the same appearances were exhibited as were formerly mentioned in experiments upon the blood; so that there can be no doubt but that the red colour of the lymph from the spleen is communicated to it by a quantity of red particles of the blood.

Sect. 74. It may be objected to Experiment II, that the animal, in being driven to the slaughter-house, might have received some blow on the spleen, by which blood was extravasated, or that extravasation might happen in the very act of killing, and that this newly extravasated blood might be absorbed and found in the lymphatic vessels in the form of red particles.

Sect. 75. To these objections we answer: first, that the spleen is so well defended from external injuries, that it is very improbable such an accident could happen. And secondly, that if such an accident had really taken place, the lymph at the greatest distance from the ligature should be of a redder colour, which is not the fact; for the colour of the lymph in any of the lymphatic vessels of the spleen, as nearly as we can determine, is the same; but that which is nearest the ligature, to a careless observer, seems to be of a deeper red; but this appearance is occasioned only by the quantity being larger from the increased size of the vessel.

Sect. 76. These experiments were frequently repeated during Mr. Hewson's lifetime, and many times since his decease, and the appearances have been uniformly the same.

Sect. 77. That the spleen is the organ ordained by nature for the more perfectly forming these red particles we shall endeavour to prove in the next chapter (cxxxiii).

(cxxxiii.) Views more or less similar to Hewson's on the use of the spleen, have been revived of late years by Tiedemann and Gmelin,[a] Schultz,[b] and Donne.[c] On the contrary, the second Dr. Monro[d] observed that the spleen of some fishes which have but few red corpuscles in the blood, is as dark and comparatively as large as in man; and that in a sturgeon six feet long, with blood abounding in red particles, though he found seven spleens, the largest was not bigger than a dried horse-bean. Professor Müller[e] too objects, with reason, that the reddish colour of the splenic lymph is not constant, and that the blood-corpuscles are formed after the spleen has been extirpated. He also mentions that in some fishes, myxine and the allied genus bdellostoma, the spleen is wanting; and that its absence in the myxine had been noticed by Retzius. It is curious that the blood of Mr. Yarrell's amphioxus lanceolatus,[f] said to be the lowest of the fishes, has lately been observed by Retzius, Müller, and de Quatrefages,[g] to be perfectly colourless, destitute of the ordinary red corpuscles, and only containing a few colourless ones, like the blood of the invertebrata.

Falconar rightly insists on Hewson's opinion,[h] that the spleen is not the only organ capable of forming the red particles. Indeed, Hewson removed it from a dog without any ill effect;[i] and in the 'Philosophical Transactions' for 1667, vol. ii, p. 521, there is an account, by Oldenburg, of a bitch that had her spleen cut out, and afterwards took a dog and bred puppies.

[a] Recherches Expérimentales sur la Digestion, tr. par Jourdan, tom. ii, p. 87, 8vo, Paris, 1827.

[b] Henle, Anatomie Générale, tr. par Jourdan, tom. i, p. 511.

[c] Physiological Journal, No. 4, 8vo, Lond. Jan. 1844, p. 118.

[d] The Structure and Physiology of Fishes explained, p. 37, folio, Edin. 1785.

[e] Physiology, tr. by Dr. Baly, vol. i, pp. 572, 567, 1st edit.

[f] History of British Fishes, vol. ii, p. 468, 8vo, Lond. 1836.

[g] Annales des Sciences Naturelles, Oct. 1845, p. 237, 3e Serie Zool. t. iv.

[h] See Sect. 82, 85, 88, 108.

[i] Letter to Dr. Haygarth, p. 289 of this volume.

CHAPTER V.

CONTAINING AN ACCOUNT OF THE MANNER IN WHICH THE RED PARTICLES OF THE BLOOD ARE FORMED, DEDUCED FROM THE EXPERIMENTS AND OBSERVATIONS RELATED IN THE PRECEDING CHAPTERS.

SECT. 78. IT hath been shown, in a paper delivered by the late Mr. Hewson,[1] and read June 17th and 24th, 1773, entitled, "An account of the Figure and Composition of the Red Particles of the Blood, commonly called the Red Globules," that these red particles are not globules, as was generally believed, but that each particle is a compound body consisting of two parts, viz. an external portion, which, from its resemblance to a small bladder, is called a vesicle; and an internal, contained in the centre of this vesicle, which is called the central particle (CXXXIV); and that these particles while circulating in the blood-vessels in their natural state, are not spheres, but round and flat, resembling a piece of money (CXXXV).

SECT. 79. This fact has been proved by experiments; first, if the red particles of the blood be diluted with fresh serum, and examined with a microscope, their flat figure may be immediately discerned, and the shape of the particles remains unaltered; but if a small quantity of water be mixed with these particles, they are immediately transformed from a plane to a sphere, the vesicle will gradually dissolve (CXXXVI), and dissolving becomes transparent, at which time the central particle may be seen rolling from side to side like a pea in a bladder; the vesicle at length bursts, and the central particle will be entirely disengaged from it; but if a drop or two of a

[1] Vide Chapter I.

(CXXXIV.) See Notes CII and CXXXVII.
(CXXXV.) See Note XCV, pp. 215-16.
(CXXXVI.) See Note XCIV, p. 215.

solution of any neutral salt be added to this mixture before the bursting of the vesicle, it immediately becomes flattened, and the particle of blood reassumes its original figure. In the natural state of a particle of blood, the vesicle is collapsed, and is in contact with, or adheres to, the central particle so firmly as to retain the particle in the middle of it; but when water is added to the blood, and the vesicle becomes a sphere, that union is broken, the central particle becomes loose in the vesicle, and then only its motion can be distinguished (CXXXVII).

SECT. 80. The figure and size of the particles of the blood differ in different animals (CXXXVIII); but the general conformation of vesicle and particle extends probably throughout animal nature (CXXXIX). That particles thus formed should not be made by the mere agitation of the chyle in the lungs seems probable; that they are not, it shall be our business to prove, and also that the lymphatic system and its appendages are the organs employed by nature to do this office. By the lymphatic system and its appendages we mean the lymphatic vessels, the lymphatic glands, the thymus, and the spleen.

SECT. 81. At the first view it may seem extraordinary that nature should have given so many and so complicated organs to form a part only of the blood, when she effects other secretions by organs apparently more simple; but our surprise must cease when we reflect, that upon a due formation of these particles, not only the various functions of the body, but also the very existence of the animal, in a great measure, depends. When we consider how liable these parts are to disease, by which their offices would be impeded, we must necessarily admire the goodness of the Author of our nature for so form-

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(CXXXVII.) This description of the nucleus is evidently taken from the blood-corpuscles of an oviparous vertebrate animal, and is not applicable to those of mammalia. In short, the nucleus is a permanent part of the red blood-corpuscle of oviparous vertebrata, and only a temporary part of the red blood-corpuscle of mammalia: see Note CII, pp. 222-3.

(CXXXVIII.) See the Tables of Measurements, Note CXVIII*, pp. 237 et seq.

(CXXXIX.) In mammals the nucleus of the red blood-corpuscle soon disappears: see Notes CII and CXXXVII.

ing these parts of our body, that the disease of a part should not be attended with the destruction of the whole.

SECT. 82. The lymphatic vessels, which arise from every part of the body, have already been described as performing the office of absorption, or taking all those fluids into the body, by which alone the animal is supplied with nourishment for its preservation and growth, and the lymphatics not only do this, but they also assist in forming the red part of the blood.

SECT. 83. Each lymphatic vessel is vascular, and, when minutely injected, appears to have more blood-vessels than are necessary for the nourishment and growth of that vessel. The coat of each lymphatic is likewise muscular, and consequently has a power of diminishing its capacity upon the application of a stimulus. (See Note LXIV, p. 125).

SECT. 84. The lymphatic glands through which the lymphatics pass secrete a fluid which, when examined with a microscope (chap. II, sect. 19), exhibits numberless small solid particles, exactly resembling in size and shape the central particles contained in the vesicles of the blood (CXL). The lymphatic vessels which arise from the cells of the lymphatic glands, into which the central particles are secreted, we have called (chap. II, sect. 17) the excretory ducts of the lymphatic glands, which convey the secreted particles into the lymphatic vessels which pass through the gland, and from thence they pass on through the thoracic duct into the blood-vessels.

SECT. 85. If we open a lymphatic vessel after it has emerged from a lymphatic gland, we find not only a great number of these central particles, but also many of the particles of the blood completely formed (CXLI); that is, the central particle

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(CXL.) See Notes CII, CV, CXXII, and CXXXVII.

(CXLI.) Red corpuscles are certainly sometimes found in the lymphatic vessels, and generally in those of the spleen of the horse and ox; but it would appear that the reddish colour of the splenic lymph is not constant: see Note CXXXIII, p. 273. Mr. Lane found the ruddy colour of the horse's chyle due to the presence of red corpuscles; and he and Mr. Ancell[a] observed imperfect blood-corpuscles, and attributed the rose-colour of the lymph to them, in the large lymphatic vessels. The thoracic duct of the horse often appears as a coloured tube from the number of these corpuscles in the chyle, which, as described in the Appendix to the English edition of Gerber's 'Anatomy,' p. 93, I have

[a] Lectures on the Blood, Lancet, 1839-40, vol. i, p. 150; vol. ii, p. 152.

is surrounded by a vesicle. We may conjecture with a great degree of probability, that the vesicle is either a secretion from the internal coat of the lymphatic vessel, or that the lymphatic vessel has a plastic power over its contained fluid, so as not only to form a vesicle round the central particle, but also to give it its red colour, for till the red vesicle is formed, the central particle is evidently white.

Sect. 86. That a lymphatic vessel, after it has passed through a lymphatic gland, contains lymph, red blood, and central particles, will not admit of a doubt to any one who will take the trouble of making the experiment. How then are these red particles formed, if not by the lymphatic vessels?

Sect. 87. In Chap. II, sect. 19, we prove that central particles are formed in the lymphatic glands; and from our finding them presently afterwards taking on their vesicular portions in the lymphatic vessels or being completely made, we cannot doubt but that the lymphatic vessel gives them the red vesicle; but in what manner this is performed, whether by a secretion from the internal coat of the vessel, or by a plastic power of the vessel itself over its contained fluid, is perhaps a circumstance among the arcana of nature too minute for human investigation.

Sect. 88. And it is amply sufficient to our purpose to prove that the lymphatic vessels and glands are of themselves capable of forming the red part of the blood.

Sect. 89. It will probably be asked, if the lymphatic glands are given to form the central particles, how are those particles

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generally found to be smaller, more irregular and less perfect in shape, than the red corpuscles in the blood; and the same observation is applicable to the red corpuscles in the splenic lymph of this animal. Dr. Simon's observations[a] on red corpuscles in the thoracic duct of the rabbit and horse, are to the same effect. Schultz and Gurlt[b] also noticed the chyle of a reddish colour from the presence of blood-corpuscles, of which they suppose, with Simon, the formation to begin in the chyle. The transition of the corpuscle of the chyle or lymph into the red corpuscle of the blood seems now to be commonly admitted in Germany: see Note CXXII, pp. 253-4. Dr. Davy informs me that he found a small portion of red crassamentum in the thoracic duct of a man who died suddenly of apoplexy.

[a] Animal Chemistry, tr. for the Syd. Soc. vol. i, pp. 120-21, 8vo, Lond. 1845.

[b] Müller's Physiology, tr. by Dr. Baly, vol. i, p. 563. 1st edit.

formed where the lymphatic glands are wanting, and yet the red blood is perfect? In answer to this, we beg leave to repeat what has been observed in a former chapter, that though animals of the amphibious class have not that circumscribed form of lymphatic gland which we find in the human body, and in the bodies of quadrupeds, yet nature has in them constituted a different apparatus to serve the same end, viz. a network of lymphatic vessels on the meshes of the mesentery found in the turtle. For, as we have already shown, the supposed cellular structure of the lymphatic glands is by no means necessary to constitute a lymphatic gland; it is very probable the ultimate branches of the lymphatic vessels in the mesentery of the amphibia may perform the same office in that class of animals which the small cells do in the human body, or in the bodies of quadrupeds; that is, as the central particles in those animals which have lymphatic glands are formed in the small cells of those glands, so nature in the amphibious class makes the ultimate ramifications of the lymphatic vessels do that office; so that the same purposes of the animal economy may be equally well effected, whether the parts composing a gland are circumscribed in a proper membrane, or whether the same parts are spread out over a large surface (CXLII).

