An account of the life, lectures and writings of William Cullen. Vol.1 / by John Thomson.

Contributors

Thomson, John, 1765-1846.

Publication/Creation

Edinburgh : Blackwood ; London : Cadell, 1832.

Persistent URL

https://wellcomecollection.org/works/bq74yfjj

License and attribution

This work has been identified as being free of known restrictions under copyright law, including all related and neighbouring rights and is being made available under the Creative Commons, Public Domain Mark.

You can copy, modify, distribute and perform the work, even for commercial purposes, without asking permission.

wellcome collection

Wellcome Collection
183 Euston Road
London NW1 2BE UK
T +44 (0)20 7611 8722
E library@wellcomecollection.org
https://wellcomecollection.org

Digitized by the Internet Archive
in 2018 with funding from
Wellcome Library

https://archive.org/details/b29347178

AN

ACCOUNT

OF THE

LIFE, LECTURES AND WRITINGS,

OF

WILLIAM CULLEN, M. D.

PROFESSOR OF THE PRACTICE OF PHYSIC IN THE UNIVERSITY OF EDINBURGH.

BY

JOHN THOMSON, M. D., F. R. S. L. & E.

PROFESSOR OF MEDICINE AND GENERAL PATHOLOGY IN THE UNIVERSITY OF EDINBURGH.

IN TWO VOLUMES.

VOL. I.

WILLIAM BLACKWOOD, EDINBURGH; AND

T. CADELL, STRAND, LONDON.

MDCCCXXXII.

WELLCOME
HISTORICAL
MEDICAL
LIBRARY

PRINTED BY NEILL & CO.

OLD FISHMARKET.

TO

JOHN ALLEN, Esq.

MASTER OF DULWICH COLLEGE,

&c. &c. &c.

Dear Allen,

I beg that you will regard the Inscription to You of this Work as a sincere though but feeble expression of my admiration of your intellectual endowments, of my esteem for your private virtues, and your inflexible integrity of character, and of the value I attach to the Friendship that has subsisted between us for more than forty years,—a friendship which has been to me the source of so many agreeable associations, and has had so powerful an influence over the fortunes of my life.

Yours faithfully,

JOHN THOMSON.

80. George Street,
Edinburgh, 13*th April* 1832.

ADVERTISEMENT.

AMONG the many eminent Teachers of Medicine to whom this country has given birth, there is certainly no one who, by his Lectures and Writings, has had a greater influence on the opinions and practice of medical men, and on the general progress of medical science, than the late Dr CULLEN. It may be justly, therefore, matter of surprise, that so many years have been suffered to elapse since his death, without the publication of any authentic or correct history of his life and studies. To account in some measure for this, it may be mentioned that his eldest son, the late Lord Cullen, entertained the laudable desire to write a biographical memoir of his father, a duty for which he was singularly well qualified, by the intimate knowledge which he had of many interesting circumstances regarding the life, studies, and habits of his distinguished Parent, as well as by his own acknowledged talents for literary composition, and for the observation and delineation of character. From having this object in view, he was led to decline several offers to execute

that undertaking made to him by Medical men, who had enjoyed the personal acquaintance of Dr Cullen, and who were desirous of being furnished with the materials necessary for writing his life. That Lord Cullen had not accepted of some of these offers is the more to be regretted, that, on his death, it did not appear that he had himself made any progress in the execution of his design.

Soon after Lord Cullen's death, Dr Cullen's papers, consisting of letters from private friends, sketches of essays, notes of lectures, and medical consultations, were placed in my hands by his surviving family, with a request, that I would endeavour to draw up from these documents, and from the information I should be able to procure from other sources, such an account of his Life, Lectures and Writings as might in some degree satisfy the curiosity of the Public. In complying with this request, I was not altogether unaware of the difficulties with which an attempt to write the life of a medical practitioner, teacher and philosopher, who held so high a place in the public estimation, must necessarily be attended,—difficulties presenting themselves the more forcibly to me that I had not myself enjoyed the advantages of Dr Cullen's personal acquaintance, and could scarcely expect, after the lapse of so many years, even to have an opportunity of conversing with any of those who had known him in the early and more

interesting part of his career. Indeed, the difficulties which I have experienced in obtaining correct information, and the consideration of the variety and extent of the scientific and professional subjects with which Dr Cullen seems through the whole of his long life to have been constantly occupied, have made me feel more sensibly, as I have proceeded, how imperfectly I was qualified for the task I had undertaken, and have rendered me sometimes doubtful whether I ought not to abandon it altogether. But my increasing admiration of his genius and labours, and the desire excited by the perusal of his papers and writings, to do justice to his memory, have urged me to persevere; assured, that however much I may fail in performing what is due to him, and in satisfying the wishes of those who take an interest in the progress of Medical Science, I shall at least have the satisfaction to know that, in the narrative, which I now submit with much diffidence to their judgment, I have not stated any thing with regard to Dr Cullen for which I have not undoubted authority; and the pleasure to hope that the statements I am about to give, may tend to correct several mistakes and misrepresentations in the very erroneous accounts of his Life which have hitherto been given to the Public.

In prosecuting the undertaking in which I had engaged, I found that there grew upon my hands a large mass of materials, in the form of extracts from Dr

CULLEN'S MS. Lectures, explanatory of his medical opinions, which would far exceed the bounds that could be allotted to them in a biographical memoir, but which, I conceived, might be advantageously submitted to the Medical Profession, if incorporated with his published writings. Under this impression, I prepared the edition of Dr CULLEN'S Works that was published in 1827,—a task, in the performance of which I received much assistance from two of my young friends, the late Dr DONALD MACINTOSH of this place, and Dr FERDINAND W. BECKER of Berlin.

I may be allowed to add, that, amidst the various distractions necessarily arising from a multiplicity of professional duties as a practitioner, a teacher, and an author, and with such interruptions and delays, some of them of several years' duration, as have from time to time occurred, I have been enabled to carry on the inquiries connected with Dr CULLEN'S Life, by the assistance which has of late years been rendered me by my eldest SON; and at length to execute that portion of it which is now published, in the hope of our being able, at no great distance of time, to bring out the remaining volume.

TABLE OF CONTENTS.

Dr Cullen's Birth and Parentage, page 1.—Education at the Grammar-School at Hamilton and the University of Glasgow, 2.—Visit to London and Voyage to the West Indies, 4.—Return to Scotland, and residence at Auchinlee, 6.—Attendance at the University of Edinburgh, 8.—Settlement at Hamilton, 11.—Connexion with Dr William Hunter, 12.—Marriage, 16.

Removal from Hamilton to Glasgow, 17.—Lectures on the Theory and Practice of Physic, 20—on Chemistry, 29—on Materia Medica and Botany, 32.—Plan of his Lectures on Chemistry, 35.—Connexion with Dr Joseph Black, 45.—Investigation of the doctrines of Heat, Evaporation, &c., 51.—Intercourse with Lord Kames on Chemistry and Agriculture, 60.—Intercourse with M. Duhamel, 60.—Origin of his acquaintance with the Duke of Argyll, 70.—Admission into the University of Glasgow, 70.—Intercourse with Dr Adam Smith and Mr David Hume, 71.—Experiments on Bleaching and on the manufacture of Salt, 74.—Intention to publish an edition of Sydenham's Works, 79.

Desire of removing from Glasgow to Edinburgh, 82.—Canvass for the Chair of Chemistry in Edinburgh, 86.—Removal to Edinburgh, 93.—Proposal to publish a translation of Van Swieten's Commentaries, 99.

Delivery of Clinical Lectures, 101.—History of Clinical Teaching, 102.—De Haen's Ratio Medendi, 105.—Dr Cullen's qualifications for teaching Clinical Medicine, and success in the performance of this duty, 107.—Prejudices against him, excited by the novelty of his doctrines, 117. Attachment

of his pupils, 122.—Correspondence with Dr George Fordyce and Dr Balfour Russel, 122.—Kind behaviour towards young persons, 134.—Account of this given by Dr Aiken and by Dr Anderson, 134.—Anecdote by Mr Dugald Stewart, 136.—Interest taken by Dr Cullen in the Philosophical Society, 137.—Intercourse with Dr Franklin, 139.

Course of Lectures on Materia Medica, 141.—Surreptitious publication of these Lectures, 143.

Canvass for the Chair of the Practice of Physic, 144.—Dr Gregory's appointment, 152.—Dr Cullen's Acceptance of the chair of the Theory of Physic, 153.—Arrangement with Dr Gregory for lecturing alternately on the Theory and Practice, 161.—Appointment as sole Professor of the Practice on Dr Gregory's death, 161.

State of Medical Science when Dr Cullen began to teach, 162.—Doctrines of Stahl respecting the influence of the Soul in the Animal Economy, 164.—Their relation to those of Aristotle, 167.—Of Galen, 170.—Of other Physiologists, 171.—Opposed to those of Des Cartes, 175.—Stahl's Theory of Disease, 177.—Influence of his opinions on Medical Practice, 179.

Doctrines of Hoffmann, 182.—Their relation to those of Aristotle and Stahl, 184.—Doctrines relative to the Properties and Nature of the Sentient Soul, 185—to its Seat and Creation, 193.—Impulse given by Hoffmann to the study of the Nervous System, 195—his Theory of Disease, 197—his opinions propagated by Rega and Dr Cullen, 200.

Doctrines of Boerhaave—his view of the difference between Mind and Matter, 202.—Account of the Mental Faculties, 203.—Signification attached by him to the term Idea, 204.—Subservience of different portions of the Nervous System to different functions, and difference between Nerves of Sensation and of Motion, 205.—Boerhaave's opinions respecting the application of Natural Philosophy and Chemistry to Physiology and Pathology, 208—his Pathological doctrines, 214.—Improvements introduced into the System of Boerhaave by De Gorter, 218—by Kaau Boerhaave, 219—by Gaubius, 220.

Doctrines of Haller, 221.—Experimental researches, and general opinions, 223.—Doctrine of Sensibility, 228.—Doctrine of Irritability, 228.—Opinions of Galen respecting Muscular Contraction, 228—of Glisson, 231.—Investigations of Haller, 234.—Attempts to prove that the muscles possess a power of contraction independently of the Nervous system, 236.

Doctrines of Whytt, 241.—Organic functions attributed by him to the agency of the Mind, 243.—Difference between his opinion and that of Stahl, 244.—Opposition to Haller's doctrine of a *Vis Insita* in the Muscles, 249—his theory of Sympathy, 250.

Dr Cullen's Lectures on the Institutions of Medicine, 258.

Lectures on Physiology, 260.—Division of the Physiological part of the Course, 261.—Observations on the Simple Solids, 263.—Account of the Nervous System, 264.—Division of it into four parts, 265.—Opinion of the Nerves of Sensation and Motion being different, opposed by Haller and Cullen, maintained by others, 266.—Of the Nervous System as the connecting medium between the Immaterial and Material parts of Man, 270—as the organ of Sensation, 273—of our Intellectual operations, 279—of the Voluntary, Involuntary, Mixed and Sympathetic motions of the animal economy, 284.—Causes exciting the Brain to produce the motions of the several parts of the body, 289.—The Will, 289.—Emotions and Passions, 291.—Principle of Imitation, 293.—Appetites, 294.—Propensities, 294.—Certain internal impressions occurring naturally in the economy, 295.—Occasional impressions and states, 300.—*Vis Medicatrix Naturæ*, Direct and Indirect stimulants, 302.—Doctrine of Consent or Sympathy, 305.—States of Sleeping and Waking, and doctrine of Excitement and Collapse, 309.—Effects of Custom on the corporeal and mental functions, 318.

Lectures on Pathology, 325.—Character of Gaubius, 326.—Object of General Pathology, 327.—Definition of Disease, 328.—Causes of diseases, 321.—Proximate causes, 332.—Remote causes, 333.—Symptoms, 336.—Divisions of Pa-

thology, 338.—Diseased states of Solids and Fluids, 340—of the Vital Solids, 342.

Pathology of the Nervous System, 343.—Morbid states of the function of Sensation, 345.—Causes producing increased or diminished Sensibility, which act on the sentient extremities, 345—those which act on the Brain, 346.—Errors of Sense referred to three classes, 350.

Morbid conditions of the Intellectual Faculties, 356.—Imbecility, 356.—Delirium; three modifications of this, 357.—Circumstances on which the Tones of Mind depend, 360.—Causes of Delirium, 363.

Morbid conditions of the Function of Motion, 364.—Increased and diminished vigour of the Muscular Fibres, 365.—Circumstances influencing their state of vigour, 366.—Increased and diminished mobility of the Muscular Fibres, 373.—Causes of increased mobility, 374.—Symptoms resulting from the morbid conditions of the Muscular Fibres, 379.—Increase of Motion, 379.—*Spasm* and *Convulsion*, their causes, 380.—Direct and Indirect stimulants, 381-2. Diminution of Motion, *Paralysis*, 383.—Its causes, 383.—Tremor and Palpitation, 387.

Lectures on Therapeutics, 388.—Therapeutics comprehends Hygiene; plan of arrangement according to Indications, 389.—Scheme of Indications, 393.—Medicines acting on the Simple Solids, 295—on the Fluids, 397—Nutrients, 400.—Medicines acting on the Moving Powers, 402.—*Tonics*, 402.—*Atonics*, 406.—*Stimulants*, 407.—*Sedatives*, 419.—Conclusion of the Course, 428.

Impulse given by Dr Cullen to the investigation of the laws of the Nervous System in the states of Health and Disease, 429.—Diffusion of his opinions, 432.—Publication of his Outlines of Physiology, 432.—Dr De la Roche's Analyse des Fonctions du Systeme Nerveux, 433.

Progress of the Study of the Nervous System in the School of Montpellier, 437.—Doctrines of Sauvages, their resemblance to those of Whytt, 438.—Doctrines of Bordeu, 439. Dependence of Secretion on the Nerves, 440.—Importance attached by Le Caze and Bordeu to the Phrenic Centre or

Ganglionic System of Nerves, 441.—Attempt to refer all the phenomena of living beings to Sensibility, 441.—Doctrines of Barthez, 442.—His employment of the term Vital Principle, 443.—Properties he ascribed to it, ibid.—His doubts as to its nature, 444.—Objections to his Doctrine, 446.—His Account of Sympathies, 455.—Medical School of Paris, 455.—M. Senac, ibid.

Popularity of Dr Cullen as Professor of the Theory of Physic, 458.—His attempt to put a stop to the traffick in Scotch Medical Degrees, 462.—Circumstances which directed his attention to this subject, 462.—Dr Fothergill's altercation with Dr Leeds, 463.—Proceedings of the College of Physicians in London, 465.—Memorial on this subject drawn up by Dr Cullen, 468.—Letter from Dr Adam Smith discouraging interference, 473.—Dr Cullen's Answer to Dr Smith's Arguments, 482.—Remarks on the System of Medical Legislation existing in this country, 486.—Useless distinctions among Medical Practitioners, ibid.—Necessity for the interference of the Legislature in regulating Medical Education, 498.—Dr Cullen's opinions on the manner in which Medical Education should be conducted, 501.—Importance of improving the Literary and Scientific Education of Medical Practitioners, 505.—Propriety of bestowing privileges on particular Teachers of Medicine, considered, 508.—Appointment and Qualifications of Examiners, 512.—Object of examinations, 513.—Apprenticeships, 515.—Trade in drugs as a part of the practice of medicine, 516.

APPENDIX.

Note A. Dr Fothergill's Account of the Edinburgh School of Medicine, ... Page 523

Note B. Dr Cullen's Account of Dr John Clerk, 525.—Correspondence with Dr David Clerk, . . . 537

Note C. Correspondence with Dr W. Hunter, . . . 539

Note D. De Dispositione Herbarum Flore Composito Observationes, ... 566

Note E. Double Elective Attractions, . . Page 570
Note F. Correspondence with Dr Black, . . 573
Note G. On the Generation of Heat and Cold by Mixture, 580
Note H. Letters from Glasgow Pupils to Dr Cullen, 583
Note I. Correspondence with Lord Kames, . . 591
Note K. Correspondence with M. Duhamel, . . 602
Note L. Letter from Lord Deskfoord, relative to Dr Reid, 605
Note M. Letters from Dr Adam Smith, . . 605
Note N. Letter from Dr Cullen to Dr Hunter on the death of Mr David Hume, ... 607
Note O. Correspondence with Dr D. Millar, . 609
Note P. Omitted.
Note Q. Correspondence relative to the surreptitious publication of the Lectures on Materia Medica, . 611
Note R. Dr Cullen's Announcement of a Summer Course on the Practice of Physic in 1768, . . 619
Note S. Medical School of Montpellier, . . 622
Note T. Letters from M. Senac to Dr Cullen, . 624
Note U. Correspondence with Pupils, . . 625
Note V. Letters from Drs Fothergill, Watson, and W. Hunter, ... 654
Note W. On the distinction of Medical Practitioners into Physicians, Surgeons, Apothecaries, &c. . 662
Note X. Prosecutions by the College of Physicians in London for the maintenance of their corporate privileges, 663
Note Y. Dr Cullen on Medical Education, . . 664
Note Z. Suggestion of an Arrangement for the course of Medical Study, ... 665
Note A A. Motives for an extended course of Preparatory Education for Students of Medicine, . . 666
Note B B. Selection of Examiners, . . . 667
Note C C. Trade in Drugs, ... 686

LIFE

OF

MANCHESTER ROYAL INFIRMARY

WILLIAM CULLEN, M. D.

Dr Cullen was born at Hamilton on the 15th day of April 1710. His father, who was a writer or attorney by profession, and factor to the Duke of Hamilton, was proprietor of a small estate in the adjoining parish of Bothwell, which had been in the possession of his ancestors for many generations. His mother was a daughter of Mr Roberton of Whistleberry, a younger son of the family of Roberton of Ernock. Dr Cullen was the second son of a family which consisted of seven sons and two daughters; his father died soon after the birth of the youngest, and his mother was afterwards married to Mr Nasmyth, writer in Hamilton, by whom she had two children. His eldest brother, who had received a classical education in the University of Glasgow, and studied law at Edinburgh, died at an early period of life. None of his other brothers appear to have followed any of the learned professions.

Dr Cullen received the rudiments of his literary education at the grammar-school of Hamilton, under the tuition of Mr Findlater, who enjoyed much reputation as a teacher. In this school, as I have been informed by one who had also been a pupil of Mr Findlater, young Cullen was particularly distinguished by the liveliness of his manner, by an uncommon quickness of apprehension, and by a most retentive memory;—qualities which he continued to possess, in an eminent degree, to the latest period of his life.

From the grammar-school in Hamilton, Dr Cullen was sent to prosecute his studies at the University of Glasgow. I have not been able to ascertain how many years he studied there, or what courses of lectures he followed, as the names of the students were at that period usually entered in the books of the University for only one of the classes which they attended. His name, however, is to be found in the list of students who attended the mathematical lectures of the celebrated Dr Simson, in the year 1727.

On commencing his medical studies, Dr Cullen was bound apprentice to Mr John Paisley, member of the Faculty of Physicians and Surgeons at Glasgow, and in extensive practice in that city. At that time, to serve an apprenticeship was almost the only way in which a knowledge of medicine could be acquired in Scotland. Professorships for teaching some of its branches had been established in our universities, but in none of them, except in that of Edinburgh, had a regular school for teaching medicine been as yet formed. In Glasgow, though a professorship of medicine had been instituted in 1714, and a professorship

of botany and anatomy in 1719, it does not appear that the persons appointed to these offices had, at the time Dr Cullen studied there, ever delivered any lectures upon the subjects they were respectively appointed to teach. It happened, fortunately for Dr Cullen, that his master, though engaged in extensive practice, was of a studious turn of mind, and had collected a large and valuable medical library. That the pupil availed himself of the advantages of his situation, and that his conduct was such as gained for him the approbation and esteem of his master, may be presumed, from the great friendship which ever afterwards subsisted between them. As a proof of this it deserves to be mentioned, that when Dr Cullen, many years after the time of his apprenticeship, became a lecturer in the University of Glasgow, the library of Mr Paisley was thrown open to all his students—a fact of which I was informed by the late Dr Wallace of Glasgow, who told me that he had availed himself of this privilege, while attending Dr Cullen's lectures. In this way, Mr Paisley was pleased to mark his regard for his pupil, and to give an example of liberal conduct towards students, which Dr Cullen, not more to his own credit than to the advantage of the public, followed closely in every future period of his life.

I have not been able to obtain any satisfactory information with regard to the persons with whom Dr Cullen chiefly associated during the time he prosecuted his studies in Glasgow; but that, even at this period, he had felt that desire of distinction which is so often the indication of superior talents, and the best

pledge of future improvement, appears from a circumstance related of him by one of his early friends, the late Mr Thom, minister of Govan. This gentleman used to mention, that if Dr Cullen happened to be in the company of his fellow students when any subject of speculation or debate was started with which he was imperfectly acquainted, he took very little share in the conversation; but when they met again, if the same discussion happened to be introduced, he never failed to shew, that, in the interval, he had acquired a more useful knowledge of the question, in all its bearings and details, than the best informed of his companions could pretend to.

On finishing his medical studies at Glasgow, Dr Cullen went to London, towards the end of the year 1729, with the view of obtaining a situation in which he might enjoy opportunities of acquiring a practical knowledge of his profession. Soon after arriving there, he had the good fortune to be appointed surgeon to a merchant ship, the Captain of which, Mr Cleland of Auchinlee, was a relation of his own. His appointment to this situation is mentioned in the following passage of a letter from his eldest brother to his mother, dated Edinburgh, 9th December 1729.—"Mr Hamilton of Dalserf got a letter last day from London from his brother Alexander, wherein he tells him that he was present with Captain Cleland when Commissioner Cleland * solicited him very strongly in favour of one Mr Cullen a son of Saughs, and used

* The friend of Mr Pope, and author of a letter prefixed to the Dunciad in the edition of Pope's Works, printed at Edinburgh in 1767, vol. iii.

very strong arguments with him to take care of him; and, among the rest, that he was a cousin of the Captain's. So the Captain promised to provide for him; but William desires that Dalserf would write to the Captain and thank him for it, for he said it was much owing to his letter.—I assure you every body thinks my brother very lucky; for Mr Alexander writes also, that the Captain has had a levee, like a General's, every day, and there have been many solicitations for that very appointment. However, for any thing I can find, William has been pretty active in the affair, and I believe has half teazed them into it."—It appears also, from a letter written many years after by a younger brother of Dr Cullen's, that, on obtaining this appointment, he underwent a medical examination, and acquitted himself so much to the satisfaction of his examiners, that they were pleased to pay him some very flattering compliments, and to encourage him strongly to persevere in that diligence which it was evident to them he had employed in the study of his profession.

The vessel to which Dr Cullen was appointed surgeon, was engaged in trading to the Spanish settlements in the West Indies, and remained during her voyage for six months at Porto-Bello, a circumstance incidentally mentioned by himself in one of his lectures on the Practice of Physic. To a mind like his, every change of situation was calculated to afford instruction. In this voyage he had an opportunity of seeing new scenes of life, and manners different from those he was accustomed to contemplate at home; and, what was to him of greater moment, an opportunity of observing the general effects of diversity of climate on the human consti-

tution, and of marking the symptoms and progress of some of the diseases which prevail in our West Indian settlements, and which in particular seasons prove so highly fatal. In the course of his lectures, Dr Cullen occasionally referred to the observations which he had made, and to the diseases he had seen in this voyage, but always with that diffidence and distrust with which he was aware that the observations of a medical practitioner so young and inexperienced as he then was, ought to be relied on by himself or received by others.

After returning from the West Indies, Dr Cullen remained for some time in London, and during his stay there attended the shop of Mr Murray, apothecary in Henrietta Street. This gentleman informed a friend of his, the late Mr Mather of Hamilton, that he had never seen a young man so diligent as Mr Cullen, for that every moment of his time, which was not employed in the business of the shop, was devoted to reading and study. It was during this period, there is reason to believe, that his attention was first turned in a particular manner to the study of Materia Medica, a subject which he seems to have had always great pleasure in investigating.

Dr Cullen returned to Scotland in the end of the year 1731, or in the beginning of 1732. His eldest brother having died during his absence, the duty devolved upon him of arranging his father's affairs, and of providing as far as was in his power for the education and settlement of his younger brothers and sisters. In these circumstances, he was invited by his friend Captain Cleland to reside with him at his family estate of Auchinlee, in the parish of Shotts, and to take

charge of the health of his son, who was affected with a lingering disorder. This situation was peculiarly convenient for Dr Cullen in commencing the practice of his profession. It was near to Hamilton, the place of his birth, and in the vicinity of the residences of many of the most considerable families in the county of Lanark. It was in the neighbourhood also of his patrimonial property, the lands of Saughs, and of another small farm which belonged to his family in the parish of Shotts. Whilst residing there, he seems to have combined with his medical practice the most unremitting application to his studies. Captain Cleland was often heard to say, that nothing could exceed his assiduity at this period; for when not engaged in visiting patients, or in preparing medicines for them, his time was wholly occupied with his books.

After having practised medicine for nearly two years in the situation described, Dr Cullen, on succeeding to a small legacy by the death of a relation, resolved to devote his attention exclusively to his studies for a certain period, preparatively to fixing himself as a medical practitioner in the town of Hamilton. With this view, he lived some time, how long I have not been able to ascertain, at the village of Rothbury, near Wooler, in Northumberland, where he resided with a dissenting clergyman, and was chiefly occupied with the study of general literature and philosophy. I was informed by the late Dr Charles Stuart, physician in Edinburgh, who at an early period of his life enjoyed much of Dr Cullen's company and friendship, that he sometimes in conversation, referred, with apparently very agreeable recollections, to the time he had spent in this retirement.

Dr Cullen passed the winter sessions of 1734–35 and 1735–36 at Edinburgh, in attendance on the medical classes, and in the prosecution of his general studies. The foundation of a new and extended medical school had been laid a few years before this time in Edinburgh, by the appointment of Dr Monro to the Chair of Anatomy in the University, and by the judicious arrangements which that excellent anatomist and experienced surgeon afterwards made with Drs Rutherford, Sinclair, Innes and Plummer for the regular and stated delivery of lectures on the different branches of medicine. Previously to this arrangement, almost the only regular lectures given upon any subjects connected with medicine in Edinburgh, were those which had been delivered in the Hall of the College of Surgeons, the chief medical school in that city from the first institution of the College, in the year 1505, till the transference of the anatomical class into the University in 1725. In reference to these lectures, Dr Græme, in an "Essay for reforming the modern way of practising Medicine in Edinburgh," printed in 1727, observes, "There has of late been taught here, and with some considerable success, at the *Surgeons' Hall*, the whole art of medicine in a systematical way; in which course, with all imaginable care of the performers, nothing has been omitted which to them seemed necessary, either to demonstrate the theory, or illustrate and explain regular practice, as drawn from certain experience and unquestionably just observations. This they have done, with all due regard to the great names of those who have furnished the world with a vast treasure of

practical observations, and of those whose great industry and accurate geniuses, have confirmed and improved the theory by many valuable discoveries; and with no less deference to the great person (Boerhaave), who, from vast volumes, has collected all relating to the one or the other, into such an exact order, and into such bounds, that what concerns either of them can be explained in the course of a seven or eight months' college; which task they own themselves unable to have performed without the aid of so great a master." Though scarcely ten years had elapsed from the first establishment of a regular school of medicine in the University of Edinburgh when Dr Cullen became a student there, the reputation of that school was beginning to be every where acknowledged, and had already attracted to it, not only a great portion of those who were preparing themselves for the profession of medicine in the British dominions, but many students from foreign universities. The state of the Edinburgh medical school at this period has been delineated by Dr Fothergill, in his essay on the character of the late Dr Alexander Russel, in a manner so lively and minute, as to render it sufficient to refer to his account of it *.

During the time that Dr Cullen studied at Edinburgh, a number of medical students, distinguished by their diligence and ardour in the pursuit of knowledge, formed themselves into a society for the purpose of mutual improvement. They met weekly, to read essays on subjects connected with their studies, and to

* See Appendix, Note A.

discuss, in the freedom of debate, the various topics and questions which arise in the progress of the natural sciences to engage the attention of those who are foremost in their cultivation, and which afterwards serve in some measure to mark the successive periods of discovery or improvement in these sciences. That Dr Cullen did not neglect to avail himself of the advantages to be derived from the intercourse of such associates, and that he was one of the original founders of this excellent institution, we learn by the following letter, addressed by him, in the 78th year of his age, to Dr James Cleghorn, Professor of Anatomy in the University of Dublin, in reply to a letter from that gentleman, enclosing an address which had been voted by the Dublin Medical Society to Dr Cullen, upon the enrolment of his name in the list of its honorary members :

" DEAR SIR, EDINBURGH, *January* 1788.

" I am very much obliged to you for the favour you have done me in transmitting the letter from the Secretary of the Dublin Medical Society. I consider your doing this as a mark of your particular politeness and attention to me, and I sincerely thank you for it.

" Your Uncle * and I are, I believe, the only surviving members of a society which existed at Edinburgh in the year 1735, and which laid the foundation of the Medical Society, which became more formally incorporated the year after, and as you know has flourished ever since, to the great advancement of medical science in this University. Tell your worthy uncle that I have what I consider, and I believe he would consider, as a curiosity. It is a manuscript book,

* Dr George Cleghorn, author of a well known work on the Diseases of Minorca, and Professor of Anatomy in the University of Dublin.

in which the discourses of the society, in 1735, are recorded; and there are I believe some of his juvenile performances to be found in it. At our breaking up in the spring 1735, we drew lots for this book, and the fortunate lot fell upon Douglas, the surgeon of the Welsh Fusileers. This gentleman, after travelling about for some years, came at length to Glasgow, and died there, leaving me this book as a legacy.

"I am very much obliged to you for your attention to Mr Black; and, with most affectionate and respectful compliments to your uncle, I have the honour to be, with the utmost regard, Sir, your most obedient servant,

"WILLIAM CULLEN."

Nothing is now known of the talents which Dr Cullen exhibited in the Medical Society as one of its early members; but it will, I believe, be universally acknowledged, that no one ever connected with that institution has supplied it with a larger fund of valuable materials for discussion, or contributed more by his example, opinions and writings, to keep alive and to cherish in the minds of its members, that spirit of liberal investigation and desire of improvement, which first called this society into existence, and which have so long continued to be its distinguishing characteristics.

On leaving Edinburgh, in the spring of 1736, Dr Cullen commenced business as a surgeon in Hamilton, and, in a very short time, as is ascertained by his legers or shop-books which have been preserved, was employed not only by the Duke and Duchess of Hamilton as their ordinary medical attendant, but by almost all the families of any consideration in that neighbourhood.

In the course of his attendance on the family of Hamilton, the Duke having been attacked by an alarming disease, which resisted the effects of the remedies first employed, Dr John Clerk was sent for from Edinburgh. This learned and judicious physician was so much satisfied with Dr Cullen's management of the Duke's case, and pleased with his intelligent conversation, that he formed a high opinion of his talents, and embraced every future opportunity to cultivate his friendship. An intimate correspondence on professional and literary subjects ensued, which was chiefly kept up, on the part of Dr Clerk, through his son Dr David Clerk, who, if we may judge from his letters to Dr Cullen, seems to have inherited his father's zeal for the improvement of his profession, to have been an excellent classical scholar, and well acquainted with medical literature. Upon the death of Dr John Clerk, which happened in 1757, Dr Cullen took occasion to evince his esteem for this excellent physician, by endeavouring to commemorate his virtues and talents, in a short Account of his Life and Character, which he read to a numerous meeting of their mutual friends assembled in the Hall of the Royal Infirmary of Edinburgh *.

Soon after he settled in Hamilton, Dr Cullen became the friend and medical preceptor of the late Dr William Hunter, whose dispositions, genius, and love of study, were every way congenial with his own. Their intercourse soon gave rise to a friendship that continued uninterrupted till the death of Dr Hunter, in the year 1783. During a great part of this

* See Appendix, Note B.

period, Dr Cullen and Dr Hunter had the mutual satisfaction of seeing each other placed, by the unanimous suffrages of their medical brethren, and by the opinion of the public, at the head of the respective schools of medicine to which they belonged. The following account which Dr Hunter's biographer, the late Dr Foart Simmons, has given of the origin of the connexion between these two distinguished teachers, is, I have reason to believe, strictly correct.

"Dr Hunter, who had been educated at the University of Glasgow, was intended for the church; but beginning to entertain doubts with regard to some of the articles of faith, which, as a clergyman, it would have been necessary for him to subscribe, he resolved to abandon the study of theology. In this state of mind, he happened to become acquainted with Dr Cullen, the present celebrated professor at Edinburgh, who was then just established in practice at Hamilton, under the patronage of the Duke of Hamilton. Dr Cullen's conversation soon determined him to lay aside all thoughts of the church, and to devote himself to the profession of physic.

"His father's consent having been previously obtained, Mr Hunter, in 1737, went to reside with Dr Cullen. In the family of this excellent friend and preceptor, he passed nearly three years; and these, as he has been often heard to acknowledge, were the happiest years of his life.

"It was then agreed that he should go and prosecute his studies in Edinburgh and London, and afterwards return to settle in Hamilton, in partnership with Dr Cullen. Dr Hunter, after prosecuting his studies for a winter at Edinburgh, went to London, where he was introduced to Dr James Douglas, who was at that time engaged in the composition of a great anatomical work on the bones, and looking out for a young man of abilities and industry whom he

might employ as a dissector. This induced him to pay particular attention to Mr Hunter; and, finding him acute and sensible, he desired him to make another visit. A second conversation confirmed the Doctor in the good opinion he had formed of Mr Hunter; and, without any further hesitation, he invited him into his family to assist in his dissections, and to superintend the education of his son. Mr Hunter having communicated this offer to his father and Dr Cullen, the latter readily and heartily granted his concurrence to it; but his father, who was very old and infirm, and expected his return with impatience, consented with reluctance to a scheme the success of which he thought precarious."

Dr Hunter retained to the latest period of his life a lively impression of the obligations he lay under to Dr Cullen, and seems never to have omitted an opportunity of acknowledging how much he owed to his attentions and friendship. Many warm expressions of this are to be found in his letters to Dr Cullen. These two eminent men do not appear to have enjoyed an opportunity of personal intercourse, after their first separation, except during a visit of a few weeks which Dr Hunter made to his relations in Scotland, in the summer of 1750; but they always continued to maintain an intimate and familiar correspondence. Their letters relate chiefly to the interchange of mutual acts of friendship, and contain much confidential communication with regard to their professional projects and prospects in life *. Dr Hunter found an additional motive for his gratitude in the continued kindness that was shewn to his family by Dr Cullen. Du-

* See Appendix, Note C.

ring a severe and lingering illness with which his mother was attacked in the year after her son's visit to Scotland, Dr Cullen's attentions were unremitting, and his letters to his friend, giving a detailed account of the progress of Mrs Hunter's disease and of its fatal termination, evince a concern and kind consideration, highly honourable to his dispositions and character.

Though, by the circumstance of Dr Hunter's remaining in London, Dr Cullen was deprived of the pleasure he had promised to himself, of having his friend and pupil associated with him as a partner in business, who was to have relieved him from the practice of surgery which he disliked, he still carried into execution his intention of acting only as a physician. With this view, he took the degree of Doctor of Medicine at Glasgow in 1740, and in the following year entered into a written contract with Mr Thomas Hamilton, Surgeon, on terms similar to those which had been formerly agreed upon between him and Dr Hunter.

During his residence in Hamilton, Dr Cullen was twice elected a magistrate of that place, first in the year 1738, and again in 1739, in conjunction with a relation of his own, Mr Hamilton of Fairholm*.

* Considerable disturbances were about this time occasioned in Scotland by the scarcity and high price of grain, and a riot took place at Hamilton, in which the leaders of the mob broke into the granaries of a farmer in the neighbourhood, forcibly carried his meal to Hamilton, where they sold it at a reduced price, and delivered over the money which they had received for it to Dr Cullen, as chief magistrate. This farmer, in bringing an action of damages against the rioters, accused the magistrates of having connived at the irregular and disorderly conduct of the mob; but the Judges of the Supreme Court of Scotland, before whom the cause

While in the magistracy, and for several years afterwards, he appears to have taken an active share in the agricultural improvements beginning at that time to be introduced into the west of Scotland. He frequently attended the meetings of the trustees appointed for the improvement of the high-roads, and was much consulted by them on the different matters which came under their consideration. Some of his papers relative to these objects, exhibit singular proofs of habits of arrangement and accuracy in transacting business, and a knowledge of rural and agricultural affairs, which must have rendered his advice particularly valuable.

In November 1741, Dr Cullen married Anna Johnstone, daughter of the Reverend Mr Johnstone, minister of Kilbarchan, in the county of Renfrew, a woman of great mental endowments, whose society he had the happiness to enjoy for a period of forty-six years. The intelligent, varied and lively conversation of Mrs Cullen, and her agreeable and fascinating manners, were, like those of her husband, the admiration and delight of all who were admitted into their family circle. She became the mother of seven sons and four daughters, several of whom inherited, in a

was brought, declared, by an unanimous decision, that "William Cullen and Charles Hamilton, Magistrates of Hamilton, instead of having any accession to, or concern with, the mob, had on the contrary done all in their power to prevent it, and to discharge the duties of their office, so far as the nature and circumstances of the case would admit; and that they had even followed out the pursuer's own directions in relation to the management of the mob, and in doing what they could to secure the meal, or the price of it, to the proprietor."

remarkable degree, the talents and accomplishments by which their parents were distinguished.

After his marriage, Dr Cullen continued to practise as a physician in Hamilton till the autumn of 1744, when he removed with his family to Glasgow. This change of residence had been suggested to him two years before, by an invitation which he received from several respectable families in Glasgow and its neighbourhood, to settle there, on the death of Dr Brisbane, a physician who had been in great employment; and, it appears, that he had even taken a house in that city, and made the necessary preparations for his removal. But he was prevented at that time from carrying his intentions into execution by the Duke of Hamilton, who held out various prospects of advantage to induce him to remain at Hamilton. Besides an annual salary, he was to have a chemical laboratory fitted up for him, and the superintendence of a botanical garden attached to the Palace. Indeed, both the Duke and Duchess seem to have been fully sensible of Dr Cullen's merits, and desirous to attach him to Hamilton by every thing which could render his situation there agreeable. These arrangements, however, were not put in execution; the Duke soon after this fell into a tedious illness, during which, for a period of four months, he required the constant attendance of Dr Cullen, who accompanied him in the summer of 1742 to the Isle of Arran, and, in winter, in a journey to London. His Grace died in the end of the following year, an event by which Dr Cullen was prevented from receiving the rewards due to his services.

From the nature and variety of the studies and pursuits in which Dr Cullen was engaged during the seven years he resided in Hamilton, there is every reason to believe that he was industriously employed in qualifying himself to occupy a situation more favourable for the exercise of his talents. Besides the study of theoretical and practical medicine, the sciences of chemistry, natural philosophy, and natural history, seem to have engaged much of his attention. In proof of this, I find among his papers several commissions, not only for the most approved books upon these sciences, but also for apparatus and philosophical instruments necessary for their practical illustration. Some of these commissions evince, in a remarkable manner, the persevering ardour with which he endeavoured to procure, from all parts of Europe, those sources of information, and means of instruction, which could in any way tend to his improvement in knowledge. His library, even at this period, contained a number of the most rare and valuable works on medicine, some of which appear to have been occasionally borrowed from him by such of his acquaintances as were occupied in pursuits similar to his own. I find a curious example of this in a letter addressed to him as "Bailie Cullen, Surgeon in Hamilton," from the late Dr William Smellie, who, as I have been told, at that time united the occupations of cloth-merchant and practitioner of midwifery in Lanark: "I have kept your book on Consumption too long, but I shall send it next week. Send me up Dr Clifton's History of Medicine; I want to see some things in him. I could not get that book from Glasgow or

Edinburgh, but I have sent to London for it." When we remember the eminence to which Dr Smellie afterwards arrived as a teacher and writer, as well as practitioner of midwifery in London, it is agreeable to perceive the laudable pains he was taking, in the obscure situation in which he was placed, to enlarge his general knowledge of medicine, and to qualify himself to fill that station in society to which the conscious possession of talent prompted him to aspire.

A variety of circumstances combined to render Glasgow a situation peculiarly eligible for Dr Cullen. He had many connections and friends there; the population of the place afforded an extensive field for practice; and the University presented to him a theatre in which he was aware that he might have full scope for the display of his knowledge and talents. That his thoughts had already been directed to this object, and that he was even prepared to teach medicine before he left Hamilton, may be presumed from the hints contained in the following extract of a letter, dated Glasgow, 7th May 1744, addressed to Dr Cullen by Mr Thom, who was at that time tutor in the family of Mr Hamilton of Wishaw: " I thought before now to have passed a night with you at Hamilton, which I never do without a great deal of pleasure and improvement; but now.I am afraid I shall not see you till we come to the country. What you were so good as to hint to me in confidence, I was mindful enough to keep a secret; but I reheard it talked of in several companies, that you are to be here, and to teach medicine next winter. On this occasion, your friends cannot help expressing that esteem of you, which a man of

distinguished abilities in science justly deserves. If you are determined to come, and make no secret of it, I should be content to know it. Nothing can give me greater joy than your success; and I think there is a probability you will meet with more, at least in teaching, than you could have had for some years past, as a good many have applied to anatomy this season, some of them foreigners, who, I know, design to study medicine." It appears, indeed, by the following letter from Dr Hunter, that Dr Cullen commenced lecturing immediately after his removal to Glasgow.

"DEAR DOCTOR, LONDON, 26*th February* 1745.

"I could not let slip the opportunity of thanking you for your kind letter; and, in answer, I wish I could say any thing that might convince you of my sense of your goodness in every thing, but particularly of your friendship in this case. You have interested yourself warmly in my brother's (James) health all along *; and, in the hurry of business and

* The following account of Mr James Hunter is given by Sir Everard Home, in his Life of Mr John Hunter:—"James Hunter was born in 1715. He was brought up to the law, as a Writer to the Signet in Edinburgh, but, in the year 1742, went to London on a visit to his brother William, who was at that time a teacher of anatomy, and so much was he captivated with the pursuits in which he found his brother engaged, that he purposed to follow them himself, and become a practitioner in physic. His health, however, was so much impaired by his application to anatomy, that he was obliged to return to Long Calderwood, where he died of a spitting of blood, in the 28th year of his age. He was a young man of uncommonly pleasing address, and of very quick parts. The late Dr Hunter has been heard to remark, that if he had lived to practise physic in London, nothing could have prevented him from rising to the top of his profession."

There is still preserved among Dr Cullen's papers a copy of a letter written by him to Mr James Hunter, in the Latin language, upon the

lecture-reading, have taken time to write me of him. Indeed, it was kind; I thank you in his name, and I thank you for myself. You see I pretend to gratitude; so love me, GOD, as I am sincere.

"The accounts Mr Macauly gives me of your situation at Glasgow, afford me a deal of joy. Macauly talks much of your lectures, too; but that does not surprise me, for I knew you before.

"Well, how does the animal economy appear to you, now that you have examined it, as one may say, with precision? I have good reason to put the question to you, because, in my little attempts that way, since I begin to think for myself, Nature, where I am best disposed to mark her, beams so strong upon me, that I am lost in wonder, and count it sacrilege to measure her meanest feature by my largest conception. Ay, ay, the time will come when our pert philosophers will blush to find, that they have talked with as little real knowledge, and as peremptorily of the animal powers, as the country miller who balances the powers of Europe. But, if I follow out this train of thought, I shall become as pert as those I blame; therefore, I'll drop the point. * * * I must end, by assuring you, once more, that I am, Sir, yours devotedly,

"WILLIAM HUNTER."

I have not been able to ascertain whether the Lectures to which Dr Hunter alludes in his letter were again delivered in the winter session of 1745–46, or whether this was prevented by the disturbed state of the country at that time; but that Dr Cullen was busily engaged during this period in preparing and improving his lectures, appears from the following letters

choice of a profession; from which it appears that Mr Hunter was at the time in doubt whether he should become a soldier or a painter.

which passed between Dr Hunter and him, in the summer of 1746.

"DEAR DOCTOR, LONDON, *May* 1746.

"I cannot say that I am fond of a very close correspondence; for a man may find sufficient employment for himself, from January to December, if he writes often to any number of friends. But really I think ours grows too remiss; and I insist upon your writing once in the twelvemonth, if it was for no other reason than keeping up the right of an old privilege. I have nothing to prompt me at present but that very motive. I have nothing to inform you of, nothing to entertain you with; but while I write, and have nothing to say, I mean to signify the continuance of my regard and esteem for you. I thought to have had the pleasure of being with you long before this time; and I hope still that Providence will indulge me in my greatest wish before the summer is over.

"I see your sister Mrs Almack often; she is always complaining of your silence, and I always taking your part when we meet. But the conflict is not equal; for, setting aside the greater abilities of my antagonist, I must own that I have the worst side of the argument to manage. All your friends here are making frequent inquiries after you. Dr Smellie, Dr Armstrong, and Dr Pitcairn, particularly, always desire to be remembered to you.

"I beg the favour that you will take care of the enclosed to my mother.——* * * My best respects to Mrs Cullen, and all friends; and believe me always, your sincere friend, and devoted servant,

"WILLIAM HUNTER."

"*P. S.*—Among many things that take up my time at present, I labour most at a compendious System of Anatomy and Physiology, which will be necessary to be before me in the winter, should I at last enter upon my old scheme.

But, good heavens! What pains is required to do any thing with tolerable accuracy! Have you not found it so, for you have been employed pretty much in the same way?"

Dr Cullen to Dr Hunter.

"Dear Willie, Glasgow, 18*th June* 1746.

"Your last to your mother I delivered precisely in the manner you desired, and now return you the answer enclosed. I am extremely sorry our correspondence is not more frequent, and I should have dunned you oftener within this twelvemonth, if the situation of the country had not made me forget myself as well as my friends;—but that is now over—and I remember you and your correspondence, with anxious wishes for the renewal of it. I am now busy in preparing for next winter in the same way as you are. I wish you would send your plan for the Physiology, and you shall have mine afterwards. If you like it, we may exchange our opinions on particular points; but you will perhaps think this an unfair proposal. I may be the better for your hints, and you may be the worse by my pre-occupying the students. The first I acknowledge, but I think there is little danger from the last. You will have enough of advantages to engage students that have been with me; and I am persuaded you cannot be disappointed with respect to the Anatomy, even by the Edinburgh College, much less by any thing that can be done here; and if I ever have any students here that are to go to London, I expect rather to be useful than hurtful to you.

"Nothing could make me happier than seeing you in Scotland. I have a thousand things to say that I can say to nobody else. Make my compliments to Drs Smellie, Armstrong, Pitcairn, &c. &c. I am quite ashamed of never having wrote Dr Armstrong since I had the honour of his

compliment. I am, Dear Willie, yours most affectionately,

"WILLIAM CULLEN."

In the summer of 1746, Dr Cullen made arrangements with Dr Johnstone, Professor of Medicine in Glasgow, which enabled him to deliver, during the following winter, a course of lectures on the theory and practice of physic in the University. To be the founder of a Medical School at Glasgow, similar to that which had been established at Edinburgh, was a design worthy of the active and enterprising genius of Dr Cullen, and he appears to have engaged in it with all the ability, diligence and ardour, necessary for the accomplishment of so important an object.

The most accurate and authentic information which I have been able to procure respecting Dr Cullen's lectures, at this period, is contained in the following letter, which I received from the late worthy and respectable Dr Wallace, surgeon in Glasgow, to whom, a few months before his death, I had the good fortune to be introduced by that accomplished physician the late Dr Cleghorn.

"DEAR SIR, GLASGOW, *5th July* 1811.

"I had the favour of yours, and shall endeavour to give you every information with regard to Dr Cullen in my power, while he resided at Glasgow.

"He settled here in the end of the year 1744, or beginning of 1745. Dr Johnstone was at that time Professor of Medicine in the University, but did not give lectures. Dr Robert Hamilton was Professor of Anatomy and Botany, but gave no lectures on Botany. He had been appointed Pro-

fessor only a few years before, upon the death of Dr Thomas Brisbane, who never gave lectures. Dr Cullen, I suppose with the concurrence of Dr Johnstone, began his first course of lectures * on the Theory and Practice of Physic, in the winter of 1746, which continued for six months. I attended that session and the succeeding one. The University having fitted up a laboratory, Dr Cullen, in the session 1747, besides lectures on Physic, gave lectures on Materia Medica and Botany, along with a Mr John Carrick, surgeon, I suppose with the concurrence of Dr Hamilton, to whom Mr Carrick was assistant. Both of these courses I attended. Dr Cullen continued annually to give lectures on Physic and Chemistry, while he remained at Glasgow. Mr Carrick died soon after this, and the lectures on materia medica and botany were given up.

" The number of students in the Physic Class and Materia Medica were very few, not above twenty. But in the Chemistry, the number was considerable; the lectures being calculated not only for the medical students, but for the general students of the University, and for gentlemen engaged in any business connected with chemistry. Dr Cullen, in the Physic Class, never read lectures, but only used notes; in the Chemistry, he sometimes read, but very seldom. To the medical students, as a short text-book, he gave out occasionally a manuscript half-sheet, to be copied and circulated from one to another; and in his lectures he delivered the same opinions with regard to the Theory of Fever, the Humoral Pathology, and the Nervous System, which have since appeared in his writings. His pupils were very much attached to him; indeed it could not have been otherwise, as every attention on his part was paid to their improvement. He had seemingly much pleasure in communicating his own

* By the First Course, Dr Wallace must here mean, the first course which Dr Cullen delivered in the University, as it appears from Dr Hunter's letter that he was employed in lecturing in the session of 1744–45.

extensive knowledge to the students under his care. In the following year the number of students greatly increased.

"Some years after Dr Cullen came to reside here, Dr Johnstone resigned, and Dr Cullen was appointed Professor of Medicine. Dr Johnstone survived several years afterwards. Dr Cullen very soon got into a most extensive business, which continued with him while here; indeed he was considered as the first physician in this part of the country.

"I recollect nothing further, only that I may mention, that the Doctor lived in great cordiality with the gentlemen of the profession. I am, with much respect, Dear Sir, your most obedient servant, ROBERT WALLACE.

"To Professor THOMSON, Edinburgh."

The notes are still preserved in Dr Cullen's handwriting, from which he appears to have delivered the lectures introductory to his first course on the Theory and Practice of Physic in the University of Glasgow. These notes bear the dates of the 4th and 5th of November 1746, and exhibit some of those comprehensive views of medical science, and much of that talent for generalization which were afterwards recognised as forming the peculiar character and charm of Dr Cullen's lectures. They are curious as containing the first outlines of those historical and critical disquisitions with which he was afterwards accustomed to introduce his lectures on the Theory and Practice of Physic in Edinburgh*.

In the first lecture on medicine which Dr Cullen delivered in the University of Glasgow, he explained to his audience his reasons for not adopting as text-

* See Works of Dr Cullen. Edinburgh 1827, vol. i. p. 365. to p. 464.

books the Institutions and Aphorisms of Boerhaave, works which were then very generally employed for that purpose in the different medical schools of Europe. While he acknowledged the great merit of these writings, he pleaded, as an apology for this innovation, the example of Boerhaave himself, referring to a passage in the preface to the Institutions of that celebrated author, where he points out the advantages enjoyed by a teacher who explains to his students his own ideas and writings, instead of commenting upon those of others. After reading this passage, which he has introduced also into the preface to his First Lines of the Practice of Physic, Dr Cullen added,—" I ought to give a text-book myself, but shall not attempt it till after a little more experience in teaching. In the mean time, I shall endeavour to supply its place by an easy, clear order and method, so that the want of it may be less felt." This was probably the first public expression of his intention with regard to a work which has now been so long the guide of scientific practitioners, and which, notwithstanding all the improvements that have been made in medical science since its publication, still continues to hold in this country the first place among systems of medicine. That from the time Dr Cullen first endeavoured to sketch for himself those outlines of his lectures, which were circulated in manuscript papers among his students at Glasgow, he had kept steadily in view the composition of his First Lines of the Theory and Practice of Physic, is evident from the numerous corrections and improvements which appear by his notes to have been successively introduced into those simple and accurate descriptions of diseases,

and comprehensive generalisations of medical facts, which constitute the great merit of that work, and have contributed so much to the lasting fame of its author. In allusion to his attempting to lecture from notes, Dr Cullen remarked, " Written lectures might be more correct in the diction and fluent in the style; but they would have taken up too much time, that may be rendered otherwise useful. I shall be as correct as possible, but perhaps a familiar style will prove more agreeable than a formal one, and the delivery more fitted to command attention."