Sect. 90. It may be objected by some that the appearance of central particles may be a deception, for that appearance may be seen in many fluids; but the uniformity of their figure in the same sort of animal, and the difference of their size and

(CXLII.) Falconar properly insists on Hewson's view, that the glands are appendages to the lymphatic vessels, and that the central particles, or lymph-corpuscles, may be formed in these vessels quite independently of the glands; thus anticipating the objection commonly urged against his conclusion as to the office of the glands, from the observations of Professors Burdach,[a] Müller,[b] and Henle,[c] that the corpuscles are found in the inferent lymphatic vessels. In the lymph of these vessels I, too, have often seen the corpuscles; but they are much more numerous in the lymphatic and mesenteric glands, than in any other part of the system of chyle or lymph vessels: see the Appendix to the English edition of Gerber's 'Anatomy,' pp. 92, 95, 97-8.

[a] Traité de Physiologie, tr. par Jourdan, tom. ix, p. 453, 8vo, Paris, 1841.

[b] Physiology, tr. by Dr. Baly, vol. i, p. 263. 1st edit.

[c] Anatomie Générale, tr. par Jourdan, tom. i, pp. 506-7.

shape in different animals (CXLIII), will put this matter out of dispute. In birds, as the common fowl for instance, the particles of the blood are oblong like a plum-stone (vide Plate V, fig. 3), and the central particles found in the lymphatic glands of that bird are also oblong, corresponding in every respect with the central particle found in the vesicle of the blood of that animal.

SECT. 91. In chapter III it was observed that the structure of the thymus gland is similar to that of the lymphatic glands, and that it secretes from the blood particles like those secreted by the lymphatic glands; in fine, in its office, that it is no more than a large lymphatic gland. But why should the thymus be large in the fœtus, and as the animal increases in size, become smaller and smaller until at length it quite disappears? Or, in other words, why does not the thymus, like the liver or pancreas, continue through life? The reason why the thymus is larger during the early part of life, is, we conceive, that it may act as an auxiliary to the lymphatic system, for the purpose of forming more of the central particles of the blood than could have been made by the lymphatic glands alone during that time, when nature wants them most; for the human body grows more, in proportion to its weight, from the second month after conception to the end of the third year, than it does in any future period of its existence of no longer duration; a greater quantity of blood is therefore wanted and applied by the constitution in the quick growth of the animal than is ever afterwards applied to that purpose in the same time.

SECT. 92. If the thymus gland were wanting in the young animal, the lymphatic vessels and glands must have been made considerably larger than they now are, or out of proportion to the other parts of the body; otherwise the animal could not have been duly nourished, and the purposes of nature must have been defeated; but by the assistance of the thymus, a sufficient quantity of the central particles, to be converted into blood necessary for the growth of the animal, are formed, and nature at the same time preserves a just proportion in the

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(CXLIII.) The similarity in the figure of the lymph-corpuscle in different classes of animals, and the difference in shape between this round corpuscle and the long oval nucleus of the blood-disc of birds, is explained in Note CXXII, p. 253.

lymphatic system; then, as the animal becomes larger, and of consequence the lymphatic system more extended, that alone and unassisted can now furnish a sufficient quantity of these particles for the growth of the animal and repair of the constitution; the thymus being no longer necessary, and occupying a space which by this time will become useful for other purposes, the size of it will be gradually diminished, and its parts absorbed into the habit.

SECT. 93. The thymus gland is placed in the chest, because the space it fills is not wanted by the fœtus. After birth, when respiration takes place and the animal increases in size, the growth of the lungs requires the cavity of the chest to be enlarged, which is done partly by the absorption of the thymus, but chiefly by the extension of the ribs.

SECT. 94. The thymus gland, then, we consider as being an appendage to the lymphatic glands, for the more perfectly and expeditiously forming the central particles of the blood in the fœtus, and in the early part of life (CXLIV).

(CXLIV.) Bischoff[a] partially supports Hewson's view as to the use of the thymus. From a series of interesting observations, Mr. Simon[b] includes its uses in the theory of fat-formation, concluding that "the gland fulfils its use as a sinking fund of nourishment in the service of respiration;" and that, "what the gland sequestrates from the circulation does, in gradually revisiting thither, accomplish those chemical purposes in respect of respiration and temperature, which, under other circumstances, are fulfilled by the effete products of active animal tissues." He disbelieves, that the globules in the fluid can serve as germs for the tissues or for the blood, because he declares it to be impossible that the globules, as such, can enter the lymphatic vessels without a dissolution of the membrane lining the cells of the gland, and which, he says, is a material of the most exquisite delicacy, rather a fine boundary line than a membrane admitting of isolation.

With respect to Mr. Simon's conclusion, it may be asked, if it has yet been proved that the purposes of respiration and temperature are fulfilled by the effete products of the animal tissues? In man and several animals, there is commonly a larger proportion of fat in the chyle than in the fluid of the thymus or of the lymphatic system; and, as mentioned in Note LX, p. 88, it is at least probable that fat performs an important part in nutrition and growth. As to the impossibility of the globules passing into the lymphatic vessels, it is now generally admitted that the multiplication of floating cells is rapidly effected by the minutest

[a] Mr. Simon's Essay on the Thymus, p. 9. [b] Physiol. Essay on the Thymus Gland, pp. 90, 91, 38, 34, 4to, Lond. 1845.

SECT. 95. In chapter II we considered the lymphatic vessels arising from the cells of the spleen as its excretory ducts, which term, perhaps, when applied to lymphatic vessels, may be objected to by some, but we think not with reason; for if we consider the office of an excretory duct, we shall find that there is no more impropriety in calling these lymphatics excretory ducts, than there is in using the term excretory duct to express that tube which conveys the secreted fluid of any other gland to its place of destination. The excretory duct of the liver conveys the bile into the duodenum, because that fluid is required in the intestines for the purpose of digestion. So the

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elementary molecules in a liquid cytoblastema, both produced by the old cells. This liquid and the molecules might traverse the exquisitely delicate membrane described by Mr. Simon, get into the lymphatic vessels, and keep up the supply of globules there; and it is not impossible that part of the membrane itself, as well as the highly developed epithelium[a] of the intra-glandular lymphatic vessels, and the lymph-globules themselves may be constantly in a state of dissolution and renovation.

But leaving these hypotheses, we come to the fact that the lymphatic vessels of the thymus do carry a fluid, however it may get into them, like that of the thymus, and pervaded by the same globules. At least such is the result of Hewson's observations, which, as far as I know, have never been refuted on this point; and Sir Astley Cooper, whose view of the use of the gland is mentioned in Note CXXVII, p. 261, declares that its lymphatic vessels are its absorbent ducts, and the carriers of its fluid into the veins in the lower part of the neck. I must repeat that a careful inquiry into the nature of the contents of these absorbent vessels ought, as Hewson and Cooper believed, to form a leading part of any researches concerning the office of the thymus.

In the Appendix to the English edition of Gerber's 'Anatomy,' pp. 95-100, the reasons are detailed which induce me to believe, that the fluid of the thymus differs only from that of the lymphatic glands in containing more corpuscles; that these have the characters of nuclei or immature cells;[b] and that both the lymphatic glands and the thymus are organs for the elaboration of nutritive matter, the activity of the thymus being most remarkable at that period of the economy when growth and nutrition are most active: see Note CXXVII, p. 261. Whether the ultimate destination of these embryo cells be simply to serve as central particles for the formation of blood-corpuscles, as Hewson supposed, or for the foundations, either mediately or immediately, of cells concerned in growth and nutrition generally, as the recent cell-doctrine would imply, is another question deserving of further inquiry.

[a] See Note CXIX*, p. 250. [b] See Note CXXII, pp. 223-4.

lymphatic vessels of the spleen convey the red particles of the blood into the thoracic duct, and from thence they pass into the blood-vessels, which is the place assigned to them by nature, and from whence they are to be conveyed to the different parts of the body, to answer the purposes of nutrition and vivification.

SECT. 96. That the spleen really does secrete the vesicular portion of the red particles of the blood we have very convincing proofs (CXLV).

SECT. 97. First, if the spleen be diseased, the body for a time gradually wastes.

SECT. 98. Secondly, we have proved that vast numbers of central particles made by the thymus and lymphatic glands, are poured into the blood-vessels through the thoracic duct, and if we examine the blood attentively, we see them floating in it (CXLVI). Nature surely would not make so infinitely many particles to answer no purpose! What then becomes of these particles after they are mixed with the circulating blood; are they immediately destroyed? No. They are, we believe, carried with the blood to the spleen, not that the spleen has any elective attraction over them; but that being equally and uniformly diffused through the general mass of blood, a due proportion of them is received by the spleen with its arterial blood, and that when arrived there, the spleen has a power of separating them from the other parts of the blood, and of depositing them in the cells of that gland already de-

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(CXLV.) See Note CXXXIII, p. 273.

(CXLVI.) This passage is so clear, as completely to set aside the claim made of late years by M. Mandl[a] and others, to the discovery of the pale globules of the blood. In that of mammalia, it is quite evident that Hewson had seen these globules, and considered them in all the vertebrata as lymph-corpuscles, a view which has recently been revived.[b] Senac[c] also appears to have seen the pale globules in the blood, and to have regarded them as belonging to the chyle.

That the lymph-globule is an immature cell, which may change in the blood, and even in the thoracic duct or lymphatic vessels, into the larger and more perfect pale cell of the blood, is very probable: see Note CXXII, pp. 253-4.

[a] Anat. Microscop. liv. i, p. 8. fol. Paris, 1838; et Anat. Génér. p. 252, 8vo, Par. 1843.

[b] See the App. to the English edition of Gerber's Anatomy, pp. 15, 19.

[c] Traité du Cœur, tom. ii, pp. 91, 661, ed. 1749; and 2d ed. tom. ii, p. 279.

scribed; where the arteries which are spread out in form of network upon the sides of the cells secrete from the blood the vesicular portion, and that when thus perfectly made, the lymphatic vessels which originate from the cells absorb them, and convey them thence into the thoracic duct, and so into the blood-vessels.

SECT. 99. That the spleen does secrete somewhat, is evident from the change observable in blood drawn from the splenic vein, which is distinguished by this remarkable property, that it will not coagulate like blood taken from other veins (CXLVII); the reason we apprehend is, because the coagulable lymph is employed by nature in the formation of the red vesicle; it remains therefore fluid until the thinner parts have evaporated, leaving the red particles a dry mass.

SECT. 100. Fourthly, we have frequently examined blood taken from the splenic vein, but could never distinguish any central particles in it (CXLVIII).

SECT. 101. Fifthly, in every animal which has red blood a spleen is found; but in those animals which have not red blood the spleen is wanting (CXLIX).

SECT. 102. Lastly, we find that vast quantities of the red particles of the blood are brought from the spleen by the lymphatic vessels which originate in its substance, and for this reason we have called these lymphatic vessels the excretory ducts of the spleen.

SECT. 103. That the red particles of the blood are completely formed by the spleen we have therefore as strong

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(CXLVII.) See Note CXXXII, p. 269, and Note CLI, p. 289.

(CXLVIII.) Contrary to the statement in the text, and to the similar observation of Dr. Simon,[a] I have more than once seen pale corpuscles abundantly in the blood of the splenic vein. They did not differ in structure or size from the pale globules in the blood of the heart; nor could I observe any difference between the red corpuscles from the splenic vein and from the heart, examined at the same time from the same animal; and the central spot[b] was observed as well in the red corpuscles of the splenic blood as in those from the heart. In one trial I saw the nuclei as usual in blood-corpuscles taken from the spleen of a goose.