In entering upon the duties of a teacher of medicine, Dr Cullen ventured to make another change in the established mode of instruction, by laying aside the use of the Latin language in the composition and delivery of his lectures. This was considered by many as a rash innovation; and some, desirous to detract from his reputation, or not sufficiently aware of the advantages attending this deviation from established practice, have insinuated that it was owing to Dr Cullen's imperfect knowledge of the Latin that he was induced to employ the English language. But how entirely groundless such an insinuation is, must be apparent to every one at all acquainted with his early education, course of studies, and habits of persevering industry. When we reflect, too, that it was through the medium of the Latin tongue that he must have acquired his extensive knowledge of medical science, it seems absurd to suppose that he was not qualified, like the other teachers of his time, to deliver, had he chosen it, his lectures in that language. We are not left, however, to conjecture with regard to this point; for that

Dr Cullen had been accustomed, from an early period of his life, to compose in Latin, appears not only from letters written by him in that language to some of his familiar friends, first draughts of which have been preserved, but also from the fact, that, whilst he taught medicine at Glasgow in his vernacular tongue, he delivered, during the same period, several courses of lectures on Botany in the Latin language. The notes of these lectures still remain among his papers; and I find also, written with his own hand, in the same language, two copies of an unfinished text-book on Chemistry. The numerous corrections of expression which are observable in the first sketches of Dr Cullen's Latin, as well as of his English compositions, show a constant attention on his part to the accuracy and purity of the language in which his ideas were expressed, and a mind always aiming, in whatever it engaged, at a degree of perfection higher than that which it conceived it had already attained.

Regarding the institution of a course of lectures on Chemistry as essential to the plan which he had formed of establishing a regular Medical School in the University of Glasgow, Dr Cullen made proposals to the Faculty of the University, that lectures should be permitted to be given on that branch of science by himself and Mr John Carrick, brother of the late Robert Carrick, Esq. Banker in Glasgow, who was at that time assistant to Dr Hamilton, the Professor of Anatomy. The Faculty approved of these proposals; and, as is stated in the records of the University, ordered, in January 1747, that a sum of money (£ 52) should be appropriated for the purchase of apparatus

and the erection of furnaces, &c. necessary for the teaching of chemistry, and directed that Dr Cullen and Mr Carrick should be consulted with regard to the mode in which this money was to be expended in the fitting up of a suitable laboratory and class-room. From an account kept by Dr Cullen, it appears that the sum of £136 was expended upon the chemical laboratory, during the sessions of 1747 and 1748, and that £20 were afterwards allowed annually to keep it in repair. That considerable difficulties were experienced at that time in procuring chemical apparatus, even in London, is seen by a letter from Dr Cullen's brother-in-law, Mr Walter Johnston, dated London, 8th December 1747, to whom he had given a commission to procure for him certain articles, along with the chemical writings of Beecher, Stahl, Bohn, and Pott, as well as a German dictionary and grammar.

The lectures on chemistry were begun by Mr Carrick; but, in consequence of his becoming indisposed almost immediately after their commencement, Dr Cullen found himself under the necessity of supplying his place. The following part of the address which he made, on introducing himself to the chemical class, points out the occasion on which he first came forward to lecture on chemistry, and the measures he was prepared to take for the instruction of his students.

"GENTLEMEN, Your chemical lessons have been a little interrupted by Mr Carrick's indisposition, which, unluckily both for you and him, still continues. To prevent any farther delay in the course, I am come to give you two or three lectures, which may possibly need some apology, as they are

on a subject I did not expect to be so particularly employed in. I suppose you have been told in what light you are to consider the course in which we are engaged. At least, I think it necessary to request you to notice, that this course is not offered by us as very experienced masters in the art, but as an attempt by us, for want of more able performers, to supply students in some measure with the rudiments of such an useful and necessary piece of knowledge; and both in consideration of the smallness of our pretensions, and that this, too, is a first attempt, we desire and expect a good deal of indulgence and favour." * * *

"As our lectures on this subject will comprehend a great many different particulars, which are not easily recollected, I have here made out such a plan as I hope may be useful to you. In that paper, as well as in our lectures, you will notice a great many terms that may seem affectedly pedantic. I am quite sensible of it; but the first chemists were not only persons of a low taste, but they also affected to be mysterious, and, therefore, introduced a number of uncouth terms which cannot now easily be got quit of, and it is quite necessary you should be acquainted with the meaning of them."

It is evident by the notes from which his lectures of this session appear to have been spoken, that instead of two or three lectures only, as proposed in his address, Dr Cullen delivered the whole of the remaining part of the course; and these notes shew that he was, even at that time, extensively and minutely acquainted with the writings of preceding chemists; and that, in selecting and employing for the purpose of instruction the more valuable facts which they contain, he exercised that discrimination and talent for arrangement, which manifested themselves in every matter of speculation or action in which he engaged.

That Dr Cullen's attention at this period was occupied with the other collateral branches of medicine besides chemistry, appears from the following extract from a letter addressed to him by Dr George Thomson of Belfast, dated the 21st of October 1747.

"I was favoured with yours of the 5th of September, by which I am glad to find that your endeavours are so great for promoting the study of medicine. I hope to see the College of Glasgow flourish in that part of science as it does in many others. I insist on the performance of your promise of sending me some specimens of the medicines you are introducing into the shops before not known, and shall be glad to hear from you of any thing curious occurring in practice. I beg all your medical news, and should be glad to know what is to be expected from the Edinburgh Society. I keep up a correspondence with Dr Rutty, a quaker physician at Dublin, who writes me that one Dr Jenkins has been these ten years preparing a Botanicum Hibernicum. I do not know when it will be published, but when it is you shall see it."

In the summer of 1748, Dr Cullen, in conjunction with Mr Carrick, gave lectures on Materia Medica and on Botany—branches of medical study which had not, any more than Chemistry, been previously made the particular subjects of academical prelection in the University of Glasgow. The lectures on Botany, as has been already observed, were delivered by Dr Cullen in the Latin language, which he was probably induced to employ on account of the difficulty of finding English words to express the terms used by botanists in the description and nomenclature of the different parts of plants; a difficulty which, it must be confessed,

has not yet been fully obviated. In these lectures, after giving an account of the principles upon which the different systems of botany had been founded, and explaining particularly that of Tournefort, he followed the system of Linnæus, the outlines of which had been first given to the public in the Systema Naturæ, published at Leyden in 1735. In the adoption of this system, which was strongly opposed by many contemporary botanists, Dr Cullen's superior sagacity enabled him to perceive how much that great naturalist has facilitated the study of botany, by establishing his arrangement of plants upon the number, proportion, relative situation, and connexions of the parts concerned in their reproduction. Indeed, in every view that can be taken of the analogies in external form, or in internal structure, existing among the different tribes of the vegetable kingdom, the Sexual System of Linnæus, must, from the universal occurrence, uniformity and importance of the parts on which it is founded, ever continue to be studied and admired by the scientific botanist. In pointing out the advantages of method in treating of diseases, in his lectures introductory to the practice of physic, Dr Cullen observes, with reference to the Linnæan system, "When, a little more than thirty years ago, I first got a sight of the botanical system of Linnæus, the language in which it was expressed appeared to me a piece of the most uncouth jargon and minute pedantry that I had ever seen; but, in length of time, it became as familiar to me as my mother-tongue; and, with whatever difficulties this system was received in most parts of Europe, it has

now surmounted these, and its utility has reconciled every person to the study of it."

That Dr Cullen was not, however, disposed to adhere servilely to any system of Botany, is evident from the general remarks which he introduced into his lectures on the advantages and disadvantages attending the different modes of arranging vegetables in classes, orders, and families, &c. Some of his criticisms upon the writings of botanists evince a knowledge of plants which could have been obtained only by a long and persevering study of them, and which surprises us the more when we remember the variety of other pursuits, in the midst of which it must have been acquired. As a specimen of the manner in which he was accustomed to sum up his general observations on the various classes of plants, I shall subjoin, in the Appendix, his remarks on the arrangement of the compound flowers which form the class Syngenesia in the System of Linnæus*.

Of the lectures which Dr Cullen delivered on Materia Medica at Glasgow, only a few fragments of notes have been preserved, and these are not sufficient to afford a precise idea of the general plan which he followed. The lectures on Botany and on Materia Medica were again delivered in 1749 ; how long they were continued after that period I have not been able to ascertain. It might be supposed, from Dr Wallace's letter, that they were given up in consequence of the death of Mr Carrick, which happened in 1750, after the delivery of only a very few courses ; but Dr Cullen's " Summer Lectures" are referred to in his corre-

* See Appendix, Note D.

spondence so late as 1754; and the following part of a letter, dated the same year, from Dr Chriström, a Danish physician, who had visited Glasgow a short time before, makes it evident that he still continued to take a keen interest in botanical pursuits. "Since my return to London, is arrived an answer of Dr Linnæus to my questions put according to your orders, to this purpose, that *Aira* is *Gramen Miliaceum*, *Medicago* is *Medicago sylvestris*. As to *Peloria*, the Doctor says he has none, and that the seed never ripens. But instead of these, he has sent for your service the enclosed, which I transmit just as they came, with this expression, that he is sure they will be an hundred times more welcome to you than all the former ones, and that *Vicia biennis* is worth an hundred ducats." What intercourse was kept up after this communication between Linnæus and Dr Cullen does not appear; but I find a letter from one of his pupils, Dr Spry, dated Sidney College, Cambridge, 21st June 1774, requesting the introductory letters which Dr Cullen had promised him for Linnæus.

In commencing his second course of Chemistry, Dr Cullen printed and distributed among his students, "The Plan of a Course of Chemical Lectures and Experiments, directed chiefly to the improvement of arts and manufactures, to be given in the Laboratory of the College of Glasgow, during the session 1748." A comparison of the arrangement of subjects followed in this plan, with that to be found in any of the chemical works then extant, could not fail to give a favourable impression of the talents of its author, and to procure

for him the attention and respect of those who were engaged in prosecuting the study of chemistry.

It was only a short time before this period that chemistry had begun to assume a systematic form, and to be recognised as constituting a distinct branch of natural science. The chemical facts discovered by those engaged in the practice of the arts, in the pursuits of alchemy, and in the preparation of medicines, were become so numerous as to render some arrangement of them necessary, and to induce philosophers to seek for some common principles by which they might be connected and explained. In this country, the experimental investigations of Mr Boyle, and of some eminent individuals whom he associated with him in his labours, had led to the discovery of many valuable facts, and had given new and useful directions to chemical researches. But the suggestion by Mayow and by Newton, that the phenomena of chemical combination depend upon peculiar attractions, which exist between the minute particles of different bodies, ought perhaps to be regarded as the first successful attempt to lay the foundation of chemistry as a science, and to distinguish it from the other branches of natural philosophy.

The encouragement and facilities afforded to the acquisition and communication of scientific knowledge by the establishment of learned societies in different countries of Europe, had given an impulse to chemical pursuits, and contributed to free them from much of that mystery in which they had been involved by their earlier cultivators. The attention given to chemistry by some of the more distinguished members of the Academy of Sciences at Paris, the delivery of lectures

on this subject in the French language by the elder Lemery; the curious experiments of Homberg on phosphorus, metals and salts; the discovery of the remarkable effects produced upon various bodies by the burning glass; and the ingenious method conceived by Geoffroi of framing tables, which should exhibit the relative degrees of chemical affinity between different substances,—had all contributed to render the pursuits of chemistry more generally popular in France than they appear to have been at the same period in any other country of Europe.

Several of the chemical arts, and particularly that of metallurgy, had long been practised in Germany, and lectures on chemistry had for some time been given in the different medical schools of that country and of Holland. Stahl, whose bold and original genius led him to attempt the reform of all the sciences which he cultivated, had been peculiarly successful in improving chemistry. He had simplified many of its processes, had enriched it with several new and valuable facts; and, in examining every part of the science with acuteness and precision, had given to its general doctrines a more systematic form than they had ever before exhibited. In following out the speculations of Beecher, he had also endeavoured to generalize the phenomena of combustion by his theory of *phlogiston*, which, though resting on a principle completely imaginary, was implicitly adopted by chemists, and long regarded as in every respect legitimate and philosophical. In framing this theory, however, Stahl overlooked entirely that function of the air in combustion which had been previously pointed out by Dr Hooke, in passages

of his Micrographia which contain a very remarkable anticipation of one of the fundamental doctrines of pneumatic chemistry *.

The reputation enjoyed by Boerhaave as a teacher of chemistry, was scarcely inferior to that which he had acquired as a lecturer on medicine. He had given a new interest to chemical science by his full and methodical account of the effects of heat and cold upon bodies, and by the enlightened views which he had taken of the numerous important relations of chemistry to natural philosophy, medicine, and the useful arts. In those parts of his "Elementa Chemiæ" which treat of air, fire, light, and the uses of the thermometer, he had, in the great number of facts which he collected from his own experiments and observations, as well as from those of others, presented to succeeding chemists a rich store of materials for speculation and research.

After Boerhaave's decease, the lectures on chemistry were continued at Leyden by his pupil Gaubius, whose lectures on that subject seem to have been circulated extensively in manuscript, and to have been much sought after by Dr Cullen. In a letter from Dr David Clerk to Dr Cullen, dated Edinburgh, November 19. 1751, he says: "I understand now that I cannot purchase Gaubius for you, though at the same time I suppose you will be as well pleased. Austin tells me that he is to purchase all Elliott's papers and manuscripts, without their being put up to sale; and upon my asking the use which he had for Gaubius, he hinted to me

* See Hooke's Micrographia, fol. Lond. 1665, p. 103.

that he intended to pursue the chemistry, and at the same time told me that he would give you a reading of Gaubius, which I suppose is all you want." About a month subsequent to the date of Dr Clerk's letter, Mr John Roebuck writes to Dr Cullen from Edinburgh, sending him a small part of an imperfect copy of Dr Gaubius's Lectures on Chemistry, and offering, if Dr Cullen was willing to pay for transcribing the rest, to have it done for him. In the Catalogue of Dr Cullen's Library, printed at the time of his death, there is a copy of "Gaubius' Chemical Lectures, in MS, 4 vols. 4to, half-bound, interleaved, Latin."

While these advances were making in chemistry on the Continent of Europe, the attention of scientific men in Britain was in a great measure attracted to other pursuits; and, for a considerable time before Dr Cullen began to lecture on chemistry, its cultivation had been confined almost exclusively to those engaged in partial applications of its phenomena to medicine, and to some of the practical arts. Dr Shaw had been usefully employed in facilitating the study of chemistry in England by his excellent translations of the chemical works of Stahl and of Boerhaave, as well as by his own writings and lectures on that subject. About the year 1726, chemical lectures were begun to be delivered in the University of Edinburgh, by Dr Andrew Plummer, who had been a pupil of Boerhaave, but who seems to have directed the attention of his students chiefly to that branch of chemistry which treats of the preparation and chemical properties of medicines. "The great and tempting field of philosophical chemistry," as has been observed by one well acquainted

with the history of its progress, and who has most ably contributed to promote it, " lay unexplored when it was entered on with ardour by Dr Cullen, who first perceived its value, and whose genius and industry, had they not been turned into another channel, would in all probability have been crowned with the richest discoveries *."

Some of the difficulties which Dr Cullen had to encounter in attempting to give a comprehensive account of chemical phenomena, are well described by himself in the following fragment of one of his earliest lectures on chemistry.

" Chemistry is an art that has furnished the world with a great number of useful facts, and has thereby contributed to the improvement of many arts; but these facts lie scattered in many different books, involved in obscure terms, mixed with many falsehoods, and joined to a great deal of false philosophy; so that it is no great wonder that chemistry has not been so much studied as might have been expected with regard to so useful a branch of knowledge, and that many professors are themselves but very superficially acquainted with it. But it was particularly to be expected, that, since it has been taught in universities, the difficulties in this study should have been in some measure removed, that the art should have been put into form, and a system of it attempted—the scattered facts collected and arranged in a proper order. But this has not yet been done; chemistry has not yet been taught but upon a very narrow plan. The teachers of it have still confined themselves to the purposes of pharmacy and medicine, and that comprehends a small branch of chemistry; and even that, by being a single branch, could not by itself be tolerably explained. I do not choose the invidious task

* See Dr Thomas Thomson's History of the Royal Society.

of derogating from established reputations; but were it necessary, I could easily show that the most celebrated attempts towards a system or course of chemistry are extremely incomplete, as examining but a few of the objects of chemistry; that of those examined a very scanty and imperfect account of their relations to other bodies is given; and that, even what is given, is in a method inconvenient and faulty. Now this is the case with the generality of the books on chemistry; but I must take notice, however, that Dr Stahl is one who has endeavoured to avoid these faults; he has taught chemistry with a more general view, and attempted to collect the chemical facts, and to range them in a better order. * * * Perhaps we have the substance of Dr Stahl's lessons, in a book published by a disciple of his, Dr Juncker of Halle, under the title of Conspectus Chemiæ, &c. This is the fullest collection that I have met with, and I have made a good deal of use of it, and you may do so too; but I must notice at the same time, that it is written in such a clumsy manner, is mixed with so much pedantic, trifling philosophy, and is often so inaccurate and superficial in describing experiments, that it will not contribute much to the propagating of chemical knowledge.

"From what I have now said, you will judge of the state of chemical learning, and what a difficult task I undertook when I engaged to teach chemistry; and it is very necessary to tell you, that I did not engage in it from any confidence of my abilities, but because it was thought proper to be undertaken, and nobody else was found to do it; and if I can be so lucky as to engage you to apply to the study, I dare say that the more you become acquainted with it, the less will my performance need an apology with you."

Though the general arrangement of subjects which Dr Cullen followed in his chemical lectures, did not at

any period differ very essentially from that observed in the plan which he originally formed, yet he always seems to have found something to amend, and something new to insert in each succeeding course, acting in this respect in conformity with a sentiment expressed by himself in an introductory address to his chemical students, delivered several years subsequent to his removal to Edinburgh.—"After teaching," says he, "for so many years, it might be supposed that my plan was exactly fixed and sufficiently known; but truly I am yet far from being satisfied with the perfection of my plan, and very certain that it is neither so complete, nor so exactly suited to your purpose as I could wish. It will, therefore, be a long time yet—I hope at least it will be long; for it will only be when the languor and debility of age shall restrain me, that I shall cease to make some corrections of my plan, or some additions to my course." Accordingly, we find that Dr Cullen seldom, if ever, repeated the same introductory lecture to any two courses of chemistry; and different copies remain of the notes of his chemical lectures in which the relative times of their composition are easily distinguishable by the progressive improvements that may be observed in them. In every succeeding year he endeavoured to communicate to his pupils the information afforded by the latest chemical works of the time; and indeed few teachers in any department of science ever more thoroughly kept pace with the progress of discovery than Dr Cullen seems to have done in chemistry. The following quotation from one of his introductory lectures may serve as a specimen of the manner in which he was accustomed to point out the merits, and to cha-

racterize the writings, of contemporary chemists. In speaking of the Academy of Sciences at Paris, he says:

" This academy has constantly enjoyed a number of excellent chemists, nay families of chemists, such as the Boulducs, Bourdelins, Lemerys, Geoffrois, &c. It still possesses Reaumur, Du Hamel, Hellot, Macquer, and several others. These have annually produced new sets of experiments. They have cultivated chemistry in its whole extent, and have been particularly employed in the improvement of arts. Of this, Reaumur's work on iron, and Hellot's on dyeing, are excellent instances. The French academicians have had this special merit of being accurate in experiments, of being always full and perspicuous in communicating them, and particularly of being free from that narrow, selfish temper of the chemists, which affects a mysterious secrecy. The French chemists have often discovered, by their own industry, and published freely to the world, what was too carefully kept secret in other countries. They have sometimes bought a secret in England, and immediately published it in France to all the world. I hope this account of the French chemists will not be thought improper here. They are, I imagine, an example fit to be held up for the imitation of young students. I shall not now detain you with an account of the other academies of Europe. They have all of them contributed more or less to the increase of chemical knowledge. Germany, as a country abounding in mines, and, perhaps, from the particular genius of its inhabitants, has at all times produced excellent chemists. Of late it has enjoyed the Academy of Berlin, which deserves our special notice. This society has possessed some of the best chemists in Europe. Neumann and Pott have gone through surprising labours. It at present enjoys M. Margraaf; and, if I were now to point out a model for the imitation of the students of chemistry, I could prefer none to this excellent chemist. The curiosity of his researches, his judgment in the choice of experiments, the

accuracy of his execution, the perspicuity with which he narrates them, and withal his temperance in theory, are qualities that deserve to be much admired and constantly imitated."

Dr Cullen was accustomed to divide his course of lectures into the General and the Particular Doctrines of Chemistry. Under the first division, he explained the laws of Combination and Separation, processes to which he regarded all chemical changes as referrible. In treating of these, he entered into a very full consideration of the sources of heat, the laws of its communication, and the various effects which it produces upon bodies. His second division comprehended the chemical history of bodies, which were arranged by him under the five classes of Salts, Inflammables, Waters, Earths, and Metals. To these divisions he added a view of the various properties of Vegetable and Animal substances, concluding his course with an account of the application of chemistry to some of the more useful practical arts.

From the time of his commencing to teach chemistry, Dr Cullen appears to have been fully aware of the advantages to be derived from representing in tables similar to those originally constructed by Geoffroi, the relative affinities of different substances; and accordingly it was with him a favourite object to enlarge and improve these tables, both from his own experiments and from those of others. Some of the tables of simple elective attractions which he afterwards put into the hands of his pupils, extended to thirty-one columns. The subject of double elective attractions also occupied much of his attention; and he appears to have been

the first who employed diagrams in order to illustrate this important but obscure part of chemistry. By the assistance of these diagrams, and by the use of algebraical characters, to represent the relative forces of attraction between the principles both of the original and of the resulting compounds, he was enabled to explain, with greater facility and clearness than had previously been done, the cases in which a mutual decomposition, and new combination, of compound bodies can be effected *.

The extensive and original views of several parts of chemical science which Dr Cullen gave in his lectures, the diligence and ardour with which in private he prosecuted his experimental investigations, the peculiar art which he possessed of gaining the affections of his students, and of imparting to them a portion of his own zeal, and the facilities of familiar intercourse with himself, and of ready access to his laboratory, which he so liberally afforded, soon had the effect of engaging several of his pupils in the ardent cultivation of his favourite science. Among these, there was no one to whom he became more attached, and certainly no one more deserving of his attentions and friendship, than the late Dr Joseph Black. The modesty and reserve of his pupil did not prevent Dr Cullen from perceiving his predilection for chemical pursuits, and the accuracy and diligence with which he executed the chemical operations or experiments that were entrusted to his care, made his preceptor desirous to give him every facility and encouragement to persevere in the cultiva-

* See Appendix, Note E.

tion of the science to which the bent of his genius seemed so decidedly to incline.

The general character of Dr Cullen's lectures at this period, and the success which attended his endeavours to excite among his pupils an ardour for chemical pursuits, have been described with great felicity by a distinguished and scientific philosopher, the late Professor Robison, in his interesting memoir of the life of Dr Joseph Black.

"It was fortunate for Dr Black that, when he began his medical studies at Glasgow, the celebrated Dr William Cullen had just entered upon his great career, was become conscious of his own strength, and saw the great unoccupied field of philosophical chemistry open before him. It had been treated hitherto only as a very curious and useful art, which was indeed susceptible of much improvement by means of rational inquiry and discussion. But Dr Cullen saw in it a vast department of the science of nature, which must be founded on principles as immutable as the laws of mechanism, and which may be one day formed into a great system of doctrines of various degrees of subordination and dependence. He was determined to attempt this mighty task, and promised himself great reputation by its accomplishment. Nor was he altogether disappointed. He quickly succeeded in taking chemistry out of the hands of the artists, the metallurgists and pharmaceutists, and exhibited it as a liberal science, the study of a gentleman. He carried into his medical lectures the same ideas of a great system of nature, and made his pupils perceive something of that affinity by which, as Cicero finely observes, *all the sciences are connected, tendering to each other a mutual illustration and assistance.* His pupils became zealous chemists, as well as refined physiologists. No professor took a more lively interest in the progress of an emulous student than Dr Cullen. It was his

delight to encourage and assist their efforts; and therefore he was not long in attaching Mr Black to himself in the most intimate co-operation; insomuch that the latter was considered as his assistant in all his operations, and his experiments were frequently adduced in the lecture as good authority. Thus began a mutual confidence and friendship, which did honour both to the professor and his pupil, and was always mentioned by the latter with gratitude and respect*."

Dr Black, after having passed nearly six years at Glasgow College, during the latter part of which period he enjoyed the advantage of living in habits of intimate and daily intercourse with Dr Cullen, went to Edinburgh, in the beginning of 1752, to complete his medical education, and to procure the degree of Doctor in Medicine. Previously, however, to going to Edinburgh, he visited his father, who had, a short time before, returned from Bordeaux to Ireland, his native country. The pleasure which the old man received from this visit of his son, and the gratitude which he felt towards Dr Cullen, are well described in a letter which he wrote to him a few days after his son's arrival. The value of the upright and honourable sentiments which this letter contains, is in no degree diminished by the little admixture of old spelling and foreign idiom in which they are expressed.

" Most worthy Sir,

" After my fifty-two years weary abode at Bordeaux, settling there, in my business, my eldest and seventh son, I came here some months agoe for my winter retreate, and by some rest and refreshment to prepare my fatigued southern

* See Professor Robison's Preface to his edition of Dr Black's Lectures, p. 21.

constitution for other incumbent dutys to my numerous dispersed family; and, as I had nott for many years seen, or but seldom heard from your pupil, my son Joseph, his brother George at Belfast acquainting him of my desire to have a little of his good company, I was, about eight or ten days agoe, most agreeably surprized to see him, who, butt a young stripling boy when he left France, was become soe tall and promising a personage; and above all soe properly endowed with principles of a generous education, for which, as he often testifies, he is in the greatest measure beholden to you. Permit me, dear and very worthy Sir, to expresse with him our most gratefull sense of all your goodnesse to him, which, I assure you, I and all mine will ever retaine a most thankfull remembrance of, humbly entreating the continuance of your soe honourable and distinguished patronage, which, as in duty, so I am persuaded he will ever study more and more to deserve, by a humble and obedient deference and respect for yourself, your good family, and all your commands.

" I have told him, I will ever, by the grace of God, endeavour to be a careful stewart of the small competency that kind Providence has entrusted me with for the use of my thirteen children, in an equall and as impartiall as possible distribution amongst them of what it hath pleased God to bless us with, giving them all to understand that it has, and ever shall be, my constant endeavour to lay a good and solid foundation; and that it is for them to contribute, by their piety, virtue and industry, to raise the superstructure, which I pray God may enable them to doe for his glory and their own happinesse. Recommending, in the most earnest manner, to my son Joseph, to bear with me the most sincere acknowledgment of your goodnesse to us; and to assure you, that I will ever remaine, with the most sincere sentiments of respect, most worthy and dear Sir, your most gratefull and obedient JOHN BLACK."

" BLAMONT, *9th December* 1751."

Dr Cullen kept up a frequent and intimate correspondence with Dr Black during the whole of the time that he studied in Edinburgh. Several of their letters have been preserved, and these show how much the attention of both was at this time turned to chemical pursuits, and the unreserved confidence with which they were accustomed to communicate to each other the results of the investigations in which they were engaged *. During this intercourse, however, Dr Cullen seems to have been careful to avoid entering on any field of inquiry in which he anticipated that his pupil might reap distinction. In reply to a letter, in which Dr Cullen had described some experiments that he had made on calcareous substances, Dr Black writes: "I received your packet of chemistry, which rejoiced me extremely. A new experiment gives me new life; but I wonder at the reserve and ceremony you use with respect to me. Did I learn chemistry from you, only to be a bar to your inquiries? The subject is not so limited as to be easily exhausted, and your experiments will only advance me so much farther on."

Dr Black, on graduating during the summer of 1754, embraced the opportunity which presented itself of expressing publicly the sense he entertained of the obligations he owed to Dr Cullen, in the dedication to him of his celebrated Thesis, which contains the original account of his discovery respecting the cause of the mildness and causticity of earths and alkalies, and which points out several of the properties that he had observed of *fixed air*, or, as it is now termed, carbonic acid gas. The precise time of his first observing one

* See Appendix, Note F.

of the more remarkable properties of this substance, viz. that of extinguishing flame, is marked by the following passage in a letter from him to Dr Cullen, dated 3d January 1754. "I fully intended to have wrote last post, but really I happened to be intent upon something else at the proper time, and forgot it. It was, indeed, an experiment I was trying that amused me, in which I had mixed together some chalk and vitriolic acid at the bottom of a large cylindrical glass; the strong effervescence produced an air or vapour, which, flowing out at the top of the glass, extinguished a candle that stood close to it; and a piece of burning paper, immersed in it, was put out as effectually as if it had been dipped in water: yet the smell of it was not disagreeable."—Previously to the publication of his thesis, Dr Black submitted it to the revisal and correction of his friend and former preceptor; and, from a fragment of a scroll, containing remarks by Dr Cullen on that dissertation, it is evident with how much attention he had considered every part of it.

Soon after the printing of his Thesis, Dr Black addressed the following letter to Dr Cullen:—

"DEAR DOCTOR, EDINBURGH, 18*th June* 1754.

"Your letter, which I received just now, surprises me not a little. If I understand it right, you have been told that I was ailing; but upon what grounds I cannot guess; for I enjoy as good health at present as I remember to have done these ten years. I am not, however, the less obliged to you for your anxiety about me; and, if any thing of moment should ail me, you may depend upon it, I am neither so ceremonious with respect to you, nor so careless of my own health, as to hesitate in the least about consulting you immediately.

"I sent you on Thursday last a dozen copies of my Thesis, which you will find to be less altered from the sketch you saw before than you expected. The truth is, I was quite tired of it before I gave it out of my hands. There are many things in the first part which do not at all please me, but I was obliged to write it, and I shall be satisfied if it passes without much notice. The style through the whole is excessively dry and awkward, but my subject did not allow much elegance; and, though it had, I confess myself so imperfect in the Latin, that I really could not have attempted to beautify it. I perceive that the Professors here think my experiments new, and seem pleased with them, particularly Alston and Rutherford, the latter particularly, because he looks upon himself as the introducer of magnesia into practice here? What do you think of printing the experimental part in the Physical Essays here? If the experiments are worth any thing they will be stolen by others; but yet I would rather have it so than make them public, unless you think it would be of some sort of service to me. I am hurried away, and must bid you farewell. Yours,

"JOSEPH BLACK."

An account of Dr Black's experiments upon magnesia was inserted into the second volume of the Philosophical and Literary Essays, published in 1755. That essay laid the foundation of his reputation as a chemist, and has ever since been regarded as a fine example of the happy union of ingenious experiment, and accurate reasoning, in chemical inquiry.

Of the different matters which were made the subject of prelection by Dr Cullen, while he taught chemistry at Glasgow, none appears to have occupied his attention more closely and uninterruptedly than the general doctrines of heat. Besides giving in his lec-

tures a full and minute account of the facts that had been ascertained with regard to heat, and in particular of Boerhaave's labours on this subject, he contributed several original experiments of his own to this branch of chemical science. In particular, he endeavoured to arrange, under a few general heads, in tables of heating and cooling mixtures, the variations of temperature that result from different chemical combinations. In one of these tables, he cites, as an instance of the production of cold by solution, that which arises from the melting of ice in water—" which," he remarks, " is extremely curious ; for, if the water be heated to 50°, and the ice be at 32°, the thermometer will sink to 32° during the solution." Having discovered this remarkable fact, it is singular that Dr Cullen should not appear to have examined what the result would have been on employing water at temperatures higher than 50°,—a train of investigation which could not have failed to lead him to a more general conclusion than is contained in his essay.

In attempting to discover some general law with regard to the changes of temperature occasioned by the combination of various substances, Dr Cullen found that cold is also produced by the solution in water of certain salts in their crystalline state ; but that, by the addition of water to the same salts, deprived of their water of crystallization, or in what he termed their state of deliquescence (efflorescence), heat is generated. In explaining these phenomena, he pointed out two kinds of union ;—" one of which," he observes, " is properly mixture (combination)—the other solution only ; and it is probable," he suggests, " that when a

deliquescent (efflorescent) salt is added to water, a part of this water is united to it in the way of mixture (combination); but that, when a crystalline salt is added to the same, it is united with it by solution only." From these, and other observations, he inferred, that "every mixture generates heat, and every solution cold." And he farther suggested, as an explanation of this difference of effect, that "possibly in every mixture there is a condensation, and in every solution a rarefaction." "In many cases," he adds, "it is so, though not easily examined in all."

To account for the cold which occurs during the combination of certain kinds of acids with volatile alkali, a fact which seemed to contradict his general inference of mixture (combination) generating heat, and solution cold, he mentioned his having observed, that when the acid is concentrated, heat is constantly generated during the mixture; but that cold is produced when the acid has previously been much diluted. "In such experiments," he remarks, "a double effect seems to have occurred; that is, the generation of heat by the mixture of the acid and alkali; and the generation of cold, by the solution of the alkali in a watery fluid; —the last, however, being the most considerable, appears as the result of the whole experiment."

These observations, some of which have usually been supposed to belong to a later period, and to other individuals, were mentioned by Dr Cullen in his lectures, and were also made the particular subject of an essay which he read before the Literary Society in the College of Glasgow. This essay has not been preserved, but it is referred to in a paper in his own hand

writing, entitled, " Some farther Remarks on the Generation of Heat and Cold by mixture *." Various other papers, on subjects connected with heat, are also to be found among Dr Cullen's manuscripts, that seem to have been communicated to his friends, or read to different societies of which he was a member. The only one of these which has been published is his " Essay on the Cold produced by evaporating Fluids †", &c. read to the Edinburgh Philosophical Society, May 1. 1755. He was led, as he informs us, to investigate this subject, in consequence of one of his pupils, Dr Dobson, whom he had employed to examine the heat or cold produced by the solution of certain substances in spirit of wine, having observed that when a thermometer, the bulb of which had been immersed in spirit of wine, was suspended in the air, the mercury always sunk two or three degrees. This recalled to Dr Cullen's mind some experiments made by M. Mairan, the perusal of which had led him to suspect that water, and perhaps other liquids, in evaporating, produce some degree of cold. In endeavouring to verify this supposition, he found, that, on repeatedly moistening with spirit of wine, and afterwards exposing to the air, the bulb of a thermometer, the mercury sunk from 44° of Fahrenheit's scale to below the freezing point; and that, by employing some other fluids, he could produce a degree of cold much more considerable. He examined the degrees of cold produced in the evaporation of a variety of other fluids, and found them to be nearly

* See Appendix, Note G.

† Edinburgh Philosophical and Literary Essays, vol. ii. p. 145, to p. 175.

in proportion to the volatility of the liquid employed, and to be increased by whatever hastened the evaporation,—by a rise of temperature in the surrounding medium,—by fresh currents of dry air on the evaporating surface, and also by the removal of atmospheric pressure. "I was ready to conclude," he observes, "that all fluids whatever would, in evaporating, produce cold. But I have found a seeming exception. When the ball of the thermometer is moistened with any of the fossil acids, a considerable degree of heat is produced. It is, however, to be doubted if this affords an exception. We know that these acids attract water from the air; and also, that these acids, mixed with water, always produce heat. It may, therefore, be supposed, that the heat produced by moistening the ball of the thermometer with these acids, is to be imputed rather to their mixing with the water of the air, than to their evaporation singly." In confirmation of this ingenious suggestion, he found, by experiment, that sulphuric acid, when largely diluted with water, produces cold during evaporation.

In prosecuting his experiments on the evaporation of fluids in an exhausted receiver, several new and curious phenomena presented themselves to Dr Cullen. "A thermometer," he observed, "hung in the receiver of an air-pump, sinks always two or three degrees upon the air's being exhausted. After a little time, the thermometer *in vacuo* returns to the temperature of the air in the chamber; and, upon letting air again into the receiver, the thermometer always rises two or three degrees above the temperature of the external air."—"When a vessel containing spirit of wine, with a ther-

mometer immersed in it, is set under the receiver of an air-pump, upon exhausting the air, the mercury in the thermometer sinks several degrees. This experiment, with spirit of wine, was often enough repeated, to show clearly that the evaporation of the spirit *in vacuo* produces a greater degree of cold than the evaporation of the same in the air."—" In an experiment made with the nitrous æther, when the heat of the air was about 43°, we set the vessel containing the æther in another, a little larger, containing water. Upon exhausting the receiver, and the vessel's remaining for a few minutes *in vacuo*, we found the most part of the water frozen, and the vessel containing the æther surrounded with a thick and firm crust of ice." Several ingenious variations upon these experiments, and additions to them, have since been made by philosophers, who have strangely neglected to refer to Dr Cullen's Essay, or to acknowledge the originality of the discoveries which it contains.

It seems impossible to peruse the passages I have quoted from Dr Cullen's manuscript lectures and papers, and from his Essay on Evaporation, without perceiving that his investigations with regard to the heat and cold occasioned by the combination, liquefaction and evaporation of bodies, must not only have assisted to direct the attention of his pupil Dr Black to similar inquiries, but must also have furnished him with several of the data from which his simple and comprehensive theory of *Latent Heat* was afterwards so philosophically deduced. That Dr Black was fully aware of the value of Dr Cullen's observations on the cold produced by evaporation, appears from the following part of a letter,

which he wrote to Dr Cullen, in the beginning of 1755: —"I have looked over your paper with the highest pleasure, and wish with all my heart you would prosecute the subject: it seems to be a copious and important article in the history of Heat. It immediately brought to my mind the sensation of cold which spirit of wine, but especially æther, gives when dropt upon the skin. As for improvements, I assure you I see none to be made in it, but what must necessarily follow the trying of new experiments, and this is a labour in which I cannot engage myself at present, having already bestowed too much of my time on my own chemical inquiries." The duties of the Professor's chair in Edinburgh to which Dr Cullen was subsequently appointed, and the multifarious distractions of private practice in which he was engaged, seem, after this period, to have left him but little if any leisure for experimental researches. It was reserved for Dr Black himself to accomplish the wish expressed in his letter, and to acquire lasting fame by the discovery of the general law that connects and explains the phenomena of the production of heat and cold, occurring in the combination, liquefaction and evaporation of bodies;—phenomena, several of the more important of which were first observed in the investigations so happily commenced by Dr Cullen.

In the end of the year 1753, Dr Cullen transmitted to the Philosophical Society of Edinburgh a paper entitled "Some Reflections on the Study of Chemistry, and an essay towards ascertaining the different species of Salts, being part of a letter addressed to Dr John Clerk," &c. This paper, intended to afford a specimen

of an Elementary work on Chemistry, which he proposed to publish, contains more extensive and precise information, with regard to the general properties and relations of the different species of salts, than is to be found in any chemical work of the time. In particular, the distinctive characters and peculiar compounds of Soda—a substance at that time not generally admitted in this country to differ specifically from potash, but which had always been treated of as such by Dr Cullen in his lectures,—are described with a minuteness and accuracy which evince how intimately he was acquainted with the progress of discovery in Chemistry.

The following letter from Dr Black affords some notion of the different lights in which this paper was viewed by the members of the Edinburgh Philosophical Society, before which it was read.

" DEAR DOCTOR, EDINBURGH, *January* 1745.

" I did indeed trust to Dr David Clerk's giving you an account of the reception of your paper on the different species of Salts; but I find he wants to have some chat with Dr Plummer in private before he writes to you. Mr Russell* tells me it was read aud approved of, but that no particular observations were made upon it. Lord Kames afterwards took notice that there were some errors in the composition, which you were not used to fall into, and offered to undertake the correction of it himself, and I believe would have had it printed immediately; but I thought it most proper to send it back to you again. Dr David Clerk seemed to intimate that the introduction was generally thought to be rather too bold, and the whole wrote in too careless and prolix a style—for I must tell you all the truth. But I believe few of them understood the meaning of it thorough-

* Professor of Natural Philosophy in the University of Edinburgh.

ly. He objected against your making but one species of Volatile Alkali, whereas you divide the fixed into Vegetable and Mineral; for, said he, it flatly contradicts some experiments of Dr Plummer's on the analysis of pit-coal, in which a fossil volatile alkali shewed itself. I perceived he did not understand you, and explained your meaning; but he still objected that there had not been a sufficient number of experiments made on this fossil alkali (soda) to ascertain whether it really was exactly the same with, or different from, the animal and vegetable. I am afraid it would require a course of Chemistry almost to make them fully sensible of the truth of your observations that are to follow.

"My former letter was very long, and had very little in it; but I chose to give you a full account of the matter to which it refers, so far as I knew, to let you see whether or not you had any thing to fear from another edition of Sydenham, which might injure yours. I am, Dear Doctor, yours sincerely,

'J. Black."

It was the singular good fortune of Dr Cullen, not only to foster the genius and talents, and to guide the early studies, of Dr Hunter and of Dr Black, but to continue, during a long series of years, to attach to himself in intimacy and friendship almost all those of his pupils who were in any way distinguished by their ardour or diligence in the pursuit of knowledge. Many of their letters to him have been preserved, and are interesting from the views they present of the state of science at the times at which they were written, as well as from their shewing the familiar intercourse in which he indulged his pupils, and the attachment and respect which they entertained for him as their preceptor *.

The reputation which Dr Cullen acquired by his lec-

* See Appendix, Note H.

tures on Chemistry, and by the success with which he prosecuted chemical researches, procured for him the acquaintance of several persons, both at home and abroad, who were distinguished for their talents and love of science. Among the most eminent and zealous of these, was the late Lord Kames, then Mr Home, with whom he was led, by an interchange of ideas on subjects of chemistry, agriculture and philosophy, to form an intimate friendship. Their acquaintance seems to have originated in their mutual friend Mr Arthur Martine, brother to Dr Martine, the author of "Essays on the construction and graduation of Thermometers, and on the heating and cooling of Bodies," &c. having in 1748, transmitted to Dr Cullen the draught of an essay by Mr Home on Evaporation, with a request that he would state his opinion of the hypothesis which it maintained, and assist in performing such experiments as might serve for its elucidation. In that essay, which was published in the Transactions of the Edinburgh Philosophical Society * more than twenty years afterwards, Lord Kames supports with great ingenuity the opinion that the process of evaporation is owing to an attraction between air and water, and that water is dissolved by air in a manner analogous to what takes place in all solutions. This hypothesis, which had been previously suggested by Dr Halley, was for some time very generally supposed to account for all the phenomena attending evaporation; but the discovery by Wallerius, that this process takes place *in vacuo* gradually led philosophers to question the justness

* Physical and Literary Essays and Observations, vol. iii. p. 80. 1771.

of the opinion; and the information which has since been afforded by the experiments of later philosophers, has shewn that the vaporization of liquids at low as well as at high temperatures, is entirely independent of the presence or absence of air.

In reply to Mr Martine's communication, Dr Cullen entered at considerable length into the subject of the Essay, and pointed out a variety of facts which he at that time regarded as tending to illustrate and confirm the hypothesis supported by Lord Kames, though he was afterwards led to doubt of its validity, as appears from the account which he has given of the progress of his experiments on the cold produced by evaporating fluids. "When I had proceeded," says he, "thus far, I began to consider whether the cold produced in the above experiments might not be the effect of the mixture of the several fluids with the air; and that, therefore, to a list of cooling mixtures and solutions which I was then making up, I should now add the several solutions of the air. By one who supposes the evaporation of fluids to depend upon the action of the air as a menstruum, this would be easily admitted; but as I knew that fluids evaporate in vacuo as well as in the air, I resolved to suspend my opinion till I should repeat my experiments in an exhausted receiver.

From the commencement of their acquaintance, Lord Kames always manifested the greatest esteem for Dr Cullen, and appears to have taken on every occasion a most friendly interest in his welfare. They had few opportunities of personal intercourse while Dr Cul-

* Physical and Literary Essays, &c. vol. ii. p. 152. Edin. 1756.

len remained at Glasgow; but they maintained a frequent and familiar epistolary correspondence on subjects connected with chemistry, and in particular on agriculture, to which they had given much attention, and for which each of them may be said to have retained a passion to the end of his life*. In a letter written in 1749, Dr Cullen requests Lord Kames to favour him with his advice respecting some agricultural experiments which he intended to prosecute, and makes mention of a few lectures on agriculture which he had introduced into his course of Chemistry. "As the course," he observes, "was intended to teach the elements of a chemistry applicable to arts in general, agriculture claimed a place; and though I was not well prepared on that subject, yet I thought it was proper to make a beginning, and at least to open young gentlemen's views on the subject."

Lord Kames seems to have been delighted to find in his new acquaintance an able and zealous cultivator of his favourite pursuit; and, in the progress of their intercourse, he became extremely desirous to engage Dr Cullen in the composition of a work on agriculture. "I live in the faith," says he in one of his early letters (17th November 1749), "that you are going on briskly with your experiments, and that no concerns of interest make an avocation. I propose you as a member in our Philosophical Society, and indeed we greatly want members of activity and genius. I expect to hear from you, as you give me hopes in your letter, and hope for the favour of any discoveries you make from time to time, even those of the smallest consequence.

* Appendix, Note I.

Every thing from you will be acceptable." In another letter, dated 23d December 1749, he says, "Don't think I have forgot a promise you have made me, or at least a hint you gave me, of sending me some of your lucubrations, of which I shall always be fond, however slovenly drest.—In short, my sedate purpose is, that your name shall be carried down to posterity by a treatise on agriculture, better than the world ever yet saw."

The following extract of a letter from Lord Kames to Dr Cullen, dated Edinburgh, 26th December 1752, cannot fail to be interesting to the lovers of agriculture, from its containing the first hint of an attempt, which Lord Kames was making at the time, to convert peat-moss into manure, a process which was afterwards so zealously and successfully prosecuted in the formation of compost-dunghils, by the late Lord Meadowbank *. "Do not forget your letter upon husbandry; having been entertained with no theory now for a long while, I am sinking into a mere practical farmer. I have not a single new thing at present, except one experiment I am making to convert moss into dung, by endeavouring to rot it in a dunghil, by mixing it with fresh horse-dung. I shall let you know the result. If I succeed, I shall be able to multiply my manure greatly."

In continuing to urge Dr Cullen to publish on agriculture, Lord Kames made to him the friendly offer contained in the following letter:—

* * * * "I no sooner get a moment's leisure than

* See Prize Essays and Transactions of the Highland Society of Scotland, vol. ii. p. 130.

I think of my old project for you about agriculture. I would not be surprised if you were offered L. 200 for the copy. I have many notes and useful remarks, gathered in the course of several years, which might find a place. Were we together three days, you might pick out what might serve your purpose. This might be here in September or October next, or in Edinburgh; and, at worst, if your business will not admit, I shall cause to be transcribed what may be useful and send it you; but this only after you fall seriously to work. In the mean time, I give you no consent to relax your application in behalf of our Philosophical Society. I think of enlarging my paper about evaporation, which I can do by your help, and of giving it in to be published with the others. I am certain we shall make a good collection to fill a first volume.

"I expect a great budget when my friend Dawson returns; for I long to hear how affairs go on in your College. Muirhead, I hope, and Smith, will both contribute to fill this same budget, besides what I am to have from Simson and Dick. Yours affectionately,

"KAMES, 25*th March* 1753." "HENRY HOME."

That Dr Cullen had at this period made some progress in the composition of a work on agriculture, appears from a manuscript, part of which is in his own handwriting, entitled "Reflections on the Principle of Agriculture." In this treatise, which is frequently alluded to in his correspondence with Lord Kames, and which contains proofs of the information, ingenuity and acuteness so conspicuous in his other writings, he endeavoured to investigate the nature and application of the nourishment of plants, and states, with regard to these subjects, opinions nearly the same with those which he taught in a course of lectures on agriculture, afterwards delivered in Edinburgh, in the summer of 1768.

Among Dr Cullen's papers there is also an essay, written with his own hand, "On the Construction and Operation of the Plough," which apparently was composed about the same period, and read before some public society, most probably the Philosophical Society in the College of Glasgow. The object of this essay was to explain, as he himself informs us, "the mechanical principles on which ploughs have been constructed, to find out what is the importance and effect of each part, and to examine what variation each or all of them require, according to the difference of soil in which they are employed." An allusion to this essay is made by Lord Kames in a letter to Dr Cullen, in which he says, "A thought has struck me in the head within these few days relative to *your engine*, the plough," &c. It is also referred to in a letter from Mr Patrick Alison, an intelligent farmer in Forfarshire, who, in proposing to substitute iron for wood in the construction of the plough, addresses himself to Dr Cullen, as being "the first person in Scotland who has attempted to account for the structure of that machine upon mathematical principles."

Dr Cullen never published any of the results of his inquiries or of his practical experience in agriculture; but Lord Kames, as is well known, had himself the merit of producing, many years after the idea of it had been first conceived by him, a treatise upon agriculture, in his work entitled the "Gentleman Farmer," which, by its great usefulness and popularity, has justly secured to his Lordship that claim on posterity which he was so desirous should belong to his friend.

The attempts which Dr Cullen made at Glasgow

to connect chemistry with agriculture, gave rise to a correspondence between him and M. Du Hamel de Monceau, so well known throughout Europe by his writings as an able vegetable physiologist, an indefatigable experimenter, and a zealous cultivator of all the branches of natural knowledge which have any relation to the improvement, rearing, and preservation of the productions of the soil. It is to be regretted that only a small portion of this correspondence has been preserved. The following letter to Dr Cullen from M. Dangeuse, an intelligent foreigner*, who visited Glasgow in October 1752, shews the opinion which he entertained of the kindred tastes and studies of the individuals between whom he wished to open a friendly intercourse.

"*Rue St André à Paris, le* 8 *Mars* 1753.

"Je n'ai point oublié, Monsieur, les agrémens et les avantages que j'ai recontrés dans votre conversation, lors de mon passage à Glasgow; et j'ai jugé par moi-même que je rendrois service à M. Duhamel de lui procurer l'honneur de votre correspondance: vous vous amusez quelquefois de l'étude de l'agriculture, et lui aussi. Il vient de donner au public un Nouveau Traité de la *conservation des grains*; et on vient de réimprimer en deux volumes son *Agriculture suivant les principes de Tull* avec diverses expériences: Il m'a chargé de vous les presenter, et de vous les faire tenir; ces trois volumes seront remis dans les premiers jours d'Avril, chez Vaillant bookseller in the Strand at London, avec votre adresse dessus; Vaillant les donnera a celui qui viendra les retirer avec un mot de votre part: M. Duhamel demande en

* Author of a Treatise, published in 1754, under the name of Sir John Nickol, "On the Advantages and Disadvantages of France and Great Britain in respect to Commerce."

revanche que vous aiez la complaisance de lui communiquer les experiences que vous avez faites, et que vous ferez. Rien n'instruit mieux que la comparaison des expériences, et c'est ainsi que les savans tirent de leur fonds de presents plus précieux que l'or ou les pierreries : ma recompense à moi, ce sera d'avoir formé entre vous une liaison surement agréable à vous deux, et qui peut être utile à la société des hommes : Lorsque vous aurez quelque chose à lui faire tenir, vous pouvez de même le lui adresser chez le Sr Vaillant qui le lui fera parvenir.

" Comme M. Moor et M. Black ont l'honneur d'être de vos amis, Monsieur, j'espere que vous me permettrez de faire mention d'eux ici : La politesse, le savoir, l'emulation qui regnent dans l'Academie de Glasgow m'en rendront le souvenir à jamais agréable ; mon grand plaisir seroit de leur pouvoir rendre en France le bon accueil que j'ai éprouvé chez vous, et de pouvoir vous prouver le veritable attachement avec lequel j'ai l'honneur d'être, Monsieur,

" Votre très humble and très obéissant serviteur,

" DANGEUSE."

The first letter of M. Du Hamel to Dr Cullen is lost, but the answer to it has been preserved, and is interesting chiefly as affording a specimen of Dr Cullen's manner of describing chemical phenomena*.

Dr Cullen's opportunities of prosecuting agricultural experiments, were increased by his undertaking in 1752 to manage and to improve the farm of Parkhead, situated about eight miles from Glasgow, which he had purchased for his brother Robert Cullen, Esq. who was at the time employed in a mercantile situation in the West Indies. The following letter to his brother, relative to the purchase of land in Scot-

* See Appendix, Note K.

land, evinces much of the good sense and clear judgment which Dr Cullen carried into the ordinary affairs of life.

"DEAR ROBIN,

"Yours of the 8th of April came to my hand in July last. I am sorry to find trade at present in such a bad way with you, but hope it will mend, and, in the mean time, think you are quite right in launching no farther till what is on hand can be disposed of. I am extremely glad to find your ambition so moderate, and have constantly my fears with regard to the climate you live in. I think as soon as your own wishes are satisfied, you have no temptation to spend any more of the best of your days in an uneasy situation, or to break your constitution so far as to render the rest of your life infirm and valetudinary. I had a prospect of some benefit from your present situation, both to myself and your other friends; but though that expected advantage were much greater, I would earnestly advise you not only to drop these views, but also to moderate as much as possible your own ambition, and think of returning to your native climate. With regard to the purchase you write of, I have no difficulty about gratifying you. There will be little difficulty in making such a purchase, and even of Whistleberry* itself; and I must tell you, farther, that I am glad to find your taste agree so much with my own. That place where I spent so much of my infancy, I have always had a particular affection for; but before you are engaged in any such purchase, I must take care to inform you fairly of the circumstances of it, and leave you then to judge for yourself. First, then, what remains of this little estate that can be purchased, is not above thirty acres of ground. This is too small on several accounts;—too little to serve a family;—too little to give a man employment;—too little to be conveniently laboured.

* The Property of their grandfather.