(CXLIX.) The spleen is said to be wanting in two or three cyclostomatous fishes: see Note CXXXIII, p. 273.

[a] Animal Chemistry, tr. for the Syd. Soc. vol. i, p. 202, 8vo, Lond. 1845.

[b] See Note XCVI, p. 21

proofs as we have that the liver secretes bile, or the testicles semen; we find bile in the ductus hepaticus, semen in the epididymis, and red particles of blood in the lymphatic vessels of the spleen.

Sect. 104. It may then reasonably be asked, how is the red blood formed when the spleen is taken out, if the spleen is the viscus intended by nature to form the red blood? This objection will militate equally strong against any other use the spleen is supposed to have; for that the spleen may be taken out, and the animal suffer but little inconvenience, by no means prove it to be useless, but it proves that some other part is capable of performing its office. Every philosopher must entertain too exalted an idea of nature to believe that any part of the creation is useless, much less could he suppose a viscus in the human body, so large as this is, has no office of importance assigned to it.

Sect. 105. Suppose, then, for a moment, we allow the spleen to do the office assigned to it by the moderns, viz. that it produces some change on the blood preparatory to the secretion of bile; what must do that office when the spleen is wanting? for as the animal lives and is well nourished afterwards, if that supposed change is absolutely necessary for the secretion of bile, either some other viscus must do its office, or the bile, a fluid so requisite for assimilating our food, could not be formed, and the animal for want of being duly nourished must die.

Sect. 106. If we may reason from analogy, we should say, that it is contrary to the established laws of the animal economy to suppose the use of one organ or gland to be merely subservient to another organ or gland in preparing the blood, in order to render it fit for such organ or gland to do its office; it would be asserting that the liver which nature intended to secrete bile could only do it by the intervention of the spleen; and yet if we allow that bile can be formed without the use of the spleen, we admit that intervention to be by no means necessary. But to carry our analogy still farther, nature has given to the animal body certain glands, and has assigned to each peculiar offices, that is, she has endowed them with a property of separating from the blood divers fluids, as different from each other as they are from the mass of blood from out of which they were originally separated.

Sect. 107. The lachrymal gland secretes the tears; the salivary glands the saliva; the kidneys, urine; the testicles, semen, &c. &c., without the intervention of any auxiliary gland. If then a fluid so elaborated, and so different from anything we find in the blood, as semen is, a fluid which has an office of no less dignity than to perpetuate the whole race of animals, can be formed from the blood by the vessels of the testis, without any preparatory change being produced on it, may we not reasonably conclude that the liver is capable of secreting bile from the blood without any antecedent change being made on it by the spleen? For to say that the blood must be prepared by the spleen, before bile can be secreted from it by the liver, is to deny that the liver, which is given to form bile, can do the office which nature has intended it to perform.

Sect. 108. But if we allow the spleen to make the red part of the blood, we can readily account for the reason why the spleen may be cut out of an animal, and yet the animal survive, and suffer but little inconvenience, for though the office of the spleen is to form the red particles of the blood, yet it is not the only organ in the body capable of doing that office; for we have already proved (sections 85 and 88) that the lymphatic vessels do also form the vesicular portion; the spleen, therefore, is not the only organ capable of doing it. But nature has given the spleen as an auxiliary to the lymphatic system, in order to the more commodiously, expeditiously, and completely forming the red part of the blood.

Sect. 109. If, then, the spleen be cut out, or its office obstructed by disease, nature has a resource, in exciting the lymphatic vessels to form a larger quantity of red particles than they had ordinarily been accustomed to do, and these in proportion to the exigencies of the habit; but here nature does not assign a new office to the lymphatic vessels, but only excites them to exert, in a higher degree, a power of which they were before possessed, and this notion is conformable to what we observe in other circumstances of the animal economy; as when an animal is fat and well nourished, the stomach is much longer in performing its office than it is when emaciated by long fasting, and its life is in danger from want of nourishment, or than it is when the body is wasting by disease, witness the surprising quantities of food the stomach will digest in a short time after a recovery from the smallpox, or a violent inflammatory fever; under these circumstances, it is astonishing to observe how much nature will exert herself, and how soon food taken into the stomach will be digested, and applied to the purposes of the constitution; in like manner, most probably, if the spleen be diseased or cut out, nature is capable of making the lymphatic vessels exert themselves more powerfully in the execution of their office; or, on the contrary, if the lymphatic system be diseased, the spleen is excited to form a larger quantity of blood in order to make up the deficiency: thereby the life of the animal will be less frequently endangered from a partial disease.

Sect. 110. But how much soever the manner in which the red vesicle is formed may be disputed, we think it cannot be denied but that the office of the thymus and lymphatic glands is clearly proved to form the central particles found in the vesicles of the blood; and though the operation of nature in forming the vesicular portion is more obscure, yet the probability of its being performed in the manner we have related will, we hope, be readily admitted.

Sect. 111. A system so new and so different as this is from the opinions at present so generally entertained of the blood, perhaps may meet with much opposition, and as no doctrine should be admitted in philosophy till it has stood the test of the most careful and accurate examination, it may therefore be some time before this is universally allowed; for as the experiments are numerous, and some of them not easily made, few but lovers of science will take the necessary pains to inquire into them: but we will be bold to assert, that whoever repeats these experiments will be amply rewarded for his trouble. We shall add, that when these facts are viewed with candour, and experiments of this kind are prosecuted with industry, they may probably direct the way to discover many operations of the animal economy that are at present considered among the inexplicable arcana of nature.

Sect. 112. Having now finished the relation of the facts, and the experiments to prove them, whether the conclusions drawn from them are just we shall submit to the judgment of the learned reader.

DETACHED PAPERS.

A Letter from the late Mr. William Hewson, F.R.S., and Teacher of Anatomy in London, to Dr. John Haygarth, Physician in Chester (CL).

My dear Sir,—I have now a little leisure, and shall endeavour to fulfil my promise, by sending you a sketch of my observations.

The red particles of the blood, improperly called globules, are flat in all animals, and of very different sizes in different animals. In man, they are small, as flat as a shilling, and appear to have a dark spot in the middle. In order to see them distinctly, I dilute the human blood with fresh serum. My predecessors, not having thought of this, could not see them distinctly, and Leeuwenhoek in particular, imagining a round figure fittest for motion, concluded they must be round in the human body; though he and others allowed, that, in frogs, &c. where they viewed them distinctly, from the blood being thinner, they were flat. Now, I prove that they are flat in all animals. In the human blood, where these particles are small, it is difficult to determine what that black spot is, which appears in the centre of each. Some have concluded that it was a perforation; but in a frog, where it is six times as large as in a man, it is easy to show that it is not a perforation, but, on the contrary, is a little solid, which is contained in the middle of a flat vesicle. Instead, therefore, of calling this part of the blood red globules, I should call it red vesicles; for each particle is a flat vesicle, with a little solid sphere in its centre.

(CL.) From the 'Medical and Philosophical Commentaries, by a Society in Edinburgh,' vol. iii, p. 87, 8vo, London, 1775. In the General Index to the present edition of Mr. Hewson's works, references will be found to the pages in the Third Part of the 'Experimental Inquiries,' where the different points mentioned in this Letter are more fully discussed.

I find that the blood of all animals contains vesicles of this sort. In human blood there are millions of them, and they give to it the red colour. But in insects they are white, and less numerous in proportion than in man and quadrupeds. As they are flat in all animals, I suspect that shape is a circumstance of importance; but it can be altered by mixture with different fluids. And I find that it is by a determinate quantity of neutral salt contained in the serum that this fluid is adapted to preserving these vesicles in their flat shape. For, if they be mixed with water they become round, and dissolve perfectly; but add a little of any neutral salt to the water, and they remain in it, without any alteration of their shape, and without dissolving.

Now, when it is considered that the blood of all animals is filled with these particles, we must believe that they serve some very important purpose in the animal economy; and since they are so complicated in their structure, it is improbable they should be made by mechanical agitation in the lungs or blood-vessels, as has been suspected, but probably have some organs set apart for their formation. This I shall endeavour to prove, when I have explained their structure a little more particularly, and mentioned the manner in which I exhibit it. I take the blood of a toad or frog, in which they are very large, I mix it with the serum of human blood to dilute it; I find them appear all flat; so they do in the blood-vessels of this animal, as I have distinctly seen in the web between its toes, whilst the animal was alive, and fixed in the microscope. Their appearance in these animals is not unlike slices of cucumber. I next mix a little of the blood with water, which immediately makes them all round, and then begins to dissolve them whilst they are round. I incline the stage of the microscope, so as to make them roll down it, and then I can distinctly see the solid in the middle fall from side to side, like a pea in a bladder. A neutral salt added to them at this time brings them back to their flat shape; but if the salt be not added the water gradually dissolves away the vesicle, and then the little sphere is left naked. Such is the composition of these particles. I have exhibited these experiments to a considerable number of my acquaintance, who all agree in their being satisfactory.

The microscope I use is a single lens, and therefore as little likely to deceive us as a pair of spectacles, which, as is allowed by all who use them, do not disfigure objects, but only represent them larger.

From further experiments, I am convinced that the use of the thymus and lymphatic glands is to make the solid middle pieces; and I can prove it in as satisfactory a manner as you can do the use of any viscus in the human body; that is, by opening these glands, and examining the fluid contained in their cells, which I find to be full of these little solids. I moreover find that the lymphatic vessels take them up from those glands, and convey them into the blood-vessels, which carry them to the spleen, in whose cells they have the vesicles laid over them; so that the thymus and lymphatic glands make the central particles, and the spleen makes the vesicles that surround them. That this is the use of the spleen, appears from examining the lymph which is returned from it by its lymphatic vessels, which are its excretory ducts; for that lymph, contrary to what is observed in other parts of the body, is extremely red.

But, besides having these glands set apart for making the red vesicles of the blood, I find that they are also made in the lymphatic vessels in different parts of the body, whose coats have blood-vessels properly constructed for this secretion. So that the thymus and lymphatic glands are no more than appendages of the lymphatic system, for making the middle particles; and the spleen an appendage to the lymphatic vessels, for making the vesicles which contain these middle particles.

I conjecture that it is the coagulable lymph which is converted into this red part of the blood, from a curious fact that has been long known; namely, that the blood in the splenic vein does not coagulate, when exposed to the air, as the blood of other veins does (CLI); so that it seems to be robbed of its coagulable lymph in passing through the spleen.

It is very remarkable that the spleen can be cut out of an

(CLI.) Mr. Hewson perhaps only had in mind the fact, mentioned in Note CXXXII, page 269, that the blood of the splenic vein generally coagulates less perfectly than other venous blood; and not that the blood of the splenic vein remains entirely fluid, until it dries, when exposed to the air, as Mr. Falconar's statement, at page 283, sect. 99, would imply.

animal, and the animal do well without it. I made the experiment on a dog, and kept him a year and a half, without observing his health the least impaired. From this, some have concluded the spleen to be a useless weight, which is absurd, when we consider that all animals with red blood have it. Therefore it is more consistent with what we know of the animal economy, to conclude that since an animal can do well without it, there is probably some part of the body that can supply its place.

Insects have vesicles constructed in a similar way to ours, but differing in colour. But insects have neither spleen, thymus, nor lymphatic glands, and therefore in them probably these vesicles are entirely fabricated in the lymphatic vessels. But to us, and other of the more perfect animals, besides the lymphatic vessels, nature has given those glands, that a proper quantity of those important vesicles might be the better secured to us, just as she has given us two ears, the better to secure to us hearing through life, though we can hear perfectly well with one.