To explain this better, I think a man ought to have as much ground as he may labour by himself; that is, as much as may employ four horses, or what we call a plough-gang, &c. —I also think, a man retiring from the hurry of business, if he sit down entirely without employment, is apt to become uneasy and splenetic, and very ready to fall into amusements that are either trifling or hurtful to health. I would, therefore, recommend to every body to avoid idleness; and I don't think you could be happy in a country life without something to do; and though I would not advise you to enter deeply into the employment of farming, which you have not been bred to, yet if you purpose to live in the country you may safely, and ought to have some employment in that way." * * * * * * * *

"Upon the whole of this affair, I have a farther advice to offer you; that is, if you bring any money to this country, you should lay it out on a purchase of land. Your money in this way will be better secured. The lands of this country are mostly improveable, and are really improving, and their value will rise, while it is as probable that the interest of money will soon sink. All this I cannot now explain at greater length; but I could show, that whoever purchases land with any discretion in this country, must soon find a benefit in it. * * I must likewise acquaint you with another affair:—I have been these several years struggling to get the lands of Saughs cleared of encumbrances and into my own possession. It was this engaged me in an otherwise unprofitable attendance on the Family of Hamilton; and it would take too much time to tell you what difficulties I have met with from the late Duke's death, the minority of the present, the Rebellion and other accidents, that have put me off a long time; however, the young Duke is just now returned from Lisbon very well recovered, and I am making a strong push, with very good hopes of succeeding. Now, have you any inclination to be possessed of these lands"? &c.

Towards the end of the year 1749, Dr Cullen had the good fortune to be introduced to Archibald Duke of Argyll, who at that time had much influence in the direction of political affairs in Scotland. The various anecdotes related of Dr Cullen's having become acquainted with that nobleman at an earlier period, and of his having afterwards been recommended by him to the Family of Hamilton, are totally without foundation. The precise time of his being introduced to the Duke, is marked by the following passage of a letter from Mr Martine, dated Edinburgh, August 1749:—"Mr Lind, at Mr Home's desire, talked very particularly about you to the Duke of Argyll; and your friends here desire that you will wait on his Grace upon his arrival at Glasgow, which will be to-morrow evening. When you are introduced to him, it is probable, on your mentioning Mr Lind's name, that he will be more particular with you." The more immediate object of Dr Cullen's being introduced to the Duke of Argyll at this time, was to obtain his Grace's consent and patronage to his succeeding Dr Johnstone as Professor of Medicine, an event which took place in consequence of an agreement between them in October 1749, though Dr Cullen was not formally admitted into the university till the 2d of January 1751. It does not appear what circumstances occasioned this delay in Dr Cullen's appointment by the Crown; but that the Duke of Argyll had interested himself in hastening it, is evident from various papers relating to the subject, and also from the following letter addressed by this nobleman to Dr Cullen.

"SIR,

"I have been in such a hurry of business of late, joyned with the distemper of cold raging here, that I thought I had better delay writing to you till I was a little at leisure. I was very sorry that your affair was so long delayed, which I had good reason to believe would have been finished half a-year sooner. I shall be very glad to hear what you are doing, and what experiments are going on. People are greatly surprised at the metal you gave me that melts in hot water. Pray send me the proportion of the ingredients, for I have forgot them. If you want to be informed of any thing passing here in your way, I will make my friend Dr Mitchell write to you. I am, Sir, your obedient humble servant, ARGYLL."

"LONDON, *March* 7. 1751.

A few days after Dr Cullen's admission as a professor in the University of Glasgow, Dr Adam Smith was appointed, by the Faculty of that University, to the chair of Logic there. These two eminent men, each pursuing with indefatigable ardour his own branch of study, soon conceived a great esteem for one another, and continued ever after to live in habits of the most intimate and friendly intercourse. By the election of men of such genius and talents, the respective Patrons of the vacant chairs seem on this occasion to have vied with each other in endeavouring to promote the true interests of the institution; and it must be allowed, that the same concern for the advancement of science and philosophy was evinced in the choice that was made of their successors, when we remember that Dr Black was nominated by the Crown to succeed Dr Cullen, and Dr Reid* chosen by the Faculty to suc-

* See Appendix, Note L.

ceed Dr Adam Smith. Appointments like these must always be regarded as doing more honour to those who bestow them than to the men on whom they are conferred.

Dr Smith having been appointed, on the death of Professor Craigie, which happened in November 1751, to succeed him in the chair of Moral Philosophy, that of Logic became vacant. For this situation, three candidates offered themselves, David Hume, Edmund Burke, and Mr Clow. The latter was the fortunate candidate. It appears from the following, as well as from other letters*, that, on this occasion, Dr Cullen had taken a very warm interest in Mr Hume's success.

"Sir,

"The part which you have acted in the late project for my election into your college, gave me so much pleasure, that I should do myself the greatest violence did I not take every opportunity of expressing my most lively sense of it. We have failed, and are thereby deprived of great opportunities of cultivating that friendship which had so happily commenced by your zeal for my interests; but I hope other opportunities will offer; and I assure you, that nothing will give me greater pleasure than an intimacy with a person of your merit. You must even allow me to count upon the same privilege of friendship, as if I had enjoyed the happiness of a longer correspondence and familiarity with you. For as it is a common observation, that the conferring favours on another is the surest method of attaching us to him, I must by this rule consider you as a person to whom my interests can never become altogether indifferent. Whatever the reverend gentlemen may say of my religion, I hope I have as much morality as to retain a grateful sentiment of your favours, and

* See Appendix, Note M.

as much sense as to know whose friendship will give greatest honour and advantage to me. I am, Sir, your most obedient and most humble servant,

"EDINBURGH, 21*st January* 1752. DAVID HUME."

It might afford curious matter of speculation to conjecture what effect the appointment of Mr Hume, or of Mr Burke, to the Chair of Logic in Glasgow, would have had upon the character of that University, or upon the metaphysical, moral, and political inquiries of the age in which they lived; and what consequences were likely to have resulted from the influence which the peculiar genius and talents of either of these great men, had they been exerted in that sphere, must necessarily have had in forming the minds of such of their pupils as were to be afterwards employed in the pursuits of science, or in the conduct and regulation of human affairs. It seems difficult to conceive how, as instructors of youth, they could either of them, without a considerable modification of their opinions, have taught Philosophy upon the Sceptical or the Berkeleian systems which they had respectively adopted; while the strict purity of their moral characters, and the great reverence which they both entertained for established institutions, give the fullest assurance, that, had either of them been appointed to the Chair of Logic, their academical duties would have been executed with an unceasing regard to the improvement of their pupils, and to the reputation of the Society into which they had been admitted. The interest which Dr Cullen took in Mr Hume's canvass, laid the foundation of a

friendship which continued without interruption till the death of Mr Hume, and was the source of many marks of mutual regard and esteem *.

During his residence in Glasgow, Dr Cullen was assiduously engaged in endeavouring to apply the principles of his favourite science, Chemistry, to the elucidation and advancement of some of the useful arts. Subsequent improvements have now rendered the results of his inquiries unimportant to the practical chemist, but they still possess a value in the examples they afford of the activity and zeal with which he at this period pursued experimental and practical knowledge. In the following letter, Lord Kames alludes to an Essay which Dr Cullen had written upon Bleaching, and to an improvement which he had proposed in the manufacture, or rather purification, of common Salt.

" Dear Sir,

" By this opportunity of Mr Crawford, which relieves you of postage, I assume the privilege of writing you a letter, without warranting that there shall be any thing in it. But I find myself inclined to converse with you a little, and no matter for the subject. I have got back your paper from Lord Deskford, and by his directions I am to send for John Chrystie (bleacher at Ormiston), and to see what he has to say upon the hints you suggest. This may be of some use; and, if any thing come out worth your notice, I shall be sure to acquaint you.—I think Bleaching a great object for this country; and now that a rational account is set on foot, I wish to have it carried to the utmost length. Pray, what are you doing about your project for refining Salt? If you are sure it can be made a manufactory, you must be entitled

* See Appendix, Note N.

to a premium for your discovery in one shape or other. I am willing for my part to bustle for it; and therefore, if you think I can be of use, pray employ me. Remember also to contribute to the Philosophical Society, about which I am turned extremely keen, now that I have got in a good measure the management of it. Yours,

"EDINBURGH, 26*th December* 1752. HENRY HOME."

The improvement in the manufacture of Salt, alluded to in Lord Kames' letter, consisted in precipitating the earthy ingredients contained in the brine of sea water, by a solution of common potash, and in this way obtaining from it a salt more pure and better fitted for preserving food than that prepared in the ordinary manner. The method of purifying common salt proposed by Dr Cullen, does not appear ever to have been extensively practised; the quantity of potash required to separate the magnesia and other substances contained in sea water, being such as to render the process too expensive for general use. In giving an account of this proposal to the Duke of Argyll, Dr Cullen says, "You will not be surprised to find a chemist become a projector. I have hitherto kept pretty clear of this character; but, like many other chemical enthusiasts, having spent more time and money than I could well afford, I begin now to wish that my chemical labours would afford me some returns. If your Grace shall find that my proposed improvement is likely to be of any use, I shall consider it as a mark of the greatest goodness to have your opinion in what manner I may best avail myself of the discovery—a very simple one indeed, but if real, I think the simplicity very much increases the value of it."

In his essay entitled Remarks on Bleaching, a copy of which remains among his manuscript papers, Dr Cullen gave a full account of the different operations of this process, and suggested some improvements with regard to the manner of conducting these operations, and the choice and application of the substances employed in them. This essay was not published, but a copy of it appears to have been presented to the Board of Trustees for the Encouragement of Fisheries, Arts, and Manufactures in Scotland. In the records of that institution for June 1755, it is mentioned that "three suits of table-linen had been given as a present to Dr William Cullen for his ingenious Observations on the Art of Bleaching."

Convinced that great advantages would result to the bleachers of this country from their being furnished with a steady supply of alkaline substances, Dr Cullen at various times instituted experiments to prove that these might be prepared at home, both from the combustion of wood, and from the lixiviation of kelp. His views with regard to this subject are explained by himself in the following letter to Lord Kames.

"My Lord, Glasgow, *February* 1753.

"Your letter of the 15th came to my hand only on Friday last. I am much obliged to Lord Deskford for the remarks he has made. From some of them I have received new information; by others I am properly corrected, and I am very sensible of the defects his Lordship has pointed out. One of them concerning the examination of Water I shall give my opinion of very soon. The other, the examination of Ashes, I cannot properly direct without many experiments, which I could not hitherto possibly find time to make.

"John Chrystie's notes give me great satisfaction. If he is not very complaisant, I must be better pleased with my own piece than I could be before. I am glad to find a man of judgment and experience agreeing with me in so many particulars. The most part of the instruction I had received on the subject, I had got from his directions; and from his notes on my remarks I have learned a good deal more. In some particulars he seems to differ from me, and I am ready to believe he is in the right. If there be any thing about which I am in doubt, it is not to be discussed by conjectures and reasoning, but must be referred to experiment; and on this footing I would let the whole affair rest for some time. But I must now own to your Lordship, that I have so far a good opinion of my first attempt on this subject, that I have a very strong inclination to prosecute it. It is a pretty wide field for experiments, and my fancy grasps at the whole; but I know well that my hands can only comprehend a small part of it. After the hints I have given, I would leave the experiments to be made in the bleaching-field by persons who have more skill and better opportunities for these; and I would choose to confine my views at present to the subject of ashes. The more I consider this subject, the more fully I am persuaded that all kinds of ashes, and all kinds of alkalies, may be made in Scotland; in what quantities and at what expense, I cannot judge but by trial. These trials I have an inclination to make but cannot afford the expense. If your Trustees have so far an apprehension of the utility of such an inquiry as to hazard a little money upon it, I shall hazard my pains, and, what may be a much more expensive article to me, my time, upon it. Favour me with your opinion on this subject. If the trustees are not likely to be very prompt in this affair, I would not have it mentioned. I shall not pretend to be disinterested. On the contrary, I should be very glad to find my interest connected with schemes of this kind; but at present I would be hurt by any suspicions of interested views. I have a great ambition to

establish Arts,—I have not avarice enough to practise them. If you do any thing in this affair, some memorial or plan may be necessary, and you shall have it on demand. * *

"I have a great inclination to have a little conversation with your Lordship, at least once a-year. Whether I can be so happy before your session rises, I am uncertain. Be pleased to let me know if you can have a Saturday afternoon, and a Sunday, to bestow upon me, or if you will be still at Edinburgh on Sunday se'nnight. Excuse this freedom in one who considers your Lordship's acquaintance as one of the most fortunate accidents of his life. I remain, your Lordship's faithful and obedient servant,

"WILLIAM CULLEN."

In reference to the proposal suggested by Dr Cullen to be made to the Board of Trustees, Lord Kames writes, (3d March 1753) :—"I could not think of doing better than to put your letter into Lord Deskford's hands. If the Trustees do not grasp at your offer, by defraying the expense of experiments, I shall not be at a loss to form a judgment of them." And, in a letter to Dr Cullen, written a few days after this, (11th March,) he says :—"But for the expectation of seeing you, I had left the town yesterday. I walk off to-morrow at six, without other regret than that I cannot manage between you and the Trustees. I have delivered to Lord Milton a letter from Deskford upon your proposal, and what I may call generous offer, recommending the matter to him warmly. But great bodies move slowly, and a certain habit of business leads some people into a slow and regular train, which no peculiarity of circumstances can provoke them out of. You will perhaps reckon it too sanguine an advice to go on with your experiments, with a reasonable,

or rather certain, prospect of a retribution. But this you may safely venture, to set out with the cheapest experiments, to give notice to Lord Deskford that you have done so by my advice, and that you are ready to go on if he gives you encouragement."

Dr Cullen seems to have followed the advice given him by Lord Kames, of continuing to prosecute his experiments on the preparation of alkaline substances. He had trials made at different bleachfields of the ashes which he had prepared, and there still remain several papers containing his directions to the bleachers with regard to the manner of using these ashes; and also their letters to him, giving an account of the success which they had had in employing them in bleaching. So late as the summer of 1762, Dr Cullen instituted an extensive series of experiments to ascertain the quantities and qualities of the alkaline salts obtained by burning ferns and different kinds of wood. These experiments, in which he was assisted by a pupil, Dr David Millar, from Hamilton, for whom he appears to have entertained a particular regard*, were made in Perthshire, on one of the estates in the neighbourhood of Rannoch, which had been forfeited to the Crown after the rebellion of 1745; and an account of them appears to have been drawn up by Dr Cullen, for the information of the Commissioners for the management of the Annexed Estates.

During the time that Dr Cullen taught the Theory and Practice of Medicine at Glasgow, he formed the design of publishing an edition of the works of the celebrated Dr Sydenham, with an account, in Latin, of his

* See Appendix, Note O.

life and writings. In a letter addressed to Dr Cullen, in 1751, Dr Hunter says,—" I see proposals for publishing Sydenham at Edinburgh:—An idle scheme I think; a mere edition, it seems, for fourteen shillings. Pray have you dropt your scheme? I had told your plan to Mr Sydenham * here, and two or three other people. Every body approved of it. Every body disapproves of the Edinburgh scheme. Let me soon know what you are to do in it. Pitcairn desires it." To this inquiry, Dr Cullen replied:—" I have not dropt my scheme with regard to Sydenham. It has been delayed partly on account of my hurry, and partly for the sake of a new type, which Mr Wilson is now about. The Edinburgh edition will not do us much harm; ours shall be a more splendid one; and at the same time have other advantages. I propose to join to it at least two dissertations; one *De Autoris Vita et Scriptis et Methodo Medendi;* and another, *De Causis Morborum Epidemicorum.* I expect some anecdotes from your friend Sydenham; I expect the original editions from Dr Pitcairn; and I expect your judgment upon my composition to be sent to you, and your advice with respect to the conduct of the whole."

How far Dr Cullen had proceeded in this work, and for what reason it was ultimately abandoned, are points about which I have not been able to obtain any satisfactory information. There remains, indeed, a short scroll, in his own handwriting, containing the principal dates and facts with regard to Sydenham's life, but without any general reflections such as Dr Cullen

* Mr Floyer Sydenham, Translator of several of the writings of Plato.

could not have failed to make, had he persevered in the undertaking, and which, notwithstanding the numerous editions of Sydenham's works, and the universal estimation in which they were held by the medical world, must have given to them an additional interest. The opinion, however, which he entertained of Dr Sydenham's character as a physician, and the value which he attached to his writings, may be learned from the interesting account which he has given of him in his Introductory Lectures to the Practice of Physic*. It does not appear whether Dr Cullen had ever written the dissertation which he proposed on Epidemic Diseases; but the subject was one which, in his lectures on the Practice of Physic, formed a particular object of his consideration.

From his appointment to be a Professor of Medicine in the University of Glasgow till the end of the year 1755, Dr Cullen, in addition to his lectures on Chemistry, continued to deliver annually a course of lectures upon the Theory and Practice of Physic. His private practice during this period, though extensive, does not appear to have been lucrative; and, as a considerable portion of it lay in the country, he had but little time left for the prosecution of his scientific pursuits. These circumstances seem to have induced some of his friends to propose his removing to Edinburgh, a scheme which is mentioned by himself in a letter to Dr Hunter, written in August 1751.

"I am quite tired of my present life; I have a good deal of country practice, which takes up a great deal of time, and hardly even allows me an hour's leisure. I get but little

* See Works, Edinburgh 1827, vol. i. p. 402.

money for my labour; and indeed by country practice, with our payments, a man cannot make money, as he cannot overtake a great deal of business. On this account, I have some thoughts of acceding to a proposal that was lately made to me of removing to Edinburgh. Dr Plummer, Professor of Chemistry, is a very rich man, has given up practice, and had proposed to give up teaching in favour of Dr Elliot; but this gentleman died about six weeks ago, and, upon this event, some friends of mine, and along with them some gentlemen concerned in the administration of the Town of Edinburgh, have proposed to use their influence with Dr Plummer to induce him to resign in my favour. As the income of that office cannot be very considerable, and my success in the way of practice is uncertain, I have hesitated about agreeing to their proposal; but, provided they can make the establishment such as will afford me a livelihood, the situation and manner of life there will be so much more agreeable than at present, that I resolve to hazard something, and have agreed to accept the invitation when made to me in a proper way. However, Plummer's consent, and some other circumstances, are still in doubt; and this, with other reasons, requires the affair to be kept as secret as possible. In the mean time, I shall beg to have your opinion and advice."

Whether the proposal alluded to in this letter was ever actually made to Dr Plummer, or, if made, what his objections to it were, do not appear. Lord Kames, however, in a postscript of a letter to Dr Cullen, dated 26th December 1751, says,—" The project about Dr Plummer I find is not yet ripe, but I keep my eye close upon it." And, in the beginning of 1753, his Lordship, upon an occasional illness of Dr Sinclair, took occasion to write to Dr Cullen in the following terms:—" Since my last to you, mat-

ters more *drumly* (muddy) to me, seem to clear for you more and more. Poor Sinclair, I am afraid, will scarce be alive when this comes to your hand, and Dr Lowis, another physician of note, is in a very bad way. In short, your presence here, which I formerly thought would be useful to yourself, will shortly be necessary for the benefit of the town."—" I have got no return as yet from the persons I employed upon Dr Plummer. Do not expect from the town any thing farther than countenance. Had I the leaders only to deal with, I believe I could operate more than you expect. But the town is poor, and such factions in the Council, about calling of ministers and candidates for Parliament, that there is no such thing as venturing any extraneous business." In allusion to the same proposal, Mr Arthur Martine, in a letter to Dr Cullen, dated 19th February 1754, says, " But one thing is thought proper, that you come to town on Friday, as that is the time most convenient for you, and lay the affair before Drs Clerk and Sinclair, as a proposition of your friends, which you think of acceding to with their good will. At the same time, we shall think of measures to treat with Plummer or Alston about a resignation."

In the autumn of 1754, on the death of Dr Dundas, Lord Kames again endeavoured to persuade Dr Cullen to remove to Edinburgh, by representing to him the favourable prospect of success, both as a medical practitioner and teacher, which seemed to await him there.

" My Dear Sir, Edinburgh, *September* 17. 1754.

" Upon occasion of Dr Dundas's death, I must be wanting to the friendship I have for you not to resume my former topic. Sinclair is as good as gone, and Clerk is valetudinary. Pray, whom have we? Scarce one physician of any

considerable character. It is my serious opinion you can never have a more favourable opportunity to exert yourself. It will be neglecting your own interest not to lay hold of it. In all appearance you cannot fail of coming soon into business here; and, in the interim, you may give what may be called a private college (Course of Lectures) of Chemistry, which will be successful, and will make you a favourite of the College of Physicians, because it will increase the number of students. In a word, the opportunity is so favourable that it is needless to bestow words upon it. Dr Dundas had a world of business, not only in the town, but in all the towns about it. You may naturally take his place till multiplicity of business confine you within the city. In the mean time, you may find leisure to publish a treatise upon husbandry. A word to the wise is sufficient. Yours,

"HENRY HOME."

In answer to this proposal, Dr Cullen appears to have explained the different circumstances which at this period rendered his removal to Edinburgh hazardous to himself and to his family; upon which Lord Kames again wrote to him in the following terms:—

"MY DEAR SIR, KAMES, 30*th September* 1754.

"You see I am in no hurry with my answer; and indeed it is, to me at least, extremely difficult to come to any positive resolution upon your case. I have carefully weighed the circumstances on both sides; and, putting myself in your situation, all I can make of it is, that at present I scarce would have courage for so bold a step. Twenty years ago I would not have had the smallest difficulty. In a word, I have not the least doubt that the change I propose will turn out to your advantage; but if your resolution is not great, it may fail you in the execution, and spoil all. Yours,

"HENRY HOME."

Dr Cullen having resolved not to incur the hazards

necessarily attendant upon his removal at this time from Glasgow, his zealous friend, though acquiescing in the propriety of his resolution, took occasion to rally him upon the subject in the following letter:—

"My Dear Sir,

"You'll think it now too late for me to observe that the last epistle I had from you appeared to me extremely whimsical. A plan formed,—difficulties struggled with and overcome,—every thing ready for execution,—and, when the hour was come, given up without saying why or wherefore. I was really so well entertained with the oddness of the adventure, that I did not chuse to be undeceived by having a rational cause assigned. Now that the humour is over, I should be glad, if you write an answer, that you clap in a word or two upon the subject by way of postscript.

"Change now the scene to the Philosophical Society, of whose works a second volume is preparing. A demand was made upon me for your paper upon Salts. I either dreamed it, or it was said to me, that you reserved that paper to make part of a greater work. If not, transmit it to me; and if you reserve it, I must insist upon some other paper from you in its stead. You told me you had one upon Remitting Fevers, the publishing of which may do you some service here, if you have not altogether abandoned your project. I insist upon one paper or another; and I insist upon it as a point of right.

"What are our friends doing in the College (of Glasgow) farther than teaching? Are there any discoveries? Is there any progress in science? Are they all *vox et præterea nihil?* I hope better things of them. But I want facts, and would not rest upon hopes. How goes on your farming scheme, in particular? In this science facts would be delightful. Your friend, at least your correspondent, Du Hamel is a ninny. Yours affectionately,

"Henry Home."

In the course of the summer of 1755, Dr Plummer having suffered a shock of palsy, by which he was rendered incapable of continuing his lectures, several candidates for the Chemical Chair immediately presented themselves, or were put in nomination by their friends. Among these were Dr Francis Home, Dr Black, and Dr Cullen. Dr Home had the warm support of his relation and friend Lord Milton, one of the Senators of the College of Justice, who at that time possessed considerable political influence in Edinburgh, and in particular with the Town-Council. Dr Black had distinguished himself by his experiments on magnesia, quicklime, &c. and was justly considered as a person eminently qualified to assist and succeed Dr Plummer; and, to the Medical Faculty, it was an additional and no small recommendation of his pretensions, that he had never been employed in the practice of physic, and was therefore not likely to become their rival in the emoluments of their profession. Dr Cullen, again, was not only regarded as a teacher of great scientific attainments, but also as a skilful and judicious medical practitioner, whose agreeable manners and engaging address could not fail to obtain for him the esteem and confidence of those who consulted him professionally.

In the commencement of the canvass for the Chemical Chair, Dr Black appears, from the following letters, to have taken the earliest opportunity of informing Dr Cullen of Dr Plummer's illness, and of expressing his resolution not to allow any wishes or engagements of his own to interfere with the views of his friend and preceptor.

"DEAR SIR, *Tuesday Night.*

"I write this to inform you of a piece of news, which, though it may be of no consequence to you, I thought it not amiss to acquaint you with. Dr Plummer was lately seized with an apoplexy, which has terminated in a hemiplegia and total loss of speech; so, if you have not abjured Edinburgh, now is your time. Yours in haste:

"JOSEPH BLACK."

About ten days after writing this letter, Dr Black wrote again to Dr Cullen in the following manner:

"DEAR DOCTOR, "*Saturday Night.*'

"You have been misinformed with respect to Dr Plummer. He is not dead, but in the state I told you. Dr Whytt left him on Wednesday with a palsy in his tongue, and one side of his body; and it is not the opinion of the physicians that his life is in immediate danger. As long as he is in this way, the affair cannot come before the Town-Council, but somebody will be appointed by his colleagues to teach for him this winter. What interest you will have among them is hard to tell. You know there is an evident *rub* in the way with respect to practice. From some hints I have received, I have reason to suspect that I am not excluded the possibility of an offer; but I assure you, Doctor, I am absolutely resolved to refuse it, if there is any hopes of its being of any advantage to you.

"For God's sake, do what you can as soon as possible, and let me know if I can do any thing for you. Yours,

"JOSEPH BLACK."

Lord Kames, conceiving that he had now an opportunity of realizing the plan he had formed of bringing Dr Cullen to Edinburgh, entered warmly into this canvass, and wrote immediately to Provost Drummond,

urging Dr Cullen's claims to be Dr Plummer's successor;—to Dr Whytt, recommending him for a colleague;—and to Lord Milton, pointing him out as the fittest person in Europe to fill the Chemical Chair, and as the candidate who would be most acceptable to the Duke of Argyll.

The following letter from Mr Martine to Mr William Wilson of Howden, Writer to the Signet, one of Dr Cullen's early friends, evinces how zealous and active he had been in endeavouring to promote Dr Cullen's interests on this occasion.

"DEAR SIR, "CANONGATE, *Friday*, 6 *o'clock*.

"Since parting with you, I have had conferences with Drs Sinclair, Porterfield, Macfarlane, and Monro, and said what I thought proper to each upon the subject in question. I find from Mr Russel, that Dr Black will be no sanguine antagonist. It is from Lord Milton's influence with the Town-Council that any danger is to be apprehended. I have had two full conferences with Mr Kincaid, who is very hearty and active. He has spoken to the Justice-Clerk, and is to speak to all his friends in Council, and to exhort them all earnestly not to give any promises but agreeably to the Duke of Argyll's pleasure. Till the elections in the Town-Council are over, he says it will not be easy to judge how things may go. He suggests, and that very properly, that it would be of use to apply to Baron Maule to get the Duke to write to his friends in Edinburgh. I go this morning to Fife, where I shall expect to receive your commands. I think I perceive a jealousy in some of the Doctors here of our friend, which I am not ill pleased to observe. I have not wrote to the Doctor. Please to communicate this to him, with my best wishes. Till meeting, my dear Sir, yours,

"A. MARTINE."

During the continuance of Dr Plummer's indisposition, his relations, and the Professors of the other branches of medicine, prevailed upon Dr Black to agree to teach the Chemistry class for the subsequent winter. It appears from a passage of a pamphlet, published at Edinburgh nine years after this, that in the canvass which took place for the Chemical Chair, an application had been made to Dr Cullen for a character of Dr Black. The anonymous author of this pamphlet, who was believed to be one of the Medical Professors, and who was probably very well acquainted with the nature of the opposition which had been made to Dr Cullen's introduction into the University of Edinburgh, says:—"Dr Black is said to have been a candidate along with you when Plummer fell sick, and your letter points him out as the ablest chemist of the two; yet you were chosen." It is much to be regretted that no record of Dr Cullen's letter has been preserved, as it could not have failed to afford an interesting testimony of his regard for Dr Black, and of the high opinion he entertained of those talents and attainments which afterwards procured for him so great distinction as an investigator and teacher of chemical science. Dr Cullen had the pleasure also, a short time after this, of giving a similar testimony of his opinion of Dr Black's merits in recommending him to the Duke of Argyll as a fit person to succeed himself as lecturer on Chemistry in the University of Glasgow.

The following extract of a letter from Dr Hunter, dated 13th December 1755, exposes one of the means which had at that time been employed to produce in

the mind of the Duke of Argyll a prejudice unfavourable to Dr Cullen.

"You know that I rejoice at every thing that turns to your advantage; and I do hope that I shall yet have a great share of joy on your account. * * * * Well, then, all I have to tell you is, the Duke is most sincerely your friend. He has not yet received yours and the Edinburgh recommendation of Mr Black. I did every thing I could to give him a good opinion of young Hamilton for Anatomy, and I did justice to Mr Black's character. Any thing I have been able to do, I need not tell you, was by Dr Stewart; for I have not the honour of being personally known to the Duke. Some of your friends were kind enough to represent you as a Jacobite: but it did you service; for the Duke looked upon it as a piece of underhand malice. I wish you success, and beg to hear from you soon."

It happened, fortunately for Dr Cullen's success, that the Duke of Argyll arrived in Edinburgh during the canvass for the Chemical Chair. Finding the wishes of those who had the right to nominate a successor to Dr Plummer divided chiefly between the other two candidates, his Grace was under the necessity of employing the whole weight of his influence in favour of Dr Cullen; and but for the exertions of that public-spirited and intelligent nobleman upon this occasion, it seems doubtful whether Dr Cullen would ever have obtained a seat in the University of Edinburgh.

The arrangement which had been made with Dr Black by the relations of Dr Plummer and the Professors of Medicine, was not altogether agreeable to the Town-Council, who, as patrons of the University, thought themselves entitled to the principal direction

in an affair of this kind. Some months, however, were allowed to elapse, that it might appear what course Dr Plummer's distemper would take. At length, on the 19th of November 1755, when the session was about to open, and when it was generally believed that Dr Plummer would not recover, the Town-Council appointed Dr Cullen joint Professor of Chemistry during the life of his colleague, with the succession in the event of his death, reserving at the same time to Dr Plummer all the rights and privileges of a Professor, and particularly that of teaching, whenever the state of his health would admit of it.

Immediately on being informed of this appointment, Dr Cullen addressed the following letter to Dr Black:

"Dear Jo,

"I find at length the Town-Council of Edinburgh have elected me a professor. As the conditions are not yet communicated to me, and there are some little difficulties in the way, I have not declared my acceptance; but it is very probable I shall accept, and, with your assistance, I shall get over every difficulty. I want, therefore, to have a little correspondence with you, and to know particularly what influence this election is to have on your measures. While you could expect to be elected a professor, I approved of every step you could take for that purpose, though in direct opposition to myself; but now that I fancy your hopes of that kind are over, I do not expect opposition; I do expect your favour and concurrence. By this I do not mean that you should pass from any engagements you have come under for this winter; nay, I shall be very well pleased that these go on; but I am now told you are really not to teach. I have not this, indeed, from any certain authority, but am apt to believe you may shrink from the toil of making up an entire

course of lectures in the hurry you would be put into. Pray let me know from yourself what is in all this. I am pressed from Edinburgh to come to teach this winter; but this, particularly on account of my engagements here, I would avoid if possible; however, if you really have dropt your former engagements, and would now in any shape concur with me, I think I could make the matter easy to us both. In short, if you can be concerned with me in teaching, I shall ease you of a great part of the labour,—I want no part of the profits. It would be making too much haste to explain further a scheme, which, perhaps, in every form you will reject, so I wait your answer. I shall only say further, that I hope my election is no great disappointment to you: certainly at your time of life, and in your situation, I should not think it such. You have certainly something much better to expect; and I now flatter myself, that, instead of fretting at my success, you will rather embrace the opportunity of this critical situation of my affairs, to assist as far as you can in establishing me and my family for life. My breast is full, but I can say no more. I am, Dear Jo, yours affectionately,

"WILLIAM CULLEN."

"GLASGOW, 21*st Nov.* 1755."

Previously to his having received this letter, Dr Black appears to have written the following to Dr Cullen:

"DEAR DR,

"As I have now an opportunity of returning your compliment, I sincerely wish you joy and success in your new employment, and should have done it by last post, but that I expected to have seen you here before now. It is indeed very proper you should come as soon as possible, in order to settle matters with respect to the teaching for this winter, as I am afraid you will receive but little help or encouragement from the Professors. They all seem to be very much

out of humour at the Town-Council's having managed this affair with so little ceremony, and as if the College had no sort of concern in the matter. Plummer himself will certainly be highly incensed; and as the laboratory is entirely his property, you need not expect to obtain the use of it for this winter; but you may probably get Scott's. His apparatus is probably very imperfect for a course of Philosophic Chemistry, but you need not be anxious, provided your course be better than Plummer's, which it is impossible for it not to be. I had lately the honour of dining with Lord Deskford, who spoke very favourably of you. He asked me if I had seen ————'s new book upon Bleaching, and intimated that the whole, or greater part of it, was stolen from a paper of yours. I am, Dr. Dr, yours,

" J. BLACK.

" EDINBURGH, 22*d Nov.* 1755."

The medical professors objected to the appointment of Dr Cullen, that it had been made without the consent or demission of Dr Plummer, who upon this ground had resolved to protest against Dr Cullen's admission into the University; and they stated that the Senatus Academicus would therefore decline receiving Dr Cullen into their body until he should either obtain Dr Plummer's demission, and purchase his laboratory, or until the point at issue between the Senatus and Town-Council should be determined in a court of law by a declaration of privileges.

Dr Cullen being well aware that Dr Plummer's recovery was considered by his medical attendants as hopeless, accepted the office to which he had been nominated by the patrons of the University, and entered upon the duty of teaching chemistry in the beginning of January 1756. Whether he actually took any steps on his coming to Edinburgh to obtain a formal admis-

sion into the University does not appear; but that this was delayed for some time, and that he consulted on the subject his friend George Drummond, Esq., at that time Provost of Edinburgh, is evident from the following letter, highly characteristic of the good sense, superior talents for business, and public spirit, which distinguished the civic administration of that intelligent and patriotic magistrate.

"My Dear Sir, London, 3*d February* 1756.

"I have got your favour of the 27th ultimo. It is very obliging in you to mention the part I took in bringing you into the University of Edinburgh in the manner you are pleased to do. But indeed, Sir, I cannot claim any merit in it. I had the opinion of the man on earth to whose judgment, in matters of that kind, I pay the greatest deference, that you were the man in the island best qualified to fill the vacant professor's chair; and I thought myself happy to have any share in bringing you to it.

"I observe the manner in which the medical people talk to you about your admission into the University, and that they have proposed your taking the laboratory off Plummer's hands, at the price put upon it when he purchased their shares of it from Sinclair and Rutherford, as the condition on which they will in a manner undertake that Plummer will resign along with you, in order to your being elected joint professors, as Monro and his son were lately, and then the admission will be unanimous.

"You are pleased to ask my advice what you should do in this matter; whether to push the admission, or harken to Monro's proposition, and therefore I will tell you how I think of it, not as a present magistrate, but without reserve, with the openness of a friend who is warmly devoted to serve you.

"I have before me a list of the members of the Univer-

sity, marked off in three classes :—eight who have declared against admitting you, unless the proposition is complied with; seven who have declared they will admit you; and the names of such of them as have not yet explained themselves either way, seven, whereof three do not attend meetings.

" It is probable that, by proper application to them, a majority might be got for the admission; this is only to be got by bringing three of the four of the last seven who attend over to us, which at this distance I am uncertain about; for that reason I am averse to put things upon that issue.

" I am quite sensible that there is a wide difference between the situation the professors of Medicine were in at the time they were established in the University and yours. At that time, we were only making a trial, and were somewhat uncertain about its success; and yet they thought themselves favoured by the Town-Council in giving them a preference in our choice to another set who petitioned us to appoint them. At that time I should have cheerfully come in to the Town's accommodating them with a laboratory, if our finances would have admitted of it. Your case is different. Every body is now sensible that the Medical College is an immense benefit to the community, and must continue to be so as long as the Town-Council do justice to the Town, by filling up vacancies, when they happen, with professors of established reputation. Such is the present case. You don't court us to choose you; we court you to come to us; and, of course, I certainly am of opinion that sooner or later the Town should accommodate you with a laboratory, and I should be for doing it now, if I did not think that difficulties might arise in it from the present state of their revenue, which would determine the Council not to come into it at this juncture. As to the sum the Town has a claim of upon Plummer's laboratory, I am apt to think the Council will not demand it in any other event but their seeing the house was to be used for any other purpose but a labora-

tory; so that the purchase-money will not exceed £120, and the professor who purchases it will have that sum refunded to his family from his successor, I think easily and certainly, as its situation, so near the Infirmary, is the most convenient spot that could be got for it.

"I should not approve of the Town-Council's entering into a treaty with the Professors about getting over the difficulty they have created to your admission; and yet, I think they would not do a thing below themselves if, upon your and Plummer's resignation, they re-elect you jointly.

"I am sensible, at this distance, I may be a stranger to some of the circumstances in this affair, for want of knowing the sentiments of my friends in the Council about it, from my having nobody here to advise with in it, and from my being obliged to give most of my attention at present to some matters of consequence which I am in some sort charged with the care of: Yet, hurried as I am, I was unwilling to suffer your letter to lie before me a single post. I have said all that occurs to me upon it; yet have the goodness, if you differ from me in the advice I have taken the liberty to offer, to acquaint me, and act according to your own judgment; for, I do assure you, if I were upon the spot, after laying it before two or three of my friends, who I know are heartily yours, I would go through every measure that you thought for your service; for I am, with great truth, Sir, your faithful and obedient humble servant,

"GEO. DRUMMOND, *L. P.*"

The measure proposed by Dr Monro *primus*, and approved of by Provost Drummond, in the foregoing letter, appears to have been soon afterwards carried into effect, and Drs Plummer and Cullen were, by a commission, signed on the 10th March 1756, elected joint Professors of Chemistry in the University of Edinburgh. On Dr Plummer's death, which happened in the month of July following, Dr Cullen was

elected sole Professor of Chemistry,—a situation which he continued to occupy for ten years.

Several years after he was appointed Professor of Chemistry in Edinburgh, Dr Cullen, in reviving an ancient but neglected custom of the University, delivered a Latin oration in presence of the Patrons, the Members of the Senatus Academicus, and the Students, on the nature and objects of the branch of science he had been appointed to teach, in which he took an opportunity of publicly expressing his obligations to Mr Drummond, who then held, for the fourth time, the office of Chief Magistrate, for the share he had taken in introducing him into the University.

By a list of the names of his chemical students, kept by Dr Cullen, it appears, that during the first course of lectures the number amounted only to seventeen. During the second course it rose to fifty-nine; and it went on gradually increasing so long as he continued to lecture on chemistry:—the greatest number that attended during any one session was 145. In looking into this list, it is curious to observe that several of those pupils, who afterwards distinguished themselves by their acquirements or writings, had attended three, four, five, or even six courses of Dr Cullen's chemical lectures.

Dr Cullen had foreseen that he would have many difficulties to contend with in removing from Glasgow to Edinburgh. In making this change of residence, he had to submit for a time to an almost complete loss of income. There being no salary attached to his professorship, the fees of the students were the only certain resource to which he could trust for the main-

tenance of his numerous family, till he should be able to establish himself as a medical practitioner. That he met the difficulties of his new situation with a cheerful and sanguine disposition, appears from the following account which he gives to his brother-in-law Major Johnstone, of the manner in which he spent the first months of his residence in Edinburgh.

" Dear Captain,

" I am very glad to learn, by yours of the second current, that Mrs Johnstone and you, with your two sons, are in good health. Of Mr George, I give you joy with all my heart. I have intended to write every day these six months, but by some accident or other I have been in perpetual hurry, and since my change of situation do not yet know where I am. I came in here to teach in the month of January, and was obliged to be very busy that I might overtake the session. My hurry was much increased by the frequent interruptions which my wife's illness gave me by calling me to the west country. She has had a long and tedious illness this spring, which, however, I hope is now well over. Just when I was leaving Glasgow, about four weeks ago, I got a broken shin, which was a small matter at first, but being therefore neglected and very ill used, it has since given me a great deal of trouble, and confined me to the house, and almost to my bed, ever since I came to Edinburgh. My family are still at Parkhead, and to continue there for the rest of the summer. My wife has been here to attend me; but now I begin to go abroad, she went west on Thursday to see the children, among whom there are some small ailments.

" As to the effects of my change of situation, I really as yet can say nothing. The hurry I was kept in by my teaching during the time I was here in the spring, did not allow me to take the measures for getting practice that may be necessary. The business of my removing from Glasgow, and other affairs in the west, have still hindered me from

doing any thing here. My sore leg has now spent a great part of the summer session, and in the harvest vacation there is nothing to be done; so that, upon the whole, I shall not think myself settled here till winter, and even then I must lay my account with business coming slowly. Some time must pass before I can either know myself, or tell my friends how I am to fare in this place. Only I shall tell you that I live in good hopes. Yours affectionately,

"WILLIAM CULLEN."

Part of the summer of 1756 appears to have been employed by Dr Cullen in extending and improving his lectures on chemistry, and in preparing, as an introduction to his course, a history of that science. This history occupied no fewer than seven lectures, which, instead of being spoken from short notes, as was his usual mode of lecturing, were written out in full, and appear to have been afterwards interlined with various additions and corrections. These lectures contain so many general remarks, and exhibit so just a specimen of Dr Cullen's manner of composition for his lectures, that I have judged it right to insert portions of them in the Appendix *, believing that they may still be read with interest by the lovers of chemistry.

It must have been with a view to relieve himself from the pecuniary embarrassments which he either felt or apprehended at this period, that Dr Cullen proposed to undertake the translation of Van Swieten's Commentaries on Boerhaave's Aphorisms; a labour in which it would seem he expected to have the assistance of Dr Hunter and of Dr Black. His prospects of success in medical practice in Edinburgh, however, soon became

* See Appendix, Note P.

so promising as to render the execution of this scheme unnecessary; but it is agreeable to record such unequivocal proofs as are contained in the following letters, of the interest which his two favourite pupils took in the success of the undertaking which he had projected.

"DEAR SIR, LONDON, 16*th November* 1756.

"I had the pleasure of a letter from you just the next day after I had sent off one for you. Though I cannot give you a more agreeable account of matters now, yet, considering the anxiety you show in that last letter, I thought a second push with the booksellers was indispensable on my part. I have been this day with Messrs Clark and Davidson, but have found them tenacious of their former opinion, that there is nothing at present in the medical way they should choose to engage in. I spoke of Van Swieten's Commentaries, but he said it was too expensive to print such a book, except (which may happen) it should have in time an universal good character. I daresay you do not suspect my inclination to serve you, even were my interest excluded from the point. It is therefore with great reluctance and concern that I must declare my diffidence in the booksellers here. I wish you may be able to do something with Kincaid. My bargain with Clark was to have a guinea every printed sheet. This may help you to determine your price. I hope if you were once well known to Kincaid, you may be very fortunate. I have found a specific, in the author of the Georgics, against the Spleen that is occasioned by *Pauperies*, which has been a wonderful palliative cure, at least with me, and has almost reconciled me to charms; as I have no doubt of its efficacy, in every case where there is faith enough, I will communicate it to you.

'Labor omnia vincit
'Improbus, et duris urgens in rebus egestas.'

"You have my eternal best wishes,

"WILLIAM HUNTER."

From Dr Black to Dr Cullen.

"DEAR DR, GLASGOW, *8th June* 1757.

"I own my last letter was wrote in a hurry, and I forgot many things which I intended to say; but you may remember I formerly told you I was willing to try the half of the translation, though I am afraid it will save you very little trouble, as you will be reduced to the necessity of correcting my language, so far as that the book may appear of a piece: But are you ripe yet for the attempt? Are you sure that you won't be prevented in London? And is any bookseller in Edinburgh willing to purchase the copy, or will they even venture upon it if they get it for nothing?

"As for myself, I shall take what part you please, and will begin immediately, if you are resolved upon it.

"My compliments to all friends, and believe me yours,

"JOSEPH BLACK."

Dr Cullen's design of translating Van Swieten's Commentaries seems to have been relinquished, and his attention directed in a particular manner to medical pursuits of his own, by his being engaged, soon after his removal to Edinburgh, in delivering lectures on Clinical Medicine. In this country, the merit of first proposing to explain, in clinical lectures, the nature and treatment of the cases of patients admitted into a public hospital, belongs to Dr John Rutherford, Professor of the Practice of Physic, to whom this privilege was granted by the Managers of the Royal Infirmary of Edinburgh, in the year 1748. In the following year the same privilege was extended to the other Professors of Medicine in the University, though none of them, besides Dr Rutherford, appear to have availed themselves of it till the session of 1757, when Dr Cul-

len undertook to deliver clinical lectures in the Infirmary. His colleagues, Drs Whytt and Monro, were induced to join him in the performance of that duty, which Dr Rutherford, probably on account of the infirm state of his health, declined any longer to discharge. By this arrangement, a new and strong impulse was given to the teaching and study of practical medicine in Edinburgh, and clinical lectures have ever since been delivered annually in the Infirmary by the medical professors, with a diligence, ability, and zeal, which have done the greatest honour to themselves; while their lectures in this department have contributed, perhaps more than any other circumstance, to extend and maintain the reputation and usefulness of the medical school.

Few, if any, traces of the useful and important mode of teaching practical medicine by example are to be found in the early records of physic. Hippocrates, indeed, we are told by Galen, was in the use of conducting his numerous pupils to the bed-sides of his patients, for the purpose of instruction; and from a well known epigram of Martial (which, on account of the warning it contains ought never to be forgotten by clinical teachers or their pupils) it appears that in his time a similar practice was followed by one at least of the physicians of Rome.

> Languebam; sed tu comitatus protinus ad me
> Venisti centum, Symmache, discipulis.
> Centum me tetigere manus, Aquilone gelatæ,
> Non habui febrem, Symmache; nunc habeo.
>
> *Lib.* v. *Epig.* 9.

Some centuries after the introduction of the Chris-

tian religion, the conjunction of hospitals for the reception of the sick poor, with medical schools, is said to have been effected in the schools of Alexandria in Egypt, of Gandisapora in Persia, and of Edessa in Mesopotamia; and in the great seminary of learning which flourished at Bagdad, attendance on hospitals is said to have formed a constituent and important part of the system of medical education.

In modern times, the practice of giving lectures on the cases of patients admitted into public hospitals appears to have been begun in Italy, in the schools of Padua and Pavia, about the middle of the sixteenth century; but it seems, after a time, to have been forbidden, and, consequently, suspended in that country. Before the middle of the seventeenth century, however, clinical lectures had been introduced into the medical schools of Holland, by Stratten of Utrecht, and Otho Heurnius of Leyden; and in 1658 they were zealously resumed in the latter of these schools by Sylvius de le Boe, the successor of Heurnius in the chair of Practical Medicine*. It was in the same school that, at the beginning of the eighteenth century, the method of clinical instruction was so successfully pursued by the illustrious Boerhaave, by whose pupils, Drs Rutherford and De Haen, it was introduced into the medical schools of Edinburgh and Vienna; into the former, as already mentioned, in 1748, and into the latter in 1754. For a considerable time after this, clinical

* Dr Beddoes' Letter to Sir Joseph Banks, 1808. Hildenbrand Annales Scholæ Clinicæ Medicæ Ticinensis, t. i. p. 4–28. Brera Prolegomeni Clinici, pp. 9. & 13.

teaching seems to have been confined chiefly, if not entirely, to these two schools. The labours of De Haen were continued uninterruptedly for a period of more than twenty years in Vienna, and the exertions of Dr Cullen during eighteen years of the same period in Edinburgh, assisted by those of his colleagues Drs Whytt, Gregory, and Home, contributed powerfully to excite a general interest in clinical instruction, and rendered the benefits of this mode of teaching medicine so apparent, as to lead gradually to its introduction into other medical schools in various parts of Europe. Indeed, after the advantages of this practice have been so fully demonstrated in all the situations in which it has been followed, it seems remarkable that the regular delivery of clinical lectures on physic and surgery should not everywhere be regarded as a fundamental and necessary part of the medical constitution of all hospitals for the reception of sick and diseased poor, that are erected and maintained at the public expense. That the primary object of such institutions is the relief and cure of the patients admitted into them is unquestionable; but it seems to be still very imperfectly understood how great the advantages are that would result to all classes of the community were the opportunities of professional improvement, which attendance upon hospitals is calculated to afford, extended to as great a number of those engaged in the study and practice of physic and surgery as may be consistent with the main objects of such establishments, where it is intended by the public that the poor shall receive every advantage which money can procure for the rich, from the attentions, skill and humanity of able phy-

sicians and surgeons. While clinical lectures are in an eminent degree useful to practitioners and to students, it is not less certain that they must indirectly benefit the patients who are the subjects of them; for if there be any thing which can induce the rash practitioner to pause, the inconsiderate to reflect, or the ill-informed to seek for instruction, it is the necessity of explaining the grounds of his practice, and his opinions of disease, to an audience composed of well-educated medical students.

The results of De Haen's observations and experience in the clinical school of Vienna were published by him from time to time in his "Ratio Medendi," a work consisting of sixteen small volumes,—which may be said to constitute an era in the history of practical medicine, not only on account of the accurate descriptions and cases of diseases, with the numerous *post-mortem* examinations which it contains, but also on account of its being the first medical work in which the results of the observations made, and the experience obtained, in a great clinical school, have been communicated to the public. It is not easy from these volumes to ascertain upon what particular plan De Haen conducted his clinical lectures. That he was at great pains, however, to instruct his pupils in what he regarded as the Hippocratic method of observing and of treating diseases, is obvious from every page of his writings; but if his daily instructions were at all similar to his writings, they certainly can by no means be regarded as having been adapted to those who were just entering upon the study of clinical medicine. Nothing, indeed, can be conceived less elementary in

their conception, arrangement, and execution, than the clinical reports of De Haen. These reports consist, in the greater part, of the histories of rare and curious cases, many of them more of a surgical than of a medical character, and of learned dissertations, historical, critical, and controversial, altogether foreign to, and inconsistent with, the proper business of a course of clinical instruction. Indefatigable industry, much learning, accuracy of observation, and fidelity of statement, are qualities every where evident in the *Ratio Medendi* of De Haen ; but there is a want of method, connection, and alliance, in his observations, which, however valuable they may be in themselves, renders them so many detached and isolated facts,—the *discordia semina rerum*. His dissertations, however, on critical days, on the temperature of the human body, and on the buffy coat of the blood, on the different kinds of fever, acute and malignant, on death from drowning and hanging, as well as his historical and detailed account of particular diseases, such as dropsy, colica pictonum, epilepsy, tetanus, scurvy, and plague, must be regarded as valuable contributions to medical science. Of his pathological observations, those are the most numerous and important which relate to pleurisy, to dropsy, to rupture and iliac passion, to the diseases of the kidney and urinary bladder, and to those of the heart and larger bloodvessels, with the histories of a few cases of apoplexy. The accounts given of the diseases of the other organs of the body are so few in number, as to occasion surprise how De Haen's attention could have been confined for so long a period to the pathological effects of so limited a number of diseases. He has given, for

example, only one instance of the inspection of the body after death from fever.

Besides giving clinical instruction in the hospital, where he seems to have had it in his power to select for the observation of his students whatever cases of disease he chose for the subject of clinical remark, De Haen was occupied as a Professor in the University in teaching pathology and therapeutics. In this course, he employed as his text-book the Institutions of Boerhaave, which he followed very closely, though not servilely, in almost every point of doctrine, as may be seen by comparing his posthumous lectures with the last volume of Boerhaave's *Prælectiones Academicæ*, as published by Haller.

Dr Cullen possessed in a remarkable degree the qualifications necessary for a clinical teacher. To a minute and extensive knowledge of all the auxiliary branches of medicine, and to the great experience which he had acquired by private practice, he added a peculiar talent for the observation and accurate description of diseases, accompanied with a strong desire, and the ready power of communicating his knowledge in the most interesting manner to his pupils. During the whole of the eighteen years that he was occupied in lecturing on clinical medicine in the Royal Infirmary, he bestowed much time and most uncommon pains on the proper performance of that duty. Of the notes from which he spoke his clinical lectures, those of the first five years and of the eighth year have by some accident been lost; but those of the other twelve years have been preserved. In these notes, constant reference is made to the case-books and the daily reports of

his patients, shewing how closely he followed, and what use he made of these in his lectures. Besides having the histories of the cases of his clinical patients carefully drawn out by his assistants, and giving himself daily reports of their progress, and of the medicines prescribed (all of which, conformably with the practice of the hospital, were inserted into journals open for the inspection of his students), it appears from his manuscript clinical lectures that he was accustomed to write down, previously to lecturing, full notes of all those particulars respecting each individual case, the symptoms of the disease, its diagnosis, prognosis, and probable causes, and the effects produced upon it by the remedies employed, which appeared to him to require or deserve attention; together with an account of the morbid appearances observed after death in those cases which had a fatal termination; in short, to take notice of every circumstance which could tend in any way to awaken the observation, and to extend the practical knowledge of his students. Even his mistakes and errors in practice, as well as his skill and success, Dr Cullen never failed to render available to these purposes. Of his candour in this respect, the following passage, taken from a clinical lecture delivered on the 21st April 1772, may serve as an example: "It is not improperly said that the earth hides the faults of the physician. If every patient that dies were opened, as ours has been, it would but too often discover the frivolity of our conjectures and practice. In these lectures, however, I hazard my credit for your instruction, my first views—my conjectures—my projects—my trials—in short, my thoughts

—which I may correct, and if necessary change; and whenever you yourselves shall be above mistakes, or can find any body else who is, I shall allow you to rate me as a very inferior person. In the mean time, I think I am no more liable to mistakes than my neighbours, and therefore I shall go on in telling you of them, when they occur. With regard to the present case, I might go back to consider the symptoms, and from them endeavour to account for my own ignorance; but I choose rather to acknowledge my mistakes, and to consider the case on the footing which we have now learned from dissection."