Thus, my dear friend, I have given you a sketch of my new opinions. I rather expect from this merely to gratify your longing than to convince you, for the subject is too intricate to be communicated in a letter; but I make no doubt of proving these positions when I have leisure to handle the subject more fully. Adieu! Believe me ever sincerely your friend,

WM. HEWSON.

LONDON; *July* 19, 1773.

The Operation of the Paracentesis Thoracis, proposed for Air in the Chest, with some Remarks on the Emphysema, and on Wounds of the Lungs in general; by Mr. William Hewson, Reader of Anatomy. Communicated by Dr. Hunter. Read June 15, 1767 (CLII).

ALTHOUGH the emphysema has been of late more successfully treated than formerly, yet I have been led to think that something further might be attempted towards a more certain and a more speedy cure of that distemper.

The improvement which has occurred to me, and which I shall venture to lay before the Society, is the operation of the paracentesis thoracis, in order to let the air out of the chest; for that the confinement of the air in that cavity occasions the worst symptoms in that disorder, and even death itself, I am almost persuaded, partly from considering what the consequences must be of a wound of the lungs, (for a wound there is the common cause of the emphysema,) and partly from attending to the symptoms of the disease; but chiefly from the dissection of the body of a person who died under it.

The accident which most commonly gives rise to the emphysema is a fractured rib, by which the vesicles of the lungs being wounded, the air escapes through them into the cavity of the thorax; but as the rib, on being fractured and pushed inwards, wounds the pleura, which lines the ribs and the intercostal muscles, part of the air most commonly gets through the pleura and those lacerated muscles into the cellular mem-

(CLII.) From the 'Medical Observations and Inquiries, by a Society of Physicians in London,' vol. iii, p. 372, 8vo, London, 1767. The second Dr. Monro had previously proposed to let out the air from the pleura in cases similar to those mentioned in this paper, as Hewson has more fully explained in the beginning of the Appendix to the First Part of the 'Experimental Inquiries,' pp. 91 et seq. of this volume.

brane that is on the outside of the chest, and from thence it is diffused through the same membrane over the whole body, so as to inflate it sometimes to an extraordinary degree. This inflation of the cellular membrane has been commonly looked upon as the most dangerous part of the disease; how justly, will appear in the sequel.

It is natural to suppose that the wound of the pleura and intercostal muscles may sometimes be too small to suffer the air to get readily into the cellular membrane and to inflate it, but may confine a part of it in the cavity of the thorax so as to compress the lungs, prevent their expansion, and cause the same symptoms of tightness of the chest, quick breathing, and sense of suffocation, which water does in the hydrops pectoris, or matter in the empyema. So far we may conclude from reasoning on the consequences of a wound of the lungs, and from analogy.

And that it sometimes really happens that air is confined, and produces these symptoms, appears probable from the histories of such patients as have been emphysematous: of these, the most remarkable that I have met with in my reading, are one by M. Littre,[1] another by M. Mery,[2] a third by Dr. Hunter,[3] and a fourth by Mr. Cheston.[4]

In M. Littre's case few symptoms are mentioned; we are only given to understand that the patient, who had received a wound in the side by a sword, could not breathe without making the most violent efforts, especially during the latter part of his disease: he died on the fifth day.

In M. Mery's case, we are informed that the patient had the fourth and fifth of the true ribs broken by a coach passing over his chest, that his respiration was much impeded from the first, and became more and more difficult till he died, which was on the fourth day after the accident.

In Dr. Hunter's case, the symptoms are more particularly mentioned. This patient had received a considerable hurt on his side by a fall from a horse. He had a difficulty of breathing, which increased in proportion as the skin became elevated

[1] Mém. de l'Acad. Royale des Sciences, pour l'année 1713.

[2] Ibid.

[3] Medical Observations and Inquiries, vol. ii.

[4] Pathological Inquiries.

and tense; it was laborious as well as frequent. His inspiration was short and almost instantaneous, and ended with that catch in the throat which is produced by shutting the glottis; after this he strained to expire for a moment without any noise, then suddenly opening the glottis, he forced out his breath with a sort of groan, and in a hurry, and then quickly inspired again; so that his endeavour seemed to be to keep his lungs always full, inspiration succeeded expiration as fast as possible. He said his difficulty of breathing was owing to an oppression or tightness across his breast, near the pit of the stomach. He had a little cough, which exasperated his pain, and brought up blood and phlegm from his lungs. Scarifications were made, which relieved him; the emphysema subsided; his breathing became more and more easy, and he recovered.

In Mr. Cheston's case, the man had received a blow on the chest. He had a constant cough, bringing up, after many ineffectual efforts, a frothy discharge lightly tinctured with blood; he seemed to be in the greatest agonies, and under a constant appearance of suffocation. His pulse was irregular, and sometimes scarcely to be felt; his face livid; and when he was sensible, which was only now and then, he complained of a pain in his head. On passing a bandage round his chest, with a proper compress to prevent the discharge of air into the cellular membrane, and to confine the motion of the thorax, the patient cried out that he could by no means suffer it, and that if it were bound so tight, he should burst. A strong compression by the hand alone affected him in the same manner. Scarifications were made to let out the air from the cellular membrane; and these closing, others were made. Notwithstanding bleeding, repeated scarifications, and other means, his sense of suffocation and difficulty of breathing increased. On the fourth day the air no longer got into the cellular membrane; when on a sudden inclining his head backwards, as it were for the admission of more air than usual, his breathing became more difficult and interrupted, he turned wholly insensible, and soon after died.

M. Littre, M. Mery, and Mr. Cheston opened the bodies of their several patients after death.

M. Littre in his, besides a wound of the lungs and a fractured rib, found a considerable quantity of blood in the cavity of the thorax, and was sensible of some putrid air escaping on his first puncturing the intercostal muscles and pleura. The wounded lobe was hard and black, and the other two of the same side were inflamed.

In M. Mery's patient there was not any extravasated blood in the cavity of the thorax, nor was there anything preternatural to be seen except the fractured ribs, the wound of the pleura, and that of the lungs.

Mr. Cheston, in his subject, observed a fracture of the tenth and eleventh ribs, and a wound of the lungs opposite to these fractures. The lungs below the wound were livid; and, an incision being made into them, their substance was found to be more compact than usual; but, excepting these appearances, all the other parts were in a natural state, without any extravasation, inflammation, or internal emphysema.

Now, were not the symptoms in those cases such as might be expected to arise from air confined in the cavity of the thorax? I am inclined to believe they were, notwithstanding that authors in general account for them otherwise, and among the rest Mr. Cheston himself, though in one part of his remarks he seems indeed to point that way.

On comparing these emphysematous cases, I say it seemed probable to me that it was the air which was confined in the chest that was the principal cause of the symptoms; such as the difficulty of breathing, the tightness of the chest, and the sense of suffocation, which was so much increased by external compression; and as in these symptoms this disease agreed with the hydrops pectoris and the empyema, both of which are relieved by the paracentesis thoracis, I was thence induced to believe that this operation might be performed, for letting out the air, with some probability of success.

But as in such cases no air had hitherto been actually discovered in the cavity of the thorax, it therefore might be doubted whether the symptoms were not owing to some other cause; such as the mere wound of the lungs (abstracting from its letting out air), or an effusion of blood into the cells of that organ,[1] in consequence of the wound. I thought it there-

[1] This seems to have been the cause of the lividness and compactness which Messrs. Littre and Cheston observed in the wounded lobes of their patients.

fore proper to try to ascertain, by the following experiments, the effects of a simple wound of the lungs, and the effects of air confined in the thorax; and the rather as, in some of these cases, the wound of the pleura and intercostal muscles appeared so large, that it might be doubted whether the air could be confined in the chest, when there seemed to be so free a passage from that cavity into the cellular membrane.

EXPERIMENT I.

I took a rabbit, and, pulling the skin of its chest to one side, I pushed a sharp knife into the cavity of the thorax, and moved it about so that I might wound the lungs; then withdrawing the knife, I let the skin slip back again, by which means the wound of the skin was at some distance from that of the intercostal muscles; then applying a piece of lint, a plaster, and a slight bandage to the wound of the skin, I expected to see the animal become emphysematous, but was disappointed; and though I repeated the experiment three or four times, I could not get the air to pass from the lungs into the cellular membrane. On killing the animal, I observed that the wounds of the lungs were surrounded by a small ecchymosis, and were so closed (probably by the blood which had been effused) that no air could escape.

EXPERIMENT II.

Having pushed a sharp knife into each side of the chest of a dog, with the same precautions, and with the same intentions as mentioned above, I then allowed him to run about the house. This experiment I made at eight o'clock in the morning; about ten he appeared less lively, and about twelve seemed to choose to be at rest, but had no difficulty of breathing nor emphysema. In the evening he was as lively as before; and was likewise so the next morning at eight o'clock, when I killed him. On opening his abdomen, the diaphragm was not depressed nor loose, as when air is let into the chest, nor did any air escape on puncturing it. On opening the chest and examining the lungs, I found that the wounds were small and perfectly closed with a slight ecchymosis surrounding them. On blowing into the lungs, the air did not escape through the wounds.

From these experiments I concluded that, in order to let the air out of the lungs into the cavity of the thorax, a puncture or incision (on account probably of the effusion of blood from the divided vessels) was improper; and that, in the emphysematous cases above mentioned, the emission of the air must have happened from a superficial abrasion, or laceration of the part.

EXPERIMENT III.

I tried, by pushing a blunt probe into the chest of a rabbit, and by moving it about, to produce such an emphysema by laceration, but without effect.

EXPERIMENT IV.

I then punctured the chest of another rabbit, but so cautiously as not to hurt the lungs; and, having blown into it, I immediately made a compression upon the wound with some lint, a compress, and a bandage, in order to confine the air in the cavity of the thorax. I then observed that the animal breathed more frequently and laboriously. On removing the compress, the air rushed out, and the animal gradually recovered its natural manner of breathing. It was then allowed to run about the house for a few days, and seemed not the worse for the operation.

EXPERIMENT V.

The same experiment was repeated on a dog, after tying him down to a table; but, when the air was blown into the thorax, he struggled so much, and acted so strongly with his chest, as to force it out again almost immediately; so that not being able to manage him, I was obliged to desist without having had an opportunity of observing what alteration was produced on his breathing. I kept him about a week, and did not observe him at all affected by the wound.

I made no further experiments, being indeed almost convinced from these that it was the air confined in the cavity of the thorax, which had occasioned the violent symptoms in the cases mentioned above, and not the mere wound of the lungs; and I must own, that if I was not perfectly convinced, it was because no air, in such cases, had yet been found in the cavity of the thorax. But, not long after I had made these experiments, the following case occurred, which fully satisfied me.

A young man, when the house was on fire, to save his life, threw himself out of the window of a second floor, but fractured his skull by the fall. He was taken up insensible, and immediately put under the care of an eminent surgeon. In the evening of the same day, my friend Dr. Stark (CLIII) acquainted me that, the patient having become emphysematous, and breathing with difficulty, I might now have an opportunity (which he knew I wanted) of seeing such a case. Being at that time engaged, I could not go till the next morning, when I found that he had expired in the middle of the night. His head had just been opened, and a considerable quantity of extravasated blood had been found between the skull and dura mater. On examining the chest, I found the external emphysema but just perceptible, and that only on the right side. On laying open the abdomen, the diaphragm was observed to be depressed and loose on the right side, nearly as it appears when in a dead body a wound is made into the cavity of the chest. This I showed to the gentlemen present, and desired that the body might be left in that condition till I could send for Dr. Hunter, and when he came, the examination was continued. Upon puncturing the thorax, some air rushed out; on laying the chest fully open, the lungs were found to be much collapsed, but there was not any extravasated blood nor lymph; so that it was evident there had been a considerable quantity of air in the cavity. We next examined the containing parts of the thorax, and found the first rib (reckoning from above) fractured near its middle, and the pleura there a little lacerated. We then turned to the lungs, expecting to find them wounded opposite to the fractured rib; but, to our surprise, no wound was discovered in that part. We then looked over the surface of the lungs, and could see no wound; but observed on the concave under part of the lungs, where they are applied to the diaphragm, two or three extravasations of blood, and as many more of air, which had raised the membranous coat of the lungs into vesications about the size of

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(CLIII.) Dr. William Stark and his works are mentioned in Note LX.

one's nail. Next we blew into the lungs, and found that the air escaped readily; and upon examining their surface once more, whilst the air was getting out, we discovered a laceration among the vesications just described, that is, on the middle of that surface of the lungs which lies upon the diaphragm; this wound therefore was not only at a considerable distance from the fractured rib, but remote from the other ribs also. These facts were shown to several gentlemen then present, and seemed to prove what I had supposed.

Now, from considering what the effects of a wound of the lungs must be, from attending to the symptoms of the emphysema, and from our having actually found air in the cavity of the chest, may we not conclude that air is sometimes in emphysematous cases so confined in that cavity as to compress the lungs, disturb their functions, and even to be the cause of death?

And as the paracentesis thoracis has been frequently performed with success for water, and for pus in the cavity of the thorax, in both which cases the parts contained are generally much diseased, may we not propose the same remedy for air; and the rather, as we know that this fluid can be collected there without any previous disorder of the lungs, and consequently that the operation will be attended with a greater probability of success? We shall be confirmed in this opinion when we consider that wounds penetrating the chest, without doing much injury to the lungs, are far from being mortal; insomuch that many instances of cures of such wounds have been observed within our memory, not to mention the numerous cases of this kind related by authors.

In wounds of the lungs, therefore, whether occasioned by fractured ribs or other causes, when symptoms of tightness and suffocation come on, so far should we be from dreading the emphysematous swelling of the cellular membrane, that we should rather consider it as a favorable symptom, showing that the air is not likely to be confined in the thorax; and so far should we be from compressing the wound, to prevent the inflation or emphysema, that we should rather dilate it, (if not large enough already,) or perform the paracentesis thoracis; and we may judge of the necessity of this operation from the violence of the symptoms enumerated above, such as the oppressed breathing, &c.[1] For when these symptoms are not considerable, and the air gets out of the chest with sufficient freedom, the operation then becomes unnecessary; of this the case in the Medical Observations is an evidence; for there the inflation proceeded rapidly, and less air was retained in the chest; so that all that was necessary to be done, was to let it out of the cellular membrane by scarifications, which accordingly were successfully made.

Perhaps it may be inquired here, how the lacerated air-vesicles, which once let out the air, should ever close? To this I should answer, that probably the inflammation, subsequent to the wound of the lungs, closes or unites the divided air-vesicles and small branches of the bronchia in such a manner as to prevent their transmitting air; much in the same way as the wounds made in the cellular membrane by scarifications in anasarcous cases are closed, or as those made in Dr. Hunter's and Mr. Cheston's cases of the emphysema seem to have been.[2] And this appears more evident from observing, that in Dr. Hunter's case the air appears not to have got into the cellular membrane (or out of the wound of the lungs) after the second day, by which time it should seem that the wound of the lungs was sufficiently inflamed to produce the effect above mentioned.

Is it not likewise probable, from the same principles, that the penetrating wound of the chest occasioned by the fractured rib, though at first large enough, may in some cases be so closed by the subsequent inflammation as to prevent the air

[1] It may not perhaps be improper to mention here, that we are not in every penetrating wound of the chest to suppose that the lungs are wounded, even though we see that the air rushes in and out by the wound. This I thought proper to observe, because some, to whom this paper was communicated, objected that they had seen patients in whom the air came out of the chest in expiration, so as to blow off the dressings; yet on compressing the wound, no difficulty of respiration followed. But here it is probable that the lungs were not hurt, and that the air, which had been thrown out by the wound of the thorax in expiration, had entered by it in inspiration, an appearance which will be easily explained by those who are acquainted with the structure and functions of the thorax.

[2] The closing of the scarifications, in Mr. Cheston's case, prevented the air from getting entirely out of the cellular membrane, and he was therefore obliged to repeat that operation to empty it completely.

getting from the thorax into the cellular membrane? And is not this conjecture strengthened by observing that in Mr. Cheston's case the air, on the fourth day, no longer got into the cellular membrane, and that soon after the patient died?

It has been observed that in gunshot wounds, where the ball has passed through the lungs, and in other large penetrating wounds of the chest, the patient has breathed most easily when the external wound has been covered, and has hardly been able to breathe when it was opened. In these circumstances the difficulty of breathing seems to be owing to the air getting into the cavity of the thorax in inspiration, instead of entering the lungs by the trachea, so that the lungs are not distended by the expansion of the chest; and the patient, whilst such a wound is uncovered, is deprived of the use of the lobes of that side, either partially or entirely, according as the wound of the thorax bears a less or greater proportion to the branch of the trachea of that side in which the wound is. Moreover, as the difficulty of breathing in these cases is not considerable whilst the external wound is covered, is it not probable that the divided air-vesicles in most of these deep wounds are closed, either by blood coagulated in the wound, or extravasated into the neighbouring cells, by which means the air is not transmitted through them as through the superficial abrasions? this seems probable from what was observed in Experiments I and II.

The case of the person, whose body I opened, likewise shows that the lungs may be lacerated, and air let loose, without any wound penetrating the cavity of the thorax; for in that instance the laceration was at a distance from the ribs, and was produced by the violence of the concussion only. It therefore intimates the necessity of the operation even in some cases where there is no emphysema, though it must be acknowledged it will not be easy to distinguish them.

As vomicæ and ulcerations of the surface of the lungs are attended with erosion and destruction of the air-vesicles, it may at first sight seem probable that they may also be attended with a discharge of air into the cavity of the thorax; but the probability will be much lessened when we consider, that the inflammation which precedes the formation of pus is likely to do here what it does in the cellular membrane; that is, to condense the adjacent vesicles, and to make the sides of the vomica, or ulcer, adhere to the inside of the parietes of the thorax, thereby limiting the extent of what may be contained in their cavities. Without this, it is probable air might be let loose into the cavity of the thorax, and might produce the symptoms above ascribed to it. For it is not uncommon to see even pretty large branches of the trachea eroded by matter, by which the air gets into the vomicæ or ulcers; of this my ingenious friend Dr. Stark lately showed me an instance. Whether air has ever thus been let loose into the cavity of the thorax, so as to suffocate the patient, I cannot take upon me to determine. But abscesses of the lungs, with adhesion to the parietes of the thorax, have been known to be attended with the emphysema; of this Palfyn mentions an instance,[1] and Dr. Hunter met with a case of the same kind. In these cases the matter of the abscess had probably eroded the pleura and intercostal muscles, and the air which got from the trachea into the abscess, had escaped through the erosions into the cellular membrane, and had inflated it.

Although the operation of the paracentesis thoracis is advisable in most cases where air is contained in the cavity of the chest; yet some may be so complicated with other injuries, that the operation, though in itself proper, may yet be unsuccessful. But this remark may be unnecessary here, as the same may be made of every chirurgical operation; and as men of experience will be cautious how they attribute to the remedy that want of success which may be owing to another cause.

When the operation becomes necessary, the best place for performing it, if the disease is on the right side, will be on the fore-part of the chest, between the fifth and sixth ribs; for there the integuments are thin, and, in the case of air, no depending drain is required. But, if the disease is on the left side, it will be more advisable to make the opening between the seventh and eighth, or eighth and ninth ribs, that we may be sure of avoiding the pericardium. With regard to the size of the wound, it may be proper to observe, that as large penetrating wounds of the chest are inconvenient on account of the air's entering by the aperture in such a quantity as to prevent

[1] Anatomie Chirurgicale, cap. xx, p. 2.

the expansion of the lungs, a small wound will therefore be eligible, and especially as the fluid requires not a large vent for its discharge. Lastly, as to the manner of performing the operation, I should think it more advisable to do it with a knife, by a cautious dissection, than by the more coarse and hazardous method, the thrusting in a trocar.

Fig 1.

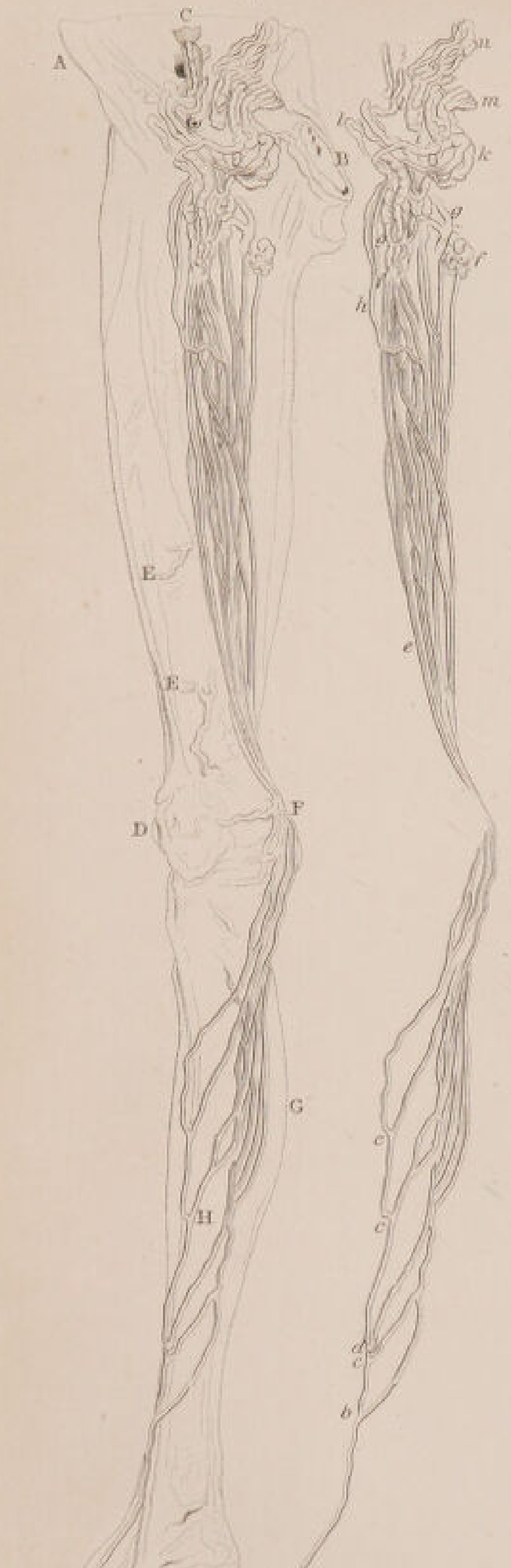


Fig 2.

H. Adlard. sc

DESCRIPTION OF THE PLATES.

PLATE I.

Fig. 1. Exhibits the more superficial Lymphatic Vessels of the lower Extremity.

A, The spine of the os ilium.
B, The os pubis.
C, The iliac artery.
D, The knee.
E, E, F, Branches of the crural artery.
G, The musculus gastrocnemius.
H, The tibia.
I, The tendon of the musculus tibialis anticus.

On the Outlines.

a, A lymphatic vessel belonging to the top of the foot.
b, Its first division into branches.
c, c, c, Other divisions of the same lymphatic vessel.
d, A small lymphatic gland.
e, The lymphatic vessels which lie between the skin and the muscles of the thigh.
f, f, Two lymphatic glands at the upper part of the thigh below the groin.
g, g, Other glands.
h, A lymphatic vessel which passes by the side of those glands without communicating with them; and, bending towards the inside of the groin at *i*, opens into the lymphatic gland *k*.
l, l, Lymphatic glands in the groin, which are common to

the lymphatic vessels of the genitals and those of the lower extremity.

m, *n*, A plexus of lymphatic vessels passing on the inside of the iliac artery.

N.B. The lymphatic vessels appear in these plates more regularly cylindrical than they are represented by Nuck, Ruysch, and others, in whose plates such vessels are painted more like chains of vesicles than I have ever seen them.