Besides possessing high qualifications as a clinical lecturer, Dr Cullen had the strongest motives to employ all the powers of his mind in this new field of professional exertion. His lectures on clinical medicine afforded the most favourable opportunity that could be desired of exhibiting publicly in Edinburgh his talents as a teacher of medicine, of evincing his skill and experience as a practical physician, and of establishing by those means a claim to the chair of the practice of physic, when it should become vacant by the resignation or demise of his colleague Dr Rutherford.

Endowed with these qualifications, and animated by these motives, Dr Cullen speedily obtained great reputation as a teacher of clinical medicine, acquired the esteem and admiration of his students, and gained by his attention and kind manners the universal confidence of his patients. His lectures were distinguished by that simplicity, ingenuity, and comprehensiveness of view, which marked at all times the

philosophical turn of his mind; and, as I have been informed by several eminent medical men who had an opportunity of attending them, and more particularly by one who acted as his clinical clerk in 1765 *, were delivered with that clearness and copiousness of illustration with which in his lectures he ever instructed and delighted his auditors. It is to be regretted that the benefits of Dr Cullen's clinical experience were for many years in a great measure confined to his own students; and that, from his other engagements and pursuits, he was prevented from giving the results of this experience in a more direct manner than that in which we now possess it in his works on Nosology, Materia Medica, and the Practice of Physic. Had these results been presented to the public in the form of clinical reports, like those of his contemporary De Haen, it is impossible that the erroneous assertion so often ignorantly repeated of Dr Cullen's being merely a speculative teacher of practical medicine, could ever for a moment have been entertained by the foreign medical public. No teacher of practical medicine, as I shall have frequent occasion to shew in the course of this narrative, was ever at more pains than Dr Cullen to distinguish between well ascertained matters of fact and the assumptions and conclusions of hypothetical reasoning. In leading his students to reflect and to reason, it was his constant endeavour to teach them to observe the course of Nature in diseases, to discriminate between their uniform and essential symptoms and their merely accidental combinations, and to ascertain, as far as is possible by

* The late Dr John Fleming, long a member of the Medical Board of Calcutta.

observation and analytical reasoning, the respective influence of the remedies employed by art, and of the operations of nature in the cure of diseases. "There is nothing," he observes in one of his clinical lectures, "I desire so much as that every disease we treat here should be a matter of experience to you; so you must not be surprised that I use only one remedy when I might employ two or three; for, in using a multiplicity of remedies, when a cure does succeed it is not easy to perceive which is most effectual; but I wish that you may always have some opportunity of judging with regard to their proper effects." In another lecture he remarks: "Every wise physician is a dogmatist, but a dogmatical physician is one of the most absurd animals that lives. We say he is a dogmatist in physic who employs his reason, and, from some acquaintance with the nature of the human body, thinks he can throw some light upon diseases, and ascertain the proper methods of cure; and I have known none who were not dogmatists except those who seemed to be incapable of reasoning, or who were too lazy for it. On the other hand, I call him a dogmatical physician who is very ready to assume opinions, to be prejudiced in favour of them, and to retain and assert very tenaciously, and with too much confidence, the opinions or prejudices which he has already taken up in common life, or in the study of the sciences. Now, I profess to be a dogmatist, but I should be sorry if any person thought me dogmatical; for there are but few theoretical opinions which I have received or offered to communicate, with regard to diseases, concerning which I am not ready to doubt, and to admit grounds for doubting, as soon as they are

offered to me. I know that there are no universal rules in the practice of physic, but there are general rules, which all admit of, with more or fewer exceptions, in theory and practice." To ascertain these general rules was the constant aim of Dr Cullen, in all his observations, speculations, and reasonings,—the great end which he proposed to himself in all his labours. Indeed, though professing to follow dogmatism, he was, in every thing which related to the practice of physic, a strict empiric. "The foundation of this," he every where asserts, "is fact and experiment." "Dogmatism and empiricism in medicine are equally founded on experience." "All our knowledge of nature, indeed, consists in experience."

In the selection of cases for the observation of his students, he was at great pains to make choice of diseases which are of daily occurrence, and an accurate and scientific knowledge of the treatment of which is of fundamental importance to the student of medicine. The different forms of febrile, inflammatory, and eruptive diseases, as they occur in the progress of the seasons, and their resemblances and differences, as they present themselves in different individuals, were with him favourite subjects of observation and remark; and, in the consideration of these, as well as of the more frequent forms of chronic diseases, such as nervous disorders, consumption, and dropsy, he made a constant reference to the clinical labours of De Haen, and to the writings of the best practical authors of his own and of former times. Every disease, however slight, seems to have been made by him the subject of observation; and the marking of its symptoms as they

appeared in concourse and succession—of its chief diagnostic characters—of its accidental combinations—of its spontaneous changes—and of those effected by the remedies employed—were constantly pointed out as objects to be particularly attended to by his students. Any discussions relative to the causes by which the disease was produced, or to those states of the functions or structure of the body upon which its principal phenomena were supposed to depend,—which Dr Cullen regarded as the proximate causes of diseases,—were either intermixed as incidental remarks, or made the subject of a separate discussion, and even occasionally of a distinct lecture.

In adhering to the greatest simplicity in the use and prescription of medicines in the treatment of his clinical patients, and in employing only those which he deemed to be necessary and efficacious, Dr Cullen was at all times disposed to avail himself of the opportunities which a clinical institution affords of trying and of verifying the medicinal powers of any new remedy recommended by practitioners of credit and experience. There can be little doubt that it is to trials made by him in the clinical wards of the Royal Infirmary that we owe, in a considerable degree, the introduction into general practice in this country of various powerful articles of the materia medica which are now in daily use, such as Cream of Tartar, Hyosciamus, Cicuta, James's Powder, or the Pulvis Antimonialis, and Tartar Emetic, the last of which he was accustomed to employ in all febrile affections accompanied with inflammatory action, in doses of several grains, given in the course of a few hours, either subsequently to blood-

letting, or, in other instances, as a substitute for that remedy. In a clinical lecture delivered 5th April 1768, Dr Cullen mentioned the effects which seemed to have been produced by this remedy in several cases of intermittent fever, in which he had administered it as a substitute for the Peruvian bark. The cases of intermittents, which he seems to have considered as most advantageously treated by emetic tartar, were those in which the intermission was so slight as to give much the appearance of a remittent, or even of a continued fever, and those in which there existed some local inflammatory affection. In the same lecture he gives an account of some cases of continued fever, in which he had employed the emetic tartar, with a view to diminish inflammatory action, and as a substitute for bloodletting. The doses in which he employed this remedy, in these instances, seem to have varied from a quarter to half a grain, and to have been frequently repeated. In another clinical lecture, delivered 8th January 1773, he remarks: "Most of our patients are affected by vomiting or purging by a grain of emetic tartar or less. In this case, three grains given in the course of the day had no sensible effect. This might be owing to the peculiar constitution of the patient; but it often arises from an insensibility produced by the disease. Where we depend upon this remedy, the dose must be increased, but with caution, as it will purge though it may not nauseate or vomit."

Another great purpose of clinical instruction, which Dr Cullen held constantly in view, was the inspection of the bodies of those who died while under his treatment. In a clinical lecture which he delivered 20th

December 1768, in speaking of the difficulty of arriving at any general theory of epilepsy, and observing that an attempt towards establishing a proximate cause, so far as we can be guided by dissection, is always the first step in framing such a theory, Dr Cullen took an opportunity of earnestly recommending industry in opening morbid bodies,—a study, he remarks, but little attended to;—and of pointing out the best guides which students at that time possessed for prosecuting this branch of medical science.

"This study," he remarks, "lies particularly in three books—Bonetus, Morgagni, and Lieutaud.

"The first is a great and useful compilation, but with many faults. Much of it is before the age of good anatomy, inaccurate and superficial. Even of that age, some things are omitted. The histories are inaccurately abridged; positive errors are committed; many cases are repeated; and, upon the whole, no pains are taken to guide the student.

"At length Morgagni's work has appeared. It renders Bonetus' work more useful, by correcting his errors, pointing out his inaccuracies, and marking his repetitions; but especially by supplying his omissions and defects by so many new cases from his own observation, and from that of Valsalva and other authors. To all this matter of fact he has added much reflection, offered ingenious speculations and valuable conjectures. This book, however, I do not find read, though I have often regretted that it was not published in my younger days. I can see some reasons why it is not perused, and can tell how it ought to be. Whoever reads Morgagni's work in the order in which it lies, immediately meets with many disjointed particulars that no memory is equal to. Nobody can read it in that manner but one who has already formed to himself some

system to which he can apply the particulars: but few of you are in this condition, and you can only read and consult it occasionally as you proceed in the system; and you may proceed much faster if you will attempt a system out of this very work. For this purpose there is the assistance of the *index morborum* and the *index visorum;* but they are both of too general titles: the supplying this should be the business of the student. Under each title of *disease*, every reference should be marked by the morbid state that was found: and, under each title of the *visa*, the reference should mark the disease or symptoms connected with it: and all this should be united by some systematic views; that is, the disease should bring together the same morbid states, *et e contrario.*

" Lieutaud has been sensible of the propriety of this, and has given his work on this very plan, and has very properly extended it beyond Morgagni. He has united Bonetus, Morgagni, and all the other dissections he could acquire; so that this, for the present, is an *instar omnium.* But it disappoints our expectations in many respects. The first part is by Lieutaud himself; he has abridged the cases and dissections very much, and in many cases with little judgment; so that it can serve but as an index, and even not as this, as he has not referred to the work, or part of the work, from which he has taken his facts, an omission which, with regard to large works, is intolerable. The second part is by a young physician, is more abridged, and not more judicious or accurate."

In commencing annually his clinical lectures, till a sufficient number of patients should be collected, Dr Cullen was accustomed to give one or more general lectures explanatory of the nature and object of clinical instruction, and of the manner in which he intended to conduct his lectures. Sometimes in place

of these, he gave one or more general lectures upon some subject of pathology, nosology, or general therapeutics. During the first nine years in which he lectured on clinical medicine, and while he remained Professor of Chemistry, he deemed it proper to give occasionally throughout the course general remarks or discussions arising out of the consideration of the cases under treatment, and which were rendered necessary by the peculiar views of the animal economy, that he himself had adopted. But after he became Professor of the Theory of Medicine, on the death of Dr Whytt, he seems to have in a great measure abandoned giving general discussions of any kind in the course of his clinical lectures, and to have confined himself strictly to the consideration of the different circumstances of the cases actually under the observation of his pupils; presuming that they had been made acquainted with the views which he took of the animal economy, and in particular of the functions of the nervous system, in the states both of health and disease, and of the action of remedies, by their attendance upon his Lectures on the Institutions of Medicine.

The novelty of many of the views which Dr Cullen took of the animal economy, when he first began to deliver clinical lectures, particularly those respecting the influence of the nervous system in the production and modification of diseases, while it attracted the attention of his auditors, and heightened the interest which they took in his lectures, seems to have had the effect of exciting in some, whom education and habit had rendered partial to the more established doctrines of the mechanical and humoral pathologies, that keen opposi-

tion which the dread of innovation, and the fear of being excelled in professional reputation, are so apt to occasion in weak minds. Dr Cullen gives himself an account of this opposition in the following passage, taken from a MS. lecture, introductory to his course on the practice of physic, delivered in the session 1783–4.

"When I studied physic in this University, about forty-eight years ago, I learned the system of Boerhaave; and, except it may be the names of some ancient writers, of Sydenham, and a few other practical authors, I heard of no other names or writers on physic; and I was taught to think the system of Boerhaave to be very perfect, complete, and sufficient. But when I retired from the University, being very much addicted to study, I soon met with other books that engaged my attention, particularly with Baglivi's Specimen *De Fibra Motrice et Morbosa*, and at length with the works of Hoffman. Both of these opened my views with respect to the animal economy, and made me perceive something wanting and to be added to the system of Boerhaave. I prosecuted the inquiry; and, according to the opportunities I had in practice and reading, I cultivated the new ideas I had got, and formed to myself a system in many respects different from that of my masters. About twenty years after I had left this University, I was again called to it to take a Professor's Chair, when I still found the system of Boerhaave prevailing as much as ever, and even without any notice taken of what Boerhaave himself, and his commentator Van Swieten, had in the mean time added to his system. Soon after I came here, I was engaged to give clinical, that is practical, lectures; and in these I ventured to give my own opinion of the nature and cure of diseases, different in several respects from that of the Boerhaavians. This soon produced an outcry against me. In a public college, as I happened to be a Professor of Chemistry, I was called a Paracelsus, a

Van Helmont, a whimsical innovator; and great pains were taken in private to disparage myself and my doctrines. This went so far, that my friend and patron, the late George Drummond, whose venerable bust you see in the hall of the Infirmary, came to me, requesting seriously that I would avoid differing from Dr Boerhaave, as he found my conduct in that respect was likely to hurt myself and the University also. I promised to be cautious; and upon every occasion I spoke very respectfully of Dr Boerhaave. I have continued always to hold the same language as I expressed in my last lecture; and I shall still do it most sincerely, as I truly esteem Dr Boerhaave as a philosopher, a physician, and the author of a system more perfect than any thing that had gone before, and as perfect as the state of science in his time would permit of. But with all this, I became more and more confirmed in my own ideas; and especially from hence, that I found my pupils adopted them very readily. I was, however, no violent reformer; and, by degrees only, I ventured to point out the imperfections, and even the errors, of Dr Boerhaave's system; and I have now done the same in the preface which I have given to the new edition of the First Lines."

That the prejudices excited against Dr Cullen, in consequence of his relinquishing the doctrines of Boerhaave, and adopting those of Hoffmann, as the basis of a system of medicine which he endeavoured to frame for himself, and the inclination manifested during his lifetime to represent him as a merely speculative teacher of practical medicine, should have been adopted by his professional rivals, or by those unacquainted with that constant reference to observation, and to the results of experience, which he observed in all his inquiries and reasonings, will not surprise any one conversant with

medical history; but that these prejudices should have been imbibed, retained, and propagated, long after his death, by no less distinguished a medical philosopher than M. Pinel, who was himself so well acquainted with Dr Cullen's writings, must be matter of equal surprise and regret.*

There can be no doubt that the reputation which Dr Cullen acquired by his clinical lectures, and the opposition which he encountered, contributed each of them to bring him rapidly into the notice of the public, and to procure for him that high estimation, both as a teacher and practitioner, which had been anticipated by his friends. "Nor," as is remarked by Dr James Anderson, who, both as a pupil and friend, had

* "Un des professeurs de médecine pratique les plus célèbres de l'école d'Edimbourg, le Docteur Cullen, étoit trop partisan des théories subtiles sur l'organisme animal et sur le développement des causes prochaines des maladies, pour s'astreindre à suivre dans ses leçons publiques une méthode sévère, et ne prendre d'autre base que l'histoire fidèle des maladies particulières observées et décrites avec soin dans les infirmeries depuis leur invasion jusqu'à la terminaison entière. Une grande habileté dans l'art insidieux de lier les faits à des hypothèses ingénieuses, et de donner ainsi à ces dernières toutes les apparences de la vérité, une certaine gravite imposante, et l'idée spécieuse de fonder une nouvelle doctrine sur les débris de celle de Boerhaave, lui firent préférer une autre méthode plus rapprochée de l'enseignement dogmatique; il ne se propose que de former un corps régulier de doctrine médicale élémentaire, sans cesse perfectionnée dans ses cours publics, et dont toutes les parties bien coordonnées et liées entre elles par certaines dispositions générales, pussent lui acquérir une sorte de stabilité à l'abri de toutes les vicissitudes: ce qui semblait lui garantir dans un âge avancé une réputation éminente."—*Diction. des Sciences Medicales, Article* CLINIQUE.

enjoyed much of his intercourse, " could it well be otherwise: Dr Cullen's professional knowledge was always great, and his manner of lecturing singularly clear and intelligible, lively, and entertaining; and to his patients, his conduct in general, as a physician, was so pleasing, his address so affable and engaging, and his manner so open, so kind, and so little regulated by pecuniary considerations, that it was impossible for those who had occasion to call for his medical assistance, ever to be satisfied on any future occasion without it. He became the friend and companion of every family he visited *."

Many expressions of congratulation on his success at Edinburgh are contained in the letters of Dr Cullen's correspondents. In August 1757, Dr Hunter writes to him:—" I give you joy of your success at Edinburgh. Every body that I see flatters and pleases me on that subject." And in a letter, dated January 1758, he says, " It makes me very happy to hear of you. Go on and prosper. Get the world of your side, as I find you do, and your rivals will humble themselves and confess your merit. I am glad you are at Edinburgh, because there is more intercourse with this place. I do assure you I have never found any thing in business so pleasing to me as to hear my patients telling me, with approbation, what Dr Cullen had done for them, and to hear my pupils speaking with the reverence and esteem of Dr Cullen that is so natural to young minds." Another letter from Dr Hunter, dated September 1759, contains similar expressions

* The Bee, or Literary Intelligencer, vol. i. p. 9.

of congratulation. "You and I have long got over ceremony with each other. We write short letters, thank God! I say so only thinking of my own. I hate writing a long letter to any body, just as much as I love reading a long one when it is from you. I hear of you often; from Mr Townshend and Lady Dalkeith last. He talks extravagantly, you know, upon all subjects; his theme now is Scotland. I dare say you Scotchmen must have flattered him abominably. I hear with immense pleasure that you carry every thing before you. When do you come to London as your last stage? If these were the days of Augustus, you would leave your bones in Rome. I wish extremely to spend the last years of life near you."

Dr Cullen's high reputation as a teacher in the University of Edinburgh was not confined to his clinical lectures. The extensive and philosophical views which he opened to his students in the science of chemistry, strongly excited their attention, and procured for him the attachment and esteem of several who afterwards became distinguished, not only in medicine, but in other departments of science and literature. One of those who, by his assiduity in study, early attracted the particular regard of Dr Cullen, was Dr George Fordyce, afterwards so well known as a teacher and practitioner of medicine in London, where he gave lectures successively on chemistry, on agriculture, and on the theory and practice of physic; thus appearing to emulate his preceptor in the variety of subjects which he taught, as well as in the originality and independence of his manner of treating them. The following letters, written about a year after Dr Fordyce

had left Edinburgh to prosecute his medical studies in London, afford an example of the lively interest which Dr Cullen took in the progress of the studies of his pupils, as well as of the habits of familiar intimacy in which he was accustomed to indulge them.

" DEAR SIR, LONDON, *Septr.* 21*st* 1759.

" I feel myself much to blame in not having wrote you long before this time, only that I waited to send the enclosed paper. I have been intending it every post these six months, as I expected to have finished it then, but various interruptions, and a number of experiments that occurred, in order to satisfy myself thoroughly that the theory I had laid down was true in practice, have prevented me till now. It is, you see, an attempt to shew in what cases double elective attractions take place; likewise to show what combinations will take place when three or more compounds are mixed. I propose to give it in to the Royal Society, but would not take any measures for that end till I first consulted you. I should be much obliged to you if you would be so good as read it over, and let me know what you think of it; and it would be doing me a great favour if you could do it in two or three posts, as I believe I shall set out for Holland in about a fortnight or little more. You may keep the copy if you choose, as I have another here which I can give Dr Pringle, who will be so good as communicate it to the Royal Society, if you approve of it; if not, I can let it lie by me, as nobody has yet seen it but my Uncle and Dr Balfour Russel. I proposed at the same time to have given in a Table of Elective Attractions, but as I heard you had published one, I have given over making experiments till I see it. The experiments upon that subject are extremely nice, difficult, and fallacious. The principal experiments I have made are upon the nitrous acid, and are as follows: Fixed Alkalies, Absorbent Earths—I am not sure if earth of alum

should or should not be accounted amongst them; zinc, tin, lead, iron, bismuth, copper, quicksilver, silver: I have left out the volatile alkali, as I have not yet determined its place. I have done nothing else in chemistry since I left Edinburgh, except some trifles for other people, such as enameling some stones for the Duke of Argyll, and giving directions for some furnaces. I should not even have meddled so far, contrary to your advice, but that something of that kind would please my Uncles, and furnish me with a pretence for being a candidate for the Royal Society, of which they wish I should be a member.

" In the winter I attended both Dr Hunter's courses, and I dissected three bodies; so that I believe, except you advise me otherwise, I have done with anatomy. After his course was over, I went out to Chelsea, and have attended the gardens there during the summer, which, with a little physiology, has been my principal employment this summer; and this puts me in mind of the death of Sir Charles Linnæus, of which, however, I am not quite certain; but Mr Millar heard it from a gentleman to whom the Swedish ambassador had told it. I have got about a thousand specimens of species of plants, which, I flatter myself, are amongst the best preserved of any, as I got a lady to take the trouble of pasting them on the paper, and drying them, who I think has done them very neatly. If I can prevail upon my Uncles, my last journey shall be to Edinburgh, where I could wish to spend a summer with you; in that case, I shall be able to shew you both them and what I may be able to pick up abroad. I do not at present know what I shall do with myself after this winter; I should be much obliged to you if you would advise me. I cannot well go to Germany for armies; and if it is true that Linnæus is dead, it is not worth while to go to Sweden.

" I have taken a good deal of pains to see collections of fossils, most of which here are of petrifactions; and this,

assure you, is the worst place in the world to buy them, as there are so many people of fortune deal in them and shells. I have seen a bit of copper-ore sell for two guineas, and a bit of common lead-ore for half a guinea. In short, people do not mind what they give for any thing they have a mind to.

" I have got a specimen of earth of alum, which was dug pure out of the ground, half of which I shall send you if I have an opportunity before I go; if not, I shall leave it with Dr Russell. I have likewise got a specimen of pure nitre, with the fixed vegetable alkali, from an efflorescence from a wall. I have not above two drachms left after making experiments with it, but hope to get some more. If I can make Edinburgh my last stage, I shall bring every thing with me, and you shall be extremely welcome to what you please.

" I hope to have several commissions from you for Holland, &c. It will give me the greatest pleasure to execute them, as I shall never be able to repay the obligations I am under to you. I ought to ask your pardon for troubling you with so long a letter; and more, for encroaching upon your time with the paper. I beg my compliments to Mrs Cullen and all the family; I hope they are all well. I am much obliged to Mr Robert for taking care of my Diploma, which I have received of Dr B. Russell. I am, Dear Sir, your much obliged humble servant,

" G. Fordyce."

To this letter of Dr Fordyce, Dr Cullen had begun a reply in which he appears to have been interrupted, but which he resumed as soon as his avocations permitted him.

" Dear Fordyce,

" Your letter last night was very welcome. I suspected that your laziness had banished you from me for ever. I

won't tell you with what regret I felt that; but the sight of your hand was extremely agreeable. I have been often vexed that I had not wrote to you; and that I might not be vexed in the same manner again, I have now sat down to answer you in course, though it may be less to the purpose than if I had taken a fortnight to consider of it. As I suspect your Essay to have been your chief purpose in writing now, I shall make it the chief purpose of my answer," &c.

* * * * * * * *

"DEAR FORDYCE, EDINB. *Oct.* 4. 1759.

"The death of my patient has set me at liberty, and I have again sat down to consider your piece. After studying it with all the attention possible, I must tell you that it is very difficult to be understood. Perhaps that is my fault, but I think not entirely; for even those parts that I do understand are by no means expressed with the perspicuity they are capable of; and I have consulted one or two of the best judges here, and they also complain extremely of your obscurity. I once intended to have been particular in my criticisms, but I now find that this would take more time than either you or I can spare at present. I must therefore be contented to give you my opinion in a general way.

"Now for your letter as it lies in order. I doubt very much of the propriety of giving your Essay to the Royal Society; but though you do not, it should not lie by you. Shew it to some persons of patience, judgment, and candour. Every body needs the corrections of his friends, and every body should consult them before he appears in public. I think it would have been very proper for you to have given in something to the Society. It would have been of use both abroad and at home; and if I was with you, I am sure I could render your Elective Attractions fit for them. My having published a Table should be no hinderance; for, in the first place, I have not really published. Instead of let-

ting my pupils transcribe from my board, I have printed what I used to put upon it, but I by no means intended it for the public. There are many things doubtful in it; there are some things absolutely improper; but such as they are, both were necessary as a text to me. Certainly Fourie (Dr Balfour Russell) had a copy of it; but as it seems he has not, I send it you enclosed. I shall expect your corrections.

" Unless your designs are very much towards Surgery, I think you have done enough in Anatomy. Your study of Botany may be continued, because it may be of use afterwards as a profession; but you must not bestow too much time on it, lest it interfere with your Chemistry and Physic, which must be your dependence; and I believe it is a surer scheme to take one thing than to grasp at more. I have so much aversion to believe the death of Linnæus, that I will doubt of it till you can be more positive. I shall be glad to see your specimens, and more so to see yourself, when you have been an exotic. What you are to do after this winter it may be time enough to determine when this winter is done. Perhaps a peace will lay open Germany, into which you must plunge headlong. If there should be no peace, though Linnæus were gone, I still think Sweden of as much use to a chemist and fossilist as ever. The Museum Tessineanum, &c. &c. still subsist; and from the preface of Wallerius, you will find there are persons in Sweden that may be of more use to you than Linnæus. You have been well employed in seeing collections of fossils, and, besides petrifactions, London certainly contains a great deal. I wish you had given me some observations upon what you have seen. You know I was intent on a method of fossils, and thought it best to make a beginning, though very imperfect. You have a copy, and I have long expected your corrections. Are the talcs and gypsums different in their composition? Are they to be distinguished otherwise than by calcination, and how? How are the crystalline bodies to

be distinguished from one another? Is there any difference in the crystalline bases? Have you formed any system of the ores? Are there any other fundamental ores of silver besides the vitrea and cornea? Is tin as much native to its crystals as lead is to the galena? Has Fourie found any fundamental ore of copper but the pyrites? What is the fundamentally native state of iron? I expected answers to these questions, and many others, both from you and Fourie long ago; but let me beg of both of you to write me, whether you have any thing to say or no. I shall always consider the loss of your friendship as a much greater misfortune than the want of your informations. Pray, could I myself learn a great deal from the British Museum? I have a strong inclination to be in it. I shall be obliged to you for your specimens of alum and nitre, or for any thing of the same kind you can spare. I see that I cannot afford to buy at London, but abroad perhaps I might; and in either place I would attempt nothing but what was necessary to my system. Things pretty in their form, or even merely rare and curious, I would absolutely reject. If I can possibly command my time and attention, I will send you a note of some things to be got, either in London or Holland.

"Wherever you go, your business is to accumulate knowledge; and let me advise you to study, too, how to communicate it to others. Study the German language, if you please, for the first, and more assiduously the English, for the other purpose. Study a more clear, full, and even diffusive expression. For your own sake, you should force yourself to write oftener to your friends; and let me seriously repeat my old advice of improving your writing. I thank you for the pains you have taken in this respect in your last letter; and I am the more obliged, as I perceive it is not yet quite easy to you. You will perceive marks of hurry in this letter, and you will not be mistaken in your judgment; but I was determined to write; and if you will give

me a little encouragement, you may always depend upon such letters as I can afford you. Believe me, most affectionately yours,

" WILLIAM CULLEN."

Dr Balfour Russell, who is alluded to in the preceding letters, was a young man of very promising talents, and the brother of Drs Patrick and Alexander Russell ; the former well known as the author of the most complete treatise which has ever been published on the Plague; and the latter, as the author of the Natural History of Aleppo, of Indian Serpents, &c. Dr Balfour Russell, after finishing his medical studies at Edinburgh and London, went out as a physician to Algiers, where he died, after a short residence, in 1761. That he was a pupil for whom Dr Cullen entertained a particular regard, is evident from the following letter, written to him some time before his setting out for Africa.

" DEAR FOURIE, EDINB. *Octr.* 4. 1759.

" I shall always be glad to have your letters, when they inform me of your own success and happiness, though they tell me of nothing else: your letter therefore was very agreeable, and I hope you shall never want enough to say to me. I should have answered you sooner, but must retort your own excuse. I have always so much to do, that I am ready to put off writing. However, I am determined for the future to be pretty punctual with some people, and you shall be of the number. I have this day finished a letter to Fordyce that might, though I shall not insist upon it, excuse me for two years to come. He has puzzled me these eight days with his compounded—I had almost said confounded—attractions; and you should have saved me, by telling him

honestly how difficult his piece was. If you did not find it so, I admire your quickness. I am sorry I have been obliged to check him a little, for I expect much good of him at last; and I excuse your complacency in saying it was all very clear, but you must shew him what I have said to you.

" I find nothing in your letter that requires a particular answer, except with regard to your studies in Africa. Of these you should now be a better judge for yourself than I can be, but I shall not for this season decline advising. I think you should always be engaged in two kinds of studies; one that you should make a regular task of; and the other as a pleasure, when opportunity offers and inclination prompts you. The first kind should be for perfecting yourself in some system. Few people but professors do so; but every one that would appear in the learned world ought to do it. The system may be what you please; but I would prefer the Practice of Physic, as connected with your daily employment; I do not mean Boerhaave's system, or Hoffmann's system, or any other, but a system which you are to make for yourself out of all of them. You will not find it possible to separate practice from theory altogether; and therefore, if you have a mind to begin with the theory, I have no objection. I think a systematic study of the Pathology and Methodus Medendi will be necessary previously to the practice, and you may always have in view a system of the whole of Physic. Of this study, you should endeavour, as I have said, to make a duty, and, as far as you conveniently can, a regular task; but, at the same time, for amusement, you may pursue the natural history of the country, and the prosecution of some more particular inquiry, experimental or literary, as conveniency, opportunity, or inclination, shall lead you.

" I still insist upon some London intelligence,—some account of the hospital practice,—some account of medicines

we do not deal in,—and some doses that we do not venture on. Tell me any thing of the present state of physic and quackery. What does your brother* think of James's Powder? After all I have heard or seen, I want the opinion of a man of candour. Have you met with nothing in chemistry or mineralogy to tell me of? I refer you to Fordyce's letter for a specimen of questions that you should have answered, or endeavoured to answer, before now. In the same hands, you will find, too, an account of your laziness. You may think that I make great demands; but it is a secret with some politicians, to demand a great deal, that they may be certain of getting a little. I would fain play off Fordyce and you against each other. Fordyce set out with as handsome a margin as you gave me, but he has very discreetly filled it afterwards. I expect no margins from you hereafter, especially after so long a letter as this is. I mean to show you that one may write a long letter to a friend without saying any thing. I may perhaps show you that I can send a letter to another quarter of the globe, with no more in it than what I would say in yawning at my own fireside. Tell your brothers I love them, because they are good brothers, and wish I knew them better. I hope I shall love you always. Farewell.

"W. C."

"*P. S.*—I find nothing more to say at present. If you had given me some account of your own deliberations and doubts, I might have said more. Let me beg of you to have no scruple with regard to what you should write. I do not expect new discoveries in every line, and I would rather bear with twenty old and common things, than miss one new fact or observation. I am sure you have met with many things in St Thomas's that I would be glad to have

* Dr Alexander Russell, Physician to St Thomas's Hospital.

your remarks upon. I hope you do not suspect that I would be either severe myself, or expose your remarks to others, if I thought it could be to your prejudice. Pray mind this, that I know nothing about Algiers or the rest of Barbary at all, and therefore take full scope in geographical, political, religious, commercial, &c. &c. communications. Let me hear from you before you go. I am," &c.

The readiness with which Dr Cullen entered into all the circumstances of the situation and views of his young friends, and the pains which he took to correct even the minutest defects he had remarked in their characters or manners, were discernible in all his intercourse and correspondence with them. Besides the examples already adduced of his habits and powers of giving advice, I find another agreeable instance of these in the following letter addressed to one of his sons, a young lad then leaving home for the first time:—

" DEAR JAMIE, EDINBURGH, 11*th November* 1765.

" I am afraid you do not consider the anxiety we have about you, or you would have contrived to give us a letter from Glasgow; but I hope you will mind better for the future to take every opportunity of giving us accounts of you. We received your letter to-day, and Mr Leitch and your brother Patrick came home before dinner. Your letter is a little concise, and I beg you will be always as full as you can, for we wish to know every particular that happens to you. Keep memorandums of what you may write; so that, when you sit down, your materials may be ready. It is common in sea journals to set down occurrences, though not relating to the reckoning. I wish you would mark as many as you can, if not in your journal, at least in another book. One cannot begin too early to make remarks on every thing. It is a very improving exercise, and, through life, attention

and observation are the foundation of success, and distinguish the able and wise from the weak and foolish.

" I hope you have got every thing necessary, and a proper chest to put them in. I beg you will study to keep it always in good order, and all your clothes in good condition; and particularly, I expect to observe that while you have perused your books, you have also kept them clean. As I hear that Mr Kippen is not well, if he is not able to go with you, it will pretty certainly take you into the store at Antigua, which I shall think very lucky; but I tremble for your handwriting, and I beg of you, in the most earnest manner, to take pains on that article. If you have any regard to my satisfaction you will; and, for your own sake, consider that nothing so much gives the appearance of mean and low bred as bad writing. Take every opportunity, therefore, to practise with attention, and, if possible, never without it. As you are not to go to Ireland, you may find the two dissertations an agreeable present to some doctors in the West Indies. I find Mr Anderson refuses to take your money, but press him to it again, and at least to keep it, to lay out for you. If he declines it in every shape, I desire you may leave it with Mr Hamilton, as I have much use for it. In the mean time, let me know what he lays out for you. Dear Jamie, I hope to write you again, and, perhaps, to send you letters for Virginia. I shall be glad to know what letters you get from your uncle. I hope few advices are now necessary. Study your trade eagerly, decline no labour, recommend yourself by briskness and diligence, bear hardships with patience and resolution, be obliging to every body whether above or below you, and hold up your head both in a literal and figurative sense. I trust that honour, truth, and discretion, shall always guide you, and give the utmost comfort to, my dear Jamie, your affectionate father,

" William Cullen."

It is impossible, I believe, to give a juster account of the general manner in which Dr Cullen conducted himself towards his pupils, than has been given by Dr Aiken, who had himself been one of them. "He was cordially attentive to all their interests; admitted them freely to his house; conversed with them on the most familiar terms; solved their doubts and difficulties; gave them the use of his library; and, in every respect, treated them with the affection of a friend and the regard of a parent. It is impossible for those who personally knew him in this relation, ever to forget the ardour of attachment which he inspired *." This representation of Dr Cullen's conduct in private towards his pupils, is well exemplified in the account which Dr Anderson has given of his first interview with him.

"Being then young," says he, "and of exceeding small stature for my age, on presenting myself, the Doctor very naturally took me for a child; and when he understood that agriculture was the profession intended, he conceived that it must have been some childish whim that had hastily laid hold of the imagination, and thought it his duty to discourage it. He, therefore, began to dissuade me from thinking of pursuing that idea any farther; but finding I had reflected on the subject, and had finally adopted a line of conduct from which I would not depart, for reasons then assigned, he at last was brought to acknowledge, that if I had steadiness and assiduity to apply properly to the study of chemistry, it might in the end prove conducive in promoting the knowledge of the principles of agriculture; and said, if I was determined to exert myself, he should do all in his power to forward my views. As his public lectures had then been for some time begun, he ordered me to at-

* Aiken's General Biography, vol. iii. p. 255.

tend a private class, with some others in the same predicament, to be instructed in those parts of his course already past, till we should overtake those in his public class, which was a common practice with him at that time.

" In these private lectures, as well as in his public class, Dr Cullen was always at pains to examine his students, from time to time, on those parts of his course that had already been delivered; and wherever he found any one at a loss, he explained it anew, in a clear, familiar manner, suited to the capacity of the student. On these, and on other occasions, he frequently desired, that whenever any one was at a loss as to any particular, they would apply to him freely for a solution of their doubts and difficulties. In this proposal he was serious; and it was understood by me in the most strict, literal sense of the word. And being very anxious to lose nothing, I had no hesitation in complying with his request with as much frankness on my part, as it was made with sincerity on his part. It thus happened, that, for a long time at the beginning, there was scarce a day that I did not run after him on the dismission of his class, to ask an explanation of one particular or another that I did not understand; nor was I to be satisfied in any case till it was made quite plain. Thus was he incessantly teased with the little prattle of a child, but without ever discovering the smallest degree of peevishness or impatience. Often have I since that time wondered at the mild condescension of that great man, who, pressed as he was for time, in the prosecution of such extensive business, was not only not offended at the frequent interruptions, but seemingly was rather well pleased with the turn of mind that occasioned them; kindly entering into discussions that were suited to my years, and listening with patience to the arguments that were dictated by youth and inexperience, and patiently removing those difficulties that perplexed me. *"

* The Bee, vol. i. p. 51.

The kind interest which Dr Cullen took in the pursuits and studies of young persons was not confined to his pupils alone. Various instances of this have been mentioned to me by those who had experienced it. The late Mr Dugald Stewart, in particular, informed me that, during a slight indisposition, which confined him for some time to his room, when a boy of fourteen or fifteen years of age, he was attended by Dr Cullen. In recommending to his patient a little relaxation from his studies, and suggesting some light reading, the Doctor inquired whether he had ever read the history of Don Quixote. On being answered in the negative, he turned quickly round to Mr Stewart's father, and desired that the book should be immediately procured. In his subsequent visits to his patient, Dr Cullen never failed to examine him on the progress he had made in reading the humorous story of the great pattern of Chivalry, and to talk over with him every successive incident, scene, and character in that history. In mentioning these particulars, Mr Stewart remarked that he never could look back on that intercourse without feeling surprise at the minute accuracy with which Dr Cullen remembered every passage in the life of Don Quixote, and the lively manner in which he sympathized with him in the pleasure he derived from the first perusal of that entertaining romance. In what degree of estimation Mr Stewart continued to hold that work may be seen by the inimitable character which he has given of it in his Dissertation on the progress of Metaphysical, Ethical, and Political Philosophy *.

* Encyclop. Brit. 7th edit. First Preliminary Dissertation, p. 96.

After coming to Edinburgh, Dr Cullen appears to have taken much interest in the proceedings of the Philosophical Society. In writing to a correspondent, in 1760, he says,

"There are just now many favourable appearances, of a revival of the spirit of that society. The meetings are regular and well attended. At each of them, of late, we have had sufficient employment, and are at present secure of employment at every meeting for a twelvemonth to come. At the meeting in June next, it is proposed that as many members as possible shall engage to read a paper or papers to the society, at one or other of their meetings in the course of the year 1761, but each member shall fix a particular day, so that every meeting of the year may be regularly provided for; and every member has at least six months to provide for it. There does not at present appear to be any doubt about the execution of this plan, and it is proposed that we shall continue hereafter, I hope, for very many years, at the meeting in June, to provide regular employment for the following year. This, however, is by no means to hinder any member from making as frequent incidental communications as possible; and particularly, every member is desired to bring to every meeting as much literary news as he can, either from new books, letters, or even conversation."

Among Dr Cullen's manuscript papers, I find an Analysis of the *Essai de Cosmologie, par M. de Maupertuis*, which appears to have been read by him to the Philosophical Society, in conformity with the plan to which he alludes in the preceding extract. The introduction to this Analysis would lead us to suppose, that if Dr Cullen was not the first to suggest to the Society this mode of diffusing information, he was at least the first to put it in execution.

"In my studies," he says "I find it of great use to be acquainted with the books that are now daily published in different parts of Europe. I do not doubt but other gentlemen of this company know the benefit, and are equally desirous of being acquainted, with new books. As all of these cannot occur to every one, nor can the reading of all of them be undertaken by any one person, I have always been of opinion, that a principal advantage to be obtained by a frequent communication between a number of persons devoted to study, is that of being more certainly and sooner informed of new books, and being better directed in the choice of them. I wish that this company may be particularly intent on obtaining such advantage, and that each member of it may zealously endeavour to be useful in this way. As the company are not otherwise engaged for this evening, I propose to lay before them an account of a new book. My discretion may perhaps be blamed, but I hope my zeal will not. The book I am to give you an account of, is a small duodecimo, printed at Leyden 1751, and is entitled "Essai de Cosmologie, par M. de Maupertuis." The purpose of the book is to give a new and more satisfying proof of the Existence of God, whence his motto is, "Mens agitat molem." The work is partly metaphysical, partly mathematical; in either respect it falls improperly under my cognizance, and works of this kind I shall hereafter put into other hands; but I had a mind to set on foot such labours; and not the subject, but the book's being the last occurring to me, determined my choice."

Of the utility of the mode of diffusing information which Dr Cullen recommended, there can be no doubt; but the multiplication of literary and scientific magazines, journals, reviews and newspapers, which of late years has taken place, has in a great measure superseded its necessity; and their circulation now communicates to

all classes of society, advantages which, even so late as the time of Dr Cullen, could be enjoyed comparatively by a few only.

Besides the Philosophical, Dr Cullen became also a member of several other societies in Edinburgh. We find his name in the list given by Dr Carlisle of the members composing the Select Society, in the year 1759. He was afterwards a member of the "Poker Club," instituted, as is mentioned by Mr Mackenzie, in 1762, "at a time when Scotland was refused a militia, and thought herself affronted by the refusal."— "The name of this club was chosen from a quaint sort of allusion to the principles it was originally meant to excite*." find also a diploma dated 1756, admitting Dr Cullen a member of the old Revolution Club.

Dr Cullen had the happiness to become personally acquainted with Dr Franklin during a visit which that distinguished philosopher and statesman made to Edinburgh in the autumn of 1759. The pleasure of their intercourse must have been greatly heightened by the interest which they took in each other's philosophical pursuits. Dr Franklin's curiosity, in particular, had been strongly excited, previously to his leaving America, by an account of Dr Cullen's experiments on the cold produced by evaporation, which had been communicated to him by Dr Simson, Professor of Mathematics at Glasgow†. We learn from the following letter, addressed by Dr Franklin to Dr Cullen, that the doctrines of heat or fire had been a subject of conversation between them during his stay in Edinburgh.

* Life of Mr John Home, p. 26.

† See Dr Franklin's Works, Lond. 1806. vol. ii. p. 75.

"DEAR SIR, LONDON, 21*st*, *October* 1761.

"I hear, that since I had the pleasure of seeing and conversing with you on the subject, you have wrote some of your sentiments of Fire, and communicated them to the Philosophical Society. If so, as it may be some time before their publication, I should think myself extremely obliged to you if I could be favoured with a copy, as there is no subject I am more impatient to be acquainted with. It should go no further than my own closet without your permission.

"I thank you for the civilities you were so good as to shew my friend Mr Shippen, whom I took the liberty of recommending to your notice the last year. Give me leave to recommend one friend more to your advice and countenance. The bearer, Mr Morgan, who purposes to reside some time in Edinburgh for the completion of his studies in Physic, is a young gentleman of Philadelphia, whom I have long known and greatly esteem; and as I interest myself in what relates to him, I cannot but wish him the advantage of your conversation and instructions. I wish it also for the sake of my country, where he is to reside, and where I am persuaded he will be not a little useful. I am, with the greatest esteem and respect, Dear Sir, your most obedient and most humble servant, B. FRANKLIN."

Dr Morgan, who is mentioned in the foregoing letter, appears to have fully realized the expectations of his friend Dr Franklin. He distinguished himself while in Edinburgh by a diligent application to his studies;—published, on receiving the degree of Doctor of Medicine, an excellent inaugural dissertation on the subject of Suppuration;—and visited the principal hospitals of France and Italy before returning to his native country. After his return to America, he took

an active share in the institution of Lectures on different Branches of Medicine in the College of Philadelphia, and in the establishment of a Dispensary and of a Medical Society in that city. The progress of these institutions is minutely described in his letters to Dr Cullen, towards whom he always appears to have felt and expressed a very grateful attachment. In one of his letters, dated London, November 10. 1764, Dr Morgan gives the following very agreeable account of the great Italian pathologist:—" Morgagni, at Rome, treated me like a brother or son, gave me the only fine copy he had left of the second edition of his work *De Causis et Sedibus Morborum*, and paid me a handsome compliment, which he wrote in it with his own hand. He is in his 83d year, has never used spectacles, is very hearty, and more lively than a young man. I never talked with him but I thought from his conversation, and his openness of behaviour and winning disposition, that I was with my good Dr Cullen."

In the winter session of 1760–61, a new opportunity was afforded to Dr Cullen of evincing to the public the great extent of his medical knowledge, and his powers as a teacher. Dr Alston, the Professor of Materia Medica, dying soon after he had commenced his lectures for that session, and there being no person immediately appointed to supply his place, the students of medicine presented a petition to Dr Cullen, requesting him to undertake this duty. He readily complied with this request, and entered upon a course of lectures on the Materia Medica in the beginning of January 1761. It was undoubtedly the circumstance of his having previously given lectures on that branch of

medical science, as well as on the Theory and Practice of Physic, at Glasgow, which enabled him to come forward so readily and so advantageously upon this occasion.

In commencing his course of lectures on the Materia Medica, Dr Cullen put into the hands of his students a catalogue, containing an enumeration of the different articles of Food and of Medicine which he was to treat of in his lectures. These articles were arranged, partly according to the sensible effects which they produce upon the body, and partly according to the divisions which they occupy in systems of Natural History. By the simple, comprehensive and scientific arrangement that was followed,—by the extensive and original views that were taken of the animal economy, and of the action of remedies upon it,—by the judicious selection that was made of topics for discussion, —by the numerous minute practical remarks that were intermixed with the general descriptions,—by the rejection of useless and inert remedies,—and by the rounded and systematic form that was given to these lectures,—Dr Cullen excited for the subject which he taught, a degree of interest that has seldom, if ever, been produced by any other course of lectures on Materia Medica. This was shewn, not only by the warm applause which his lectures received from those who heard them, but also by the eager curiosity with which manuscript notes taken by his students were long sought for, multiplied and circulated amongst the medical profession, in all parts of Europe. It must have been in some measure to gratify this curiosity that, ten years after the period at which they were de-

livered, an incorrect edition of these lectures was, without Dr Cullen's knowledge, published in London from the manuscript notes of some of his pupils; and not only translated into several of the languages of the Continent, but republished in Dublin and in Edinburgh.

The unwarrantable liberty which had been taken by the publication of his lectures, induced Dr Cullen to apply to the Court of Chancery for an injunction to stop their sale, which was immediately granted. His agent in London, in informing him of this, writes (13th December 1771).

"Mr Solicitor-General (Mr Wedderburn, afterwards created Earl of Rosslyn), after giving you the character which he thinks you justly deserve, moved the Lord Chancellor for an injunction against Lowndes.* His Lordship, after mentioning the great consequence of this affair to the Professors of every science, and the similarity of the case to that of Sir William Blackstone's Lectures, published without his knowledge, ordered the injunction immediately. Mr Solicitor-General has entered warmly into this business, and thinks it a matter of great consequence to you, as any of your publications must sell well; and he says, that as you are almost certain of success, you had better incur the expense of a Chancery-suit, than suffer yourself to be deprived of your property and the fruits of your study, genius, and industry. He also says, he would be very well pleased to have a reading of the book during the Christmas holidays."

As, on inquiry, it appeared that the physician who

* The Bookseller.

had furnished the bookseller with the manuscript of the lectures was sensible of the error he had committed, and had had no pecuniary object in view by their publication, and as a considerable number of printed copies had already got into circulation, Dr Cullen at length agreed to allow of the sale of the remaining copies, on condition that he should receive a share of the profits, and that the grosser errors in the work should be corrected, by the addition of a supplement. The unsanctioned publication of his opinions is an evil to which every public teacher is necessarily exposed, and this evil is rendered doubly severe, when the editor, in concealing the source from which they are derived, appropriates the merit of these opinions, whatever it may be, to himself. But though on this occasion a great injury was done to Dr Cullen's feelings, there was apparently none intended, and there certainly happened none by it to his professional reputation *.

The high character which Dr Cullen acquired as a teacher of Practical Medicine by his Clinical labours in the Infirmary, as well as by the course of lectures which he had given on Materia Medica, soon procured for him, not only a considerable share of private practice in Edinburgh, but also numerous consultations from medical practitioners in all parts of Scotland; so that, in the advanced years and declining health of Dr Rutherford, the Professor of the Practice of Physic, he was very generally regarded as the person in this country best qualified to succeed to the Chair of Practical Medicine when it should become vacant. Unfortu-

* See Appendix, Note Q.

nately, however, for the accomplishment of this object, Dr Rutherford had imbibed prejudices, which, though he was desirous to retire from the University, rendered him unwilling to resign in favour of Dr Cullen.

In these circumstances, Dr John Gregory, who had held for several years the professorship of Physic in the King's College, Aberdeen, but who had not had occasion in that capacity to deliver any lectures on Medicine, was invited by his friends to come to Edinburgh and to offer himself as a candidate for Dr Rutherford's Chair. On its being understood that Dr Rutherford was disposed to resign in his favour, the friends of Dr Cullen endeavoured to prevent this arrangement by an appeal to the public, in which they stated the various successful efforts which Dr Cullen had made to promote the science of Medicine, and the great services which he had rendered to the Medical School of Edinburgh in particular, as claims which in their opinion entitled him, in the choice of the electors, to a preference over any less distinguished candidate. This appeal did not long remain without various replies, in which abuse and ribaldry supplied the place of argument. The following extract, taken from one of these publications, entitled a "Letter from a Citizen of Edinburgh to Dr Puff," while it exhibits a specimen of the spirit and manner in which the opposition to Dr Cullen was conceived and conducted, contains, at the same time, what must now be regarded as a just and well merited, though unintentional, tribute to the great powers which he actually evinced as a lecturer on Medicine. The author of this letter seems to have been little aware, that what he had written in derision

might, at a future period, serve to elevate the character of him whom it was meant to injure.

" It is now time," says he, " we should come to the point, and consider your particular fitness for the Medical Chair in question, which gives rise to our present correspondence. Upon this chapter, it is said, ' That you taught this branch of the profession at *Glasgow* many years; that you have a happy talent in communicating your thoughts; and that a man in the practice of teaching, is preferable to one who has not been used to it.' This seems to be the import of what is urged to support your preference. With respect to this plea, it must be acknowledged a very uncommon instance of genius, to be ready to fill every vacancy that may happen in the Medical department, as any one branch of it is thought sufficient for most men; and allowing that your students at *Glasgow* formerly applauded your lectures on this branch of Physic, does it follow that therefore you are fitter than any other person? By-the-by, I cannot help observing here, how much you are to blame in not informing us, in what language these lectures were given. Here they are delivered in Latin, to the study of which, I am told, you have applied since your settling in this town; so far undoubtedly there is some merit in making up the deficiencies of youth. But as it seldom happens that people in years are endowed with the gift of tongues, which they had not imbibed when young, it is to be regretted, that, in enumerating all your other accomplishments, this, which is said to be the key to science, is not once mentioned. Surely it was highly necessary to inform us with respect to this point, that we might the better judge what chance you had to draw foreigners to this class.

" Well, but it is said that the students at *Glasgow* applauded these lectures. And is this any thing remarkable, that a professor, possessed of popular talents, and who lays himself out to engage the good will of his scholars, should

obtain that end? But in a competition, whether your lectures, or those of another, are preferable, the only way we can judge is, by your publishing them to the world, by which every man of knowledge will have an opportunity of giving his opinion, and of judging what part thereof is your own, and what may be borrowed from others, so that justice may be done both to yourself and creditors, if you have any. If this advice is followed, I may likewise venture to assure you, that you will in all probability procure as honest a friend as *Blas of Santilane* was to the Archbishop of *Grenada;* though it is not impossible but the friend might incur the same censure from you as that faithful servant did from his master.

"Further, if you are so exceedingly fit for this Chair, as is averred, pray, what reason can be assigned why Dr R———d, who, every body must allow, is a man of discernment to discover the fitness of a successor, and of probity not to be influenced by any selfish motives, should be so blind, not only to reject your repeated solicitations, but likewise to prefer another?

"However, in order to obtain a change, Dr B——k is next brought on the stage, as one of the *dramatis personæ*, and recommended as a person proper to succeed you. The Doctor's skill in chemistry nobody will controvert. But is this a reason why the medical chair should suffer? Dr R———d certainly thinks the interest of his class depends on the choice he has made. Ought not his recommendation to have as much weight in the scale as yours?"

In reference to these publications, Dr Hunter writes to Dr Cullen, in February 1765.

"I have heard of all that has been passing at Edinburgh, from a variety of different hands, and always to your honour." —"Mr Oswald thinks as I do, that no power of politics

could or should prevent your wish when Rutherford resigns." —" Wherever I can, I bring in the subject, and talk strongly upon it. Among other things, I say that the love of my country, and of increase of knowledge in the profession, for the sake of the sick and the lame, makes me of all things long to see so happy a change, at the same time that my private views would incline me powerfully to have you and Black mortified upon the occasion. I have read the Citizen's letter with pleasure; and since that, the letter to Dr Puff, with great mortification to myself. Good God! that my friend should by any mortal be put into any sort of balance or comparison with the author of such a puerile performance. I hope and presume it never gave you pain of any kind. Were I in your place, I should be very proud of it."