Fig. 2. Exhibits a Back View of the lower Extremity, dissected so as to show the deeper-seated Lymphatic Vessels which accompany the Arteries.

N.B. This extremity was dried before the plate was made from it, and the muscles are therefore much shrunk.

A, The os pubis.

B, The tuberosity of the ischium.

C, That part of the os ilium which was articulated with the os sacrum.

D, The extremity of the iliac artery appearing above the groin.

E, The knee.

F, F, The two cut surfaces of the triceps muscle, which was divided to show the lymphatic vessels that pass through its perforation along with the crural artery.

G, The edge of the musculus gracilis.

H, The gastrocnemius and soleus, much shrunk by being dried, and by the soleus being separated from the tibia to expose the vessels.

I, The heel.

K, The sole of the foot.

L, The superficial lymphatic vessels passing over the knee, to get to the thigh.

On the Outlines.

M, The posterior tibial artery.

a, A lymphatic vessel accompanying the posterior tibial artery.

b, The same vessel crossing the artery.

c, A small lymphatic gland, through which this deep-seated lymphatic vessel passes.

d, The lymphatic vessel passing under a small part of the soleus which is left attached to the bone, the rest being removed.

e, The lymphatic vessel crossing the popliteal artery.

f, g, h, Lymphatic glands in the ham, through which the lymphatic vessel passes.

i, The lymphatic vessel passing with the crural artery through the perforation of the triceps muscle.

k, The lymphatic vessel after it has passed the perforation of the triceps, dividing into branches which embrace the artery *l*.

m, A lymphatic gland belonging to the deep-seated lymphatic vessel. At this place those vessels pass to the fore part of the groin, where they communicate with the superficial lymphatic vessels.

n, A part of the superficial lymphatic vessels appearing on the brim of the pelvis.

PLATE II,

Exhibits the Trunk of the Human Subject, prepared to show the Lymphatic Vessels and the Ductus Thoracicus.

A, The neck.

B, B, The two jugular veins.

C, The vena cava superior.

D, D, D, D, The subclavian veins.

E, The beginning of the aorta pulled to the left side by means of a ligature, in order to show the thoracic duct behind it.

F, The branches arising from the curvature of the aorta.

G, G, The two carotid arteries.

H, H, The first ribs.

I, I, The trachea.

K, K, The spine.

L, L, The vena azygos.

M, M, The descending aorta.

N, The cœliac artery dividing into three branches.

O, The superior mesenteric artery.

P, The right crus diaphragmatis.

Q, Q, The two kidneys.

R, The right emulgent artery.

S, S, The external iliac arteries.

g, *d*, The musculi psoæ.

T, The internal iliac artery.

U, The cavity of the pelvis.

X, X, The spine of the os ilium.

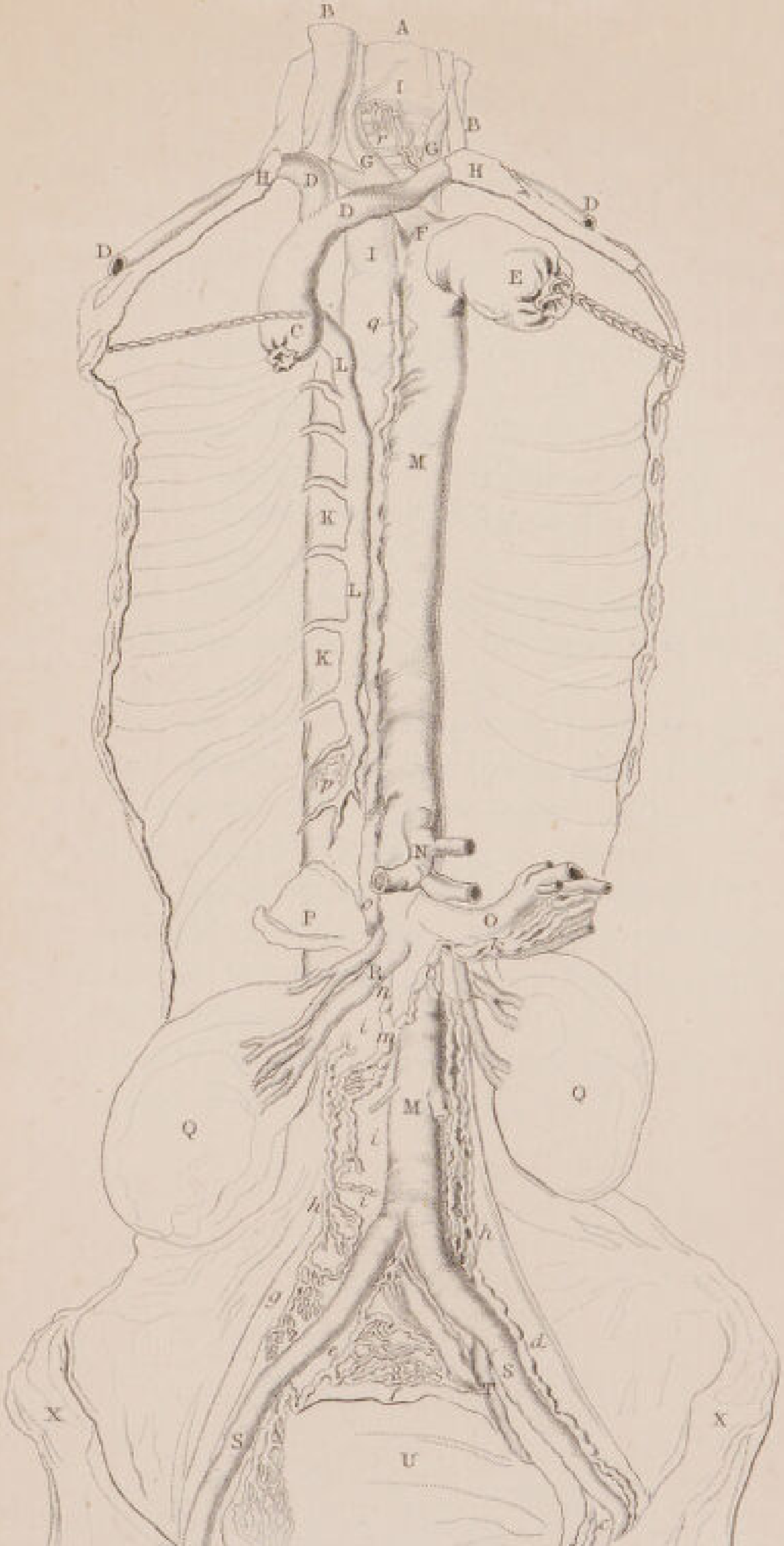
Y, Y, The groins.

a, A lymphatic gland in the groin, into which lymphatic vessels from the lower extremity are seen to enter.

b, *b*, The lymphatic vessels of the lower extremities passing under Poupart's ligament.

c, *c*, A plexus of the lymphatic vessels lying on each side of the pelvis.

d, The psoas muscle with lymphatic vessels lying upon its inside.



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H. Adlard sc

e, A plexus of lymphatics, which having passed over the brim of the pelvis at *c*, having entered the cavity of the pelvis, and received the lymphatic vessels belonging to the viscera contained in that cavity, next ascends, and passes behind the iliac artery to *g*.

f, Some lymphatic vessels of the left side passing over the upper part of the os sacrum, to meet those of the right side.

g, The right psoas muscle, with a large plexus of lymphatics lying on its inside.

h, *h*, The plexus lying on each side of the spine.

i, *i*, *i*, Spaces occupied by the lymphatic glands.

k, The trunk of the lacteals lying on the under side of the superior mesenteric artery.

l, The same dividing into two branches, one of which passes on each aide of the aorta; that of the right side being seen to enter the thoracic duct at *m*.

m, The thoracic duct beginning from the large lymphatics.

n, The duct passing under the lower part of the crus diaphragmatis and under the right emulgent artery.

o, The thoracic duct penetrating the thorax.

p, Some lymphatic vessels joining that duct in the thorax.

q, The thoracic duct passing under the curvature of the aorta to get to the left subclavian vein; the aorta being drawn aside to show the duct.

r, A plexus of lymphatic vessels passing upon the trachea from the thyroid gland to the thoracic duct.

PLATE III,

Exhibits the upper part of the Thoracic Duct, the Lymphatic Vessels of the upper part of the Chest and Arms, and of the lower part of the Neck.

A, The neck.

B, B, The two shoulders with the pectoral muscles turned over them.

C, C, The arms.

D, D, The cut ends of the clavicles.

E, E, The extremities of the first two ribs.

F, F, The subclavian muscles.

G, G, The ribs.

H, The trachea.

I, The aorta ascendens.

K, The aorta descendens.

L, The trunk of the carotid, and subclavian artery of the right side.

M, The left carotid.

N, N, The left subclavian artery.

O, The vena cava superior.

P, P, The trunks of the subclavian and jugular veins.

Q, The right subclavian vein.

R, The right jugular vein.

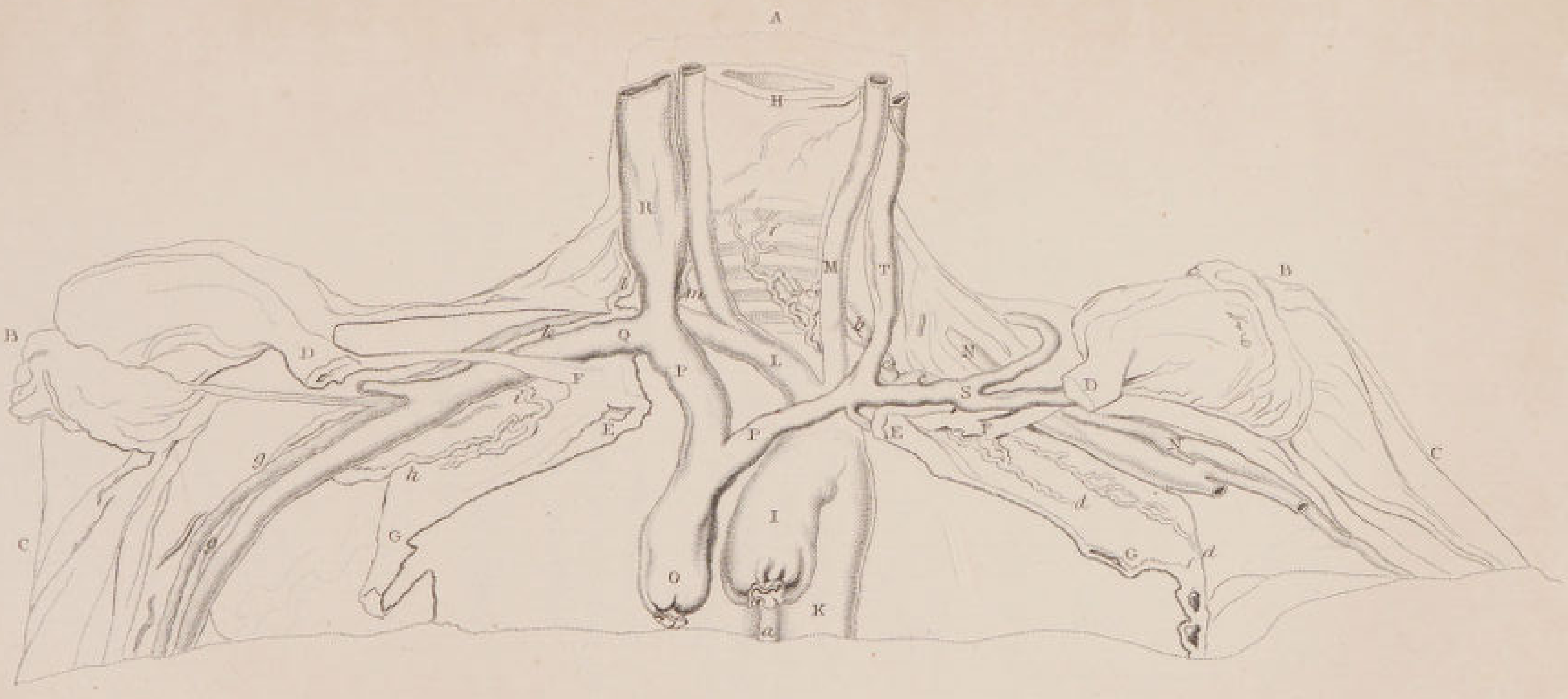
S, The left subclavian vein.