Whether the publication to which Dr Hunter alludes really gave any uneasiness to Dr Cullen cannot now be known; but it is certain that this, as well as every other attempt to asperse his character or to ridicule his doctrines, was met by him with that silent neglect which is in general the most effectual repeller of injurious and malevolent personal attacks. As a proof how little the strictures made on his opinions influenced Dr Cullen's conduct towards those from whom they were in general believed to emanate, we have the following testimony of Dr Anderson: " During three years that the writer of this article attended his public lectures, and for upwards of thirty years that he has been indulged with his private acquaintance, he can with truth aver, that neither in public nor in private did he ever hear a single expression drop from Dr Cullen that tended, directly or indirectly, to derogate from the professional character of any of his colleagues, or that

could induce a student to think lightly of their talents as professors, or of their abilities as physicians *."

The following letter from Dr Hunter, so characteristic of his vivacity of feeling and desire of professional distinction, was written about this time, when it would appear that Dr Cullen had lost the expectation of succeeding to the Chair of the Practice of Physic.

" Dear Sir, *April* 1. 1765.

" Your friend Dr Pitcairn has had an opportunity of doing his best offices with Mr Oswald, but to no purpose. He at their first meeting had the pleasure to hear Mr O. begin upon the subject, and say *almost* every thing he could have wished : that is, Mr O. talked in the strongest terms, and was well acquainted with the subject, of the propriety of doing what you desire, and of the cruelty and absurdity of endeavouring to do otherwise, &c. &c. ; but still persisted in the resolution he told me he had taken, viz. not to intermeddle, except Lord Bute or Mr Mackenzie should give him an opening or opportunity. In that case, he would not only speak his mind freely, but give advice strenuously : but, at the same time, he said he hardly expected such an occasion, and rather imagined it would be avoided ; because he believed his way of thinking was too well known, and that it was their own way of thinking, likewise, though politics obliged them to seem to be of another opinion ; which delicate situation would make them shy upon the subject.

* * * * * *

" Nothing is to be done here, at least at present. Go on with the applause of those whose good opinion you can be proud of. You have it universally. If you mind the envious and the wrong-headed herd, you have not so much manliness as I thought you possessed of. The more oppo-

* The Bee, or Literary Intelligencer, vol. 1. p. 9.

sition you meet with, the more you should exert yourself; and you will always find contempt for your adversaries a most excellent cordial. You know that you triumph over the poor things in the minds of all men who can judge.

"With this I have sent you another letter which will startle you. I have a great inclination to do something considerable at Glasgow some time or other. If you think proper (and you are the best judge), you may talk of the scheme as a great secret with two or three different people, and then you know all the town will know it. God bless you. I am, Dear Sir, most faithfully yours,

"WILLIAM HUNTER."

"DEAR SIR, *2d April* 1765.

"I have felt disappointment myself; and therefore can easily imagine what you must feel in meeting with such opposition to a scheme which I know to be of so much more consequence to the public than it can be to yourself. Mine was to have founded a perpetual school for Anatomy here. I will one day or other send you a copy of my Memorial to the Ministry. In short, without asking any thing for myself, but, upon the contrary, declaring I would have nothing, I only asked a piece of ground to build upon, and offered to lay out £7000 immediately on a building, with every convenience for anatomy, and obliging myself to give it to the public the moment it was built: and said, further, that I *meant*, but would not now oblige myself, to give likewise my preparations and books. My library has already cost me I presume between £3000 and £4000; and my anatomical museum is of more worth to the public, because they are things that cannot be bought. I am resolved never to be much richer than I now am. I am independent, and wish to do something that shall be mentioned when the few years which I have to live are gone. Now, you shall see to what purpose all this—I should like to be joined with you in the

end of life as in the beginning. You have been ill-used at Edinburgh, as I have been at London. Could you make a sacrifice of the few more guineas you would receive by practice at Edinburgh, and join with me to raise a School of Physic upon a noble plan at Glasgow? I would propose to give all my Museum and Library, and build a Theatre at my own expense: and I should ask nothing for teaching but the credit of doing it with reputation. You and Black and I, with those we could chuse, I think could not fail of making our neighbours stare. We should at once draw all the English, and, I presume, most of the Scotch, students. Among other reasons, I should not dislike teaching anatomy near my two friends, the Monroes, to whom I owe so much.

"I will say no more at present upon this subject. I mean only to throw it out to you for your consideration, and shall be glad to have your thoughts in general upon the plan. I would not have you say any thing of it to any person, except in confidence to any sensible friend whose opinion you may choose to take. I shall explain myself further whenever you please. Let me now only say, before I put my name to this, neither of us have been well used; and it is in our power to do ourselves immortal honour. I am, as you well know, dear Sir, always your most obliged

"WILLIAM HUNTER."

"*20th April* 1765.

"*P. S.*—This letter was really begun almost three weeks ago.'

The advantages likely to result from engaging Dr Cullen in teaching the practice of physic, appear to have been anticipated, not only by the public at large, but also by the Patrons of the University, who, though they omitted the opportunity presented by the resignation of Dr Rutherford, in February 1766, of elect-

ing Dr Cullen to that Chair, seem to have imagined that they might still have this in their power at no very distant period. That such at least was the impression of some of Dr Cullen's friends in the Town-Council, appears by the following letter from Lord Kames, written about a month after Dr Gregory had been admitted Professor of the Practice of Physic, and a few days after the Chair of the Theory of Physic had become vacant by the death of Dr Whytt, on the 15th of April 1766.

"My Dear Sir, Edinburgh, 18*th April* 1766.

"It is vexatious to find the Town-Council acting so loosely as to suffer themselves to be disappointed by Dr Gregory. I was assured by Millar, that the purpose in choosing Gregory was to remove the obstruction made to you by Rutherford; and that the Chair should be yours so soon as Gregory should be otherwise provided for; and I had not the opportunity of conversing with any other upon the subject. Most heartily I wish you success; not so much for your sake as for that of the Town of Edinburgh. Yours,

"H. Home."

"*P. S.*—If you be at home on the 29th of this month, I shall have the pleasure to sup with you."

Of the existence or nature of the arrangements alluded to by Lord Kames it is impossible for me to speak, without more precise and authentic information than any I have been able to acquire. The result of the opposition so unjustifiably made to the pretensions of Dr Cullen, was his exclusion for some years from the Chair of the Practice of Physic. It was fortunate, however, for the interests of the University, that in

the interval its duties were allotted to a physician who, to an excellent understanding and cultivated taste, united much practical skill in his profession, and who had shortly before distinguished himself by a popular work on the State and Faculties of Man, compared with those of the Animal World.

Dr Cullen was so much disgusted with the conduct of the Patrons of the University, and with the treatment he had received in relation to the Chair of the Practice of Physic, that, on the death of Dr Whytt, he seems to have wished to remain Professor of Chemistry, and to have at last consented to accept the Chair of the Theory of Physic, rather in compliance with the wishes of his friends, and in order to give the electors an opportunity of bringing Dr Black into Edinburgh, than from any desire of his own to occupy that situation. The opinions which were entertained of Dr Cullen's merits by his medical colleagues, and by the students of medicine attending the University of Edinburgh at that period, and the wishes which these two bodies respectively and publicly expressed in relation to his appointment to the Chairs of the Theory and Practice of Physic, are recorded in the two following documents.

"To Dr Cullen.

"The Professors of the several branches of Medicine in this University, after maturely considering the present critical situation of the School of Physic here, and the great importance to the Town and University, that the profession of the Theory of Physic, vacant by the death of the late very ingenious Dr Whytt, be taught by a physician of extensive genius, learning, and established reputation, and who has given

unquestionable proofs of his talents as a teacher; and fully convinced that their colleague Dr Cullen possesses all these qualifications in the most eminent degree, and that he is also thoroughly master of this subject, think it a duty they owe the Town, the University, and the Students of Physic, and themselves, to request of him, in this most public and earnest manner, that he will resign the professorship of Chemistry, and offer himself to the Honourable Patrons of this University, as a candidate for the profession of the Theory of Physic.

"Besides the many and great advantages which they think will directly arise from the nomination of Dr Cullen to that Chair, they are hopeful that, freed from the burden of an experimental college, and which a person otherwise less engaged in business might prosecute with still greater benefit to the art and to the students, he would have more leisure and attention to bestow on the Clinical Lectures given in the Royal Infirmary. And as this is an object of the utmost consequence in the study of Physic, and a branch in which Dr Cullen has already given very high satisfaction, they are of opinion, that it would be so much for the improvement of the students, that he were engaged still farther in explaining to them this immediately useful and practical part, and in pointing out to them the application of the theory and principles of medicine in the treatment of the sick, that they think it their duty to request of him also, that, on the event of his being elected Professor of the Theory of Physic, he will bestow as much time on the Clinical Lectures as his other avocations will admit.

"Alex. Monro, *Med. & Anat. P.*
"Alex. Monro jun. *Med. & Anat. P.*
"Jo. Hope, *Med. & Bot. P.*
"Thos. Young, *Art. Obst. P.*"

"Edinburgh, 24*th April* 1766."

"ADDRESS of the Students of Medicine to the Right Honourable the Lord Provost, Magistrates, and Town-Council of the City of Edinburgh, 1766.

"The following address is presented with the sole view of promoting the good of the University and City of Edinburgh. When such is its intention, there is no occasion to solicit, from the guardians of the welfare of both, a candid consideration of its contents, nor to entreat, that, what is meant only to declare the *sentiments* and *wishes* of those who are more immediately interested in your determination of the point to be considered, should not be construed into a vain and ill-judged attempt to infringe any of your undoubted rights.

"We who make this application are Students of Medicine in your University. By the death of the late Dr Whytt, we have been deprived of a truly learned and justly celebrated preceptor. By the resignation of Dr Rutherford we can no longer enjoy the instructions of a man, whose years and long services have justly entitled him to retire, with honour, from the burden of Academical duties. We are sensible that, from the manner in which their seats are supplied, the reputation of the University and Magistrates, and our hopes of farther improvement here, must entirely depend.

"We are humbly of opinion, that the reputation of the University and Magistrates, the good of the city, and our improvement, will all, in an eminent manner, be consulted, by engaging Dr Gregory to relinquish the Professorship of the Practice for that of the Theory of Medicine, by appointing Dr Cullen, present Professor of Chemistry, to the Practical Chair, and by electing Dr Black Professor of Chemistry.

"As Students of Medicine, we are led to adopt these sentiments from the following considerations:—

"The first is, the merits and abilities of Dr Cullen.—How

much that Gentleman has contributed to the advancement of almost every branch of Medical Science, whoever is in the least acquainted with the Medical Annals of your University, abundantly knows. To pretend to enumerate in detail how much we owe to him, how far he has contributed to diffuse an ardour for improvement and spirit of liberal inquiry in Medicine, were to waste your time and our own. We cannot, however, forbear to mention, that Chemistry, formerly little studied here, has been by him new-modelled, and raised to the highest reputation; so that his plan has been successfully followed in different parts of the British dominions, and that here there are now more Students engaged annually in the pursuit of this collateral branch of Physic, than in any other part of it, except Anatomy.—More particularly, it tends to our present purpose to observe, that all who have attended the Clinical Lectures in the Royal Infirmary, have, in them, heard him give proofs of his profound knowledge in the Practice of Physic, and candidly deliver the result of an experience of more than thirty years. There, from the justness and importance of his observations, from the accuracy and chastity of his reasoning, from his extensive knowledge of the practice of other countries, from the wideness of his views, from the perspicuity of his method, from the ease and elegance of his delivery, every hearer has obtained the most thorough conviction of his abilities as a Practical Professor, and conceived the most ardent desire of hearing his instructions from that Chair.

"Nor is this our opinion of Dr Cullen meant, in the least, to detract from the merits of Dr Gregory. On the contrary, a principal motive to our expressing the sentiments we do, on this occasion, is the high opinion we entertain of that Gentleman's capacity. By a late very elegant and ingenious performance, by every body attributed to him, we imagine it is evident what advantages the University must reap from Lectures on the Theory of Medicine, delivered by a thinker so just and original, and so universally acquainted with hu-

man nature. With pleasure, too, we reflect, that his character is not less respectable as a man, than as a philosopher; we therefore cannot suppose, that, were the public emolument to be obtained, even at the expense of his private interest, he would not rejoice to make the honourable sacrifice, far less that he would, in the least, hesitate to favour a scheme for promoting the public utility, when his private advantage is consistent with it.

"That the public utility *will* highly be promoted, by agreeing to an exchange, which leaves room for Dr Black's being elected Professor of Chemistry, will hardly be denied. Dr Cullen in his lectures on Chemistry, will always be heard with attention and applause. Those numerous experiments, however, which he is almost daily pointing out to us, as necessary to be made, for attaining farther excellence in that science, require, for their performance, such a long and unwearied attention, as is perhaps only consistent with an almost total abstraction from other avocations. How much Dr Cullen does in this way, even amidst the constant distractions of other business, is not easy to say. But the prosecution of the science still more amply upon that plan, may, with confidence, be expected from Dr Black, a man whose name as a Chemist is justly celebrated throughout all Europe, and whose reputation, at present, supports the fame of an inferior, but rival University. In him, the vigour and enterprise of youth, is conjoined to a well known and peculiarly preferable attachment to Chemistry. He, we apprehend, is the only person, who, in the Chemical Chair, can acquit himself with the same applause, that the present Professor has done for a number of years.

"Such are the considerations, that, as *Students of Medicine*, engage us to entertain these sentiments, with regard to the disposal of the Professors' seats: but were we for a moment to consider ourselves as *Citizens* of Edinburgh, additional motives would crowd upon us: the honour of the University; the honour of the City, intimately connected with it; the

great advantages arising from the confluence of students; the certain diminution of that confluence upon decay of the University's reputation; the necessity of raising that reputation to the highest pitch, in order to attain pre-eminence over rivals in other Universities; the fame of having contributed to that additional reputation; the disagreeable reflexion of having lessened it, would then be a few of the many arguments, on which we would enlarge to our Representatives and Governors, as incitements to their adopting the scheme we propose.

"But, without considering ourselves in that, to us, imaginary relation, we presume to hope we have suggested arguments, which will induce the Council to do every thing in their power to bring our proposal to execution.

"We end, by again assuring the Right Honourable Lord Provost, Magistrates, and Town-Council, that our intention here is only to inform them of the *sentiments* and *wishes* of the Students; that we are entirely sensible that our opinion, unless from its tendency to public benefit, should have no influence on the unquestioned independency of the Council's conduct; and that it is alone from the conviction of such being its tendency, that we have been prompted to make this address. What other motives *could* prompt us? We have no private enmity to gratify, nor successful rival to oppose. Though we should profess to have been guided only by private interest, our private interest, in this affair, is intimately and inseparably connected with the public good.

"Finally, whatever be the success of our endeavours, we are happy in having this opportunity of publicly expressing our gratitude to our Professors, and regard to the City.

"We, therefore, whose names are hereto subjoined, unanimously concur in declaring to The Right Honourable Lord Provost, Magistrates, and Town-Council of Edinburgh, that we are thoroughly convinced, that Dr Gregory's relinquishing the professorship of the Practice for that of the Theory of Medicine, that the appointment of Dr Cullen to the

Practical Chair, and the election of Dr Black as Professor of Chemistry, will add to the reputation of the University and Magistrates, and contribute highly to the good of the City and of the Students of Medicine.

"Possessed of this conviction, we therefore also unanimously concur in entreating the Right Honourable Lord Provost, Magistrates, and Town-Council of Edinburgh, in the most humble and earnest manner, to do every thing in their power to put in execution the scheme above proposed.

("*The above was signed by one hundred and sixty Students.*")

"Since the above address was approved and signed, a plan of arranging the professorships has been communicated to us, of which, thus late, it is still thought necessary to make mention. By this plan it is proposed, that Dr Cullen and Dr Gregory should be appointed conjunctly and severally Professors both of the Theory and Practice of Medicine, and Dr Black be elected to the Chair of Chemistry.

"Guided only by the faint light of our own prudence in our ardour for the public cause, proceeding with the most reverential deference for established forms, intending to propose no change but what might be effected by the compliance of one man, and which our exalted opinion of his character gave us the most just grounds to expect; there is no wonder that *we* should not have ventured to suggest an *alteration* in teaching the Theory and Practice of Physic, which, however expedient, has hitherto been inexperienced in this University.

"But when we consider the conformity of that plan with our own, in so far as producing an arrangement, by which Chemistry, Theory and Practice, are taught by Dr Black, Dr Gregory, and Dr Cullen; when we reflect, while choice is still left to the student, of prosecuting physic in the manner in which it was formerly taught, that, at the same time, peculiar advantages arise from its being in his power to learn the-

ory and practice from the same person; when we foresee the immense and rapid progress to be expected in sciences thus taught by two men of genius, contesting with noble emulation the prize of fame; finally, when we consider the sanction this proposal has received, by taking its rise among the Professors of Medicine themselves, we cannot but most heartily approve and sincerely admire it.

"Without, therefore, enlarging on a scheme, the propriety of which will doubtless be urged to you by a weight of argument worthy of that illustrious body where it took its rise, we whose names are subscribed, with the most perfect unanimity, declare our concurrence with it, and that the execution of it will be to us equally agreeable and advantageous as of that which we have before suggested.

("*The above was signed by one hundred and fifty-one Students.*")

"A report having been malignantly propagated, that the framing and presentation of this address were suggested and conducted, not by the Students of Medicine themselves, but by one of the Professors of Medicine; we, the committee appointed for presenting it, who have had access to be acquainted with the particulars relative to these matters, hereby declare, in the most solemn manner, that such report is entirely without foundation.

"James Maddocks, M. D.
"Thomas Smith.
"Alexander Monro Drummond.
"James Blair."

Dr Cullen having consented to become a candidate for the Chair of Dr Whytt, was elected to that office, and admitted Professor of the Institutes or Theory of Medicine on the 1st November 1766; and on the same day, Dr Black was admitted into the University

as his successor in the Professorship of Chemistry. The proposal suggested by some of the Professors in the University, and approved of by the Students in the latter part of their address, having been adopted and urged on the consideration of the Patrons and Professors, by succeeding students, Dr Gregory, after delivering three courses of lectures on the Practice of Physic, during the winter sessions of 1766–7, 67–8 68–9, was at length induced to comply with the general wish of those interested in the prosperity of the University, that Dr Cullen should be permitted to lecture upon that subject. Accordingly, we find that, with Dr Gregory's permission, Dr Cullen delivered a short course of lectures on the Practice of Physic, in the summer of 1768 *, and during the remainder of Dr Gregory's life, Drs Cullen and Gregory continued to give alternate courses of the Theory and Practice of Physic. On Dr Gregory's death, which happened on the 10th February 1773, Dr Cullen was appointed sole Professor of the Practice of Physic.

Such were the difficulties to be overcome, and such the exertions required to procure, first, a place in the University of Edinburgh, and afterwards the proper situation in it, for the man whose genius, talents and industry shed such a lustre over the Institution, and contributed in so remarkable a degree to extend and to perpetuate the fame of its Medical School!

* See Appendix, Note R.

At the period when Dr Cullen first began to deliver lectures on medicine in Glasgow, there prevailed in the medical schools of Europe three great systems of physic, those of Stahl, Hoffmann, and Boerhaave,—teachers not less distinguished by their peculiar and original powers of intellect, than by their attainments in literature and philosophy, their proficiency in the mathematical and experimental sciences, and their extensive knowledge of theoretical and of practical medicine. The lectures and writings of these eminent men, besides affording useful summaries of all that was known in medicine before the beginning of the eighteenth century, laid open various new and interesting views of the animal economy. Stahl and Hoffmann, in particular, recognised more distinctly, and recommended more emphatically, than had been done by any of their predecessors, the study of the living powers, and the laws by which they are governed, as the proper and legitimate objects of medical investigation.

The ancient doctrines of the four elements and their corresponding temperaments—of the separate functions of the vegetative, sentient, and rational souls—and of the agency of the natural, vital, and animal spirits—had continued to be taught in the schools of medicine with very little variation, from the time of Galen till after the middle of the seventeenth century. It was, indeed, but a short time before Stahl, Hoffmann and Boerhaave, began to lecture on medicine, that a solid foundation had been laid for the extension and improvement of medical science, by the introduction of the experimental and inductive method of prosecuting philosophical inquiries, so well explained and strenuous-

ly inculcated in the writings of Lord Bacon,—by the clear, precise, and logical distinction made by Descartes between mind and matter, as the respective subjects of properties essentially different from each other,—by the accurate analysis which had been given by Locke of mind and its operations, in his Essay on the Human Understanding, and his recognition of sensation and reflection as distinct sources of knowledge,—by the discovery by Newton of the universal law by which the motions of masses of matter placed at sensible distances from one another are regulated, and his distinction of this class of motions from the chemical changes which the different species of matter produce upon one another when their minute particles are brought into immediate contact,—by the application (though at first necessarily imperfect, and in many respects erroneous) of the principles of natural philosophy and of chemistry to the investigation of the phenomena of the animal economy,—by the discovery of the circulation of the blood by Harvey, and of the absorbent system by Asellius and Pecquet,—by the minute examination of the structure, distribution, and functions of the nervous system by Willis, Vieussens, Baglivi and others, —and by the development by Glisson of the contractile or irritable power inherent in muscular fibres, by the operation of which the various motions of the animal economy are performed ;—advances in knowledge all tending to facilitate the proper investigation of the vital susceptibilities and energies inherent in organized bodies, and of the operation of the external agents by which these susceptibilities and energies may be excited, modified or destroyed.

STAHL *, in professing to adopt the ancient hypothesis of the absolute incapacity of matter to move or to be moved without the immediate influence of mind, and the opinion so explicitly stated by Plato that the body is the organ of the soul, was led, in the formation of his *Theoria Medica Vera*, to ascribe all the organic changes or motions that occur in the human economy, involuntary as well as voluntary, to the direct operation of the rational soul, which he supposed to be endowed with the power not only of producing these motions, but also of governing and directing them according to their various final destinations †.

To the very obvious objection which presents itself to this theory—that, in the organic motions of the human economy, upon which the functions of circulation, nutrition, secretion, &c. depend, we are not only unconscious of any exertion or interference of the mind, but have no perception whatever of these motions themselves, and infer their existence solely from the effects which result from them,—Stahl replied, that it was by no means necessary to the truth of his theory that the soul should be conscious of these organic motions, or that, in producing them, it should always act by reflection ; for the mind, he remarked, is not conscious of many of its most indubitable acts. Corporeal motions are performed in the human economy, he alleged, *first*, by a certain kind of necessity inherent in

* Stahl was born in 1660 ; appointed Professor at Jena in 1684, and at Halle in 1694 ; went to Berlin in 1716, and died in 1734.

† Platonis Operum editio Bipont. *Alcibiades*, 1^mus^, tom. v. p. 59 ; *Phædon*, tom. i. p. 181, &c. Stahl, *Theoria Medica Vera*, edit. Halæ, 1708. De Mechanismi et Organismi Diversitate, p. 43. seqq. ; p. 51. Physiologia, pp. 18, 19, 37, and 325.

the soul, which he denominated *logos*, or reason; and, *secondly*, by reflection, which he termed *logismos*, or reasoning. In illustration of this distinction, he referred to the actions which we perform by *instinct*, and to those which we at first perform from intention, but afterwards come to perform from *habit*, without being conscious of them. But it is evident that, in adopting this mode of defending his theory from the objection made to it, Stahl was chargeable with an unwarrantable liberty in the use of language, in employing the words Reason and Reasoning as synonymous in their meaning with Instinct and Habit, and with a palpable infringement of a fundamental rule of logic, in substituting motions or actions that are voluntary, and therefore mental, in their origin, in place of the purely material and organic functions of plants and animals, to which alone the objection that had been urged relates *.

Besides the influence which the rational soul is universally allowed to exercise in the production of voluntary motions, Stahl adduced likewise, as proofs of the action of that principle upon the corporeal organs, those palpitations of the heart and irregular distributions of the blood in the system of circulation, which are occasioned by the emotions of fear, shame, anger, &c.; the convulsive motions excited in the nervous and muscular systems by those emotions of the mind which produce particular or universal tremors, such as epilepsy, hysteria, &c.; the changes produced in the action of the various secretory and excretory organs of the body by the influence of the passions; and the fainting, nausea

* De Mechan. et Organ. Div. *Argumentum.* Physiologia, p. 24.

and vomiting, which occur from the perception, or even from the recollection, of disagreeable objects. It must be acknowledged that, in all these instances, motions are produced in the body by causes operating through the mind; but it is at the same time certain, that, neither in the occurrence of these motions, nor in the action of the purely organic functions of the economy, are we conscious of any influence whatever proceeding from the foresight, intelligence, or intention of the mind; and, therefore, the conclusion that such an influence is exerted cannot be deduced from any knowledge of which we are possessed*.

The abstract notions of the inactivity of matter, and of the activity of mind or soul, on which Stahl's theory was made to rest, seem to have been adopted by physiologists in the very infancy of their science. Conscious that the locomotion of their own bodies was effected by the influence of the mind, they were naturally led to believe that motion in all other bodies must be produced by the operation of a similar active and intelligent cause. Accordingly, the doctrine that the functions of organized and living bodies are produced, maintained and regulated, by the influence of vivifying, animating, and governing principles,—emanations from the *anima mundi*, or soul of the universe,—was taught by the ancients; and, under different modifications, and various forms of expression, is to be found incorporated with almost every system of physiology and metaphysics, from the time of Plato† and Aristotle to that of Stahl. Indeed the doctrines upon

* Physiologia, pp. 24, 56, 203–208.

† De Legibus, lib. x. vol. ix. p. 89.

this subject taught by physiologists and metaphysicians, since the time of Aristotle, will, I am convinced, on a careful examination, be found to differ from those contained in the writings of that philosopher less in the novelty of the ideas which they convey, than in the variety of the terms in which they have been successively expressed.

Aristotle, in his Treatise on the Soul, the first regular treatise on Psychology that has come down to modern times, divided natural bodies into those which have life, the organic, and those that are without life, the inorganic; the former he denominated Animated, and the latter Inanimate bodies. Soul, generically considered, he defined to be the vivifying principle, entelecheia, or vital energy, common to all natural organic bodies, distinguishing this principle as it exists in plants, in animals, and in man, into the vegetative, the sentient, and the rational soul*. The organic functions of nutrition and generation, as they occur in plants and in animals, Aristotle attributed to the agency of the vegetative soul; the animal functions of sense and of voluntary motion, appetite, and passion, he attributed to the operation of the sentient soul; and the exercise of those powers of intellect by which

* Aristotle's threefold division of the Soul into the Vegetative, Sentient, and Rational, has been attributed by Nemesius to Plato, and he has been followed in this misconception by the late Dr Barclay. Plato likewise made a threefold division of the soul, but it was into the Appetites, the Passions, and the Cognitive Faculty, (Timæus, vol. ix. p. 387); and it was to these, and not to the vegetative, sentient, and rational souls, that he assigned separate residences in the belly, chest, and head.—See Barclay on Life and Organization, pp. 7, 8.

man is distinguished from other animals, he attributed to the agency of the rational soul. The vegetative soul, which Aristotle denominates the first and most common faculty of the soul, or the power by which all organized beings live, exists in plants, he conceived, without the sentient faculty, and the sentient soul in animals, without the rational faculty; but the rational soul cannot exist in man without the sentient faculty, nor the sentient soul in animals without the vegetative faculty *.

These great generalizations of Aristotle appear to have been the result of his extensive observation, reading, and reflection with regard to the functions of organized and living beings, and of his well directed and laborious endeavours to ascertain the nature and distinctive characters of those primary powers or principles in the constitution of plants, of animals, and of man, upon which the phenomena of life, of sense and voluntary motion, and of intellect or reason, more immediately depend. They form one of the more interesting of the many remarkable monuments which that philosopher has raised to his own fame; and, whatever defects or errors may now appear in them, they must continue to be useful land-marks to physiologists and metaphysicians in their inquiries into the phenomena and functions of vegetable and animal life, and into the attributes, modifications, and operations of mind

* Aristotelis *Opera Omnia, a J. Pacio edita*, 1597. De Anima, Lib. ii. Theses 3, 13–27. Ibid. Lib. ii. Thes. 24, 42. Magna Moralia, Lib. i. Cap. 4, 5. De Anima, Lib. ii. Thes. 31, 32, 34. Ibid. Lib. i. Thes. 95. De Moribus, Lib. i. Cap. 13.

in the different orders of beings in which that principle is perceived to exist.

The classification, by Aristotle, of the vital and purely organic phenomena of vegetable and animal bodies, under the general functions of Nutrition and Generation, and his attributing the production of these phenomena to the operation of a principle specifically distinct from the sensitive and intellective faculties of the soul, was unquestionably a great and important step in physiology. But it may be doubted how far his extension of the term Soul (ψυχη) to the cause of the vital and purely organic phenomena of plants and animals was correct in itself, or has been fortunate in its physiological applications. The ancients, as he himself informs us, all concurred in regarding sense, voluntary motion, and incorporiety, as the peculiar characters of mind or soul, and in viewing the body merely as the mechanical or organic instrument of this sentient, self-moving, and incorporeal principle. Several of his more immediate predecessors, and particularly Hippocrates, had employed the word Nature to indicate the vital and organic functions of plants and animals, though, as not altogether exclusive of sense and voluntary motion, and consequently in a meaning less definitely restricted than that in which Aristotle employed the term Vegetative Soul. It must be allowed, therefore, that Aristotle's generic definition of Soul, as a term synonymous with life, or the principle of life, besides being an innovation in language, has proved the source of much confusion in the notions adopted by succeeding philosophers respecting the cause of life or animation in organized beings, and has given occasion to the invention and personifi-

cation of numerous imaginary vital and archæal principles in the science of physiology *.

Galen seems to have been the first medical philosopher who perceived that the objectionable part of Aristotle's physiological generalizations of vital phenomena, consisted chiefly, if not solely, in his comprehending, under the generic term Soul, the phenomena and functions of organic or vegetative life, along with those functions that are peculiar to sentient and intelligent beings,—functions which he regarded as essentially connected with, or dependent upon, the presence of a nervous system †. In the commencement of his Treatise on the Natural Faculties, Galen observes that, to feel and to be moved voluntarily are properties peculiar to animals, but to be nourished and to grow are common to them with plants; the former are operations of the soul, the latter the work of nature. Instead, therefore, of comprehending the vegetative, sentient, and voluntive faculties of the mind, as had been done by Aristotle, under the common term Soul, Galen thought it more conformable to the ordinary use of language to designate the first of these faculties by the term Nature, and the second by that of the Sentient or Animal Soul; and, accordingly, under the appellation of the natural faculties, he has given a full account of the phenomena of nutrition and generation.

From the preceding statement, it would appear that the distinguishing peculiarity of the theory of Stahl, consisted in his ascribing to the rational soul those vital and purely organic functions which had been at-

* De Anima, Lib. i. Thes. 19, 33, 34.

† De Motu Musculorum, lib. i. cap. i. tom. iv. p. 372, edit. Kuhnii.

tributed by Aristotle to the agency of the vegetative soul, and by Hippocrates, Galen, and others, to the operation of nature *. But, in using the term Rational Soul to designate the cause of all the vital phenomena of the human economy, Stahl has nowhere explained, in a satisfactory manner, why the faculty of reason should be required, in this economy, for the execution of its organic or vegetative functions; while functions in every respect analogous are performed in plants and in brute animals without the agency of that principle. These functions, however, could not have escaped his notice, and we must therefore believe either that Stahl employed the words Rational Soul, as a generic term, implying necessarily, as Aristotle had conceived, the co-existence and co-operation of the vegetative and sentient faculties along with the intellective, or that he used the words rational soul in a sense little if at all different from that in which Aristotle had employed the term Vegetative Soul. This last supposition appears to be in some measure justified by the account which he has himself given of the reasons that induced physicians to make use of the term Spirits, and by a passage in his preface to Juncker's *Conspectus Medicinæ*, in which he states that the introduction of the rational soul into his medical theory was not at all

* Opinions regarding the operation of the soul in the animal economy, in some respects similar to those of Stahl, are to be found in the writings of Borelli and of Perrault; but these authors admitted the operation of other principles, both mechanical and chemical, besides the soul, in the explanation of vital phenomena. *Borelli, de Motu Animalium*, p. 1, 2, 3; et Pars secunda, Propos. 80.—*Perrault, La Mechanique des Animaux*, Avertissement; *Des Sens Exterieurs*, 1re Partie, &c. &c.

necessary to its validity; and assigns, as a reason for having recourse to that principle, his fear of being suspected to maintain that corporeal motions or actions could be performed without an agent *. Besides, Stahl himself informs us, in various parts of his writings, that his *anima* or rational soul was the same principle with the *nature* of Hippocrates †, a principle which, from the particular phenomena and functions of organized beings that have been uniformly ascribed to it by a long succession of physiologists, must be regarded as the same in every respect, except in name, with the vegetative soul of Aristotle, the natural faculties of Galen, Fernelius, and Sennertus; the archæus of Paracelsus and Van-Helmont; the formative, organizing, and plastic power of Harvey, Glisson, and Cudworth; the vis essentialis of Wolff; the life of Mr Hunter; the nisus formativus of Blumenbach; and the vital principle of Barthez, and of other modern authors.

Of the physiologists who have adopted Aristotle's doctrine of a vegetative soul or vital principle, some appear to have regarded this soul merely as a term significative of the organic constitution of plants and animals, and of the vital functions which they perform; others as indicating the existence of a distinct substance or principle, superadded to vegetable and animal organizations, which possesses properties different from the physical and chemical qualities of matter, and of a nature intermediate, as it were, between the properties of matter and the attributes of mind. But whether for the production of the organic and vital phenomena

* Physiologia, p. 22. Junckeri Conspectus Medicinæ, p. 8.

† Stahl, Ad Aliena a Medica Doctrina arcendum, p. 25.

of plants and animals, any other properties have been conferred upon the elementary molecules of which they are formed, in addition to the physical and chemical properties which these molecules possess when they enter into the composition of unorganized masses of matter, is a point concerning which the researches of past physiologists do not appear to afford any satisfactory information. By some physiologists, however, it has been maintained that the production of the organic and vital phenomena of plants and animals must depend on the operation of properties quite different from those with which the constituent elements of unorganized matter are endowed; while other physiologists, again, have been led by various considerations arising out of the progress of discovery in the sciences, to believe that the vital properties which are perceived to exist in the elementary textures of plants and animals, and the changes occasioned in them by the operation of external agents, may be the results of chemical combination in organized structures, which can only be regarded as the infinitely diversified products of a Divine Architect.

The following observations of Boerhaave, which had professedly the Archæus of Paracelsus and Van Helmont for their object, are still applicable, it is conceived, to the different modifications which have since been proposed of the doctrine of vital principles as existences distinct from the organizations in which they inhere.

"These philosophers conceived it to be necessary to admit the existence of a certain being that directs all the

functions of the body in the states of health and of disease, —which is neither the cogitative mind, nor dull and common matter, but an agent of an intermediate nature,—from thinking it impossible that so many different functions, mutually depending upon one another, as are carried on in organized bodies, could be properly performed, unless they were all regulated by some intelligent director. They were unwilling to attribute that office to the immortal (or rational) soul, because in that case we must be conscious of those things that take place within us, and even be able to regulate the functions which we call vital. There seems to be no need of a serious refutation of this hypothesis. I can scarcely believe that Helmont was so insane as to credit all those things which he wrote about the Archæus; and as often as he says that the archæus desires, selects, digests, and expels food, he seems to have meant nothing more than that food is desired, selected, digested, and expelled by some unknown power. For what difference is there between confessing that you are ignorant of the cause of a particular action, and attributing it to an obscure and fictitious agent, of which you cannot ascertain either the existence, or the nature, or the properties, or the mode of operation? We know, indeed, many mechanical causes of the functions, and we likewise know in general that life, health, and all the functions of the human economy depend on innumerable physical causes, so combined into one organic body by the First Mind, that the body can preserve and restore itself; nor does it stand more in need of any subordinate mover than a clock, after it is once set in motion, requires a new principle of motion during the whole of the time for which the different wheels, that enter into its structure, suffice for carrying round the index *."

Were any additional evidence required to shew that

* Boerhaave's Physiology, § 107. Note 5.

the rational soul of Stahl was but another name for the vegetative soul of Aristotle, it might easily be obtained from a review of the particular functions which he has every where assigned to that principle. Indeed, his dissertations on the differences that exist between mechanism and organization, and on the peculiar properties and functions which distinguish living from inorganic bodies, and the accurate account which he has given in his Physiology, of life as resulting entirely from motion, of vital activity, and of vital actions, of digestion, circulation, secretion, &c. as concerned in the vegetative functions of nutrition and generation, can be considered in no other light than as an exposition of the properties and functions of the vegetative soul or vital principle, more full, correct and definite than any which had been previously given, and as complete, in all its parts, as the state of the natural sciences, and the knowledge of organic structure, and of vital functions at the time he lived, could possibly admit of. Stahl seems to have insisted the more strenuously on the doctrines of organization, vitality, and vital actions, in consequence * of the very novel and singular view which had been taken of the animal economy by Descartes, particularly in his posthumous work, the *Tractatus de Homine et de Formatione Fœtus*. This philosopher had endeavoured to prove that all the functions of organized bodies, animal as well as vegetative, result entirely from the mechanical disposition of their organs, in the same manner as the motions of a clock, or of any other automaton, arise from the arrangement of

* Stahl, De Mechan. et Organ. Div. p. 31.

its springs, weights and wheels; and, therefore, that it was unnecessary to attribute these functions to the operation of a vegetative or sentient soul, or, indeed, to any other principle of motion or life besides the heat of the blood and animal spirits, which operates unceasingly during life in the brain and heart. In rejecting the generalizations of vital phenomena which had been made by Aristotle, and in maintaining that the animal functions, as well as the purely organic, are performed in brute animals without the interference of a conscious mind, merely from the automatic constitution or internal mechanical arrangement of their bodily organs, Descartes was led to conclude, that though these animals possess a nervous system and sensitive organs, similar in their structure to those of man, they are nevertheless altogether devoid of sense, memory, knowledge and desire,—a conclusion obviously not less absurd than that to which Stahl, in rejecting the same generalizations, was conducted in imagining that the organic, as well as the animal functions of all living beings, are begun, carried on, preserved, and governed, by the exercise of an intelligent, rational, and yet unconscious soul *.

By admitting with Descartes only two substances in the human system, an organized body and a rational soul, and by supposing that the soul acts directly upon the body without the concurrence of any intermediate agent, Stahl was enabled, in the view which he took

* Descartes, De Homine (Amstelodami, 1677), § 105, 6. De Formatione Fœtus, § 1, 5. De Homine, § 14. De Passionibus, Art. 8, 16. De Form. Fœtus, § 7, seqq. Responsiones Quartæ (Meditationes 1663), p. 126. Resp. Sext. p. 157. De Methodo, § 5.

of the functions of the human economy, to lay aside all explanations founded on the operation of such imaginary principles as natural, vital, and animal spirits, which had formed so large a portion of the physiological and pathological doctrines of the schools, and had so long diverted the attention of philosophers from the proper objects of physical and medical inquiry. He rejected also all explanations founded on the chemical composition of the fluids, and on the alterations to which they are subject in diseases, and considered the different theories of putrescency and of fermentation, of acrimony and of cacochymy, of acid and of alkali, &c. as utterly irreconcilable with the sound state of the blood in the living body, and with the operation of those vital motions by which that state is preserved amidst all the causes of variation to which it is exposed. The only changes in the blood which Stahl recognised as occurring during life, are those that relate to its quantity and consistence *.

As Health, according to Stahl, depends on the sound state of the vital organs, and on the due performance of their respective functions under the governing influence of the rational soul; so Disease arises from the efforts of this soul, to resist whatever either immediately or remotely tends to injure the composition or structure of these organs. In conformity with this opinion, Stahl considered diseases as arising from the combined operation of two distinct causes; *first*, from the direct action of noxious powers upon the body; and, *secondly*, from the reaction of the organic system itself, consisting in

* Physiologia, p. 22. Ad Aliena, &c. p. 18, seq. Physiologia, p. 152, 3.

the vital and tonic motions excited in it by the soul, in order to oppose and counteract the effects which the noxious powers produce, and to purify, preserve, and repair the system. Diseases, accordingly, were regarded by Stahl as not in themselves pernicious, but as apt to become so, from the mistakes of the soul in the choice of the motions which it excites for their removal, or from errors in the proportion and degree of these motions, and in the time of their production. Even death itself did not appear to Stahl to arise from any inevitable cause in the structure of our bodies, but merely from the indolence of the soul leading it to desist from its vital motions, and from its unwillingness to continue its painful and troublesome functions in the corporeal derangements arising from the operation of noxious powers *.

The term Nature had been used by Hippocrates to express the cause, not only of all the healthy operations that take place in the animal economy, but also of those actions of the organic system by which diseases are spontaneously cured. Stahl made these actions the particular subject of his consideration; and in his celebrated thesis *De Autocrateia Naturæ*, has given a very full enumeration of the salutary effects they produce in repairing the injuries, and in mitigating and curing the diseases to which the body is liable. Though it be undoubtedly an ancient opinion, that nature cures diseases, yet Stahl was, so far as I know, the first pathologist who explicitly stated and maintained, that diseases, such as fevers, inflammations, hæmorrhages,

* Pathologia, p. 37–41. De Scriptis suis, p. 65.

spasms, &c. proceed chiefly from the reaction of the organic system, excited by the operation of noxious powers, and are to be considered as salutary efforts of the soul, to defend the vital economy from the dangers that threaten it. This view of pathology which represents morbid actions as efforts of nature to avert some greater evil, led Stahl to consider it to be the principal duty of a prudent and intelligent physician to observe carefully the natural course of diseases,—to superintend and direct the motions which the soul excites in order to effect their cure,—to moderate these motions when they appear to be too strong,—to strengthen them when too weak,—and to employ for these purposes all the means, medicinal, dietetic, and regiminal, which may assist in restoring the diseased economy to the proper integrity of its structure and functions *.

Dr Cullen has alleged that "both Stahl and his followers have, in the whole of their practice, been very much governed by their general principle. Trusting much to the constant attention and wisdom of nature, they have proposed the art of curing by expectation; have therefore, for the most part, proposed only very inert and frivolous remedies; have zealously opposed the use of some of the most efficacious, such as opium, and the Peruvian bark; and are extremely reserved in the use of general remedies, such as bleeding, vomiting, &c." † This censure of the Stahlian practice must be allowed to be in several respects just, but at the same time it is perhaps too indiscriminating. The remedies which Stahl recommended may be said in general to

* De Script. suis, p. 64. † Works, vol. i. p. 405, 6.

have consisted of the antiphlogistic regimen, and the proper regulation of the non-naturals, the former adapted for the treatment of acute, and the latter of chronic, diseases. It is often indeed, in the practice of the healing art, a matter of extreme difficulty to say at what point a cautious and prudent abstinence from interference passes into ignorant or careless negligence. If trusting to the spontaneous operations of the animal economy leads to a feeble and inert practice, it must be admitted that the neglect of these operations not less frequently leads to a preposterous confidence in the powers of remedies that is the source of much rash and presumptuous empiricism.

The simple and sublime conception that all the motions of the human body are produced and governed by an intelligent principle inherent in it, was well calculated, by its novelty and by the easy and comprehensive generalization of vital phenomena which it seemed to afford, to excite and promote the speculative inquiries of medical philosophers, and to free the science of medicine from many of those erroneous and absurd mechanical and chemical doctrines with which in its progress it had become encumbered. But the adoption of this hypothesis led Stahl, in the framing of his system, to be too easily satisfied with the imperfect and erroneous physiological view which he had taken of the human economy,—to neglect the phenomena of life, as they present themselves in the nutrition and generation of plants and of irrational animals,—to content himself in accounting for the phenomena of the organic functions, with applying the term Rational Soul to the principle which had been, by almost all

former physiologists, denominated the vegetative soul or nature; and almost wholly to omit in his view of the animal economy, the consideration of the peculiar and distinguishing susceptibilities and energies of the Nervous System. These errors and omissions prevented Stahl from perceiving the fixed boundary which has been established by nature between the operations of the material and mental faculties of our frame, in that consciousness of unity and personal identity, by which all the various modifications of sense, memory, intellect, and passion appear to be constantly and inseparably accompanied; while, at the same time, his ambition to be the founder of a new sect in medicine disposed him to be less just to the merits of his predecessors and contemporaries than is required of one who undertakes to make any addition to the opinions or to the experience of past ages.

It is but just to Stahl, however, to acknowledge, that he had the merit of directing the attention of medical practitioners, in a more particular manner than had been done before his time, to that resistance to putrefaction which exists in the solid and fluid parts of the body during life,—to the vital activities by which the state of health is preserved, and its functions duly performed,—to the influence which the mind indirectly exercises over the different functions of the body,—to the effects of the different passions in exciting diseases,—to the natural course of diseases,—and especially to those powers of the animal economy by which diseases are spontaneously cured or relieved.

This bold and enterprising philosopher continued with an industry and zeal which have rarely been

equalled, for a period of more than thirty years, to explain and defend his peculiar medical doctrines in his lectures, and in an extensive series of academical prolusions and inaugural dissertations. His *Theoria Medica Vera*, first published in 1708, contains a full account of all his medical opinions, and of the improvements which he conceived he had introduced into the teaching and practice of physic; but the abstract method in which he treated every subject, the obscurity of his language, and his studiously avoiding every form of expression, as well as of thought, that he did not believe to be peculiar to himself, have rendered the perusal of this work difficult and disagreeable, and have induced many to seek for information with regard to the system of Stahl, in the writings of his pupils, rather than in those in which his ideas were originally communicated and explained. Stahl has had many able and intelligent followers, and though few, if any, physicians now profess to adhere strictly to his doctrines, yet almost all use in their writings more or less of his phraseology, and of the modes of explanation and reasoning which he employed.

HOFFMANN *, who had studied medicine along with Stahl, under the learned Wedelius, at the University of Jena, and who, in founding the medical school of Halle, had procured for him one of its professorships, was the author of a System of Medicine, differing in many respects from that of his colleague. Rejecting

* Hoffmann was born in 1660; entered on his duties as Professor of Medicine at Halle in 1693; went to Berlin in 1708, as physician to the King of Prussia; but returned again to Halle in 1712: died in 1742.

the hypothesis that matter must, in every instance, be put in motion by the direct agency of an immaterial principle, Hoffmann believed with Leibnitz that all the sensible motions observable in different masses of inorganic matter, and the changes which take place in the arrangement of their constituent parts, result necessarily from the physical and chemical qualities with which they were endowed in their original formation, and that material bodies act and react upon one another without the intervention of any immaterial principle *.

In the human economy, according to Hoffmann, there are three principal agents to be considered, differing from one another in their essence and operations, and requiring to be distinguished by different appellations. These are, Nature, or the Organic Body; the Sentient Soul; and the Rational Soul;—a classification corresponding, as he conceived, to that mentioned in Scripture, of Body, Soul, and Spirit. By the term Nature, Hoffmann understood the whole of the circulatory, secretory, and excretory motions occurring in the solids and fluids, by which the life and health of the body are preserved:—by the Sentient Soul, that principle common to man and brute animals, by which they perceive, remember, desire, and move:—and by the Rational Soul, or Mind, which distinguishes man from other animals, that noble principle which, being conscious of its own modifications and operations, learns to

* Leibnitz, Acta Erudit. Ann. 1694–5–8. See Appendix to "A Collection of Papers which passed between Mr Leibnitz and Dr Clarke," Lond. 1717, p. 365–383. Hoffmann, Prolegomena de Medicinæ Natura, fundamentis, &c. cap. iv. § 14. Scholion.

compare the ideas arising from these sources, and to judge of their relations, and which has the additional power of beginning and regulating its own actions, according to its deliberate determinations *.

From the general resemblance of this enunciation of the three great powers inherent in the human economy, to the distinction which had been made by Aristotle of the vivifying principle in plants, in brute animals, and in man, into the vegetative, sentient, and rational souls, one would be led to conceive that Hoffmann must, with that philosopher, have considered the organic functions of nutrition and generation in plants to be vital processes. But having defined life to consist in the circulatory, secretory, and excretory motions produced by the action of the heart and arteries, it became necessary for him to affirm, that since plants are destitute of these organs, life cannot with propriety be ascribed to them;—an opinion in opposition to that of Aristotle and of most other physiologists, and not very consistent even with the view which he has himself taken of the vegetable economy in the ninth chapter of his treatise *De optima Philosophandi ratione*. That the circulation of the nutritious fluids is in man, and in many other animals, though not in all, connected with, and dependent upon, the action of a heart and vessels arising from it, to which nothing analogous is to be observed in plants, must be allowed; but, on the other hand, the processes of nutrition, growth, secretion, and generation, as they occur in plants, bear so many analogies to these processes in the

* Philosophia Corporis Humani Vivi et Sani, lib. i. sect. iii. cap. 3. § 1, Scholion; cap. 7. § 1, Scholion.

animal economy, and are so different from any phenomena exhibited by unorganized bodies, as to have led to their being justly and almost universally considered as vital operations *.

Instead of supposing, with Stahl, that the organic functions of the human economy depend for their production on the agency of an intelligent soul, Hoffmann endeavoured to prove that they do not depend upon any immaterial agent whatsoever, but are motions resulting from the mechanical and chemical properties of the elements of which the organic body is composed. As the circulatory, secretory, and excretory motions, which he considered to be the functions most essential to life, appear to depend in the human economy on the dilating and contracting powers of the muscular fibres of the heart and bloodvessels, he conceived these powers to be the true cause of the organic functions, and regarded their action as depending upon the animal spirits, a thin, subtile, elastic fluid which he supposed to be contained in the minute tubes of the nerves, and in the blood itself †.

The faculties of sense, memory, imagination, passion, and volition, which Hoffman ascribed to the Sentient or Animal Soul, are precisely the same with those by which this principle had been characterized by Aristotle. The Greek philosopher, in his different treatises on the History, on the Parts, and on the Generation of Animals, as well as in his Treatise on the Soul itself, has enumerated a great many curious and

* Philos. Corp. Hum. lib. i. sect. i. cap. 2. § 2.

† Ibid. lib. i. sect. i. cap. 2. § 21 ; cap. 3. § 25. Prolegomena, cap. 8. § 11, 12.

important facts relative to the faculties and operations of the sentient principle, as they are to be observed in the different orders of animals; and has considered that principle in many of its most interesting physiological and metaphysical relations. The number of faculties included under the term Sentient Soul appeared to him to vary in the different species of animals. All animals, he frequently remarks, have one of the senses, touch; and with this, the feelings of agreeable and disagreeable, and, consequently, of desire and aversion. In some animals the sense of touch seems to be accompanied with more or fewer of the other senses; in other animals, again, the senses seem all to be present, in a state little, if at all, less perfect than in man; and accompanied with certain degrees of memory, imagination, appetites, passions, capacity of acquiring knowledge from experience and discipline, and the power of acting from the apprehension of immediate good or evil. Aristotle had observed the remarkable fact, that when some of the lower species of animals are divided, the divided portions not only continue to live, like some plants when similarly treated, but seem each to retain their powers of sense and voluntary motion; and from his observation of this fact, as well as of the continued development of the same mental faculties in the successive generations of the same species of animals, he was led to believe that the sentient soul is, as well as the vegetative, essentially connected with, if not the product of, material organization *. Long before his

* Aristotle, De Anima, lib. ii. Thes. 17, 23, 27, 31. De Sensu et Sensili, cap. 1. Historia Animalium, lib. i. cap. 2; lib. viii. cap. 1; lib. ix. cap. 1, 7. De Anima, lib. i. Thes. 68–93.

time, some of the Greek philosophers had taught that the souls of brute animals are to be regarded as portions of the divine incorporeal power, or *anima mundi*, by which they supposed the motions that occur in every being contained in the universe, inanimate or animate, to be produced and regulated; while others maintained, that these souls are individual, immaterial existences, coeval in their origin with matter, and, like it, neither created nor annihilated in the successive generations of animal systems, but passing by transmigration after death from one species of animal to another. On each of these hypotheses, the incorporeal sentient soul was supposed to manifest different powers, according to the peculiar organization of the animal systems with which it is conjoined.

But though Aristotle conceived the sentient soul, or soul of brute animals, to be inseparably connected with the organic body in which it resides, and dependent upon it for the exercise of all its faculties, he seems to have regarded the human intellect, or rational soul, as a principle of a more divine nature, derived from without, and consequently capable of acting independently of the body, and of existing in a state separate from it *. The respective operations of the sensitive, representative, intellective, and voluntive faculties of the rational soul in man, as united in him in one individual consciousness, are defined, described, and

* De Anima, lib. ii. Thes. 21, 22.; lib. iii. Thes. 19, 20. De Moribus Animalium, lib. vi. cap. 2. De Partibus Animalium, lib. i. cap. 1, 5; lib. iv. cap. 10. Magna Moralia, p. 184, 5. De Generatione Animalium, lib. ii. cap. 3. De Historia Animalium, lib. i. cap. 1.

physiologically considered by Aristotle in his Treatise on the Soul, and in some of his other writings, with an accuracy that is truly astonishing. His analysis, in particular, of the information afforded by each individual sense,—his distinction of conception, phantasy, or the representative faculty from sense on the one hand, and from understanding on the other, and his account of the order observed in its reminiscences and combinations,—his description of the analytical, synthetical, and ratiocinative processes of the understanding or intellect, and his division of this faculty into the contemplative or theoretical, which has truth or error for the object of its study, and into the practical, prudential or moral, which has good or evil for the more immediate objects of its consideration,—and his enumeration and description of the appetites, desires and affections which influence the determinations of the will,—form indeed some of the most original and important contributions that have ever been made to metaphysical science *.

From the time of Aristotle to that of Hoffmann, and particularly during the progress of the sixteenth and seventeenth centuries, the consideration of the nature, properties, and seat of the sentient soul, and of the manner in which this soul is produced and continued in the successive generations of the different species of animals, occupied much of the attention of

* De Anima, lib. ii. cap. 7–11 ; lib. iii. cap. 3. Thes. 158–162 ; cap. 5. Thes. 1–16 ; cap. 8. Thes. 27–36. De Memoria et Reminiscentia passim. De Moribus, lib. i. cap. 4 ; lib. iii. cap. 3 ; lib. vi. cap. 2. De Animalium Motionibus, cap. 6, 7. Magna Moralia, lib. i. cap. 18. Rhetorice, lib. ii. De Anima, lib. iii. Thes. 40–59.

physiologists, metaphysicians, and theologians; and a great interest was given to the discussion of these subjects, from its being erroneously supposed that the opinions formed regarding them must have a tendency to uphold, or to undermine, the principles of natural and revealed religion. Of the various hypotheses invented to explain the origin of sensation, memory, passion, intelligence, and voluntary motion, as they appear in brute animals, three, sufficiently distinct from one another, seem to have been eagerly embraced and strenuously defended by their respective admirers: the *first*, that these phenomena are not mental phenomena, but mere material changes occurring in animal machines;—the *second*, that they proceed from the operation of a principle intermediate in its nature between mind and matter;—and the *third*, that they are the necessary results of the action of a principle, purely immaterial, united to animal organizations.

The hypothesis, that the phenomena usually ascribed to the sentient soul are not mental phenomena, but mere material changes occurring in animal machines, was proposed by no less distinguished a philosopher than Descartes. He seems to have been led to deny the existence and co-operation of a sentient soul, in the production of the animal functions, in some degree from his conceiving, and that justly, that the corporeal part of these functions may be exercised independently of the mental part, and in some degree also from an unwillingness to admit that brutes possess a sentient, thinking, and conscious principle, of the same immaterial nature as the rational soul of man. It has been

shewn by Bayle, that Descartes had been anticipated in the hypothesis, that brutes are mere machines, by Pereira, a Spanish physician; but this absurd and paradoxical hypothesis has had few followers, and scarcely any of distinction, if we except the names of Paschal and Malebranche *.

A very slight observation of the manners and habits of the different species of animals by which man is every where surrounded, must at all times have enabled him to perceive that these beings are provided with organic and sensitive frames, similar to his own; and that their actions appear to be governed by feelings, appetites, and passions, in many respects analogous to those by which he is himself influenced. Observations of this kind must have led necessarily to the conclusion, that brutes have souls as well as organised bodies, and that the soul of man can differ from theirs only in one of two respects—either in its nature or essence, or in the number and degrees of the mental faculties with which man and brute animals are respectively endowed. It has, however, been only by slow degrees that metaphysicians have been brought to perceive, that, to determine whether the soul of brutes be in its nature specifically different from that of man, is a problem placed beyond the reach of the human faculties; and to acknowledge that we neither know nor can know, by the use of our reasoning powers, any thing of matter besides the properties which we perceive in it by our senses, nor any thing

* Descartes, De Passionibus, prima pars, art. 16. Bayle, Dictionnaire Historique et Critique, Art. Pereira.

of soul or mind besides the different feelings of which we are conscious. For the suggestion of these fundamental principles of philosophical inquiry, we are particularly indebted to the writings of Descartes; and for their fuller development and complete demonstration to Mr Locke's excellent account of our complex ideas of substances, of the use of general terms, and of the names of substances. By no author since their times have these important truths been more clearly perceived or more accurately expressed than by Mr Dugald Stewart, in his introduction to the Elements of the Philosophy of the Human Mind; in his Observations on the Metaphysical Theories of Hartley, Priestley, and Darwin; and in his statement of the Argument for a Future State, derived from the Nature of Mind *.

But if the principles laid down in the writings which have been referred to are admitted to be just, it would seem to follow that, in the present state of our knowledge, we ought to regard every attempt to demonstrate, by reasoning *a priori* that the animal functions of sensation and voluntary motion must necessarily depend upon the operation of a principle altogether different in its real nature from matter, as no less absurd than it is to attempt to prove that the function of intellection in all its relations is purely the result of material organization. The diffidence and modesty with which Mr Locke has expressed his opinion on the question whether God can confer consciousness or the power of

* Descartes, Meditatio Secunda. De Principiis Cogitationis Humanæ, p. 2. Locke, Essay on the Human Understanding, book ii. chap. xxiii; book iii. chap. iii. & vi.

thinking upon matter *, form a striking contrast to the presumption with which other metaphysicians have boldly decided this question in the negative; and, in their ignorance of the real nature of material and mental substances, have presumed to set limits to the power of Him by whom these substances were contrived and created. "Men are not capable," as Boerhaave has justly remarked, "of understanding beings and their natures, like their Creator; nor were they present at the first formation of things, and yet are they proud enough to judge, censure, and determine in these matters †." How much more usefully might metaphysicians have been employed, if, instead of attempting to resolve a problem, which, from its nature, seems to be insolvable by human reason, they had occupied themselves, like Aristotle, in observing and comparing the operations of the mental faculties of brutes with those of man. Such an investigation could not have failed to force upon them, irresistibly, the conviction that, between the souls of brute animals and the rational soul of man, there exists a great and an essential distinction; and that man is better distinguished from brutes by the employment of the faculty of speech as an instrument of thought and a medium of communication with his species;—by the faculty of perceiving physical, metaphysical, and mathematical truths, and judging of their relations;—and by the power of adapting the operations of mechanical, chemical, intelligent, and moral agents to the attainment of his purposes,—faculties all co-operating with

* Loc. cit. book iv. chap. iii. § 6, and Notes.

† Institutions, § 669.

his innate desire of immortality, to the progressive improvement of the human species in knowledge and virtue,—than by any subtle distinctions, founded on incomprehensible speculations regarding the material or immaterial nature of the principles employed by Divine Wisdom in the production of the mental feelings of which man is conscious in himself, and the indications of which he perceives in other animals.

The results of the inquiries of philosophers, with regard to the seat of the sentient as well as of the rational soul, and the manner of their union with the body, were not more satisfactory than those had been which related to their nature or essence. Some, with Plato, imagined that the appetitive part of the soul occupies the region of the liver; the passions, that of the heart; and the cognitive faculty, that of the head. Some, as Descartes, conceived that the soul occupies only a very small portion of the brain; others, that it is extended over, and capable of perception, in every part of the nervous system; and others again, as Leibnitz, imagined that the soul and body, being the one of an immaterial and the other of a material nature, are incapable of being united; and that in the actions in which they appear mutually to influence each other, they correspond merely, like the movements of two contiguous time-pieces, in consequence of the pre-established adjustment of their contriver *.

Whether the sentient soul of animals is produced

* Descartes, De Passionibus, pars prima, art. 31. *seqq.* Leibnitz, Essais de Theodicée; Disc. Prel. § 55; Essais, § 59–65, § 400. See Appendix to Collection of Papers which passed between Leibnitz and Clarke, p. 387.

by an immediate creation, or by successive reproduction, had also been the subject of much fruitless controversy. The very learned but elaborate answers given by the Theological Faculties, in eight of the German Universities, to the questions relative to this subject, addressed to them by Sennertus *, seem to contain all the information which their own or former times had collected respecting this inscrutable physiological point; and are at least sufficient to instruct those who may be disposed to enter on its re-investigation, what opinions, regarding the generation of the sentient soul of brute animals, may be safely adopted, and what are to be rejected as dangerous; what may be received as orthodox, and what ought to be proscribed as heretical.

From the account which Hoffmann has given of the sentient or animal soul, he seems to have been inclined to consider it as a principle of a nature intermediate between mind and matter, being unwilling either to deny altogether the existence of mind in brute animals, or to admit that they possess minds purely immaterial. Of what nature the sentient principle is, exceeds, he confesses, the reach of human intellect to ascertain; but he regarded it as sufficiently distinguished, by its operations, both from matter and from the rational soul. It belongs, he says, to a higher species of metaphysics than medical inquiry can reach, to determine whether, besides body and spirit, there be not another substance which, as a pure, homogeneous, simple element, is fitted to receive the impressions of

* Sennerti Opera Omnia. Lugduni 1650, vol. i. p. 851.

external objects, and to produce corresponding reactions. The sensitive soul, in performing its functions, employs, he imagined, as its principal organ, a fluid secreted from the blood in the brain, by the motions of which in the nerves all the phenomena of sensation, memory, passion, and locomotion, are produced. This nervous fluid, which Hoffmann supposed to be the organ of the sentient soul is, as he himself states, the same in its nature and properties as the animal spirits of the ancients, and the ether of more modern physiologists *.

The great and prominent merits of Hoffmann as a medical philosopher, undoubtedly consisted in his having perceived and pointed out more clearly than any of his predecessors, the extensive and powerful influence of the Nervous System, in modifying and regulating at least, if not in producing all the phenomena of the organic as well as of the animal functions in the human economy, and more particularly in his application of this doctrine to the explanation of diseases. Galen had recorded many facts which had been observed before his time, by Erasistratus, Herophilus and others, relative to the nervous system, considered as the organ of sense and voluntary motion, and to these he had added several new observations and experiments of his own. But it was not till the publication of the elaborate works of Willis and Vieussens, that the structure, distribution, and functions of that system seem to have become the objects of very general attention among medical men. These authors

* Philos. Corp. Hum. lib. i. sect. iii. cap. viii. § 3. scholion; § 7. et scholion.

ROYAL

pointed out many examples of sympathies existing between different parts of the human body through the medium of the nervous system, in the states both of health and disease; and Mayow, Baglivi, and Pacchioni, endeavoured to account for some of these sympathetic actions, by a contractile power which they erroneously supposed to be lodged in the fibres of the dura mater. It was reserved for Hoffmann, however, to take a comprehensive view of the Nervous System, not only as the organ of sense and motion, but also as the common centre by which all the different parts of the animal economy are connected together, and through which they mutually influence each other. He was, accordingly led to regard all those alterations in the structure and functions of this economy, which constitute the state of disease, as having their primary origin in affections of the nervous system, and as depending, therefore, upon a deranged state of the imperceptible and contractile motions in the solids, rather than upon changes induced in the chemical composition of the fluid parts of the body.

In speaking of the writers who had treated of the pathological doctrine of the nervous system previously to Hoffmann, Dr Cullen has very justly remarked, that " in these writers it was either not extensively applied to diseases, or was still so involved in many physiological errors, that it had attracted little attention; and Dr Hoffmann was the first who gave any tolerably simple and clear system on the subject, or pointed out any extensive application of it to the explanation of diseases." Hoffmann has himself given an excellent summary of his pathological doctrines in that remark-

able passage of his *Medicina Rationalis Systematica*, which has been quoted by Dr Cullen in the introduction to his First Lines of the Practice of Physic, and which exhibits a concise, and at the same time a juster and more comprehensive view of the animal system in its morbid state than had been taken by any preceding pathologist.

The ideas with regard to the nervous origin of diseases, which pervade the numerous writings of Hoffmann,—and which he has explained and illustrated in a very distinct and luminous manner in the fourth and fifth chapters of his Therapeutics, where he treats of the Genealogy of Diseases, and of the Sympathies existing between the different parts of the Nervous System,—form the great basis of the pathology at present taught in the schools of medicine; and which, though at first slowly received, has at length gradually supplanted the doctrine of the four humours, and their corresponding temperaments, that had prevailed for a period of two thousand years. It must be admitted that Hoffmann amply realized the expectations expressed by Leibnitz in a letter which he wrote to him in 1699, in which, in complaining of the inattention of authors to strictness of definition, he remarks: "You appear to me to be one of the few who are at particular pains to speak of things the meaning of which is understood. I expect from you, therefore, at some future period, some elementary principles of rational medicine, not too nearly allied to those theoretical speculations, remote from practical utility, which prevail amongst the Cartesian physicians; nor founded on mere sports of the imagination, as is usually the

case with the doctrines of the chemists; but such as shall bring to light the intelligible causes of sensible things, where this can be done; and where this is impossible, shall at least deduce useful results from those things that are certain in fact, though not yet referrible to their causes*."

The followers of Stahl have accused Hoffmann of borrowing sometimes, without acknowledgment, the opinions of his colleague; but it may be questioned how far this charge is in any respect well founded; and it is beyond all doubt that Hoffmann cannot have owed his opinions regarding the nervous origin of diseases to Stahl, since this author makes very little mention of the nervous system, either in the physiological or pathological parts of his *Theoria Medica Vera*. The most accurate view which we possess of the differences existing between the medical systems of these two professors, is that given by Hoffmann himself in his *Commentarius de differentiis inter ejus doctrinam Medico-Mechanicam et Stahlii Medico-Organicam*. This commentary, which was published some years after the author's death, is distinguished by the great extent, minuteness and accuracy, of the physiological, pathological, and therapeutical knowledge which it contains, and by the instructive example it affords of the exercise of candour and discrimination in estimating the merits and in refuting the doctrines of a rival. Yet it is impossible to read this commentary, without regretting that Hoffmann should have admitted into the explanation which he has given of the

* Hoffmanni Operum Supplementum, pars 1mo, p. 51.

nervous origin of the phenomena of health and disease, the operation of a principle so hypothetical and imaginary as the animal spirits or nervous fluid, which had been entirely rejected by Stahl. Hoffmann has, however, it must be allowed, amply atoned for this imperfection of his system, by the expanded and correct view which he has taken of the true foundations of medical science, and of the legitimate mode of conducting medical inquiries, in several paragraphs of his *Opusculum de erroribus Medicorum in theoria et præcipue in praxi medica*, in which a variety of important philosophical maxims are inculcated, that cannot be too deeply impressed upon the minds of those who are employed in cultivating and in endeavouring to improve the science of medicine.

Instead of regarding Medicine, as had been done by Stahl, as a branch of knowledge separate from, and totally independent of, the other arts and sciences, Hoffmann conceived it to depend upon these in a great measure for its improvement and progress; and he employed all the powers of his capacious mind, and his extensive learning, in borrowing from anatomy, and from natural philosophy and chemistry, whatever could tend to elucidate the study or improve the practice of medicine. His writings, in six volumes folio, form an immense store-house of theoretical and practical knowledge, collected from his own observations and reflections, and from all the treasures of ancient and modern learning. This laborious mode of conducting his inquiries, and the anxiety which Hoffmann evinces to do justice to the opinions of his predecessors and cotemporaries, has in some degree obscured the origi-

nality which actually exists in his own compositions,—a quality the semblance of which is not unfrequently given to the speculations of inferior authors, merely by their omitting to refer to the sources from which their ideas have been borrowed or their information more immediately derived.

Professor Rega of Louvain was the first writer on the continent of Europe, after Hoffmann, who adopted the doctrine of the Nervous Pathology. He has given a useful and somewhat popular view of this pathology in his *Tractatus Medicus de Sympathia seu Consensu partium corporis humani ac præcipue Ventriculi in statu morboso,* published at Haarlem in 1721. In this country, it was Dr Cullen who first perceived the value of the doctrine of the Nervous Pathology introduced by Hoffmann; and from the time he began to lecture on medicine in Glasgow, he not only adopted that doctrine, but uniformly endeavoured to render to the memory and writings of its author that commendation and applause to which the importance of his improvements in pathology, and in the descriptions of diseases, and his candour and impartiality to others, gave him so just a claim.

Shortly after Hoffmann and Stahl had laid the foundation of the Medical School at Halle, BOERHAAVE * began to teach medicine at Leyden. He had been educated originally for the church, but in consequence of being unjustly suspected of having adopted the metaphysical opinions of Spinoza, he found it necessary to abandon his intention of entering into the

* Boerhaave was born in 1668, appointed Professor at Leyden 1709; and died in 1738.

clerical profession, and applied himself zealously to the study and practice of medicine.

No teacher of medical science ever entered upon the duties of his office with qualifications greater than those which Boerhaave possessed, or performed them with more ability, diligence, and success. His erudition was varied and profound; he was conversant with the opinions of the different sects of ancient philosophers, and with the writings of the Greek and Arabian physicians; and to this store of learning he added an acquaintance with all that had been discovered in medicine previously to his own time. He was also eminently distinguished by his knowledge of the collateral sciences of natural philosophy, chemistry, botany, and anatomy; and to great mental endowments he united the most indefatigable industry in the acquisition of experimental and professional information. It is impossible for any one acquainted with the life and writings of Boerhaave to peruse his oration commemorating the talents and virtues of his deceased colleague Professor Bernard Albinus, without perceiving that, in delineating the character of his friend, he was all the while unconsciously giving a picture of his own mind, and pronouncing an eulogium which is in every respect justly applicable to himself *. Thoroughly instructed in the modes of investigation and reasoning pursued in the mathematical sciences, Boerhaave, in commencing to teach medicine, endeavoured to simplify its study, by rejecting all vain and useless hypotheses, and by adopting, as the basis of his

* Boerhaave, Opuscula Omnia, 4to.; Hagæ Comitis, 1738, p. 44.

reasonings, those facts only which he believed to have been ascertained and verified by observation and experience. After having taught for a few years, he reduced his medical doctrines to a regular system, which he published in two separate volumes; one entitled the Institutions or Theory, and the other the Aphorisms or Practice of Physic. These short treatises, which gave to medicine a more systematic form than it had previously exhibited, are remarkable for brevity, perspicuity, and elegance of style, for great condensation of ideas, and for the number of important facts which they contain relative to the healthy and diseased states of the human economy.

The fundamental principle of Boerhaave's Physiology consisted in regarding man as composed of two distinct substances, Mind and Body. The former he defined to be, that within us which feels, thinks, and wills; the latter, that which has extension, figure, and mobility. It is the essential nature of Mind, he observes, to be conscious, or to think; but to think of this or of that particular thing is accidental to it. The essential nature of Body is to be extended and impenetrable. These attributes of mind and body have, he conceived, nothing in common, nor can any analogy between them be inferred from their existing together in the same class of beings. "When I think of extension," says Boerhaave, "it does not involve any idea of consciousness; and when I reflect upon consciousness, I cannot perceive any connexion which it has with extension. The idea or definition of body, therefore, has nothing in common with that of mind; nor has the idea of mind, on the other hand, any thing

in common with that of body." But though these two principles differ essentially in their nature, they are capable of mutually influencing each other, and their reciprocal action he regarded as an ultimate fact, to a knowledge of which we are led by observation and consciousness, but which does not admit of explanation *.

Boerhaave considered the study of Mind to form an important part of physiology; and, accordingly, in his lectures on this subject, he gave an account of the Mental Faculties under the two distinct heads of the External and Internal Senses—a distinction which had been previously followed by Fernelius and other physiologists, and which has of late years been revived by several metaphysicians, under the appellations of the External and Internal Affections of the Mind †.

In his explanation of the process of sensation, Boerhaave did not admit, with the Peripatetics, the intervention of any sensible species, idea, or image, between the impression produced by an external object upon a sensitive organ, and the affection of the mind by which such an impression is followed. "Our bodies," he observes, "receive nothing else from sensible objects, to produce sensation, than a change in the surface of the nerve, excited by the contact of the object."—"I do not say that this is to feel, but I say that we feel when this takes place."—"Sensation, therefore, is nothing either in the object or in the nerve affected, but

* Institutions, § 27, and Comment.

† Ibid. § § 481, and 566. Elemens d'Idéologie par Destutt Tracy, Paris 1804. Lectures on the Philosophy of the Human Mind by the late Dr T. Brown, Edin. 1820.

a certain idea which God has assigned to this particular corporeal change. This is acknowledged by Newton in the last edition of his Optics. It must be remembered, however, that, by a certain law, a definite idea is connected with each corporeal change, so that the same action of the same object, upon thesame organ, always produces the same idea." From these, and other passages in his writings, it is obvious that Boerhaave employed the term idea to signify, not corporeal images of external objects transmitted from the sensitive organ to the mind—the sense in which it has been understood by some metaphysicians—but sensations, conceptions, and thoughts, occasioned sometimes by external and sometimes by internal causes. Indeed, M. Claude Perrault, in his *Essais de Physique*, had previously pointed out, in a very distinct manner, the equivocal meaning of the term idea or image—the literal, in which a painting, or the impression of a seal, are called images of objects, and the metaphorical, in which speech is said to be the image of thought, and writing the image of speech—and had affirmed that, though the word image had been employed in both of these senses by physiologists and metaphysicians, it is only in the metaphorical sense that the expression of the mind preserving the images of objects ought to be understood *.

But though Perrault and Boerhaave agreed in rejecting the hypotheses of substantial forms or images, they differed widely as to the part of the body in

* Boerhaave, § 566, and Comment.; § 570, Comment. Perrault, Œuvres de Physique, Amsterdam, 1727, tom. i. p. 268.

which they conceived the process of perception to be completed by the affection of the sentient mind. Perrault supposed that the mind is united to the whole of the animated body, and is consequently affected by the impressions of external objects, in the organ upon which they more immediately act. Boerhaave, again, conceiving, with most other physiologists, the more particular seat of the mind to be in the brain, was led, from the well known effects of the compression and division of nerves in destroying the sensibility of the parts to which they are distributed, to believe that the change produced upon the extremity of a sentient nerve must be propagated along the nerve to the brain, before sensation can be produced. Of the two mechanical explanations that had been given of the manner in which the change produced upon the extremity of a sentient nerve is propagated along it to the brain, according to one of which the nerves are to be considered as solid vibratory cords, and according to the other as hollow undulatory canals, Boerhaave adopted the latter, conceiving that the change in the common Sensory can be nothing more than a repulsion of the nervous fluid or animal spirits against their origin in the brain *.

That different portions of the nervous system are subservient to different functions in the animal economy, is an opinion which was entertained by some of the ancient physicians. Erasistratus, as we are informed by Rufus Ephesius, conceived that there are

* Perrault, loc. cit. p. 265–6. Boerhaave, § 568, and Comment.

different nerves for sensation and for motion. The sensory nerves he supposed to arise from the membranes, and the motory from the substance of the brain. Galen, also, believed that the nerves of sensation and the nerves of motion are separate, and have different origins; and assigns this as the reason why those organs of sense which are susceptible of voluntary motions, such as the eye and the tongue, are provided with two sets of nerves. "If," says he, in his Treatise on the Use of Parts, "one of these nerves is at any time injured, the organ loses only that function which the injured nerve performed for it. Thus we often see the tongue in one case impeded in its motion, and in another in its power of recognising and distinguishing tastes." The sensory nerves, Galen supposed to arise from the cerebrum, and the motory, with but few exceptions, from the cerebellum. It is mentioned by Dr Friend, that Gulielmus de Saliceto, "in speaking of wounds in the chest, has a very remarkable passage concerning the nerves of that part; he says, those from the 6th and 7th pair, which arise from the brain and the nucha, serve for its voluntary motion, the others for the natural or vital motion. I take notice of this," continues Dr Friend, "because it is exactly the notion of the famous Dr Willis, the first inventor of the nervous System." Willis considered the cerebrum to be the seat of the intellectual faculties, and the source of that portion of the animal spirits on which the voluntary motions of the body depend. To the cerebellum he attributed the production of those involuntary actions, which go on in the animal economy in a regular manner, without our knowledge, and

independently of our volition. The nerves presiding over the voluntary motions, therefore, he supposed to arise chiefly from the cerebrum; and those presiding over the involuntary motions, chiefly from the cerebellum or its appendages. Boerhaave agreed with Willis in believing, that the cerebrum presides over the voluntary, and the cerebellum over the involuntary motions of the animal economy. But as, in many parts of the body, both of these two classes of motions are performed together, he conceived that the nerves of these parts must be composed of fibres, derived partly from the cerebrum and partly from the cerebellum, and retaining, throughout their whole course, a peculiar function according to the part from which they arise. He was likewise disposed to believe that each of the senses has not only its own peculiar external organ, but also its distinct seat in the common sensory or brain *.

Boerhaave gave an uncommon degree of interest to his lectures on the External Senses, particularly to those on Seeing and Hearing, by curious details with regard to the varieties observable in the structure of the visual and auditory organs of man and other animals, and by a happy application of the principles of optics and acoustics to the explanation of the phenomena of these functions: and he rendered his lectures

* Rufus Ephesius, de Partibus Hominis, p. 65, edit. Clinch, Lond. 1726. Galen de Anatomicis Administrationibus, lib. iii. De Usu Partium, lib. viii. Friend, History of Medicine, part ii. p. 315. Willis, Anatomy of the Brain, chap. xv.–xvii. Boerhaave, § 395, Comment.; § 600, Comment.; § 401, Comment.; § 273, and Comment.; § 568, and Comment.; § 570, and Comment.

on the Internal Senses, and on the states of Sleeping and Waking, no less interesting, by the selection of short, striking and apposite illustrations, borrowed from almost all the departments of human knowledge. There are, indeed, few portions of medical writing of the same extent as Boerhaave's account of the mental faculties, which exhibit such a happy union of varied, curious, and useful information.

Conceiving the human body to be a combination of all manner of machines, arranged by its Divine Architect into one compendious system, so as to be capable of maintaining its existence for a long series of years, of repairing the waste it undergoes from its own motions, and of producing systems similar to itself, Boerhaave endeavoured to explain the phenomena of the body, both in its healthy and morbid states, chiefly by the principles of Natural Philosophy and Chemistry; and though he admitted the existence in it of vital and animal powers, he seldom, if ever, had recourse to these powers in his explanations of the functions of the animal economy. Hence his physiological system was represented by some of his contemporaries as founded upon principles purely mechanical. He denies, however, the justness of this representation. "It is incorrect," says he, "to attribute to me the opinion that all medical phenomena are to be explained upon mechanical principles. This is certainly very far from being my opinion; for as I am persuaded that, even in simple bodies, general laws are insufficient to explain all the individual phenomena, much less do I suppose them capable of accounting for those in the human body, the most complex of all." "But if some portions

of the human body," he justly remarks, " correspond in their structure with mechanical instruments, they must be governed by the same laws. For all the power of these parts is in the motion which they produce; and motion, by whatever body it is performed, takes place according to the universal laws of mechanics. There are some who think that those actions should not be explained by mechanical laws, the mechanical causes of which are unknown to us. But this is said without an accurate examination of the matter, for we do not speak of the *causes* but of the *effects* as being governed by mechanical laws. There are many and considerable motions performed in nature, of the causes of which we are ignorant; but the motions themselves are governed by supreme and universally diffused laws. The magnet, the cause of whose action is wholly unknown to us, performs its motions agreeably to a certain and ascertained law, which, when once known, can be applied, without danger of mistake, to future experiments. The human body, in like manner, exhibits motions, the causes of which are unknown to us; but their effects are the elevation of weights by cords affixed to them, the propulsion of fluids through determinate vessels, and other effects like those produced by mechanical causes, and they are not governed by any other laws."—" So that it appears," he adds, " that both parties have erred,—the mechanicians, in attempting to define all things from their art, without being sufficiently acquainted with the structure of the parts, the powers of which they expressed by numbers,—and those who, hating the very name of mechanics, have declared that our body is independent of those very laws

by which all bodies whatsoever are governed."—" The misfortune is that such physiological subjects are usually handled either by mathematicians unskilful in anatomy, or by anatomists who are not versed in mathematics*."

To explain in what manner that contraction or shortening of the muscular fibres is produced, upon which all the sensible motions of the animal economy depend, Boerhaave adopted the hypothesis generally received by the Mathematical School of Medicine, with the doctrines, demonstrations, and calculations of which he was intimately acquainted, and of which he must be regarded as one of the most able and most discriminating adherents. According to this hypothesis, the minute fibres of the nerves and of the muscles are continuous tubes, containing a greater or less quantity of nervous fluid; and the contraction of the muscular fibres was conceived to depend on their dilatation by an increased influx of this fluid. The sufficiency of this supposed cause of muscular contraction, to account for the immense force which the muscles are capable of exercising, Boerhaave endeavoured to establish, by a reference to the hydrostatic paradox demonstrated by Mariotte, which, as he remarks, shews how considerable a weight may be overcome by a small quantity of water, if the moving power be applied through a small tube to the resistance. With these views of the manner in which muscular contraction is produced, and of the nature of its dependence on the nervous system,

* Institutions, § 19, Comment.; § 40, Comment.; § 29, Comment; § 212, Comment.

it is scarcely to be wondered at that Boerhaave should have noticed Glisson's opinion, that this phenomenon depends on a peculiar power inherent in the muscular fibres themselves, with only a simple expression of dissent*.

The attempt to explain, upon Mechanical principles, the cause of muscular contraction, now justly considered as a vital phenomenon, must be regarded as a fundamental mistake on the part of the mechanical physiologists, in the employment of their favourite science, and as the primary source of many of those erroneous and discordant calculations which have tended to throw discredit on the application of mathematical principles to the phenomena of the animal economy. A more legitimate object of inquiry, and one which was pursued with the greatest ability and success by Borelli, the founder of the Mathematical School of Medicine, was to ascertain the resistances which any given muscle or muscular organ must overcome—or, in other words, the force which it must exert—in order to produce a given effect. And it must be acknowledged, that, in applying the general laws of moving powers to the voluntary motions, these physiologists were extremely successful in determining the circumstances upon which the amount of force exerted by the voluntary muscles must depend, and in pointing out the various mechanical contrivances by which their actions are promoted and regulated. By no physiologist have the results of their labours been more briefly and distinctly stated than by Boerhaave, in

* Institutions, § 395–407 ; § 411 ; § 415, Comment.

the 410th and 412th paragraphs of his Institutions *. That the attempts of natural philosophers to apply the principles of mechanics, and particularly of hydraulics, to the motions of the muscular organs subservient to the vital and natural functions, should have been far from being equally successful with the application of these principles to the voluntary motions, is to be attributed not to their inapplicability to those motions, as has often been erroneously supposed, but to the difficulty of ascertaining the different elements upon which such calculations must be founded.

The application of Chemistry to the explanation of the phenomena of the animal economy, though of an earlier date than that of Mechanics, had, at the time when Boerhaave taught, been by no means equally successful. This was a necessary consequence of the entire ignorance of the chemists respecting the elementary composition of the solids and fluids of the

* In taking notice of the great increase of strength produced by the multiplication of fibres in muscles of a penniform structure, Boerhaave has omitted to mention the greater extent of motion produced by a given degree of shortening in oblique muscles, as compared with that which is produced by an equal degree of shortening in straight muscles, a fact which had been pointed out by Borelli, in the 110th and 111th propositions of his work, and afterwards by Dr Monro *primus*, in his remarks on the articulations, &c. of the lower jaw; and of which a variety of demonstrations have been since given by Heringa, Dr Monro *secundus*, and Sir Gilbert Blane—demonstrations all tending to shew the advantages to be derived from geometrical knowledge in the explanation of the phenomena of muscular contraction.—Dr Monro 1^{mus}, Works, p. 279. Heringa, Dissertatio Inauguralis, Lugduni Batavorum, 1741. Dr Monro 2^{dus}, Observations on the Muscles, 1794. Sir Gilbert Blane, Croonian Lecture, 1788, Select Dissertations, p. 281.

human body, and of the effects which may be produced upon these by the operation of different chemical agents. No physician seems ever to have been more completely aware than Boerhaave of the folly of the attempts which had been made by chemists, to account for all the phenomena of the animal economy, by the operation of some predominating chemical element, or of the advantages that may be derived from the judicious use of chemical knowledge in all the different branches of the healing art. "We are obliged," he says, "to confess, that there are many truths, and those too of the greatest importance in the whole of medical physiology, a knowledge of which can be acquired only by the assistance of chemistry. But the greatest glory of this valuable art is, that it alone is able to expose and correct those errors, which some whimsical dabblers in chemistry had introduced into medicine, as Boyle, Bohn, Hoffmann, Homberg, and others, have shown by beautiful examples. Those vain trifling chemists were certainly in the wrong, who have pretended by their art alone to explain physiology in all its parts; nor, however, are those less mistaken, who imagine they can do this without chemistry. Let anatomy faithfully describe the parts and structure of the body; let the mechanician apply his particular science to the solids; let hydrostatics explain the laws of fluids in general, and hydraulics their actions, as they move through given canals; and, lastly, let the chemist add to all these, whatever his art, when fairly and carefully applied, has been able to discover; and then, if I am not mistaken, we shall have a complete account of medical physiology." From this

statement, it is obvious that Boerhaave did not, in his Physiological System, take any very enlarged or correct views of the vital properties of organized beings—of the dependence of these properties in animals upon the state of the nervous system,—of the reciprocal or sympathetic action of all parts of the living body upon one another through the medium of that system, or of the more immediate connexion which it has with the exercise of our sentient, intellectual, and moral faculties.

The Pathology of Boerhaave was even more defective than his physiology, from his, if possible, still greater neglect in it of the peculiar vital powers inherent in the human body; so that, notwithstanding the great caution, and in many respects correct method, which he observed in accounting for its morbid conditions upon mechanical and chemical principles, his explanations of these were, in general, fully more applicable to the body, considered as an inert hydraulic machine, than as an organized, living, and sentient system. There are, however, in the pathological writings of Boerhaave, some passages in which he recognises the existence of vital motions in the animal economy, that cannot be referred to the operation either of mind or of unorganized matter. Thus, in speaking of the sudden and wonderful effects of certain vegetable and animal poisons, he observes, "the force which they exert is not mechanical, but relative to the animal organs and humours, which makes me at a loss to know what to say concerning them." The motions, too, by which diseases are spontaneously cured, he denominates *automatic*, in order to distinguish them from those of simple mechanism.

These salutary motions occurring in all the diseases to which man and brute animals are subject, and in every stage of their progress, arise, he conceived, from the peculiar constitution of the organic body, without the interference of the mind, which can neither produce nor destroy them. But, as if afraid in maintaining the corporeal origin of automatic motions, that the charge of Spinozism might be renewed against him, he deprecates being accused of believing that the animal machine is guided in its operations by a fatal necessity. "The doctrine of automatic motions," he affirms, "is certain, however displeasing it may be to some philosophers *."

Besides the cures which nature produces by means of the automatic motions, Boerhaave conceived that animals, when affected by disease, are often led by instinct to the use of appropriate remedies; and that man is impelled, by the constitution of his mind, in the states of pain and of sickness, to seek for relief, by making use of every object in nature that can be supposed to possess sanative virtues. These endeavours of the mind to free itself from present uneasiness by following the indications of nature,—by availing itself of past experience,—and, in want of this, by making promiscuous experiments,—he regarded as the origin of Therapeutical knowledge, and the basis on which all practical medicine must necessarily rest.

"In forming a system of physic," as has been well observed by Dr Cullen, "Dr Boerhaave seems to have studied dili-

* § 725, Comment.; § 1144, Comment.; § 5, Comment.; § 4, Comment.

gently all the several writings of both ancient and modern physicians; and, without prejudice in favour of any former systems, he endeavoured to be a candid and genuine Eclectic. When he came into the School of Leyden, about the beginning of this century, he found that school divided between the chemical system of Sylvius de la Boe, and the mechanical one of Bellini and Pitcairn. His sagacity perceived the general utility of both, and his discernment selected very properly from each. From Bellini he took the doctrine of obstruction, and at the same time the doctrine of lentor, which Bellini had taken from the Cartesians. From the chemists Boerhaave took the doctrine of acid and alkali, but he very properly limited and corrected it. He thus combined the doctrines of the Mechanicians, Cartesians, and Chemists, and also admitted the doctrine of Plethora, the only remains of the Galenical theory, which the discovery of the circulation of the blood tended to support. Possessed of an excellent systematic genius, he gave a system superior to any that ever before appeared. As in the great extent, and seemingly perfect consistency of system, he appeared to improve and refine upon every thing that had before been offered; and, as in his lectures he explained his doctrines with great clearness and elegance, he soon acquired a very high reputation, and his system was more generally received than any previous one had been since the time of Galen. Whoever will consider the merits of Dr Boerhaave, and can compare his system with that of former writers, must acknowledge that he was very justly esteemed, and gave a system which was at that time deservedly valued *."

The unrivalled celebrity of Boerhaave as a teacher, and the valuable information communicated in the lectures, in which he continued, for a period of thirty years, to explain his text-books, with a felicity and

* Works of Dr Cullen, vol. i. p. 411.

variety of illustration derived from every possible source of knowledge, attracted to Leyden almost all those who, during that period, were engaged in the scientific study of medicine. Even after the death of this celebrated teacher, his reputation seemed rather to increase than to decline; and it was supported, not only by the intrinsic value of his text-books, but by the publication of Commentaries upon them by two of his most distinguished pupils, Haller and Van Swieten. Haller, who had taken notes of the lectures of Boerhaave during an attendance on them of three years, published the Commentaries upon the Institutions of Medicine nearly as they had been delivered, with the addition of a few valuable notes and illustrations of his own. Van Swieten, after having diligently attended for twenty years upon the lectures of Boerhaave, new-modelled the Commentaries of his master upon the Practice of Physic, into those useful but ponderous volumes which are now so well known under his own name.

The mechanical doctrines of the Simple Solid, and of the Humoral Pathology, as modified by Boerhaave, continued for many years, in the writings emanating from the School of Leyden, and from most of the other Medical Schools of Europe, to afford the greater part of the explanations that were given of the phenomena of the animal economy in its sound and morbid states; while but little if any notice was taken of the influence which the vital powers, particularly those of the nervous system, exert in the production of these phenomena. Some portion, however, of the physiological and pathological opinions which had been taught by Glisson, Stahl, and Hoffmann, appears to have been in-

troduced earlier into the medical schools of Holland than into those of most of the other countries of Europe. De Gorter, professor of medicine in the University of Harderwic, was the first distinguished disciple of Boerhaave who attempted to ingraft these opinions upon the system of his master. The principal alterations which he suggested are detailed in his *Exercitatio Medica de Motu Vitali*, and in his *Oratio de Animi et Corporis Consensione mirabili tam in secunda quam adversa valetudine.* In the former of these essays, De Gorter distinctly attributes the vital motions of the animal economy to the operation of a contractile power inherent in the living solids, differing in its nature from the powers of unorganized matter, and governed by laws peculiar to itself. A slight examination of the functions which he ascribes to this power, is sufficient to shew that they are the same with those which Glisson had attributed to the principle of Irritability, though De Gorter has neither made use of the term irritability, nor referred to the writings of that author. The most original part of De Gorter's essay on Vital Motion, appears to be that in which he endeavours to account for the production of local inflammations by the increased action of the contractile power inherent in the capillary arteries. The doctrine of the irritability of the muscular fibre was soon afterwards formally introduced into the School of Leyden itself by Professor Winter, and illustrated, in a succession of medical theses, by several of his more distinguished pupils, but without any reference being made in these to the doctrines of Glisson *.

* Winter de Certitudine in Medicina Practica, Franeker,

A still greater innovation on the system of Boerhaave was made by his nephew Kaau Boerhaave, in his celebrated work entitled *Impetum faciens dictum Hippocrati,* &c., published at Leyden in 1745. The title of this work, which is the common Latin translation of the *To Enormon* of Hippocrates, or the principle of motion in the animal economy, can be regarded only as another name introduced into medical writings for Nature or the Vital Principle; and, accordingly, we find that the term *impetum faciens* is used by Kaau Boerhaave to express sometimes the functions which had been ascribed to the vegetative, and at other times those which had been ascribed to the sentient soul; sometimes to designate the cause of the contractile power of the muscular fibre, and at other times the source of the sensibility of the nervous system. In collecting from all sources, from poets, historians, philosophers, physicians, and divines, proofs of the reciprocal action of the mind and body upon each other, and in superadding to these, many curious and original observations with regard to the functions of the nervous system, and the energies and sympathies which this system exercises in the states both of health and disease, one is surprised to find that Kaau Boerhaave should have carefully avoided making any mention of the writings of Stahl and of Hoffmann, with which he must have been familiar, and from which he must have derived much of his information, and several of the improvements which he attempted to make upon the system of his uncle Dr Boerhaave. These im-

1746. Bicker, De Natura Corporis Humani. Vanden Bos, De Vivis Corporis Humani Solidis.

provements, which relate chiefly to the Physiology, were certainly well calculated to promote the medical inquiries of the period at which the *Impetum faciens* appeared; but this work is now chiefly remarkable as a monument of the learning, industry, and professional zeal of its author.

Gaubius, the favourite pupil of Boerhaave, and his successor in the Chairs of Chemistry and of the Theory and Practice of Physic, continued for a period of twenty years to employ, as text-books for his medical lectures, the Institutions and Aphorisms of his master. At length, however, he ventured to compose and to publish his *Institutiones Pathologiæ Medicinalis*, a work which, from its intrinsic merits, kept possession of the schools of medicine for a long series of years, and has served as a model for most of the systems of general pathology which have since been produced. In his Institutions, Gaubius has retained a portion of the mechanical and chemical explanations of Boerhaave; but to these he has made several valuable additions in the fuller and juster views which he has taken of the agency of the living solids—of the reciprocal influence of the mind and body upon each other—and of the healing powers inherent in the animal economy. Without falling into the error of supposing, with Stahl, that the purely organic and vital, as well as the animal functions, are carried on under the governing influence of the mind, Gaubius has attributed to this principle its full and just share of importance in the production of the morbid as well as of the healthy phenomena of the human economy. It is not without some justice, therefore, that he has been said by his biographer M. Vicq

d'Azyr, to have established the true principles of Medical Psychology, not only in his Institutions of Pathology, but in his two academical orations, entitled *De Regimine Mentis quod Medicorum est.* The merits of Gaubius's Institutions of Pathology first became known in this country, in consequence of the great use which Dr Cullen made of them in his Lectures upon this branch of the Theory of Medicine.

But the pupil of Boerhaave, who made the greatest changes upon the system of his master, and perhaps the most important additions to the theory of Medicine that have ever been made by a single individual, was HALLER*, of whom it is difficult to say whether we ought to admire most the universality of his genius, his unremitting industry, or the useful direction of his labours. "For the principal object of his studies," observes the eloquent and scientific Condorcet, "Haller made choice of Physiology, that branch of medicine which, by penetrating into the intimate structure of the different parts of the body, endeavours to ascertain by what laws man is formed, is developed, grows, lives, reproduces his species, decays, and dies; how each organ performs its appropriate motions and fulfils the functions for which it is destined; by what means the organs, which the exercise of their functions tends continually to destroy, may be repaired by nourishment and sleep; by what mechanism a power, the principle of which is unknown to us, sometimes performs, at the command of the will, actions that are necessary for the preservation or for the happiness of man, and some-

* Haller was born in 1708; called as Professor to Gottingen in 1736; retired to Berne in 1753; and died in 1777.

times produces, independently of volition, operations that are essential to his existence; how changes in the organs are sometimes the cause and sometimes the effect of derangement of the vital functions; what the relations are which exist between the derangement of these functions, and the diseases of the parts that perform them;—and, finally, how remedies of every kind, may, by acting upon these organs, re-establish order in the animal economy. Haller was aware that the science of Physiology, long abandoned to the spirit of system, had become an object of distrust to natural philosophers, and it was with him a principal object to remove this prejudice. He hoped to render physiology a science as certain as any other physical science, a science by means of which philosophers might acquire a knowledge of the constitution of man, and physicians find a basis upon which to found their practice. For this purpose, it was necessary to establish the foundations of physiology upon the correct anatomy of man, as well as upon comparative anatomy, which has so frequently revealed to us secrets respecting the animal economy, that the study of man himself had failed to discover. It was necessary to banish from Physiology, both that kind of metaphysics, which, in all the sciences, has long concealed real ignorance under scientific terms, and those mathematical and chemical theories, rejected by mathematicians and chemists, and always employed with the greater confidence, and adopted with the greater respect, in proportion as teachers, or their disciples, have been ignorant of mathematics and chemistry. It was necessary to substitute, in place of all these systems, gene-

ral facts ascertained by observation and experience; to have the prudence to be satisfied with these facts, and to submit to remain ignorant of their causes; and to know that in all the sciences, there are limits beyond which it is doubtful whether the human mind can ever penetrate, and which it certainly can only pass by the aid of time, and a long series of labours *."

By his exertions as an experimentalist, a teacher, and an author, Haller gave a most powerful impulse to the study and progress of Physiology. His experimental researches on the formation of the chick *in ovo*,—on the phenomena attending the growth of bone,—on the circulation of the blood,—and on the sensible and irritable parts of animals,—though forming but a small part of his labours, furnish a series of results unequalled in number, accuracy, and importance, by those of any other physiologist †. But Haller's claims to the gratitude of posterity rest less perhaps upon the discoveries, valuable as these were, which he himself made in the animal economy, than upon that spirit for experimental physiological research which his example contributed so powerfully to engender, and to animate in his contemporaries and successors. His zeal and industry as a teacher procured for him the warm and steady attachment of a numerous body of pupils, all eager to co-operate with him in his researches, to defend his opinions, and to extend his fame. By bringing together in his Elements of Physiology all that was known of this science,—by adding to the labours of his

* Eloge de M. De Haller, Œuvr. Compl. de Condorcet. t. I. p. 379. edit. Brunswick et Paris 1804.

† Halleri Opera Minora, 3 tom. 4to: Lausannæ, 1762–68.

predecessors the results of his own observation and original experimental researches,—and by arranging the materials of this stupendous work upon a plan as strictly philosophical as it was entirely new,—Haller has acquired to himself the title of Father of Physiology, a title by which, in the progress of discoveries, and amidst all the future changes of medical opinions, he must ever continue to be distinguished.

In the general view which Haller took of the animal economy, he seems to have embraced opinions with respect to mind and matter, their distinctive attributes, and their reciprocal influence upon each other, the same or nearly the same with those of his preceptor Boerhaave. "That the nature of the mind," he remarks, "is different from that of the body, appears from an infinite number of considerations, but more particularly from the consideration of those abstract ideas and affections of the mind which have nothing in common with the organs of sense *."

Of the different theories which had been formed to account for the nature of the connexion existing between mind and matter, Haller informs us that Boerhaave was inclined to adopt the system of *pre-established harmony* suggested by Leibnitz, in preference to that of *occasional causes* proposed by Descartes; and he owns that he himself felt the same predilection. "It is established," he observes, "as a perpetual law by the Creator, that certain changes made first in the nerve, and then in the common sensory, shall produce certain new correspondent thoughts in the mind, which changes

* Primæ Lineæ Physiologiæ, sect. 569.

and thoughts have an indissoluble connexion with each other; so that though what we perceive in the world be arbitrary, yet that it is real and not false, appears plainly from the perpetual agreement of similar thoughts, arising from similar affections of the sensitive nerves, in all persons at the same time from one object, or in the same person at different times." That Leibnitz himself would readily have admitted this modification of his doctrine, may be presumed, not only from his well known tolerant character in matters of philosophy, but likewise from the pleasure which he seems to have had in regarding the following passage of Mr Locke, as a tacit concession in his favour. "That we cannot discourse and reason about them (the several ways whereby we come by ideas) farther than we conceive, is a great truth; and it would be well if we would not, but would ingenuously own the shortness of our sight where we do not see. To say there can be no other, because we conceive no other, does not, I confess, much instruct. And if I would say that it is possible God has made our souls so, and so united them to our bodies, that upon certain motions made in our bodies by external objects, the soul should have such and such perceptions or ideas, though in a way inconceivable to us, this perhaps would appear as true and as instructive a proposition as what is so positively laid down *." Notwithstanding his predilection for the system of Leibnitz, Haller seems to have caught the true spirit of Mr

* Leibnitz, Examen du Sentiment du Pere Malebranche, Œuvres Philosophiques, Amsterdam et Leipsic, 1765, p. 501. Locke's Examination of P. Malebranche's Opinion, &c. Works, Lond. 1812, vol. ix. p. 215.

Locke's philosophy in an observation, the truth of which cannot be too strongly impressed upon the minds of those who engage in psychological speculations. "Those," he remarks, "have behaved modestly, who, confessing themselves ignorant as to the manner in which the body and mind are united, have contented themselves with proceeding no farther than the known laws which the Creator himself has prescribed, without inventing and supplying us with conjectures unsupported by experience *."

In the account which Haller has given of the process of perception, he states distinctly that "Thought is not the express image of the object by which the sentient nerve is affected. The idea of redness has nothing in common with the slightly refrangible ray separated from the other portions of the whole ray; and far less is it consistent with optical principles, that an object painted by rays upon a soft white nerve, can be, in a long tract of perfect darkness, conveyed through a very opaque body to the origin of the optic thalami." —"Though a motion occasioned by a body that is perceived by sense, is propagated to the brain, the mind neither perceives this motion, nor the tremors of sounds, nor the percussions of the rays of light, but something perfectly distinct from these †."

The theory which Stahl had proposed with regard to the necessary agency of the mind in the production and regulation of the natural and vital functions of the

* Boerhaave, Institutions, § 27. Comment. Haller, Primæ Lineæ, sect. 556, 571.

† Primæ Lineæ, sect. 556.

human body, having been adopted in various parts of Europe by several of the more active and intelligent of those who were occupied in investigating the laws of the animal economy, Haller deemed it necessary to enter into a refutation of this theory; and he executed the undertaking in so able and complete a manner as to leave little, if any thing, to be accomplished by his successors. Our not being conscious of the mind's exerting any such influence over the organic functions, as that supposed by Stahl,—the impossibility of its attending at one and the same time to all the various trains of material phenomena which occur in the human economy,—and the performance of the organic functions with perfect regularity and full effect, in circumstances in which the rational soul is either very feeble or wholly deficient, as in infants, idiots, madmen, and brute animals,—Haller regarded as sufficient to disprove that dependence of the corporeal functions upon the agency of the mind, which had been supposed to exist by Stahl and his followers. The value of the brief and comprehensive view which he has given of this subject, in his *Primæ Lineæ*, can be properly estimated only by those who have perused the historical details and more enlarged discussion into which he has entered in his *Elementa Physiologiæ* *.

In adopting the opinion of Galen, that the brain, spinal marrow, and nerves, are the organs of sense and voluntary motion, Haller has, in the 4th volume of his Elements, adduced a great number of physiologi-

* Primæ Lineæ, sect. 572, seqq. Elementa Physiologiæ, Lib. iv. sect. v. § 11; Lib. xi, sect. iv. § 4.

cal and pathological facts to prove, that there are not any modifications of the faculties of sensation, memory, judgment, or volition, that are independent of the sound and morbid states of the nervous system, and more particularly of its encephalic portion. He has given a very full enumeration of the observations upon record, which tend to shew the connexion between organic alterations of the brain, and an impaired condition of the intellectual faculties, particularly of the memory. At the same time he seems to have been perfectly aware of the difficulty of ascertaining, from pathological observation, the existence of any very fixed relations between the kinds and degrees of the mental derangements, and the particular seat and extent of the cerebral disorganization.

The part of Haller's labours, however, which has thrown most light upon the functions of the animal economy, is that in which he investigated experimentally the respective powers of the Nervous and Muscular systems,—endeavoured to ascertain the various proportions in which these powers are possessed by the different textures and organs of the body, into the composition of which the nervous and muscular fibres more or less largely enter,—and attempted to point out the phenomena occurring in living animals that are referrible to these two primary vital powers, called into operation and modified in it by different physical and mental agencies.

Dr Boerhaave had affirmed that there is no part of the human body which is not sensible, and this opinion had been very generally received. Haller, however, in the course of his experiments, was led to adopt the

opinion of Galen, that the different parts of the body differ very much in the degree of sensibility which they possess, proportionally to the number of nerves they receive, and that there are some parts, such as bones, cartilages, ligaments, membranes, &c. into which few if any nerves can be traced, and which, therefore, are nearly if not altogether destitute of sensibility. Haller's experiments on sensibility related wholly to the degree of this power possessed by different parts of the body in their natural or healthy state; and notwithstanding the violent opposition that was made to his opinions on this subject by his contemporaries,—which probably proceeded in a great measure from their not sufficiently distinguishing, in their experiments, between those nerves that everywhere accompany the arteries in their distribution, and those which enter, distinct from the arteries, into the composition of different textures and organs,—his estimate of the sensibility of the different parts of the body is now very generally acknowledged to be correct. It was ascertained, however, in the course of the inquiry, and particularly by Dr Whytt, that some parts which, in their healthy state, are nearly or altogether destitute of sensibility, become exquisitely sensible when they pass into the state of inflammation.*

Galen, in his treatise on the Motion of Muscles, had considered these organs as being the instruments of voluntary motion only, without seeming to have

* Galen, de Motu Musculorum, lib i. cap. ii. Haller, Memoires sur les Parties Sensibles et Irritables du Corps Animal, Lausanne, 1756, Dissert. sur la Sensibilité, t. i. p. 3; t. iv. p. 153. Elem. Physiolog. lib. x. sect. vii, § 2–12.

been aware of the share which muscular fibres have in the performance of those functions which he denominated the Natural and Vital. By the limited view which he took of this subject, he was led even to deny that the heart is a muscle, from its motions being independent of the will. He states explicitly, that the muscles derive their principle of motion from the brain through the nerves; adducing, in proof of this opinion, the difference, in regard to the moving and sensitive powers, of the parts above and below the place where a nerve, or the spinal marrow, has been divided.