T, The left jugular.

a, The thoracic duct passing on the right side of the descending aorta K, behind the ascending aorta I, and behind the lower part of the left carotid artery M, and then appearing at *b*.

b, The upper part of the thoracic duct lying between the left carotid and the left jugular vein, and passing behind that vein downwards and outwards towards the angle between the left jugular and the left subclavian.

c, The extremity of the thoracic duct entering the angle between the left jugular and the left subclavian vein.



T. Bonner, del[t]

H. Adlard, sc

d, d, Two of the trunks of the lymphatics of the left arm lying upon the outside of the chest, and passing under the subclavian muscle F, and the subclavian vein S, the clavicle itself being removed, and its cut extremity seen at D.

e, A trunk formed by the lymphatics of the upper extremity, which trunk joins the extremity of the thoracic duct *c*, and enters the angle between the left jugular and the left subclavian vein.

f, Lymphatics from the thyroid gland running upon the trachea, and passing under the aorta to get to the thoracic duct, just where that duct enters the veins.

g, g, A trunk of the lymphatics of the right arm lying on the outside of the right brachial artery.

h, A branch of this trunk making a network on the outside of the thorax just under the clavicle.

i, That network passing under the right subclavian vein Q, and under the subclavian muscle F, the clavicle itself being removed.

k, The common trunk both of that plexus and of all the other lymphatics of the upper extremity of the right side, which trunk lies between the right subclavian artery and vein, and passes into the angle between the right jugular and the right subclavian.

l, The trunk of the lymphatics of the right side of the neck lying on the outside of the right jugular vein, and passing into the angle between that vein and the subclavian of the same side.

m, One of the lymphatics of the right side of the thyroid gland going under the right jugular vein.

PLATE IV.

Fig. 1, exhibits a Back View of the Fore-arm and Hand. The Preparation from which this View was taken having been previously dried, the Muscles appear very slender.

A, The hand.
B, The lower extremity of the radius.
C, The lower extremity of the ulna.
D, The muscles on the back of the fore-arm turned aside to exhibit a deep-seated lymphatic vessel, which perforates the interosseous ligament to get to the fore part.
E, The olecranon.

On the Outlines.

a, a, a, Lymphatics appearing on the back of the fore-arm, immediately under the skin.

b, Some of the lymphatics bending over the upper extremity of the radius to get to the fore part of the arm.

c, A lymphatic passing over the ulna, immediately under the olecranon, and under the inner condyle of the os humeri, to get to the fore part of the arm.

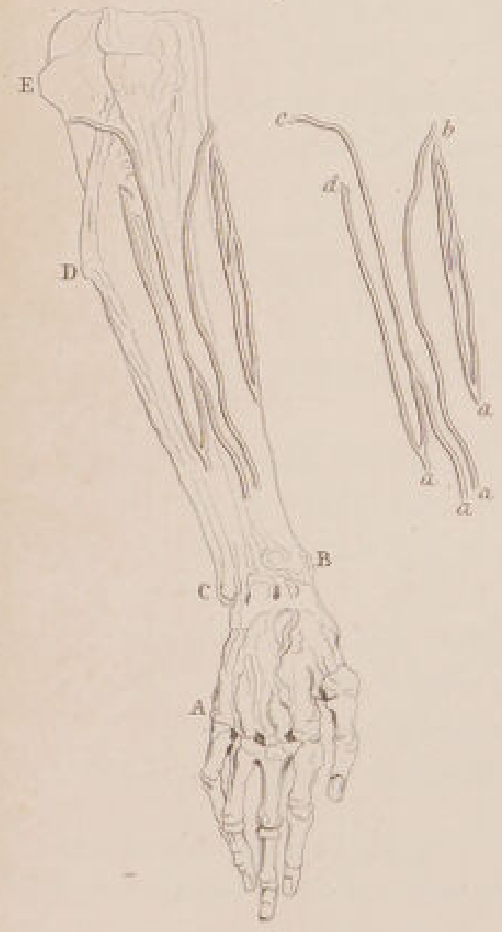
d, A lymphatic which has penetrated the muscles, perforates the interosseous ligament, and gets to the fore part of the arm near the radial artery.

Fig. 2, exhibits a fore View of the upper Extremity. This Plate was likewise made from a dried Preparation, and the Muscles therefore appear very small. It has a peculiarity in the ulnar artery running over the muscles instead of under them.

A, The scapula.
B, The clavicle.
C, The extremity of the brachial artery.
D, The muscles lying on the inside of the arm.

Fig 2.

Fig 1.



H. Adlard sc.

E, The inner condyle of the os humeri.

F, The lower extremity of the radius.

On the Outlines.

a, A lymphatic vessel which lies in the cellular membrane immediately under the skin, and passes up on the inside of the arm to the axillary glands.

b, Superficial lymphatic vessels passing over the muscles from the back of the fore-arm, and likewise over the biceps to the glands in the axilla.

c, A superficial lymphatic from the back of the fore-arm.

d, A gland through which it passes.

e, The lymphatics from the anterior and the posterior part of the fore-arm uniting.

f, f, Lymphatic glands in the axilla.

g, A deeper-seated lymphatic vessel lying close to the radial artery, which it accompanies all the way to *h*.

h, The deep-seated lymphatic vessel passing under the inter-osseous and ulnar arteries, and appearing again on the arm at *i*.

i, The deep-seated lymphatic vessel lying close to the brachial artery.

k, k, Two small lymphatic glands through which it passes.

l, The same vessel now become much larger, and passing under a branch of the artery and some cellular membrane, and appearing at *m*.

m, The trunk of the deep-seated lymphatic vessels passing upwards to the axilla, where it enters the glands *f, f*.

f, f, Three axillary glands, which are common both to the superficial and the deep-seated lymphatic vessels.

Note. In the figures of this plate the lymphatic vessels are shown somewhat larger in proportion to the limb than natural.

PLATE V.

A comparative View of the Flat Vesicles of the Blood in different Animals, exhibiting their Size and Shape (CLIV) as they appear through a Lens $\frac{1}{23}$ of an inch focus.

FIG. 1, Their size in an ox, a cat, an ass, a mouse, and a bat.

FIG. 2, Their size in a man, in a rabbit, a dog, and a porpoise (CLV).

FIG. 3, Their size in birds; viz. a pigeon, a hen, a chaffinch, and a duck.

FIG. 4, Their size in a chick, from the egg, on the sixth day of incubation (see Note CXVI, p. 233).

FIG. 5, Their size in the common fish, as the salmon, carp, eel.

FIG. 6, Their size in a full grown viper and in a turtle.

FIG. 7, Their size in a small viper taken from the belly of its mother (see Note CXVI, p. 233).

FIG. 8, Their size in a slow-worm (CLVI).

(CLIV.) See the copious Tables of Measurements of the blood-corpuscles of vertebrate animals, Note CXVIII*, pp. 237 et seq.; and the Notes there referred to on the size and shape of the corpuscles.

(CLV.) The central spot in the blood-corpuscles of mammalia should not be confounded with the nucleus of the blood-corpuscle of oviparous vertebrata; see Notes XCVI and CII.

(CLVI.) The Tables of Measurements, at p. 242, show that the red corpuscle of the slow-worm (anguis fragilis) is a narrower ellipse than either the corpuscle of the snake, of the adder, or of the boa, as explained in my Paper on the Blood-Corpuscles of Ophidian Reptiles.[a] Other well-

[a] Proceedings of the Zoological Society, August 9, 1842.

Fig. 1.

Fig. 2.

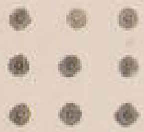


Fig. 3.

Fig. 4.

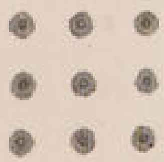


Fig. 5.

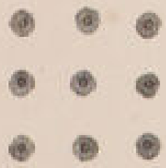


Fig. 6.

Fig. 7.

Fig. 8.

Fig. 9.

Fig. 10.

Fig. 11.

Fig. 12.

Fig. 13.

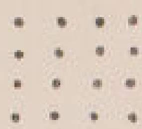


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Fig. 9, Their size in a frog.

Fig. 10, Their size in a skate.

Fig. 11, Their size in a lobster.

Fig. 12, The vesicles of the same lobster, as they appear after being a short time exposed to the air.

Fig. 13, The size of the globules of milk.

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marked examples of a difference of shape in the blood-corpuscles of nearly allied animals occur among birds: the snowy owl, the passenger pigeon, the butcher bird, are all distinguishable by the longer oval form of their blood-corpuscles from their congeners. See Note xcviii, pp. 218-19.

PLATE VI,

FIG. 1, Exhibits the Lymphatic Vessels of the lower Extremity, injected with Mercury, passing through a Cluster of Lymphatic Glands taken from the Groin. The Arteries and Veins were not injected in this Preparation.

a, a, a, a, a, a, a, a, a, a, a, a, a, a, a, a, a, a, the lymphatic vessels.

b, b, b, b, b, b, b, b, b, the lymphatic glands seen of different sizes and shapes, with the lymphatic vessels passing through them.

A, B, C, D, E, Lymphatic glands that have a cellular appearance.

F, G, Lymphatic glands evidently formed of convoluted lymphatic vessels; these likewise show the manner in which the vessels enter and pass through the glands.

H, H, Lymphatic vessels running to the edges of the two lymphatic glands not injected.

I, I, Lymphatic glands uninjected.

FIG. 2, Exhibits a Lymphatic Vessel that forms three small Lymphatic Glands.

A, A lymphatic vessel.

B, A gland made by a subdivision of the trunk A into nine or ten branches, which are convoluted; these reuniting, form the lymphatic vessel C.

C, A lymphatic vessel ascending to form the gland D.

D, A lymphatic gland.

E, A lymphatic vessel formed by the union of the lymphatic vessels that composed the gland D, which divides into the two branches F and G.

F, The lymphatic vessels subdivided into small branches to form the third lymphatic gland H.

H, The third lymphatic gland.

Fig.1.

Fig.2.

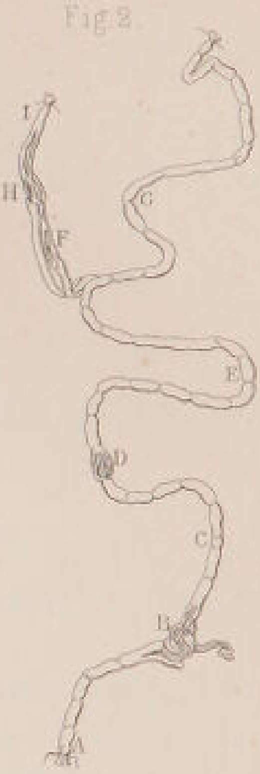


Fig.3.

Fig.4.

Fig.5.

Jas. Turner. del.

H. Adlard. sc.

I, A lymphatic vessel formed from the gland H, which passed on to communicate with other lymphatic vessels, not expressed in this plate.

G, A lymphatic vessel.

FIG. 3, Exhibits the manner in which a single Lymphatic Vessel enters and passes through a Lymphatic Gland.

a, The trunk of a vessel filled with mercury.

b, The division of the trunk into four branches before it enters the gland.

c, The gland, with the lymphatic vessels passing through it.

d, The lymphatic vessels having passed through the gland, form four vessels on the opposite side, to which they had entered the gland. These vessels unite to form the trunk *e*.

e, The lymphatic vessel, having passed through the gland, is become larger than before it entered the gland.