"All the muscles," says Galen, "have a communication, and that not an unimportant one, with the brain and spinal marrow; for they must receive, either from the brain or from the spinal marrow, a nerve, which, though small in appearance, is by no means inconsiderable in power. This is shewn by the effects of injury and disease; for when a nerve is divided, compressed, contused, included in a ligature, affected with scirrhus, or otherwise diseased, the muscle to which it goes is deprived of its powers of sense and motion. From inflammation of a nerve, many are attacked with convulsions and delirium, in whom these affections cease immediately on the nerve being divided; but the muscles to which the nerve had been distributed remain afterwards insensible and incapable of motion. There is certainly, therefore, some great power in the nerves, flowing into them from their primary origin; for they have it not of themselves, neither is it innate in them. This is most distinctly proved by the fact, that if you divide any of those nerves, or the spinal marrow itself, the part above the incision, and in continuity with the brain, will still retain the powers of its origin, but the part below will be incapable of producing either sense or motion. The nerves, therefore, like streams from a fountain, convey to the muscles their powers from the brain."

Galen had also observed, that when a muscle is divided in a living or recently killed animal, the divided portions retract towards their attached extremities; and that if one of two antagonist muscles be divided, the other enters into contraction. This species of muscular contraction he attributed to a natural power inherent in them, which he called Tonic Power, and the contraction itself Tonic Action or Energy, in order to distinguish it from that contraction which is produced in muscles by the influence of the mind, communicated to them through the nerves, and which he denominated their *psychical* or mental tone or energy. But Galen does not appear to have had any notion whatever of the possibility of the contraction of muscles being produced by the application of mechanical or chemical stimuli, either to their fibres directly, or to the nerves with which they are supplied *.

Glisson seems to have been the first physiologist who applied the term Irritability to the peculiar power of contraction possessed by muscular fibres. He was led to believe that the operation of this power must, in all instances, be preceded by the application of stimuli or irritants to the muscular fibres, either directly, or through the medium of the nervous system; and that these fibres are spontaneously relaxed when the irritation of the stimulus ceases. Muscular fibres may, according to Glisson, be set in motion in three different ways:— 1*st*, In consequence of the application to the fibres themselves, of external stimuli which do not occasion

* Galeni Opera Omnia, edit. a Kuhn Lipsiæ, vol. iv. p. 367. De Motu Musculorum, lib. i. cap. iii. p. 377–379; cap. i. 370; cap. viii. p. 404–5.

sensation; *2d*, In consequence of a sensation produced by an external agent through the medium of the nerves; and, *3d*, In consequence of an appetite, desire, or volition originating in the brain. The first source of muscular contraction he terms Natural Irritability, or *Constitutio Insita;* the second, Sensitive; and the third, Appetitive Irritability. As examples of the first power, or that in which stimuli operate on the muscles through natural perception, and altogether independently of sensation, Glisson mentions the pulsation of the heart, which, he says, is neither performed nor varied by sensation,—the tremulous motion which continues in the muscular flesh of animals for some time after decapitation,—the peristaltic motions observed in the intestines when the abdomen has been recently opened, and they still retain some degree of warmth,—the contractions occasioned in the muscles of animals recently killed by touching them with acrid and pungent fluids,—and the efforts to contract made by the hearts of some animals after they have been taken out of the body, and even when cut in pieces. "What need," he adds, "is there of more examples? From these, it may certainly be sufficiently inferred that the fibres may, without the aid of the senses, perceive irritation, and move themselves accordingly." Glisson has not attempted to give any illustration of irritability excited through sensitive perception or sensation. Indeed he expresses himself doubtful whether, in any instance, a sensation produces muscular contraction directly, or whether it does not primarily tend to the brain, and through that organ excite animal desire. With regard, again, to motions induced by animal ap-

petite or desire, Glisson remarks that this does not move the limbs directly, but through the medium of the nerves and the muscular fibres. " We do not, therefore," he observes, " call in question whether or not the fibres of the muscles be subjected to the dominion of the phantasy and of animal appetite, but we only doubt as to the manner in which the commands of the brain are conveyed to the muscles ; for we allow that the moving fibres of the muscles, when in their natural state, are always moved according to the pleasure of the animal appetite or desire ; but we observe that they may be immediately excited by other causes also, as is sufficiently proved by the convulsive motions and spasms, to which they are occasionally subject." He states, likewise, that muscular action may be either excessive, deficient, or in a middle state ; and these different conditions he refers partly to the different degrees of vigour and of irritability possessed by the muscular fibres, and partly to the different degrees of intensity of the stimuli. Deficient irritability he calls *Torpor*. Excessive irritability, he remarks, has no proper name, but it may be termed *Restless* or *Prurient* *. That the opinions of Glisson, relative to irritability, should not have attracted a greater share of notice than they appear to have done, from his contemporaries and immediate successors, is perhaps to be attributed to the obscure manner in which several of these opinions are expressed, and to their being involved in subtle metaphysical distinctions, which could

* Tractatus de Ventriculo et Intestinis, Tract. Posterior. cap. v. § 14 ; cap. vii. § 2 ; cap. viii. § 1 ; cap. ix. § 1. § 7

be understood only by those who had attentively perused his *Tractatus de Natura Substantiæ Energetica*,—a work in which he combats, with great ability and success, Descartes' hypothesis of the inactivity of matter, by a most singular union of original philosophical reflection and abstruse scholastic argumentation.

But though many of the phenomena proceeding from the contractile power of the muscular parts of the body had been observed by Galen, Glisson, Swammerdam, Steno, Harvey, Lower, and other physiologists previous to the time of Haller, to him certainly belongs the merit of having been the first who made these phenomena the subjects of particular and extensive experimental research, and who pointed out this property as distinct from all others in the animal economy. It is evident, from his own statement, that Haller had, at an early period, adopted the opinion, that the different motions which occur in the animal economy are dependent on a primary vital power peculiar to muscular fibre, by virtue of which this fibre contracts whenever a stimulus is applied to it, and again relaxes spontaneously. "In my Commentaries," he remarks, "upon the Institutions of Boerhaave, published in 1739, I have said that the heart is moved by some unknown cause, which depends neither upon the brain nor upon arteries, and which is concealed in the structure of the heart itself. The nature of the thing obliged me to abandon the opinion of my master. Three years afterwards (in the fourth volume of the same work, published in 1743), I announced that every muscular fibre when irritated contracts itself, that this character distinguishes it from the vegetable fibre, and that perpe-

tual irritation alone was the cause of the continuation of motion in the vital organs, after the animal organs had ceased to move. In my abridgment of the Physiology published in 1747, I have distinctly attributed the motion of the heart to the power of stimulus; and in the second edition (published in 1751) I have confirmed, with more precision, the irritability of the muscular fibre, and I have taught that it is independent of the nerves, and of every known property. Since that time, numerous experiments have been made confirmatory of the truths which I have advanced." "Irritability is different, both from elasticity and from the dead contraction common to all fibres: it seems to constitute a power which is quite peculiar to the muscular fibre, and characteristic of it, so that every muscular fibre may be said to be irritable; and, on the other hand, every thing which is irritable may be said to be muscular fibre. It is a power distinct from every other power, and to be referred to the sources of motion—of the ultimate cause of which we are ignorant *."

The property which the muscular fibre possesses of contracting upon the application of a stimulus, and by which it is distinguished from every other animal texture, Haller with Glisson denominated Irritability, or the *Vis Insita*. This property, he ascertained, can be brought into action in both the voluntary and involuntary muscles, by the direct application of stimuli to their fibres; and in the voluntary muscles, though

* Memoires sur les Parties Sensibles et Irritables du Corps Animal, Diss. sur l'Irritabilité, tom. i. p. 87. Elem. Physiol. lib. xi. sect. ii. § 11.

not in the involuntary, by the application of the same stimuli to the nerves with which they are supplied. The office of the nerves, to excite motions in the voluntary muscles, which Glisson had denominated Appetitive Irritability, Haller termed their Nervous Power. From the results of numerous and reiterated experiments, as well as from pathological observations, he was led to believe that this nervous power proceeds from the encephalon and spinal marrow through the nerves to the voluntary muscles, and that the irritability, or *vis insita*, is not derived from that source, but resides constantly and independently in the muscles themselves. "The will," he observes, "excites and puts a stop to the action of the nervous power, but has no influence over the *vis insita*. Hence it happens, that in apoplexies and palsies, the voluntary muscles become paralytic, in consequence of being deprived of the influx of nervous power from the brain, while the involuntary muscles of the vital organs, not requiring the same influence of the brain, continue to be excited to contraction by their appropriate stimuli,—the heart, for example, by the blood, and the intestines by air and aliments *."

The experiments made by Haller, which seem to afford the greatest degree of support to his opinion, that irritability, or the *vis insita*, is independent of the nervous system, are those in which he found that contractions are not excited in the heart and other involuntary muscles by the application of stimuli to the nerves with which they are supplied, though these

* Elem. Phys. lib. xi. sect. ii. § 4. § 15. Primæ Lineæ, § 404. & 410.

organs can be readily excited to contraction by the application of the same stimuli directly to their muscular fibres. "Nor have I seen," he observes, "that when the eighth pair of nerves, or any other nerve, was irritated, the motion of the heart was accelerated, the pulse became stronger, or the blood spouted out with more force." "Nor has irritation of the spinal marrow appeared to me to recall the motion of the heart." "Nor is it the will only that has no authority over the heart—there is none in the nerves themselves, by which alone the commands of the soul are conveyed to all the muscles; for when its nerves are irritated, the motion of the heart is not excited; when they are tied, it is not destroyed. It follows, therefore, that the heart is differently regulated from the voluntary muscles, since neither the will nor the nerves have any command over it."—"We possess accurate experiments with regard to the heart, but even the nerves of the mesentery when irritated produce no contraction in the intestines of a living animal; and we have no experiment that proves that the urinary bladder, or the stomach, empty themselves, when their nerves are irritated *."

Contemporary and succeeding physiologists, in attempting to determine whether the stimuli, which produce contractions in muscles, must in all instances act through the nerves, or may produce these motions by their direct action on the muscular fibres themselves, do not seem to have given sufficient attention to the very striking differences mentioned in the foregoing passages

* Mem. sur les Parties Sens. &c. tom. i. p. 390. Elem. Phys. lib. iv. sect. v. § 2; lib. xi. sect. iii. § 7.

by Haller, with regard to the effects which result from the action of stimuli, according as they are applied to the nerves of the *voluntary* or to those of the *involuntary* system of muscles. Indeed the doctrine of the dependence of the irritability of muscular fibre upon the nervous system, seems to have been inferred chiefly from the results of experiments made upon the system of voluntary muscles, in which mechanical, chemical, or galvanic stimuli are found to act as readily, for as great a length of time, and even more powerfully in producing contractions, when applied to their nerves, as when applied to the muscular fibres themselves. But in the system of involuntary muscles,—as was ascertained by Haller, Fontana, and Caldani, and has been more fully confirmed by experiments since their time,—the application of stimuli to their nerves, and the irritation of the brain and spinal marrow, have not been observed to be followed by any distinct contractions, though such contractions may at all times be very readily produced by the application of mechanical and chemical stimuli to the muscular fibres. Had physiologists, in entering on this inquiry, performed their experiments on the nerves of the involuntary, instead of making them on those of the voluntary muscles, it seems doubtful whether they could ever have been led to the conclusion, that the agency of the nervous system is, in all circumstances, essential to the production of muscular contraction; for, to suppose that when a stimulus is applied directly to any involuntary muscle, it acts only secondarily on the muscular fibres, through the intervention of the nervous filaments distributed among them, would be to take it

for granted, that these filaments are endowed with a power of receiving impressions from stimuli that is not possessed by the trunk of the nerve from which they are derived *.

But though Haller had observed that contraction cannot be produced in the heart, or other involuntary muscles, by the application of stimuli to their nervous trunks, he was fully aware that the action of these organs may be very materially modified by the different passions and emotions of the mind, and that the influence of these emotions must be conveyed from the brain, to the muscular organs, through the medium of the nerves. "The nerves of the involuntary muscles," he observes, "convey to them from the brain the influence, not of the will, but of those laws assigned to the animated body, in virtue of which certain motions are produced when certain stimuli are applied. In anger they convey to the heart violent emotions that give rise to palpitations—not in consequence of the commands of the will, but yet from the brain, and in consequence of a stimulus of a disagreeable nature presented to the mind, from which it desires to be freed as speedily as possible. The nerves, therefore, of the involuntary muscles, are media of communication between the functions of the soul and those parts of the body, although they do not convey the dictates of the will †."

Haller was led, by the results of his own observations and experimental researches, as well as by those of his disciples and followers, to believe, 1st, That ir-

* Mem. sur les Parties Sens. &c. tom. iii. pp. 213. & 470.

† Elem. Physiol. lib. xi, sect. iii. § 3.

ritability, or the *vis insita*, of the muscular fibre is a vital property, peculiar to the muscles, of a nature entirely different from the sensibility of the nervous fibre, and not derived from the nervous system. 2d, That the voluntary muscles possess, besides the *vis insita*, an additional contractile power, the *vis nervosa*; in consequence of which, they can be made to contract as readily by stimuli applied to their nerves, as when applied to the proper fibres themselves. 3d, That the involuntary muscular organs, which, in the living economy, are excited to action each by its own peculiar stimulus, can all be made to contract readily by the application of certain artificial stimuli directly to their fibres; but that distinct contractions cannot be produced in these organs, as in the voluntary muscles, by the application of stimuli to the nerves with which they are supplied *.

The general correctness of the opinions expressed in these fundamental propositions of the *Hallerian* doctrine of irritability, though strongly contested by a succession of zealous, and some of them very able, opponents, seems to be confirmed, in a remarkable degree, by the researches of those physiologists who have been more lately engaged in investigating the relations of the nervous and muscular systems, with the aid of galvanism—a test of irritability much more delicate than any which Haller, or his more immediate successors, had an opportunity of employing in their experiments. With the exception, perhaps, of his representing the *vis nervosa* as a property of the muscular in-

* Mem. sur les Parties Sens. &c. tom. i. pp. 237–255.

stead of the nervous fibre—an inadvertency which has been the source of an apparent inconsistency in his own opinions, and of much misconception in those of his opponents,—Haller's theory of Irritability may, in the present state of our knowledge, be regarded as complete; and his accurate and philosophical distinction between the irritability of the muscular, and the sensibility of the nervous fibre, must ever continue to form one of the most remarkable epochs in the history of the investigation of those functions which are immediately connected with, and indicative of, the existence of animal life.

The opinions of Haller respecting the Sensibility and Irritability of the different parts of the body, which were so new and indeed so contrary to the doctrines prevailing at the time in the different medical schools of Europe, called forth a host of opponents, some of whom endeavoured to refute his opinions by reasoning, and others by direct experiments. Among these opponents no one distinguished himself more by his ingenuity, knowledge, and talents for controversial discussion, than Dr WHYTT *. This eminent physician, who had studied medicine in the schools of Leyden, Paris, and London, was appointed to teach the Theory of Medicine in the University of Edinburgh in the same year in which Dr Cullen began to teach the Theory and Practice of Physic in Glasgow, and he continued to perform this duty, with great vigour of intellect, for a period of twenty years. In commencing

* Dr Whytt was born in 1714; appointed Professor in 1747, previous to which time he had officiated in the University for Dr Andrew Sinclair; and died in 1766.

to teach, Dr Whytt adopted, and continued ever afterwards to employ, Boerhaave's Institutions as a textbook in his lectures, though he gradually came to entertain opinions with regard to the animal economy, particularly respecting the causes of its vital and involuntary motions, inconsistent in various respects with the views which these Institutions exhibit. His opinions on the nature and causes of these motions were first published in 1751, in his celebrated Essay on the Vital and other Involuntary Motions of Animals; and were defended by him, in the controversy which he afterwards carried on with Haller relative to the sensibility and irritability of the different parts of man and other animals, in his Physiological Essays, published in 1755. In this controversy Dr Whytt had undoubtedly the merit of shewing that several parts of the body, which Haller regarded as insensible, ought to be considered as possessing sensibility in a very slight degree only, rather than as being altogether destitute of that property, since these parts often become exquisitely sensible in the state of disease.

Dr Whytt regarded the Nervous system as the peculiar seat of the Mind; which, with Perrault, he believed not to be confined to any particular region of this system, but to be equally present in the extremities of the nerves distributed throughout the whole body, as in the brain itself. In the nerves, he conceived the soul to be only capable of feeling, or simple sensation; whilst in the brain it exercises the power of reflex consciousness and reason *.

In the view which Dr Whytt took of the causes of

* Observations on Irritability, Works, p. 288. Note.

the different motions of the animal economy, he agreed with Stahl in regarding matter as a substance incapable, by any condition or arrangement of its particles, of commencing motion, or of performing any of the functions of life. He could not therefore agree with Glisson and Haller in regarding the irritability of the muscular fibre as capable of being called into action merely by the application to it of a physical stimulus. "It seems to be improper," he observes, "to attribute active powers to that which, however modified or arranged, is yet no more than a system of mere matter, powers which are not only confessedly beyond those of mechanism, but seemingly *contrary* to all the known properties of matter." In order that the application of stimuli to involuntary muscular organs should be followed by contraction, these stimuli, Dr Whytt imagined, must first affect the mind or sentient principle, and this principle, which, he remarks, "in a peculiar manner displays its powers in the brain, and by means of the nerves, moves, actuates, and enlivens the whole machine," must react on the muscles, by determining the influence of the nerves into them more strongly than usual; so that, according to his view of muscular action, the *involuntary* motions of the animal economy are, as well as the *voluntary*, produced by the intervention of the mind. "Such," he remarks, "is the constitution of our frame and the law of that wonderful union between the soul and the body, that the former, upon any uneasy perception, produces such motions and changes in the latter as naturally and most effectually tend to lessen or expel the irritating cause." Dr Whytt did not, any more than Stahl, think

it necessary, to the establishment of this doctrine, that we should be conscious of the intervention of the mind in producing involuntary motions, for the mind, he said, may be influenced to continue its actions without our being conscious of it, and even contrary to the will*.

In attributing the contractions of stimulated muscles to the energy of an active sentient principle, Dr Whytt stated that he did not consider the involuntary motions in man to be owing to a principle distinct from the rational soul; but that, on the contrary, he considered the sentient and rational souls to be one and the same principle, acting in different capacities. "There is in man," he says, "one sentient and intelligent principle, which is equally the source of life, sense, and motion, as of reason, and which exerts more or less of its power and influence as the different circumstances of the several organs actuated by it may require." He considered the mind to act, in producing the voluntary motions, both as a sentient and rational agent; but he supposed that, in producing the involuntary motions, without our being conscious of its interference, the mind is not influenced by intelligent or rational views, as had been supposed by Stahl, but merely by the stimuli affecting the several organs: and that in these motions, consequently, the mind is an involuntary agent, acting necessarily and without consciousness of intention, or, in other words, merely as a sentient, not as a rational principle—acknowledging that, if reason were concerned in the production of these motions, we could not fail to be con-

* On the Vital and other Involuntary Motions, Works, pp. 127, 128, 150, 153, 156.

scious of it. From this statement, it is obvious that while Dr Whytt denied the separate existence of a sentient soul, he contended for the separate agency of the sentient faculty of the soul in the production of involuntary motions *.

The absurdity of supposing that the soul should carry on a number of organic operations requiring the exercise of reason, without its being conscious of doing so, Dr Whytt has ridiculed very freely and happily in his observations on the *Prælectio de Anima Medica* of Dr Nicholls. "To imagine," says he, "that the soul should, with the wisest views and in the most skilful manner, at first form the body—a work far above the efforts of human art and contrivance!—and afterwards, when it is disordered, should, with the same skill and wisdom, often remedy the evil, and restore it to a sound state; but finding it in the end, or sometimes suspecting it only, to be no longer tenable or comfortable, should, instead of repairing, either whimsically or wisely desert it; to conceive, I say, of the soul as performing all this, without in the mean time being conscious of such intentions, or of the exertions of its power in pursuance of them, is at least as fanciful as to suppose that an architect might raise a stately edifice, in which nothing should be wanting that could contribute either to its usefulness or ornament; that he might frequently repair such damages as it sustains from the weather, or from the decay of any of its materials, and at last, apprehending it to be in danger of falling, might abandon it, without being con-

* On the Vital and other Involuntary Motions, Works, pp. 148, 152, 169.

scious of ever having once exercised either his skill in contriving, erecting, and repairing it, or his prudence in quitting it, when, as he thought, it was ready to bury him in its ruins." Yet, notwithstanding this ridicule of the doctrine of Stahl, Dr Whytt, in explaining his ideas with regard to the agency of the mind in the production of the involuntary motions, found it difficult to avoid relapsing occasionally into the language of that author. Thus, in describing agreeable or disagreeable sensations as the immediate causes of the mind's reaction upon organs to which stimuli are applied, he almost invariably speaks of the mind being *determined* by an ungrateful sensation or stimulus to exert its power in bringing about motions; and of "the soul endeavouring by all means, and in the most effectual manner, to avoid or get rid of every disagreeable sensation." In general, however, he endeavours, by the introduction of some such phrase as, "without reasoning," or "without ratiocination," to establish a distinction between his own doctrine of the sentient soul and that of the rational soul of Stahl and his stricter followers; a distinction which has been happily enough expressed, by applying to the doctrine of Stahl the term *Animism*, and to that of Whytt, *Semi-Animism*. That a physician so acute and skilful in reasoning as Dr Whytt undoubtedly was, should not have perceived that the use which he made of the unconscious agency of the sentient soul is liable to all the objections that had been so justly and successfully urged, by Haller and others, against the use which Stahl had made of the unconscious operation of the rational soul in his explanation of the phenomena

of the animal economy, can now be only matter of surprise to those who are capable of estimating duly the value of Dr Whytt's physiological labours *.

In conformity with his general theory, that muscular motions can be produced only through the intervention of the sentient soul, Dr Whytt did not hesitate to attribute the motions which can be artificially excited in the different parts of animals after they have been separated from the bodies to which they belonged, to the presence of that principle in them, and to its continuing to act for a longer or shorter period in these separated parts. He conceived that the opinion of the soul being present in every part of the nervous system of animals during life, and of its continuance for a time in parts which have been separated, do not afford any presumption in favour of materialism; for "may not," he asks, "the soul be extended without being divisible, and may it not continue to act in the separated parts of animals, and yet be only one mind?"—"Upon the whole, it appears certain," he observes, "that, after death, or an entire stop of all motion in the bodies of animals, the soul still remains present with them, and can be again brought to exert its influence, by various kinds of stimuli applied to their different parts. May not, then, the same principle continue present with the several muscles after they are separated from the body, and be the cause of their motions when irritated?"—"If the soul, without extension, be present at one and the same time in different places of the brain; and if, in many animals, it

* On the Vital and other Involuntary Motions, Works, pp. 146, 147, 162, 138.

can act along the spinal marrow for a great while after the head is cut off, why may not it also actuate parts separated from the body, without being extended? On the other hand, if we allow the soul to occupy space, I do not see why it may not continue to be present with the parts of the body after they are separated, as well as when they were united. And, with respect to the divisibility of the soul, which is generally thought to follow from the supposition of its being extended; why may it not be a substance so perfectly and essentially one, as that a separation or division of its parts would necessarily infer a destruction of its essence? Further, if the soul can be present in all or in any considerable part of the body at one and the same time, without being discerptible, its sphere of existence being so much increased as to act upon the parts, when separated, will not infer its divisibility*." How far these opinions are correct, or are capable of being reconciled with those maintained by the celebrated Dr Clarke, in his controversy with Mr Collins, which Dr Whytt professes to adopt, and whether the love of physical truth, which is every where apparent in this author's writings, and the respect he at the same time entertained for established metaphysical dogmata, have not sometimes involved him in inextricable difficulties, it may not be easy to determine. In reference to these opinions of Dr Whytt, Haller has made the following reflections:—" The soul is that being which feels itself, which represents to itself its body, and by means of its body, the whole universe of things. I am

* On the Vital and other Involuntary Motions, Works, pp. 183, 200, seqq. Observations on Irritability, Works, p. 287. seqq.

myself, and not another, because that which is called *I* experiences changes in all the variations which occur to the body which that *I* calls his own. If there be a muscle, or an intestine, the changes of which produce an impression on a different soul than mine, and not on mine, the soul of that muscle is not mine, it does not belong to me. But a finger cut from my body, a bit of flesh taken from my limb, has no connexion with me. I do not feel any of its changes; they do not produce in me any idea or sensation. It is not, therefore, inhabited by my soul, nor by any of the parts of that soul. If it were, I should feel its changes. I am not in that limb; it is entirely separate, both from my soul, and from that of all other men. Its amputation has not occasioned the smallest injury to my volition; this remains quite entire. My soul has lost nothing of its powers, but it has no longer any command over that limb, and yet that limb continues to be irritable. Irritability, therefore, is independent of the soul and of volition."

In opposition to the opinion of irritability being a property of the muscular fibre, independent of the nervous system, and regulated by laws peculiar to itself, to which Haller had been led by his observations and experiments, Dr Whytt endeavoured to shew that the motions of stimulated muscles proceed from their sensibility, or are closely connected with it. In proof of this hypothesis, he remarked,—1*st*, That we almost always observe the irritability of the muscles, or muscular organs of the body, to bear a proportion to their sensibility; 2*d*, That whatever increases the sensibility of the muscles, or moving organs of the body,

also increases their irritability; *3d*, That whatever lessens or destroys the sensibility of the muscles of animals, also lessens or destroys their irritability; *4th*, That, when a cold-blooded animal is pricked with the point of a knife, some time after being deprived of its head, heart, and other viscera, it moves not only the muscles whose fibres are touched, but also the other muscles of its body which have no connexion with those that are stimulated; *5th*, and lastly, he remarked that It will appear probable that the motions of irritated muscles are owing to the sensation excited by the stimulus applied to them, if we consider that we are in fact conscious of many involuntary motions in our bodies proceeding from a particular sensation, either in the organs moved, or in some neighbouring part. In illustration of each of these very doubtful propositions, Dr Whytt adduced a number of well-known and curious phenomena; but many of them, it must be confessed, are not very relevant to the question at issue; and others are of so uncertain and equivocal a nature, as not to furnish any satisfactory data for its determination *.

In the introduction to his Treatise on Nervous Disorders, Dr Whytt directed his attention in a particular manner to the doctrine of Sympathy, with which, as well as with Sensation and the power of Motion, he conceived that we are endowed by means of the Nerves. The term Sympathy has been employed by metaphysicians and physiologists in two very different significations. By metaphysicians it has been used to denote our fellow-feeling with the passions of others, or the

* Observations on Irritability, Works, p. 295, seqq.

springing up in the mind of a bystander of emotions corresponding to what he imagines should be the sentiments of the sufferer on any occasion of grief or of joy. This is properly termed Mental Sympathy. By physiologists the term Sympathy is properly employed to denote certain sensations, motions, or other vital changes arising in the animal economy, suddenly and without the intervention of the will, from impressions made upon, or irritations excited in, parts distinct or at a distance from those in which these sensations, motions or changes, actually occur, this being what constitutes Corporeal Sympathy. Corporeal sympathy, Dr Whytt remarked, " is either *general*, and extended through the whole system, every sensible part of the body having a sympathy with the whole; or it is *particular*, and confined in a great measure to certain parts." Hippocrates seems to have been well aware of that general sympathy which Dr Whytt has described as extending through the whole system, and has expressed it in his celebrated maxim, *Confluxio una, conspiratio una, consentientia omnia;* and in various other passages of his writings *. That this general sympathy had not been lost sight of by medical men since the time of Hippocrates, has been shewn by Rega in his second chapter *De Consensu Partium generatim*, a chapter well deserving the attention of those who may wish to trace the origin of the modern doctrines of the susceptibility of constitutional affections from local impressions; or of the production of diseases by local *Irritations*. Rega has justly remarked, that the term Particular Sympathy, or consent

* De Alimentis. De Locis in Homine.

of parts, had generally been employed by medical men in a double signification, 1st, To denote a concurrence of several parts co-operating in the performance of some particular action; and, 2d, A certain co-suffering, or con-dolence of different parts, in consequence of which, when one part is morbidly affected, some different part is at the same time affected, either in the same or in some different but painful manner. To the former kind of sympathies Rega gave the name of *Consensus actionum;* they seem to comprehend the class of phenomena which Stahl designated by the term Synergia or Co-operation,—a term which has been revived by some late continental physiologists. To the morbid sympathies, again, Rega gave the name of *Consensus passionum;* and it appears to be this class of phenomena which medical men now generally wish to comprehend under the title of Sympathies. In conformity with this division of the particular sympathies existing between the several parts and organs of the body, Dr Whytt observed, that, "by means of sympathy many operations are carried on in a sound state; and pain, convulsive motions, and other morbid symptoms, are often produced in such parts as have no near connection with those that are immediately affected."

A very slight analysis of the various phenomena enumerated by Dr Whytt as examples of particular sympathies, would be sufficient to shew that many of these phenomena bear too little resemblance to one another to admit of being included under a general term or explained on one general principle. Some of them are examples of sympathies existing, not between different parts of the same body, but between different

animal systems, as when yawning, vomiting, or convulsive affections are induced by instinctive imitation. Some of them cannot be referred to the head of sympathy under any of the various significations of that term, being the effects of those peculiarities of individual constitution which have been denominated idiosyncracies, predispositions, and diatheses; such, for example, as "when one eye is affected with an inflammation, a cataract, or the *gutta serena*, the other being often soon after attacked with the same disease." Others of these phenomena are obviously effects of the influence of the mental emotions or of the appetites on the different functions of the body;—on the Natural functions, as when grief, vexation, or fear are said to lessen the secretion of the saliva, destroy the appetite, and sometimes occasion a diarrhœa, and the sight or smell of grateful food to occasion an uncommon flow of the saliva in a hungry person;—or on the Vital functions, as when it is said that the great consent between the brain and the heart appears from the sudden and remarkable effects of the passions on the latter. Many, again, are examples of motions performed in obedience to the will, but in which the interference of that faculty is apt to be overlooked in consequence of the rapidity of its determinations, or in consequence of the motions we perform being contrary to our usual inclinations, or even extremely disagreeable to us. Of this kind are, it is conceived, two of the motions mentioned by Dr Whytt as instances of sympathy, viz. our shutting both eyelids as often as any thing threatens to hurt either eye; and the convulsions into which, "not only the muscles of the legs, but those of the

whole body, may be thrown by tickling the soles of the feet." It is, likewise, to the head of Voluntary motions that many of those cases of motions denominated Sympathetic are to be referred, in which a sensation occurring in one part of the body has been supposed to occasion motions in a distinct part or organ; for though a sensation may act as the indirect cause of a muscular motion by giving rise to some other mental affection, capable of exciting muscular contraction, such as a volition or an emotion, there seems no reason to believe that a sensation acts in any instance of itself as the immediate cause of such a motion, without the interposition of a desire or an emotion. Whether some of the later writers upon sympathy have been more fortunate than Dr Whytt in enumerating the phenomena that ought to be included under the term Corporeal Sympathies, or in assigning definite characters, by which this class of phenomena may be distinguished from others bearing a more or less close degree of resemblance to them, appears to be very doubtful.

In endeavouring to discover some general principle or principles capable of accounting for the phenomena of Sympathy, medical men had been led to believe that sympathetic connexions exist principally between parts bearing a close resemblance to one another in their structure, or having a community of function, and between parts connected with one another by means of vessels and nerves, or situated in the vicinity of one another. Finding that the two former of these circumstances, resemblance of structure, and similarity of function, did not lead to any satisfactory explanation of the phenomena of Sympathy, attention was more particularly

directed to the two last of the circumstances mentioned, especially to the communication existing between parts, whether near to, or remote from, one another, through the medium of the nerves. It was soon observed, that there are four ways in which a connexion may be established between distinct or distant parts, through the medium of nerves; 1st, The same nerve may, in its course, pass through different parts; 2d, A nerve may send branches to different parts; 3d, Two or more nerves going to different parts, may arise from the same portion of the central part of the nervous system,—the brain and the spinal marrow; and, 4th, Two or more such nerves, though arising from different portions of the common sensory, may be connected with one another in their course by anastomosing branches. These different possible modes of nervous communication suggested to physiologists the further inquiry, whether, when a sensation, motion, or other vital action arises in some part of the body, in consequence of an impression made on some other distinct part, the impression must first be communicated from the part primarily affected, to the brain, and from thence be transmitted to the part in which the resulting sensation or action occurs; or whether the impression may pass directly from the part on which the external agent operates, to the part ultimately affected, without passing through the brain.

Dr Whytt extended his hypothesis of the necessity of the intervention of the sentient principle in the production of muscular motions, to those phenomena of Sympathy, in which organs are put in motion by the application of stimuli to remote parts. To account for these phenomena by connexions existing between dif-

ferent parts of the body, through the medium of nerves, appeared to him to savour too much of Mechanism, or in other words, of Organization; and, accordingly, he maintained that the sentient principle is the only channel through which impressions made on one part of the body can occasion muscular contractions in a distant part. Regarding the nervous system as the organ through which the sentient principle acts, he of necessity allowed to this system an important share of influence in the production of sympathetic motions; but he did not deem it necessary for the explanation of Sympathy, that it should be possible to trace in the brain any anatomical connexion between the nerves of the organ to which the stimulus is applied, and those of the part of the body in which the resulting action takes place. The agency of the sentient principle of the soul he considered to be sufficient, independently of such a connexion, to account for the mutual influence of different parts of the body upon one another. "All sympathy or consent," he observes, "supposes feeling; and therefore must be owing to the nerves, which are the sole instruments of sensation."—"If the various instances of sympathy cannot be accounted for from any union or anastomosis of the nerves, in their way from the brain to the several organs; and, if there are many remarkable instances of consent between parts whose nerves have no connexion at all, it follows that all sympathy must be referred to the brain itself and spinal marrow, the source of all the nerves."—"If it should be objected, that it is as difficult to account for a sympathy between the nerves, at their origin in the brain, as in their course to the several parts where they

happen to be connected; I answer," says Dr Whytt, "that the purpose of these observations is not to explain how the different parts of the body can be endowed, by means of the nerves, either with a sentient or a sympathetic power; but, to endeavour to trace the sympathy of the nerves to its true source, which I take to be the brain and spinal marrow. It would be in vain to inquire further into this matter, unless we knew the minute structure and connections of the several parts of the brain, and were better acquainted with the laws of union between the body and soul, to whose sentient power the sympathy of the nerves at their origin, must be at last referred. For, if consent or sympathy supposes feeling, and if feeling cannot, any more than intelligence, be a property of matter, however modified, it must follow that sympathy depends upon a principle that is not mechanical, and that to suppose it may be owing solely to the particular situation, arrangement, or connexion of the medullary fibres of the brain, or to the union of the nerves proceeding from it, is as unreasonable as to imagine that thought may be the result of a motion among the particles of the animal spirits, or other subtile matter in the brain *."

However questionable and unsatisfactory Dr Whytt's doctrine of the interference of the unconscious sentient soul, in the production of the animal motions usually denominated Sympathetic, may be, still it would be unjust to his memory not to acknowledge that his writings have contributed powerfully to the progress of

* On the Vital and other Involuntary Motions, Works, p. 97, 98. On Nervous Disorders, Works, p. 510.

medical science; and a late re-perusal of them has only tended to confirm me in an opinion which I ventured to express some years ago in alluding to his excellent observations on Nervous Disorders; that, "notwithstanding any errors in theory which he may have committed, the writings of this author display an extensive acquaintance with the phenomena of health and disease, habits of accurate observation, and a talent for abstract reasoning and philosophical analysis not surpassed by any of those who have been employed in investigating the laws by which the economy of living systems is governed." It has been gratifying to me since to find that Dr Cullen entertained a similar opinion of Dr Whytt's merits. In his notes of a clinical lecture for the month of December 1765, in alluding to the progress which had been made in the investigation of the laws of the Nervous System, in the states of health and disease, and to the share which Dr Whytt had taken in it, he observes, "Helmont, Willis, Baglivi, Wepfer, Hoffmann, Haller, and Gaubius, have all done something, but Dr Whytt more than them all, though he has not exhausted the subject, nor removed every difficulty."

Dr CULLEN, in succeeding, in 1766, to the Chair that had been so ably filled by his colleague Dr Whytt, entered on the duty of teaching the Institutions of Medicine with a reputation which it required the fullest exertion of his talents to support. To an intimate acquaintance with the theoretical and practical writings of his predecessors and contemporaries, he was known to unite a vigorous and discriminating

understanding, matured by long-continued observation and experience, with the most felicitous powers of exciting the curiosity and directing the pursuits and studies of his pupils. The success with which he taught the Institutions of Medicine during the time he held that Chair, redounded so much to the honour of the University and to his own fame, that his friends and admirers had ultimately more reason to rejoice at, than to regret, the temporary disappointment he had experienced in not obtaining the Chair of the Practice of Physic, which had been so long the object of his ambition.

Dr Cullen arranged the different subjects of which he had occasion to treat in his lectures on the Institutions of Medicine, under the three general divisions of Physiology, Pathology, and Therapeutics, comprehending the consideration of Health, Disease and Remedy. The consideration of Hygiene and Semeiotics, or the general doctrines of the preservation of health and the signs of diseases, which, from the time of Galen, had usually been treated of as distinct branches of Medical Science, he introduced under Therapeutics and Pathology *. In explaining the general objects of the course on which he was about to enter, Dr Cullen observed, "The Practice of Physic, is the art applied to particular diseases and cases; but before considering this art in its application to particular diseases, certain General Doctrines are necessary to be premised which we call the Institutions of Medicine. Some are pleased to call our present course the Theory of Physic. With

* See Works, vol. i. p. 4.

regard to the common notion affixed to the term Theory, I must say that I mean to deliver nothing in this course but what is applicable to the cure of diseases, which is the ultimate end of all our studies: what we are to deliver in this course will be applicable to practice." "When gentlemen call this course a course of Theory, if they mean by that term, a deduction of reasoning, and that founded upon hypothesis, we refuse the appellation; but if by Theory, they mean the general doctrines of the art of Medicine founded upon experience and observation, I allow you to call this course the Theory of Physic." "My general doctrines are to be only so many general facts."

The definition which Dr Cullen has given of Physiology, in the 4th paragraph of his Institutions, appears to me to be one of the most accurate and comprehensive which has yet been proposed. "The Doctrine," he says, "which explains the conditions of the Body and of the Mind necessary to life and health, is called Physiology, or the doctrine of the Animal Economy." In pointing out the consideration of the phenomena of Mind as forming so important a part of Physiology, he seems to have thought some explanation necessary. "I must take notice," he remarks, "that to many of you I may appear to deal in Metaphysics; and I acknowledge that in so far as some analysis of the faculties of the human mind, some account of its general operations is to be so called, I must necessarily speak in the language of Metaphysics; and I think you will perceive that there is hardly any phenomenon of the Nervous system in which the operations of the mind are not more or less concerned; if, therefore,

you will call the consideration of these operations Metaphysics, their use is altogether unavoidable. I employ Metaphysics, because every physiologist has employed them; they have been employed to corrupt and destroy Physiology to a great degree, and they are not to be counteracted but by means of their own weapons.—I think that every part of the history of the human mind may be, upon one occasion or other, useful in Physic. The physician must at times be the moral philosopher also; and he will sometimes practise with little success, unless he can apply himself to the mind."

Dr Cullen divided the Physiological part of his course into seven sections,—The *first*, comprehending an account of the general facts known with regard to the Solid matter of which every organ of the body consists; the *second*, an account of the Nervous system; the *third*, of the Motion and Circulation of the Blood; the *fourth*, of the functions employed in supporting and repairing the several solid and fluid parts of the Body; the *fifth*, of the Organs employed in receiving and modifying the impressions of external bodies necessary to Sensation; the *sixth*, of the Motions of the several parts of the body which depend upon the Action of Muscles; and the *seventh*, of the Functions peculiar to the Sexes *.

The great number and variety of the facts and reasonings requiring to be stated and explained, relative to the different subjects that ought to be treated of in a Course of Lectures on Physiology, and the li-

* See Works, vol. i. p. 6. seqq.

mited period of time allotted for this purpose, rendered it impossible for Dr Cullen to go through each of these subjects in the way of detail, and obliged him to give only general views of some of them, and entirely to omit the consideration of others. Though he devoted nearly one-half of the winter session to the consideration of the Physiological part of the course, it does not appear that he had ever been able to overtake more of this than the subjects included under his first four sections. This defect he endeavoured to supply by referring his students to the *Primæ Lineæ* and *Elementa Physiologiæ* of Haller, which he justly regarded as works containing all that was known in Physiology at the time they were written. As a proof of the estimation in which Dr Cullen held the writings of Haller, it deserves to be mentioned, that, in 1766, the year in which he commenced his lectures on the Institutions of Medicine, a Latin edition of the *Primæ Lineæ* was published at Edinburgh under his inspection, the value of which was considerably increased by the addition of a copious, accurate, and well digested index, planned and executed by him, with the assistance of two of his pupils and friends, Dr Alexander Monro Drummond, and Dr John Fleming, late of the Medical Board of Calcutta.

In commencing his lectures on Physiology, after giving a short but comprehensive sketch of the History of Medicine *, Dr Cullen seems to have thought it necessary, in conformity with the doctrines which were then prevalent, to premise some general observations on

* Works, vol. i. p. 365 to p. 415.

the Simple Solids, and on the diseases to which they are liable. The ancients had distinguished the different solid parts of animal bodies into Simple or Similar, and Compound, Dissimilar, or Organic parts,—a distinction the same, or nearly the same, with that which is now made of the solid parts of the body into textures and organs. By Gaubius, the solid parts of the body had been distinguished into Simple and Vital; and Dr Cullen, as he himself informs us, in adopting these terms, included, under the simple or inanimate solids, all those textures and organs of the body, the physical properties of which are the same in the dead as in the living, and the same in the animated as in many inanimate bodies. His observations on the properties of the solid matter of which every organ of the body consists, certainly evince great knowledge and ingenuity; but the manner of considering this subject has of late years been so completely changed by the discovery of the constituent parts of animal matter, and by the more accurate examination of the chemical and physical properties of each of the solid parts composing the bodies of man and of other animals, as to render it now as unnecessary as it would be uninteresting to detail his particular opinions upon these subjects. It may be remarked, however, that he was aware that all animal matters may be ultimately traced to a vegetable origin; and he observes, that, if we would inquire into the production of animal matter, we must first inquire in what manner vegetable matter may be converted into animal. The justness of this observation, and the general accuracy of the statements respecting the animal Mixt, or animal matter, contained in the 10th

11th, 12th, and 13th paragraphs of his Physiology *, have been fully confirmed by the singular discoveries of Pneumatic Chemistry. But that the animal mixt is formed of carbon, hydrogen, oxygen, and nitrogen, and is capable of being resolved, in its ultimate analysis, into these principles, was a discovery reserved for the genius of M. Berthollet, and to which he was naturally enough led by the discoveries that Lavoisier and others had made with regard to the composition of vegetable matter. In the ingenious experiments by which M. Berthollet proved, that the addition of nitrogen to vegetable matter converts it into an animal substance, he may be justly said to have laid the foundation of Animal Chemistry.

After finishing his account of the Simple Solids, Dr Cullen proceeded to the consideration of the Nervous System. The investigation of the functions of this system, he regarded as constituting the fundamental and most important part of the study of the animal economy. "The animal economy," he remarks, "is a system consisting of many parts mutually necessary to one another, and to the subsistence of the whole; but I shall have occasion to shew, that there is some difference in the importance of these parts; that the most important part to the economy is the Brain, comprehending, as I do, under this term, the whole medullary substance within the cranium and vertebral canal; that upon the functions of this organ life depends, and that its energy, therefore, is necessary to the whole."

In combining the information relative to the Nervous System, contained in the writings of his contem-

* Works, vol. i. p. 11.

poraries, Haller, Whytt, and Gaubius, with the opinions which he had early adopted from the works of Hoffmann, Dr Cullen seems to have constantly endeavoured to extend, improve, and methodize his knowledge of the functions of this system; and, certainly, in no part of his labours do the powers of his mind appear to have been more successfully exerted. His speculations with regard to the different functions of the Nervous system, but more particularly with regard to that of the Animal Power or Energy of the Brain, were incorporated with every opinion which he taught concerning the phenomena of the animal economy, the causes of diseases, and the operation of medicines; and they may be said to constitute a most important part, if not the sole basis, of that system of the Practice of Physic, which he made the subject of prelection, as well as of study, for a period of nearly forty years before he ventured to give it to the public.

Dr Cullen divided the Nervous system into four parts:—1*st*, The Brain or Medullary substance contained in the cranium and vertebral cavity, "the whole of which," he says, "seems to consist of distinct fibres;" 2*d*, The Nerves; 3*d*, The Extremities of the Nerves distributed upon the different organs of sense,—these he calls the sentient extremities of the Nerves; and 4*th*, The Moving or Muscular fibres;—these, Dr Cullen was disposed to regard as a continuation of the medullary substance of the brain and nerves, and he therefore termed them the Moving extremities of the nerves. To each of these parts of the Nervous system Dr Cullen ascribed a distinct function. To the Sentient extremities, that of receiving the impressions of

external bodies, which, by motions conveyed along the nerves to the Brain, give rise to sensations; to the Brain, the susceptibility of those motions with which sensation and the consequent operations of thought are connected; to the Moving extremities, the faculty of contraction, and of having this contraction excited by an energy communicated to them from the Brain; and to the Nerves, the power of conveying motions from the sentient extremities to the Brain, and the commands of the will from the Brain to the moving extremities.

In mentioning the opinions of Boerhaave, with regard to different functions being assigned to different portions of the nervous system, it has been stated (p. 205–7) that several Anatomists, previously to his time, had been led by observations to entertain the belief that the nerves of sensation and of motion are connected with different parts of the common sensory. This opinion was greatly discouraged by the authority of Haller, who, though aware of the fact, that in paralytic affections motion is frequently destroyed while sensation remains, and that in other cases, sensation is lost while motion continues, thought that these occurrences might be explained without supposing that the nerves of sensation and motion are different. Dr Cullen supported the same opinion as Haller respecting the identity of the sensory and motory nerves; and maintained, that we must either allow these nerves to be the same, or suppose that there is nowhere a nerve of motion which is not accompanied with a nerve of sense that is inseparable from it; which he regarded as a gratuitous supposition. The opinions of Haller on

this subject were ably combated by M. Pouteau, in a memoir to be found in the second volume of his Posthumous Works, entitled, " Researches on the difference to be established between the Nerves of Sensation and the Nerves of Motion; suggested by some Observations on that rather uncommon species of Palsy, which deprives a limb of all sensation without depriving it of the use of motion." In this memoir M. Pouteau was led to conclude that the nerves of sensation arise in the cerebrum, and those of motion in the cerebellum, though in the spinal marrow, he observes, the two appear to be confounded, in running *pêle mêle* alongside of each other. " The confusion of the peduncles of the cerebrum and cerebellum in forming the medulla oblongata is, however," he remarks, " only apparent. The feebleness of our vision does not allow us to trace them further; for we must suppose that all that is in common to the nervous filaments coming from these two sources, the cerebrum and the cerebellum, is their being enclosed in common sheaths, furnished by the dura and by the pia mater, and their contributing more or less to the formation of every visible nerve." In speaking of the portio dura and portio mollis of the seventh pair as two nerves that animate the ear, and both of which come nearly from the same origin, viz. the medulla oblongata, where the nervous filaments furnished by the cerebrum and by the cerebellum are confounded, M. Pouteau remarks, " In all other parts Nature has always bound, under common sheaths, the filaments furnished in different proportions by the cerebrum and by the cerebellum; here, on the contrary, they are separate in all animals."

It appears from the statement of M. Pouteau that M. Lecat had previously adopted the distinction between the nerves of sensation and of motion; and in the 44th volume of the Journal de Medécine for 1775, there is a paper by M. Morin, in which it is remarked, that "our observing in palsy the diseased part preserve its sensibility without mobility, and *vice versa*, proves that there are two different species of nerves." The anonymous editor of the "Clinical Lectures delivered by Dr Cullen during the years 1765–66," also remarks in a note, "It is surprising that, when the nerves that go off together from the sensorium are the cause of both sensation and motion in a muscle, yet the one should be destroyed and the other remain entire; this affords a proof that these nerves are distinct, even in the sensorium *."

These incidental notices of the opinions of preceding authors, like the quotations from Prochaska referred to by Sir Charles Bell in the last edition of his work on the Nervous System, shew "how much importance may be now given to passages which heretofore had little interest attached to them;" while they in no degree detract from the singular merit of the physiologist who first established by experiment the diversity of the sensory and motory nerves, and who has applied this important discovery with so much ingenuity and precision to the explanation of many phenomena of the animal economy in the states of injury and disease.

Though Dr Cullen rejected the hypotheses of the

* Haller, Primæ Lineæ, § 384. Elementa Physiologiæ, lib. x. sect. viii. § 22. Cullen, Comment. on § 116. of his Physiology. Pouteau, Œuvres Posthumes, tom. ii. p. 480. Paris, 1783.

communication of motions through the different parts of the Nervous System by means of vibrations of the nerves, or of a fluid secreted in the Brain, he seems to have conceived with Hoffmann and others, that the agency of some subtile fluid in this system is necessary, in order to fit it for the communication of impressions from one part to another. This fluid he termed the *nervous fluid;* but without meaning, as he himself informs us, to determine any thing with regard to its source, nature, or manner of acting. "By nervous fluid," he says, "I mean nothing more, than that there is a condition of the nerves which fits them for the communication of motion."

It is difficult to comprise, in a narrow compass, the various and extensive views which Dr Cullen was accustomed to take of the functions of the Nervous system; but the general results of his speculations upon this subject may perhaps be comprehended under the following heads:—1*st*, Of the Nervous system considered as the connecting medium between the soul and body, or the Immaterial and the Material parts of man; —2*d*, Of the Nervous system considered as the organ of Sensation;—3*d*, As the organ of our Intellectual operations, Memory and Judgment;—4*th*, As the organ of the Voluntary, Involuntary, Mixed, and Sympathetic motions of the animal economy;—5*th*, Of the different conditions of the Nervous system in the states of Sleeping and Waking, and the doctrine of Excitement and Collapse;—6*th*, and lastly, The effects of Custom upon our corporeal and mental functions.

I. *The Nervous system considered as the connect-*

ing medium between the Immaterial and the Material parts of man.—In considering Mind and Matter as respectively endowed with qualities essentially different, Dr Cullen regarded the Nervous system as the connecting medium between the mind, or immaterial thinking part of man, and the different organs of which his body is composed; and believed, that certain motions or unknown changes excited in this system give occasion to thought, and that thought, however occasioned, produces new motions or changes in the Nervous system; or, in other words, that certain changes in the state of the Nervous system, and certain affections of the mind, are, during life, reciprocally the invariable antecedents and consequents, or causes and effects, of each other. "This mutual communication or influence," he remarks, "we assume with confidence as a fact; but the mode of it we do not understand nor pretend to explain; and, therefore, we are not bound to obviate the difficulties that attend any of the suppositions which have been made concerning it." In expressing his opinions upon these subjects, Dr Cullen was particularly careful to guard himself from the charge of materialism;—a charge which has been so often rashly and ignorantly preferred against those who have been engaged in tracing the order of succession of the material and mental phenomena of the animal economy, and the reciprocal influences established by Nature between them.