FIG. 4, Exhibits a Lymphatic Gland with the Lymphatic Vessels injected with Mercury, in which the Subdivision of the larger Vessels into smaller Branches, running in a serpentine Direction through the Gland, is apparent to the naked Eye.

FIG. 5, Exhibits Lymphatic Vessels and Glands, taken from the Axilla in which the Subdivision of the Trunks into Branches, and their frequent communication with each other, may be traced distinctly.

a, *a*, *a*, The trunks of five lymphatic vessels that come from the arm into the axilla, and as they ascend, divide into many branches, which make frequent anastomoses, and then form the plexus A.

A, A plexus of lymphatic vessels dividing into two parts *b*, *c*.

b, A cluster of lymphatic vessels that run parallel to each other.

d, Lymphatic vessels that surrounded the artery, and passed on to lymphatic glands not represented.

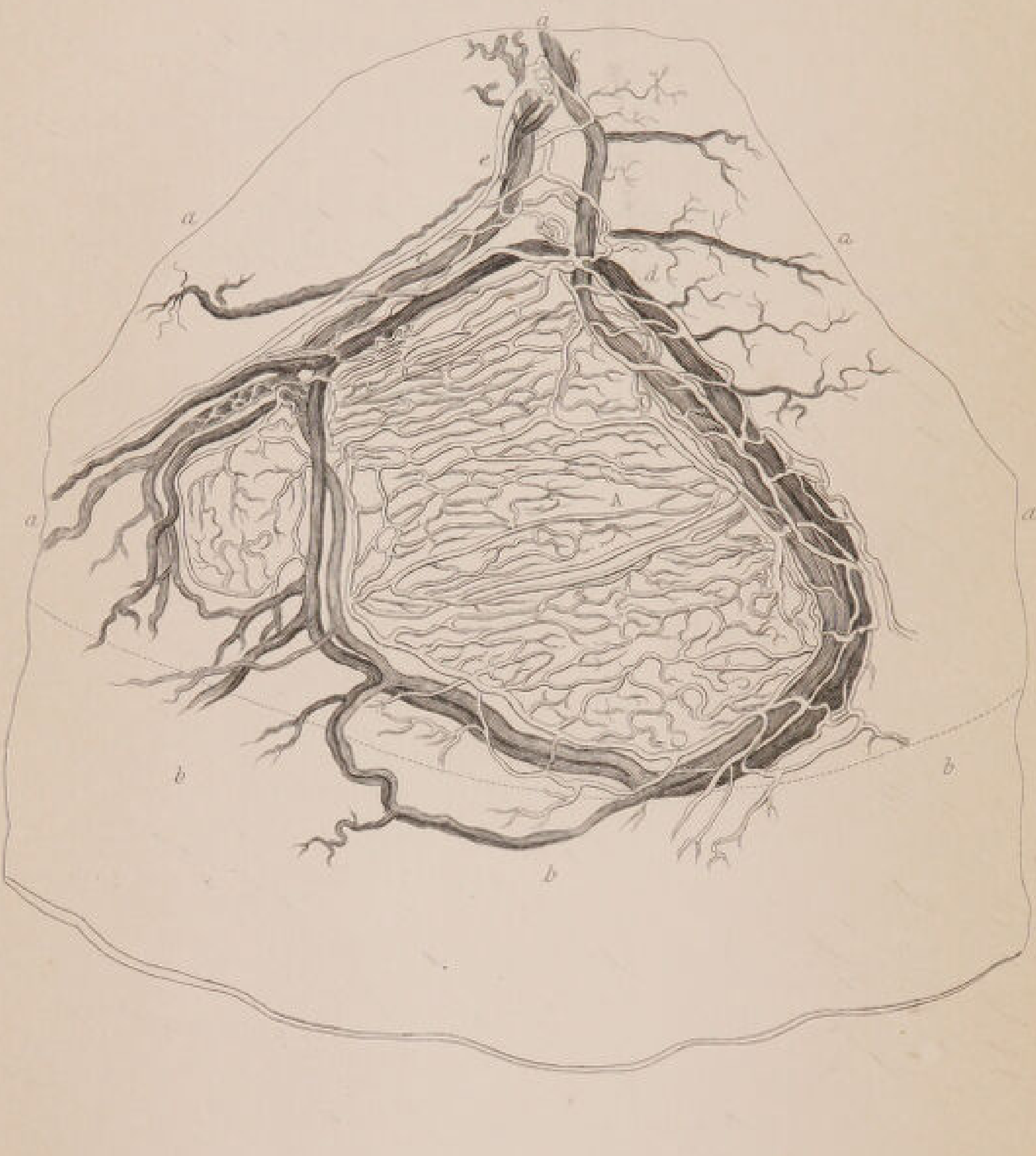
e, *f*, *g*, Lymphatic vessels that form the gland *h*.

i, The lymphatic vessel arising from the gland *h*.

c, A plexus of lymphatic vessels communicating frequently with each other, and then ascending, divides into the branches *k*, *l*, *l*.

k, *l*, *l*, Lymphatic vessels that passed on to open into the angle between the jugular and subclavian vein, on the right side, without entering any lymphatic gland in their passage.

Note. All the objects in this plate are exhibited of a reduced size.



Jas. Turner, del. R. Allard, sc.

PLATE VII,

Exhibits a reduced view of a part of the Mesentery of a Turtle injected. The Arteries were filled with red Wax, the Veins with black, and the Lacteals with Mercury.

The mesenteric artery and vein, as they pass on to the gut between two folds of the peritoneum, divide into branches that make frequent anastomoses. These larger vessels leave spaces that resemble a network, and form meshes of different sizes. These in the unprepared state are transparent; but when the blood-vessels are minutely injected, we observe several small arteries and veins ramifying to the utmost minuteness. And from the transparency of the peritoneum, we can here distinctly trace the artery terminating in the vein by continuity of canal. It is on these meshes that the lacteals, as they come from the gut upon the mesentery, divide into innumerable branches, that communicate frequently with each other, and form a beautiful plexus of lacteal vessels. That in its office, we consider analogous to the lymphatic glands, seen in the mesentery of other animals.

a, a, a, a, a, The outline of the mesentery.

b, b, b, The intestine.

c, The artery.

d, The vein.

e, The lacteals surrounding the artery and vein.

A, A mesh of the mesentery on which a plexus of lacteal vessels is delineated.

PLATE VIII.

Fig. 1, Exhibits the Chest of a Still-born Child, opened to show the Situation of the Thymus Gland.

a, a, a, a, The skin, muscles, and ends of the ribs, the sternum and cartilaginous part of the ribs being removed.

b, b, b, b, The edge of the diaphragm.

c, c, The right and left lung.

d, The pericardium.

e, The heart.

f, Part of the first bone of the sternum.

g, g, g, The thymus gland.

Fig. 2, Exhibits the Size and Figure of the Vesicles of the Human Blood, as they appeared in the microscope when viewed through a lens the $\frac{1}{23}$ of an inch focus. The solid central particle, as formerly described, is seen distinctly in the centre of each vesicle (see Notes xcvi and cii).

Fig. 3, Represents the Size and Figure of the Particles (see Note cxxii, p. 253) found in a Lymphatic Gland, taken from the Human Body, as they appeared viewed by a strong sun-light, through the same lens used in the former experiments.

Fig. 4, Represents a portion of a Lymphatic Gland from the Human Subject greatly magnified, in which the cells of the Lymphatic Gland are shown.

Fig 1.

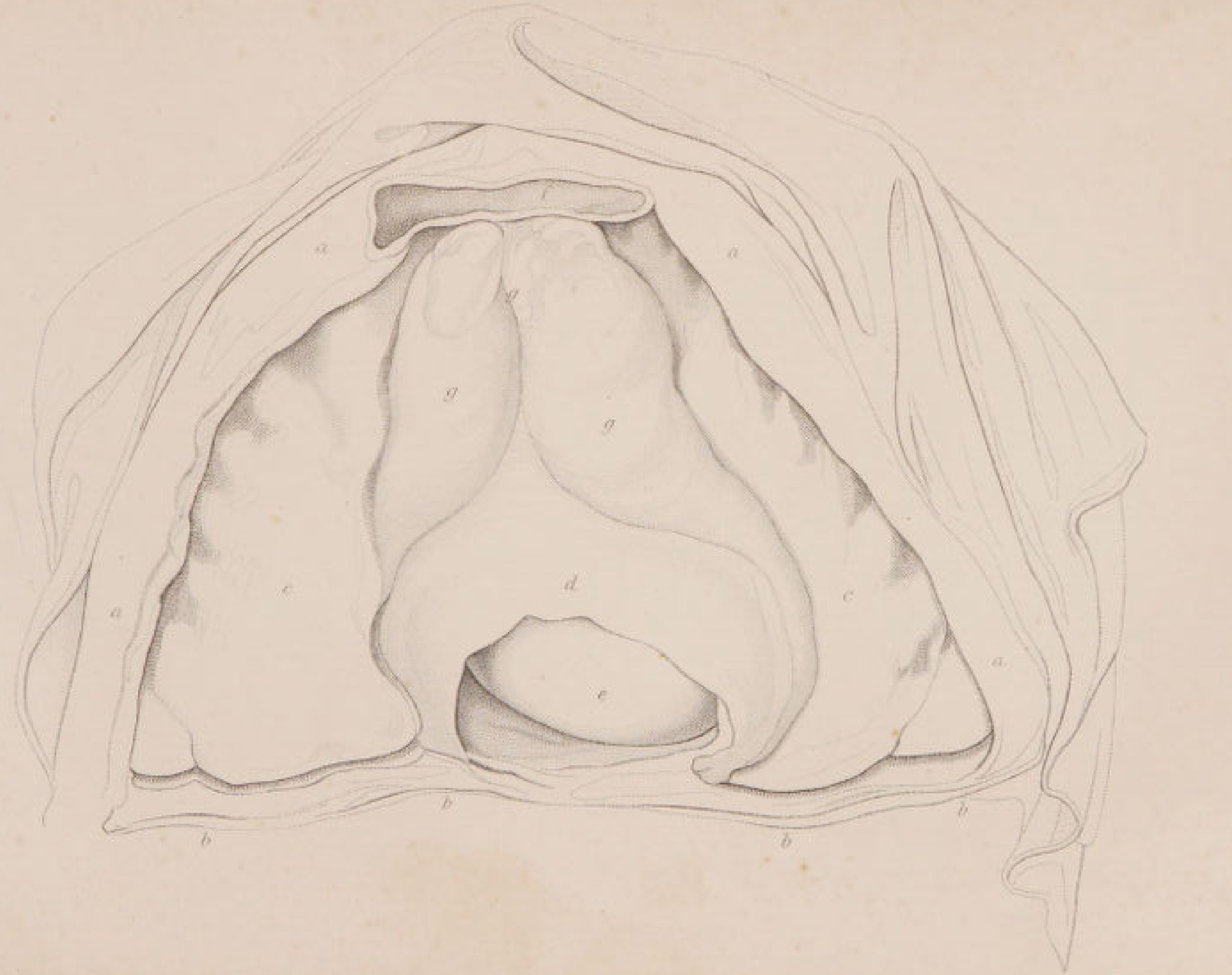


Fig 2.

Fig 3.

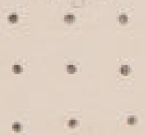


Fig 4.

Fig 5.

Fig 6.

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H. Adlard sc.

Fig. 5, The Size and Figure of the Vesicles of the Blood, taken from a common fowl.

Fig. 6, Exhibits the Size and Form of Particles (see Note cxxii, p. 253) taken from a Lymphatic Gland, found on the neck of the fowl whose blood is expressed in fig. 5.

Note. All the Experiments were made by a clear daylight, and the objects viewed through the same lens, viz. $\frac{1}{23}$ of an inch focus, except Plate viii, fig. 4, which was examined with a lens the $\frac{1}{30}$ of an inch focus.

GENERAL INDEX.

THE END.

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31