"With regard to the question," he observes, "whether our intellectual part depends on a certain constitution of matter, or upon an immaterial substance connected with our material part, we are of the latter opinion, and think it fully proved, by observing, that there are laws of the animal eco-

nomy absolutely incompatible with the ordinary powers of matter and motion. But, on this subject, I refer you entirely to Dr Whytt, who, in his book on the Vital and Involuntary motions of Animals, has put the matter beyond all doubt. I will only add one argument, which is, that the association of ideas, a very fundamental part of all our intellectual operations, cannot be explained upon any supposed organization of matter." "The existence of a soul being admitted, we must next inquire into the circumstances of its operating in the system. With respect to this, we find that the notion of the soul's governing the whole animal functions is very ancient, as we meet with it in the Timæus of Plato. It has since been revived, in different shapes, by Helmont, Wepfer, and Dolæus, and especially by Stahl, who plainly maintains, that the soul, as in a great measure independent of the body, and as in itself a rational being, presides over and directs most of the animal functions. In this he has been followed by many physicians in Germany, by Dr Nichols in England, as appears from his dissertation, entitled *Anima Medica*; and our countryman Dr Porterfield has also shewn a strong bias to the same opinion. We cannot, however, join in this opinion with these respectable persons, as we neither find that it is founded in truth, nor that it can have any good influence on the theory or practice of physic. Although the soul is certainly a distinct substance from the body, it appears highly probable that the soul, while connected with the body, seldom acts, but in consequence of motions first excited in the latter; and that it is generally true, that "*Nihil est in intellectu, quod non prius fuerit in sensu.*" Even our reflex sensations are always necessary consequences of previous impressions. The impossibility of a perpetual motion or automaton being demonstrated with respect to matter, is considered as a proof that the seeming automaton in a living animal must depend upon a soul animating all its motions. That the soul is constantly necessary to the mo-

tions of the body we readily admit; but the argument is pushed too far, when it is supposed that all of these motions are supported by the power of the soul alone; for it appears, that motions excited by the impulse of external bodies are absolutely necessary to that support. Take away all impressions on the external senses, and most persons will fall asleep, and that sleep would probably continue and end in death, were it not for impressions being renewed. The power of the soul appears, especially in what are called the *voluntary* motions; but these, properly considered, do not show that power exerted in an arbitrary manner, or with any attention directed to the motions of the body. The soul is indeed a necessary part of these motions, but it cannot be said to direct and govern them independently of the conditions of the body. This is still more clear with respect to the internal and what are called *involuntary* motions, with which physic is more especially concerned. The Stahlians would have us believe that the increased action of the heart and arteries which appears in fevers, does not arise from the physical powers of the causes of fevers, but from hence, that the soul, perceiving the noxious tendency of some matters present in the system, excites the action of the heart and arteries as a means of expelling such noxious matters. This cannot be proved; and it appears to us more probable that, analogous to a general law of the system, the material causes of fevers have a physical power of exciting those motions that are fitted to expel them out of the body. In short, whatever share the soul may have in supporting the motions of the body, we can have no art of physic, but in so far as we suppose that the causes operating upon the body act by a physical necessity; and that, by a knowledge of such causes, we can produce certain changes in the state of the matter and organization of the body."

I have introduced into the first volume of Dr Cullen's Works, pp. 18. and 103, his commentaries upon

the 31st and 117th paragraphs of the Physiology, as expressing his opinions with regard to the mutual relations of the mind and body, and as affording a specimen of his general manner of lecturing upon physiological subjects, as well as a proof of the great pains which he took to make his pupils acquainted with the opinions of other teachers.

II. *The Nervous System as the Organ of Sensation.*—Dr Cullen agreed with Boerhaave in believing that the brain, and not the nervous system generally, as Perrault and Whytt had supposed, is the immediate organ of the mind in the function of sensation.

"The Stahlians and some of the physiologists," he remarks, "have taken a fancy that the soul has no seat, no particular part of the body with which it is connected more immediately than with the rest; they conceive that it is coextensive with the nervous system, perceives in the organs of sense, independently of any communication with the brain, and operates in the muscles without any motions being propagated to them from the brain. But in saying this, they cannot but be stumbled by the fact, that if a ligature or compression be applied to a nerve of sense, no sensation will be produced by any application between the ligature or compression, and the sentient extremity of the nerve; though, if we can go beyond the ligature, and apply a puncture between it and the brain, this will occasion a sensation. Since, therefore, the impulse or impression of bodies on the sentient extremities of a nerve does not occasion sensation, unless the nerve between the sentient extremity and the brain be free, we conclude that sensation, so far as it is connected with corporeal motions, is a function of the brain alone; and we presume that sensation arises only in consequence of external impulse producing motion in the sentient extremities of the

nerves, and of that motion being thence propagated along the nerves to the brain."

According to Dr Cullen, our Sensations may be considered as of two kinds: 1st, Those which arise from the impulse or impression of external bodies, which he therefore names *Sensations of Impression.* 2d, Those which arise from the mind's being conscious of its own actions, and of the motions which it excites; and these he names *Sensations of Consciousness.* In enumerating the sensations of Impression, Dr Cullen distinguished those of Sight, Hearing, Smell, and Taste, from those of Touch; each of the first four senses constituting a genus, the particular sensations of which, though various, possess something in common, arise from impressions made upon one part of the body only, and are produced by the action of one kind of external bodies, or rather by the action of one and the same quality, as by that of light, tremor, volatility or solubility. No such characters, he remarks, concur in establishing one genus of the sensations referred to Touch. These, therefore, he attempted to distinguish into different genera, such as sensations from mechanical impulse—from chemical acrimony—and from heat and cold.

The term *Sensations of Consciousness* was, as Dr Cullen remarks, a new term in physiology; and it must be allowed that some such term was necessary, in order to comprehend under it a variety of feelings of the mind of which we are conscious, but which we are not able to trace to the impression of bodies external to the Nervous system. "It is first asserted by Boerhaave," he observes, "and in a great measure by Gaubius,

that there is no mental operation whatever that has not some state of the body corresponding to it; and that our sensations are merely reflections of some changes that go on in our corporeal parts,—so that sensations of consciousness should not be separated from those of impression. But I hold the general proposition to be doubtful; and whatever may be in it, it is certain that, in the examples which I am to give of sensations of Consciousness, we neither perceive nor are conscious of any such changes in the state of the body; so that we must take them as they appear in the mind only."

"The sensations of consciousness," Dr Cullen remarks, "may be referred to the following heads:—1st, Those of Apperception, by which we are in general conscious of perceiving, remembering, judging, and willing, and thereby of our existence and identity; 2d, The sensations arising from the particular state of thinking, according as perception, memory, and judgment are more or less clear, ready, or exact; 3d, The sensations arising from the particular state of volition, and its various modes; 4th, The sensations arising from the general state of muscular action, as vigorous or weak, easy or difficult. An example of this," he observes, "is, the sense of alacrity and of weight in moving the limbs, also the sense of lassitude. I find that I cannot move my limbs with the same force as I used to do. I find that I cannot continue their action so long as I have been able to do. I find a sense of weight and uneasiness in endeavouring to do it. This, it is plain, is merely a sense of an affection of the mind, and without any thing that can be called external impression; 5th, The sensations arising from particular actions, or a consciousness of the actions excited, and of the motion of the different parts of the body. These also probably take place with regard to all the internal functions. The action of the heart and arteries, for instance, is in gene-

ral obscurely, but sometimes very clearly perceived, as is also the state of respiration, or the action of the lungs; and still more evidently the actions of the stomach and intestines, which we cannot consider, with any propriety, without having thus distinguished the sensations of consciousness. I own that this head will admit of some doubt, whether these sensations belong to those of consciousness or to those of impression. I am willing to allow that they are sensations of impression, in so far as the action of the muscles overstretches certain parts; but sometimes they are without any such impression, and we are conscious only of the degree to which our motions are exerted. Thus I judge of the distance of a body, which I measure by stretching out my arm to come into contact with it, which gives only the consciousness of the degree of motion excited; but I am not conscious of the agency of the several muscles which are put into motion to produce this action. I am sensible of this only when these muscles are exerted with some unusual violence, or with spasmodic contraction. Internal motions also may come to be perceived when they are performed with a spasmodic force. Thus we take no notice of the action of the heart, unless when any cause excites it in an unusual manner, which we call a palpitation; or, when there is an interruption or intermission of the pulse, which many persons can mark in the heart. And so with respect to respiration, the usual train of which we are not conscious of willing or directing; but when it is performed with unusual frequency or force, we are.—6th, The sensations arising from the diminution or absence of impressions. I will not say that darkness is visible, but it is a positive sensation. The sensation of darkness or blackness is the absence of impression: it takes place when no rays of light fall on our eyes, so that the sensation of consciousness may be here separated from that of impression."

It is evident from the 40th and 61st paragraphs of

his Physiology, that Dr Cullen occasionally employed the word Sensation as a general term, synonymous with feeling of the mind; and, accordingly, in the 62d paragraph, where he says, "We are disposed to combine our sensations as united in one object, and thus form what are called complex ideas," he uses the word Sensation as synonymous with the term Conception; and, again, in the 63d paragraph, "We compare our several sensations, and from thence acquire new sensations of relation," he uses the word sensation as synonymous with the act of judging, or with what Hobbes and Malebranche formerly, and more recently M. Destutt Tracy and Dr Thomas Brown, have denominated a feeling of relation. Though it may be doubted how far our consciousness of perceiving, of remembering, of judging, and of willing, and of the greater or less facility with which these mental operations are performed, can with propriety receive the appellation of Sensations, which Dr Cullen has given to them, there can be no question that we ought to regard as sensations the bodily feelings which arise from the various emotions and passions of the mind, as well as those that accompany the various motions arising from volition, which Dr Cullen justly remarks is a very fruitful source of sensations;—feelings the more deserving of our attention, that they appear to be the natural means or signs by which we are made acquainted with the magnitude, figure, and distance of external bodies.

In tracing the relations of the corporeal and mental phenomena of sensation, Dr Cullen appears to have been particularly desirous to ascertain and to point out the general circumstances or laws of that process;—an

attempt which had been previously made by Boerhaave, in the 575th, 6th, 7th, and 8th paragraphs of his Institutions;—and he has certainly succeeded in stating the most important of these laws with greater brevity, perspicuity, and accuracy, than had been done by any preceding physiologist. He states, that the different parts of the body are sensible only by means of the nerves present in them;—that particular sensations arise from impressions on certain parts only;—that no sensations arise originally in the mind, without a previous change in the state of the body;—that, in order to produce sensation, a certain limited force and a certain duration of impression are required;—that a certain degree of attention of the mind is necessary to give an impression its full effect;—that the mind seems to be determined to attention by the force of impression, by the pleasure or pain arising from it, by the degree of emotion or passion produced by these, and by the emotion's being more or less related to the person feeling;—that the mind can attend to one sensation only at one time, but that several impressions, producing sensations of the same genus, if they occur either simultaneously or in succession, may unite in producing a single sensation different from either of the separate sensations;—that sensations remain for some time after the external impressions that produced them have been withdrawn;—that the sensations, produced by particular impressions, are modified not only by the original constitution and accidental state of the organs of sense, but also by the different degrees of force with which the impressions act, and by the degree of impression to which the organ had

been immediately before accustomed;—that the same impression, after it has been often repeated, does not produce the same strength of sensation as at first;—that different sensations are accompanied with different judgments concerning the bodies making impression, and the part of the body upon which it is made;—that sensations are referred either, 1st, to the place where the impression is made, or, if this be an internal part to the incumbent external part; 2d, to a part sympathizing with the part where the impression is made; or, 3d, to the nervous extremities from whence the sensation was in use to arise;—that sensations are pleasant or painful according to the quality of the impression from which they arise, and the degree of its force;—that sensations, at first painful, may, by repetition, be changed into pleasant, and the pleasant into insipid and uneasy; and that impressions and sensations are often rendered the objects of desire or aversion, by combination, succession, and relation. Each of these laws of sensation Dr Cullen endeavoured to establish and illustrate in his lectures by numerous and apposite examples *.

III. *The Nervous System as the Organ of our Intellectual Operations.*—Of the intellectual faculties or internal senses, Dr Cullen took occasion to refer principally to those of Memory and Judgment †. Of Memory he points out three modifications, under the names of Reminiscence, Memory, and Imagination. By Reminiscence, he understands the renewal of sensations formerly experienced, by the same objects that

* Works, vol. i. pp. 31–49. † Works, vol. i. p. 46.

produced them, accompanied with the consciousness of their having been formerly perceived; by Memory, the renewal of perceptions formerly received, without the presence or action of the objects which had given occasion to them, attended with the consciousness of a difference between the vividness of the two perceptions, and particularly of the absence of the original objects; such a renewed perception, he says, is called an Idea; and, lastly, by Imagination he understands the renewal of perceptions formerly received, also without the presence of the original objects, but in such manner that the mind does not perceive any difference between the renewed and the original perception, so that such renewal is always attended with the persuasion of the presence of the object. "The term Imagination," he remarks, "has been very variously applied; and particularly to that mode of memory which renews ideas with more vivacity than we commonly experience in our remembrance of sensations. But it is necessary for us to confine the term, as we have done, to that renewal of perceptions, without the presence of the object, that is *equal* in its effects to what the presence of the object itself would give, as in the case of dreams and delirium."

Judgment Dr Cullen defines to be "that faculty by which we compare our several perceptions, and from thence acquire new sensations or feelings of relation." "When any two sensations," he observes, "immediately succeed one another—that is, are presented nearly together—we form a comparison of them, and perceive resemblance or difference between them, in various respects; hence a new feeling arises in the

mind, which is called a sensation of relation, or a perception of the judgment." Dr Cullen has pointed out, in different parts of his lectures, how much the exercise of memory is dependent on the associations that are formed between our different perceptions, between perceptions that are present and perceptions formerly experienced. "What are the various relations observed, which connect the perceptions of sense, of memory, and of judgment, I leave to the metaphysicians to determine. They may perhaps be referred to a few heads; but under each of these are comprehended a great number of particulars; and the slightest differences are sufficient to form a relation; and it is enough to our purpose to observe, that every relation marked forms an association for the purposes of memory. So that every sensation renewed by impression, does at the same time renew the idea or conception of a sensation formerly associated with it by some relation that has been marked; and this renewed idea is also capable of renewing others that had been associated with it in the same manner; and thus one single impression can renew a great number of ideas formerly received. This is the ordinary cause and operation of memory."

With respect to the mutual dependence of perception, memory, and judgment upon one another, and the circumstances upon which may depend the similarity or dissimilarity of the judgments formed upon the same subjects by different individuals in a sound state of mind, Dr Cullen gives the following explanation:

"The external perceptions of men are nearly similar; and for this reason, there is also a similarity in their internal per-

ceptions, or perceptions of relation. Simple internal perceptions and their relations are the materials on which the intellect is exercised. They are laid up in the mind by associations, and it is in following these associations that the mind brings back before it the relations which it is to judge of. But if the perception of relations is similar, so will the associations be in common, with the most part of men; and as the perceptions, relations, and associations, are founded on the nature of things, so the judgments of men will be similar, and in the exercise of this faculty they will follow the same trains of associations. Notwithstanding this similarity, there may be a great diversity in the judgments of men,—from want of some perceptions,—from some difference in perceptions, especially in such as are complex,—from a great difference in the number of relations marked, and their exactness,—and, lastly, from the number and variety of associations; whence a different state of the mind in different individuals, or in the same individuals at different times. But, at the same time, there are so many circumstances of human life in common to all men, that there must be so much similarity as to establish a common sense; that is, perceptions, relations, associations and judgments, in which all agree; and when any particular man differs from all others in these respects, we say he is not in his senses, but insane *."

That the Brain is the organ of the intellectual faculties, Dr Cullen conceived to be proved not only from our naturally referring the operations of memory and judgment to the Encephalon, but also from the effects that are produced upon these faculties by injuries, by temporary diseased states, and by permanent morbid alterations of that organ.

" Though there are," he observes, " cases of derangement of the intellectual faculties, cases even where a delirium or mania had subsisted for a great part of life, the causes of which we cannot find upon dissection, in any

* Works, vol. ii. pp. 510, 511.

change of the state of the brain, yet in ninety-nine instances of the hundred of such affections, we can perceive a change in the organization of the brain,—we can perceive causes that act there, and there only; and if in the remaining hundredth I cannot account for it, it will not disturb the conclusion. I am averse to establish universal propositions, but this, which is general enough, I insist upon, to prove that the brain is the Sensorium Commune, the seat of our intellectual faculties; or, if we use the language of an organ of the soul, that the brain is such an organ. We may perhaps be told that the brain may be taken away, and the intellectual faculties remain; and it certainly may be taken away entirely without immediately destroying vitality; we know that a bird will run the length of the room, and show some signs of a pretty lively vitality, after its brain has been removed. But in none of these cases is there any certainty that the intellectual faculties are not entirely destroyed; or if these remain after the brain has suffered any lesion, I say that it amounts only to a lesion of a certain extent. This certainly makes it difficult to say what portion of our brain is necessary to our intellectual faculties, for in several vitiated states of that organ they do continue, if the causes producing the lesion operate slowly; but if you affect the brain with a certain degree of pressure, you will at length destroy them. In hydrocephalus, though a fatuity has ensued, the senses have often remained to a surprising degree; but none of these instances amount to so much as to disturb the general conclusion, that the brain is necessary to sense, and that its organization, and the intellectual faculties, are immediately connected with one another."

In adopting, however, in common with most authors who have treated of the faculties of the human mind, the opinion that all the operations of thought are carried on in the brain, and are modificd by its various conditions, Dr Cullen was well aware of the difficulties

which present themselves to the physiologist, in attempting to ascertain what those particular parts and states of the brain are, upon which depend either the different degrees of intellect that different men are originally endowed with, or those various modifications of the intellectual faculties that are observed to occur in the same individual during the progress of life, from age or from disease. "Although," he remarks, "we cannot doubt that the operations of our intellect always depend upon some motions taking place in the brain, yet these motions have never been the objects of our senses, nor have we hitherto been able to perceive that any particular part of the brain has more concern in the operations of our intellect than any other. Nor have we attained any knowledge of what share the several parts of the brain have in that operation; and, therefore, in this situation of our science, it must be a very difficult matter to discover those parts and states of the brain that may give occasion to the various states of our intellectual functions." These remarks of Dr Cullen have obviously a reference to the attempts which had been made, at different times, to discover the more immediate seat of the soul, or Sensorium Commune, as it has been termed, and to the experimental researches undertaken with a view to this object by Haller and his disciples.

IV. *The Nervous System as the organ of the Voluntary, Involuntary, Mixed, and Sympathetic Motions of the animal economy.*—Dr Cullen agreed with Haller in regarding the muscular fibres as the organs of motion in all parts of the body; and though

he conceived them to be a portion of the nervous system, he was disposed to attribute the power of contractility which they possess, and which, he observes, is not possessed by any other portion of this system, to some peculiarity of their organization, without professing to explain in what this peculiarity consists, or on what physical change in the condition of muscular fibres, their contraction depends. He seems to have clearly perceived the fallacy of one of the arguments by which Haller's distinction between Sensibility and Irritability had been opposed, viz. that muscular fibres cannot contract unless they *feel* the stimuli applied to them, and that muscular contractility is therefore immediately dependent upon, if not the same power with, nervous sensibility. "Very generally indeed," he remarked, "the motions of the animal economy begin in sensation; but it is not necessary, as some have supposed, that they should always do so;. for there are powers which, when directly applied to the moving fibres, excite their action without any previous sensation being excited, or without any intervention of the brain; which appears clearly from hence, that the motion of moving fibres can be excited so long as the living principle subsists in them, though they are entirely separated from the rest of the body, and entirely, therefore, removed from all sense *." To express the constant tendency which muscular fibres have to contract, when the powers stretching them are taken off, Dr Cullen employed the term Tone, which had been originally applied to that tendency by Galen. "In respect of the causes by which it may be excited," he observes, "the

* Treatise on the Materia Medica, 1789, vol. ii. p. 131.

contractility of muscular fibres has been called Irritability; but there are two qualities of this property, which it is of importance to distinguish from each other, 1st, The *Force* with which the contraction of muscular fibres can be performed; and, 2d, The *Facility* with which it can be excited. These two circumstances are indeed variously combined in different cases; and, in many cases, it is extremely difficult to mark how far they are combined or separated. But we observe that the facility with which the contraction of muscular fibres can be excited, is often increased in the same proportion as the force with which they are contracted, is diminished; and, upon that ground, they ought to be distinguished by particular terms." These two states, which plainly correspond with what Glisson had termed the *Robur fibrarum* and the *differentiæ Irritabilitatis graduales*, Dr Cullen termed respectively the Vigour and the Mobility of muscular fibres.

Dr Cullen employed the Hallerian terms of *Vis Insita* and *Vis Nervosa*, not to signify, as Haller had done, two different powers of contractility, one possessed by muscle, in virtue of its own organization, and the other communicated to muscle by the nerves, in which it originates,—but to denote the two ways in which muscular irritability can be called into action,—by stimuli applied directly to the muscular substance itself, or by stimuli obviously acting through the medium of the nerves. "Dr Haller," he remarks, "has taken a partiality for a doctrine of which he thinks himself the author, viz. that the *vis nervea* and the *vis insita* of muscular fibres are totally distinct powers."

"Whether the contraction be excited by applications made to the muscles themselves, or to the nerves connected with them, the effects of such application are so exactly the same, as to allow us to conclude that the nervous and the inherent powers are somewhat of the same nature. A very ingenious gentleman, Dr Thomas Smith, a pupil of mine, has written an inaugural dissertation to demonstrate this very fact. He applied to muscles and to nerves every variety of powers, mechanical or chemical, which are supposed to act upon them. He employed a great variety of different substances in this way; and he could alternately, and in circumstances well suited for comparison, apply them to both nerves and muscles. The general result of his experiments is, that the same matter applied to the muscle or nerve, constantly produces either contractions or insensibility, and shews either stimulant or sedative powers; and this equally, whether applied to the nerve or to the muscle itself; and in no one instance did any diversity appear. The experiments were nicely executed, and indeed they required a great deal of nicety and attention *." These observations, it deserves to be remarked, Haller had ascertained to hold good only in the case of stimuli applied to the voluntary muscles and their nerves—the involuntary muscular organs not being excited to contraction by stimuli applied to the nerves with which they are supplied.

Though Dr Cullen acknowledged it to be possible that the property of irritability may, as Haller and others had believed, reside entirely in the muscular fibre itself, independently of any influence derived from

* Works, vol. i. pp. 67–70.

the nervous system, yet he maintained that, in the production of all the voluntary, involuntary, and mixed motions of the animal economy, the muscular fibre is always more or less influenced by an energy derived from the nervous system. "The vigour of the inherent or contractile power of the muscles," he observes, "cannot be evident in a living animal, because we cannot be certain of its acting entirely without the nervous power; and, with regard to the strength with which a muscle contracts, I can undertake to show that, in different conditions of the body, as it is weakened by disease or by various other causes, or as it is increased by powers which have that effect, this contraction will be perceived to be manifestly different; and I shall have many occasions to prove that the tone of the inherent power is increased or diminished by causes acting upon the brain, and upon the brain alone." To this influence of the nervous system upon the muscular, involuntary as well as voluntary, Dr Cullen gave the name of the "Animal Power or Energy of the Brain." "This term," he observes, "has not been common in our systems of physic; but if, in all the communications that take place between the different parts of the body, much depends upon the present state of the brain, we will not be in any difficulty in lodging a particular power there, of which we shall speak under the name of Animal Power, or Energy of the Brain." "This animal power we suppose to be seated in the brain, and only there seated. Whether you consider it, as Dr Whytt did, as a sentient principle, or as an automatic vital energy, depending upon the peculiar organization of all the several parts of the nervous system, it is

probably the fundamental part of the system, without which the functions cannot long remain." "It is highly probable that in the brain, as being the principal seat of the nervous system, to which all the other parts are in some measure united, there is a common centre of motion and power; from whence, in consequence of certain circumstances, the nervous fluid is determined with greater force, and perhaps in larger quantity, into some parts than into others. This is what I would term the Action or Energy of the Brain; and it is particularly evident in the operations of the nervous power in the case of voluntary motions."

Believing that the motions of the several parts of the body, whether voluntary, involuntary, mixed, or sympathetic, arise from the action of the brain upon the muscular system, Dr Cullen has enumerated under seven distinct heads, the causes by which that organ seems to be excited to produce these motions, viz. 1st, The Will; 2d, Emotions and Passions; 3d, The Principle of Imitation; 4th, Appetites; 5th, Propensities; 6th, Certain Internal Impressions occurring naturally in the economy; 7th and lastly, Various occasional impressions of external bodies, and various occasional states of the general system, or of its particular parts.

1. It is in the exercise of the *Will*, Dr Cullen remarks, that the power of the soul over the body chiefly appears.

"The organs of voluntary motion have this in common, that their actions are in consequence of a peculiar action of the brain, which excites them, while the vital functions need only that general energy which is constantly operating in the system. In consequence of their dependence on the ex-

ertion of the animal power, the organs of voluntary motion are subject to lassitude and debility, and require a state of rest and of sleep. Hence, also, various irritations of the brain exciting its action, are especially exerted in producing convulsive motions of the organs of voluntary motion; hence palsy takes place in the same organs, and many of their diseases are explained by this their peculiar connexion with the brain."

Of the voluntary motions, Dr Cullen made two divisions.

"Volition," he remarks, "arises in two ways; 1st, When sensations, arising either without or with very little reflex sensation, give occasion to the exercise of the judgment in marking their various relations, and their consequent fitness or unfitness for human affairs, they thereby give occasion to various desires, and therefore to volitions for producing those motions of the body that are suited to the ends desired. A second case of volition, excited and exciting, is, when the sensations, either altogether without or with very little intellectual operation attending them, produce those various modes of volition which we distinguish by the names of Appetites, Propensities, and Emotions or Passions." "The voluntary motions of the body," he remarked, at another place, "may be considered either as *Rational* or as *Instinctive*. They are said to be *rational* when, in consequence of a certain train of reasoning, one end or purpose is chosen in preference to another; and they are to be considered as *rational* also, when the means are selected that are best fitted to obtain the ends proposed. They are *instinctive*, again, in two respects; first, When they arise, without any reasoning, in consequence of sensations producing a desire, and have for their end merely the gratification of that desire; secondly, When, though they may be calculated to accomplish some end, they do not arise in consequence of the perception of that end. Every volition, therefore, arises

from direct sensation, or from the perception of relation. When the volition is from direct sensation, it is said to be *instinctive;* when it is from the perception of relation, and especially from a relation perceived by the intervention of many others, it is said to be *rational.* In most of the voluntary motions, we are conscious of willing the end proposed rather than the motions excited; and, of the motions produced, we are conscious chiefly of those of a whole member, or, in other words, of the general effect, and but very little of the many particular motions that concur to produce it. We are never conscious of the action of the particular muscles employed. In many cases of voluntary motion we have an end in view, and we likewise choose the means; but, in the motions of our body, by which we are to obtain the ends, there is a mixture of the instinctive, since they often follow from a conception of the end without our being conscious of willing the particular means. Custom enables us to perform motions with more or less agility, without our willing any more than the effect."

2. *Emotions and Passions.*—Dr Cullen agreed with Haller in dividing our sensations and ideas, according to the nature of the reflex feelings by which they are accompanied, into those the repetition of which we desire, and those to the repetition of which we are averse. These reflex feelings or peculiar qualities of sensations, when arising from other bodies, he named *agreeable* and *disagreeable;* when from our own body, *pleasant, painful,* and *uneasy.* The painful and uneasy, he conceived to differ from one another, in the former being always sensations of impression referred pretty accurately to a particular part of the body; whilst the uneasy are sensations of consciousness, and are either referred to the whole body, or to some considerable part of it—as to the head, thorax, or abdo-

men. Of this latter kind, he remarks, are the sensations of debility, lassitude, difficulty in exertion, and particularly the sensation which we term anxiety, and which we refer obscurely to internal parts. Without attempting any enumeration of the particular sensations belonging to each of these classes, Dr Cullen has shortly pointed out some of the circumstances upon which their agreeable or disagreeable, pleasant, painful, or uneasy character depends—such as the degree of force in the impressions producing them, their novelty, their peculiar qualities, and the associations that are connected with them. "Every reflex sensation being necessarily attended with desire or aversion, these two general modes of volition are accompanied," he remarks, "by different emotions and passions, according to the causes from which they arise, or the objects to which they are directed. When the mind is under the influence of the more violent emotions or passions, the consciousness of willing the particular motions produced is always very indistinct, as in actions performed under the influence of anger or of fear." Besides the voluntary motions succeeding to emotions and passions, these states of the mind, in many instances, produce motions independently of any exercise of volition. Involuntary motions of this kind may occur either in muscles usually obedient to the will, as is the case in the examples referred to by Dr Cullen, of the expressions of the passions in the countenance and gesture, as well as in trembling from fear; or they may occur in muscular organs not subject to the will, as in the heart in palpitation, and in the arteries in blushing; or, lastly, they may occur in that class of muscles by which re-

spiration is performed, and which, though subject to the will, usually carry on their operations independently of its interference, as in laughing, crying, sighing, &c. "As the exertions of the energy of the brain," Dr Cullen observes, "are especially under the influence of the will, so it is well known that those modifications of the will which are named Passions and Emotions have a powerful influence on the energy of the brain in its actions upon the heart, either in increasing or diminishing the force of that energy. Thus, anger has the former, and fear the latter effect; and thence it may be understood how terror often occasions a syncope, sometimes of the most violent kind, named Asphyxia, and sometimes death itself*."

3. *Principle of Imitation.*—The third circumstance mentioned by Dr Cullen as exciting the brain to produce motions, is the disposition of human nature to imitation. As in the case of the emotions and passions, the motions to which imitation gives rise may be voluntary or involuntary. In many of the voluntary motions arising from imitation, to which we may refer the acquisition of speech by children, and the various species of mimickry, Dr Cullen conceives that we are often conscious of willing the general purpose only, and not the particular motions produced; or if we will these, it is only, he says, as a general effect. That our ordinary voluntary motions may be modified by imitation without any consciousness or intention on our part, is rendered obvious, he observes, by the similarity of voice and gesture which prevails in families and countries, and by the proneness of strangers to catch the

* First Lines, § 1180, Works, vol. ii. p. 366.

tones and gestures of persons among whom they may happen to reside for some time, even although they may be desirous to avoid this. As an example of an involuntary motion arising from imitation, Dr Cullen referred to the difficulty which every one experiences in resisting yawning, when practised before him by another; and he remarks that we have many instances of involuntary imitation going still further in convulsive diseases.

4. *Appetites.*—The fourth class of causes which Dr Cullen mentions as exciting the brain to produce motions, are the appetites. As a proof that the motions arising from the appetites are instinctive, he refers to the action of deglutition. Though we have, says he, an arbitrary power over this action, yet most persons are as ignorant of the different successive motions that occur in its performance, as they are of the motions of the stomach and intestines. They cannot be said, therefore, to will those particular motions, but merely the general effect of swallowing. He also points out, as marking how strictly this class of motions is connected with the desires that excite them, the difficulty which a person without appetite experiences in attempting to swallow food, or even a person with appetite, in attempting to swallow what he nauseates.

5. *Propensities.*—The term Propensities is employed by Dr Cullen in a meaning somewhat peculiar, to signify our desires to remove uneasy or painful sensations, in consequence of which motions of a mixed nature are excited in the economy that are not directed to external objects, like those arising from the appetites, but confined to the body itself. Of motions of this kind, the

greater part consist of irregular and temporary actions of the respiratory muscles, such as those of sneezing, coughing, sighing, hiccuping, voiding urine and fæces, yawning, and vomiting, if this last action be, as it has been supposed to be, performed by means of the diaphragm; others occur in the muscles usually subject to the will, as that of stretching *(pandiculatio)*, and those motions of restlessness and inquietude which pain and uneasiness produce, and which have been usually termed the *fidgets*. These motions, Dr Cullen conceived to be instinctive; but, in all of them, he remarked, as well as in those arising from the appetites, some volition is concerned. This is proved, he says, not only by the fact that they can often be prevented by another volition presenting itself, but also by the circumstance that the several motions which occur in executing these propensities, are more or fewer, and more or less forcible, according to the vehemence of the propensity or effort; in many instances, however, the stimulus to these propensities is irresistible.

6. *Certain Internal Impressions occurring naturally in the Economy.*—The class of motions comprehended by Dr Cullen under this head, are the involuntary motions concerned in the performance of the vital and organic functions, and of which, in ordinary health, the mind is altogether unconscious. To their production he considers the brain to be excited by certain internal impressions which do not occasion either sensation or volition. It is only, he remarks, when they are exercised in an unusual manner, or in an inordinate degree, that we are conscious of the occurrence of these motions. That they are not the mere phy-

sical effects of the action of their several causes upon the inherent power of muscular fibres, but also depend upon an energy or action of the brain, is proved, Dr Cullen conceived, by the influence which the passions exercise over many of these motions, as well as by the effects that result from the destruction or compression of the nerves that connect the muscular organs, by which they are produced, with the brain.

It was from the automatic motions of the heart and of the stomach that Dr Cullen principally derived his illustrations of the dependence of the vital and organic functions upon a nervous energy. "Though the muscular fibres of the Heart," he observes, "be endowed with a certain degree of inherent power, or *vis insita*, they are still, for such action as is necessary to the motion of the blood, very constantly dependent upon a nervous power sent into them from the brain. At least this is evident, that there are certain powers acting primarily, and perhaps only in the brain, which influence and variously modify the action of the heart. I suppose, therefore, a force very constantly during life exerted in the brain, with respect to the moving fibres of the heart, as well as of every part of the body; which force I shall call the Energy of the Brain; and which I suppose may be, on different occasions, stronger or weaker with respect to the heart *." The influence of the emotions and passions over this organ had been long known. "Fear," says Dr Cullen, "is allowed to weaken the action of the heart as much as anger can increase it; and, when these operate so immediately upon the heart, it must be regarded as a proof that

* First Lines, § 1175, Works, vol. ii. p. 363.

the action of that organ depends upon some energy derived constantly from the brain." As proofs of the same energy, Dr Cullen referred to the slow pulse occurring in cases of compression of the brain, and to the quick and irregular motions of the heart in various states of disease. The cessation of the heart's action taking place some time after the par vagum and intercostal nerves are tied or divided, is another fact which Dr Cullen adduced to prove the dependence of that action upon an influence derived from the cerebral organ. "It is true," he remarks, "that the action of the heart may, in some degree, subsist after all communication between that organ and the brain is destroyed; that there is an inherent power to this extent we readily allow, and in a living and entire animal, with a very few nerves, or, if you will, with none at all, that power may give some action. But this does not disprove that the nerves are constantly necessary to the action of the heart; for, in these experiments its actions were disordered, and the pulse became intermittent. The slight palpitations which continue in the heart after it is separated from the nervous system, are not that vigorous healthy action which is necessary to the functions of life*." But as it has been found by many experiments that the heart continues to propel the blood for some time after the division of these nerves, provided respiration be artificially maintained, it may be doubted whether their direct influence or nervous energy be not exerted chiefly upon the respiratory organs rather than upon the heart. The other Muscular part of the Sanguiferous system, viz. the Arteries, Dr Cullen consi-

* Works, vol. i. p. 81.

dered to be also constantly under the influence of the energy of the brain. "The tone and action of the arteries, as a muscular part," he remarked, "may be increased by stimuli immediately applied to them, or by the increased force of the nervous and animal powers with regard to them; and they may be diminished by sedative powers applied, or by weakening the nervous and animal powers*."

"The action of the Stomach," according to Dr Cullen, "is not so constant as that of the heart and organs of respiration, but it is pretty constant. It differs, however, in this, that it is subject to a great variety; the difference between the action of a full stomach and one almost empty is very considerable; and this organ must be exposed to all the variety of states between its strongest and weakest action. The variety of its action depends upon the energy of the brain, which indeed constantly operates, but must admit of some inequality: in the stomach the influence of the brain is greater than in any other part of the system; and the brain, as it is liable to any variety of its energy, must more especially affect this organ †." "That the stomach requires the constant energy of the brain is proved," Dr Cullen observes, "by its immediately becoming paralytic when its nerves are destroyed. This effect, was commonly found to occur in the experiments made upon the heart, by cutting the par vagum: the appetite was destroyed, the contents of the stomach stagnated, and were variously corrupted." As additional proofs of the influence of the state of the brain upon the muscular organs of the alimentary canal, Dr Cul-

* Works, vol. i. p. 162. † Ibid. p. 150.

len mentions the difficulty of exciting vomiting in mania, and of producing purging in compression of the cerebrum.

Dr Cullen was aware that the Secretory organs are also in a considerable degree susceptible of being influenced in their actions through the medium of the nervous system. In the notes of a clinical lecture, delivered 25th March 1762, I find the following hint written down for commentary: "Instances of the nervous power exciting various secretions, such as saliva, and others. This an important fact in physiology, and the same applies to inflammation and hæmorrhage." In his Outlines of Physiology he remarks, "It would seem that no other secretion but those of perspiration and sweat are manifestly increased by the increased action of the heart and arteries, and that most of the other secretions are increased only by stimulants applied to their organs. These stimulants may be either such as are immediately applied externally or internally to the excretory, or perhaps to the secretory vessels; or they may be such as are applied to the Sensorium, or to distant parts of the nervous system, which, by the laws of the animal economy, have a connexion with the organs of secretion *."

"From all these considerations," adds Dr Cullen, "I think it should appear that some energy of the brain is required even in the involuntary motions, where it appears to be less necessary, and where a renewed action does not indeed always concur, as in the voluntary muscles."

7. The seventh and last class of causes mentioned

* Physiology, § 282, Works, vol. i. p. 207.

by Dr Cullen, as exciting the brain to produce motions, are *various occasional impressions of external bodies, and various occasional states of the general system, or of its particular parts.* To this head he referred the effects produced upon the animal economy, by noxious powers or morbific causes, and by therapeutic powers or remedies.

Among the noxious powers acting upon the different muscular organs through the medium of the nervous system, Dr Cullen pointed out, 1st, The effects of *Heat* and *Cold* in increasing and diminishing the sensibility of the nerves and the energy of the brain, as evinced by an increased temperature giving rise to an increased action of the heart and arteries, and affecting the distribution of the blood through the different parts of the body; and by a reduced temperature diminishing the mobility of the voluntary muscles, weakening the action of the heart and arteries, inducing sleep, and when carried beyond certain limits, ultimately extinguishing the vital functions of the brain; 2d, The effects of *Food and of Drinks containing alcohol,* the first operation of which upon the voluntary, vital, and organic muscular systems, Dr Cullen considered to be, in many instances, so instantaneous as not to admit of its being supposed that it can be produced in any other way than through the immediate agency of the energizing influence of the brain; 3d, The effects of *Poisonous substances,* the speedy and universal influence of which upon the animal, vital, and natural functions of the body, when they are taken into the stomach, must, he conceived, depend on their operation being communicated to the brain; 4th, The

effects of *Contagions*, as evinced in the phenomena produced upon the different muscular organs of the animal, vital, and natural functions, by the contagion of typhous fever, and in the very peculiar state of the nervous system and of the voluntary muscles, produced by the poison of rabid animals; and, 5th, The effects of *Emotions* and *Passions* in producing, through the medium of the nervous system, irregular actions of the voluntary muscles and of the vital and organic muscular organs, as manifested in the production, by these causes, of epilepsy, palpitation of the heart, and syncope.

With regard to the action of Therapeutic powers or remedies upon the animal economy, Dr Cullen remarks that it is sufficiently probable that the peculiar action of medicines depends upon the sensibility and irritability of the human body; or, in other words, that it universally depends upon motions excited and propagated in the Nervous system. "What is," he continues, "the nature of the matter in which these motions take place, or in what manner it is inherent in the Nervous system, is not well known, but we think it may be justly held as existing there, and that we may speak of it under the appellation of the Nervous or Animal Power. As it appears only in the living, and disappears entirely in the dead state of the body, it may be otherwise properly enough termed the Vital Principle of animals." The remedies capable of increasing or diminishing motion in the animal economy, Dr Cullen divided into those which operate on the system more generally, including Stimulants and Sedatives, and those operating upon particular parts,

as Emetics, Cathartics, Diuretics, Diaphoretics, &c. In speaking of Stimulants taken into the stomach, Dr Cullen expresses his belief that the most common and very general manner in which they operate is by their stimulus being communicated to the Brain; and that by exciting its energy, the various effects which follow their administration are produced in the different parts of the system. Of the class of Sedatives which act more immediately upon the Nervous system, viz. Narcotics, Dr Cullen observes that their operation extends to every function depending upon the Energy of the Brain, manifesting in all of them a sedative power; in the Animal functions by inducing sleep, in the Vital functions by weakening the force and diminishing the frequency of the pulsations of the heart and arteries, and in the Natural functions by diminishing the activity of the alimentary canal, the chief organ of this class of functions. "All of these powers," he remarks, "noxious and salutary, operate not only upon the parts to which they are applied, but also upon distant parts of the system, and that by the intervention of the Brain; and these causes are therefore to be considered as exciting the action of that organ."

After having enumerated the various causes by which the action of the Brain in moving the several parts of the body is at different times excited, Dr Cullen observed, that

"In all or any of these cases in which the action of the brain takes place, we cannot perceive the manner, that is, the mechanical means, by which the several causes produce their effects; and we perceive only an institution of our Maker, establishing a connexion between the several causes and the motions that ensue. At the same time we, for the

most part, perceive that the connexions established are suited to the purposes of the animal economy, and particularly to the purpose of supporting the system in a certain condition for a certain time, and of averting what might hurt or destroy it. This constitution of the animal economy we call Nature, or the reaction of the system; and everywhere in the economy we perceive the *Vires Conservatrices* and *Medicatrices Naturæ*, so justly celebrated in the schools of physic*."

"There is in the animal economy a power capable, in many cases, of obviating or redressing its own morbid deviations, or, as has been observed at every period since the beginning of physic, there is a *Vis Medicatrix Naturæ*. Some impute this power to the mechanism or organization of the body alone; others, such as the Stahlians, impute it to the exertions of a rational soul, independent of the organization of the body, perceiving such things as have a noxious tendency, and obviating their operation in order to restore the body to a healthy state. Now, whether this power is to be imputed to the body or mind it is not necessary to inquire here; the fact is enough, that there are motions which may be imputed to a Vis Medicatrix Naturæ, to a tendency in the animal economy to redress its own deviations. It is in consequence of this constitution, that not only impulse, and other causes, which may be supposed to produce motion, do accordingly excite motions in the animal economy; but that also many causes which seem calculated to diminish motion, do, however, produce an increase of motion in animal bodies. Thus several passions which, in their first tendency, diminish motion, several propensities arising from debility and difficulty of action, the absence of usual impressions, evacuations, and other causes of relaxation, cold and narcotic powers, are all of them causes of considerable motions arising in the animal system. They do not, however, excite contractions by their direct application to the moving fibres, but as applied to the system

* Physiology, § 120; Works, vol. i. p. 111.

in general so as to affect the brain; and if they excite the animal power it must be in this manner. The first of these classes of agents may be called the *Direct*, the last the *Indirect* Stimulants."

As an additional illustration of the difference which Dr Cullen conceived to exist between direct and indirect stimulants, " a doctrine," he remarks, " which I have first started," I may quote his observations on vomiting as a symptom of dyspepsia.

" Vomiting, as an increased action, has been commonly supposed to be always occasioned by a direct stimulus applied to the fibres of the stomach; and it may often be so: but I maintain that it is more frequently occasioned by a disagreeable sensation exciting such motions as are proper to throw off the cause. Such sensations often excite motions, without any irritating matter being applied to the part moved; at least, without any matter being applied that has any direct tendency to excite motion, or that can produce this effect except in consequence of a sensation being carried to the brain, and from thence exciting the motion. But as a motion is thus excited, we name the power of such a sensation a *stimulus;* but, to distinguish it from the proper *direct* stimulus, we call it an *indirect* stimulus. The operations of such indirect stimuli are frequent in the system, both in health and sickness; and, as they are often exerted for salutary purposes, they are considered as evidences of a *Vis Medicatrix Naturæ**."

With regard to the connexions established in the General system, through the medium of the Brain, Dr Cullen considered it as of great consequence in Pathology, to be observed, that

" Certain parts of the body which have a common function and peculiar constitution have thereby a peculiar rela-

* Works, vol. i. p. 270.

tion to the brain, so as to be more liable to be affected by its different states, and, in their turn, by the difference of their condition, to affect the brain. Such are especially the organs of voluntary motion; the alimentary canal, and especially the stomach; the circulatory system, and particularly the extremities of the vessels on the surface of the body; the generative organs, and of these particularly the Uterus in females." "With regard to these," he adds, "remember that the facts are certain, and therefore an object of our attention and curiosity; the reasons are doubtful: I do not pretend to explain them either certainly or completely."

That property of the nervous system, in consequence of which impressions made upon one part affect the whole system, has, Dr Cullen remarks, been called General Consent or Sympathy. But it has besides been alleged, he continues to observe,

....." that there are certain parts which have a particular connexion with one another more than with other parts; and though they may be situated at a distance, or *remote* from one another, motions or changes excited in the one part produce motions or changes in the other. It is instances of this kind that have especially given occasion to the terms of Particular Consent or Sympathy." "I need not tell you that this consent or sympathy has been very much spoken of, as you will see in Dr Hoffmann's Works, in the *Tractatus Medicus de Sympathia* of Dr Rega, and in Dr Whytt's Essay on Nervous Diseases. Employed as a term for this fact, as expressing a communication between particular parts of the body, I am willing to admit of it; but as it was used formerly it implied an occult quality—it implied that such a phenomenon took place without the communication of motion. Every term which is like establishing a power between bodies at a distance, without contact and motion, and therefore a term for an occult quality, is now properly rejected; I would wish, therefore, that the

terms Sympathy and Consent were rejected from our system." "As long as we have no idea on what the connexion depends, the term may be used; but if we can find out its foundation, and the means of communication—which ought to be attempted—the term will be no longer proper. Thus, it would be absurd to talk of a sympathy between the pendulum of a clock, and a finger that points the time; because here we understand the means of communication between them. In the same manner here, if a sentient principle is interposed, or if the motions follow, not indeed by a mechanical power, as in the clock, but by a physical or automatic necessity, all occasion for supposing occult qualities, and consequently for using this term, immediately ceases." "Our modern physiologists, that is for these hundred years past, have attempted to account for these particular sympathies by a particular connexion of the nerves of the different parts, either in their course or from their having their origin in the same part of the brain; but we, with Dr Whytt, are of opinion that the communications of motion between the several parts of the nervous system which have been mentioned as instances of a particular sympathy between these parts, are very seldom to be explained by any contiguity or contact, either in the origin or in the course of the nerves of the communicating parts. Dr Whytt has, in his last Treatise, destroyed the very foundation of particular sympathies, by shewing that they are not owing to any direct communication of particular nerves with one another, but that they come through the intervention of the Sensorium Commune *."

In endeavouring to reduce as much as possible the number of phenomena that ought to be regarded as particular sympathies requiring to be explained by a communication of nerves, Dr Cullen took occasion to point out that many of what had been regarded as such

* Printed Clin. Lect. p. 29.

were in fact instances of the general consent or sympathy improperly understood, which must be explained by supposing that the primary impressions of external agents act on the brain, and that it is in consequence of the reaction of this organ, that the secondary or subsequent motions are produced in some other part of the system; and that others are examples of a connexion between several parts, the action of which is necessary to produce one effect, which connexion also, he conceived, is established through the intervention of the brain. For examples of general sympathies mistaken for particular ones, Dr Cullen refers to the list given by Dr Whytt, who, he remarks,

....." has made this list much larger than I would have wished, and has brought in a number of instances that are more easily explained upon the principle I have mentioned. Thus, he says that Hippocrates has observed that the unexpected sight of a serpent will make a person pale; and he considers this as an instance of sympathy between the eyes and other parts, particularly the face. But I say that it is no instance at all of any such connexion, but only shews that fear may be excited by objects of sight, as it may also be by objects of hearing and of touch. The touch would, in many persons, equally produce the paleness of the face; and even the description of some of the pernicious effects of serpents would produce the same general effect of paleness. These, then, are all different means of producing fear, and the paleness of the countenance is an expression of that emotion. The same operation goes on in many others of the instances of particular sympathy adduced by Dr Whytt." " The motion of particular parts, in consequence of such impressions, depends only on the intervention of the sensorium commune, and the particular irritability of the part *."

* Clinical Lectures, p. 30.

"A second way," continues Dr Cullen, "of accounting for some of those phenomena which have been considered as particular sympathies, is this,—When the action of several parts, together or successively, is necessary to produce one effect, these parts *concur*, though the stimulus exciting the action of the brain is applied to one single part only; and commonly no other cause of communication can be assigned, but the several motions being necessary to the execution of the propensity, desire, volition, &c. arising from the stimulus. Thus Dr Whytt, in enumerating the instances of the consent of the nose, has said, that acrid substances applied to this organ bring into consent the diaphragm, intercostal muscles, and other parts. But they will have the same effect if applied to the glottis or to the trachea, to the parietes of the stomach, or to the extremity of the rectum; and, in like manner, the same muscles will be brought into convulsive motions by acrimony of the urine or distention of the bladder. But this, in my opinion, establishes no particular connexion: it is to be explained merely upon the principle that has just been mentioned. By the laws of the animal economy, which we cannot further explain, certain impressions produce certain propensities and volitions, which again produce the concurrent action (Synergeia) of certain muscles; so that it is by the intervention of the brain, or, as Dr Whytt says, of the sentient principle, that the connexion in these instances is established. I do maintain that you will find no other explanation of what may be strictly called sympathy or consent, namely, the necessary concurrence of several actions to a propensity or volition, and the concurrence of several motions of the system produced through the intervention of the brain, in consequence of propensities and volitions."

Besides the principles that have been already stated, as affording an explanation of a large share of those phenomena, which have been referred by many phy-

siologists to the head of Particular Sympathies, Dr Cullen thought that, for the explanation of some of these phenomena, it may be necessary to admit the doctrine of nervous communication, though, he remarks,

"These are exceptions to the rule with which we set out, that the sympathies are very seldom to be explained by communication of nerves. Dr Whytt, in enumerating the instances of consent between the ear and the teeth, mentions the disagreeable sensation experienced in the teeth, in consequence of the noise of a file and other sharp sounds. In this instance, the connexion between the nerves of the teeth and those of the ear does evidently appear. Whether this is an objection to what we have said with regard to there being no communication between nervous fibrils, I dare not say. There is a communication of motion between parts whose membranes are *contiguous*, such as between the neck of the bladder and orifice of the urethra, and between the kidneys and orifice of the urethra; between the intestines and the abdominal muscles; and, perhaps, between the internal ear and the glottis, where irritation of the ear produces coughing. There are many instances of these motions being carried along *continuous* membranes, but the *contiguity* of parts is also to be taken into account for the particular sympathy and consent. You will be able, I think, to distribute such instances in the manner I have pointed out, and be relieved from much confusion and appearance of mystery which this language of sympathy has introduced into our system of physic."

V. *The different conditions of the Nervous System in the states of Sleeping and Waking, and the doctrine of Excitement and Collapse.*—The natural states of Sleeping and Waking, as well as those conditions of the animal economy, which he was accustomed to comprehend under the terms of Excitement and

Collapse, Dr Cullen conceived to depend upon the more or less *excitable* state of the Nervous system. His speculations upon this subject, which pervade all parts of his writings, are, in many respects, original, and must be regarded, I conceive, as the foundation of those doctrines which have since been considered as peculiar to the Medical School of Edinburgh, and usually designated, by Continental writers, under the appellation of the *Theory of Excitement.*

Boerhaave and Haller, with most preceding physiologists, in adopting the hypothesis of Animal spirits, imagined that, during the waking state, these spirits are gradually exhausted, whilst, during sleep, on the other hand, they are recruited; and that these alternate states of the exhaustion and accumulation of the Animal spirits act as the immediate causes of the states of Sleeping and Waking. "This," Dr Cullen observes, "has been the opinion of the learned and of the vulgar; it has been the most common one in every System of Physic, with very few exceptions, but it is embarrassed with many difficulties; there are many circumstances in the states of Sleeping and Waking, that will not correspond with it." In rejecting this hypothesis, Dr Cullen seems to have been led by the great deference which he entertained for the genius of Sir Isaac Newton, to conceive a partiality for the explanation of the phenomena of Sense and Motion, by the operation of an Ethereal medium, which Sir Isaac had suggested; and he endeavoured, accordingly, to account for the alternate exercise and suspension of sense and motion which occur in the states of Sleeping and Waking, by an increased or diminished mobility,

or a more or less *excited* state of this medium. But, in giving this explanation, he regarded it as hypothetical, and wished it to be received as such by his students; for, in speaking of it, he says—

"I take it for granted, that when you consider the weakness and manifest mistakes of any other hypothesis, you will readily, with me, think that the condition of the nerves fitting them for the communication of motion, consists in some state of the matter of the nervous fluid itself, and of its having more or less mobility; in some cases being capable of being moved with more ease and vigour, while in other cases it is unfit for either. Now, I say, merely to avoid long expressions, I shall choose shorter ones, and shall speak of the moveable state of the nervous fluid, or of that condition of the nervous system which fits it for the communication of motion, under the term of its *Excitement*, and a deficiency or lesser degree of this I shall call its *Collapse*. Now, you must merely consider these as terms employed for what I take to be matters of fact, the increased or diminished force of the animal power or energy of the brain, and not as importing matters of theory, or as expressing any thing with regard to the nature of the nervous fluid, or wherein these different states of the nervous system consist. Whatever hypotheses I may have fancied to myself, I consider these as hypotheses still, and dare not trust you with them, unless you take them as they pass in my mind, and be very certain never to apply them in particular cases."

In the explanation of his doctrine of *Excitement* and *Collapse*, that is, of the increased and diminished Energy of the Brain, Dr Cullen assumed the waking state of a man in health as the standard point from which the higher and lower degrees of excitement are to be estimated, though he acknowledges that there are difficulties in fixing these degrees.

" I have been a little at a loss," he observes, " in the application of the terms of Excitement and Collapse. There is nothing more manifest than that the degree of excitement is very different upon different occasions; if we take the lowest, every higher degree than that must be called a degree of excitement; and if we take the highest degrees, and consider the lower degrees that may take place while life still subsists, every lower may be called a degree of collapse. The same ambiguity occurs with regard to the terms of Heat and Cold, which may be absolutely applied to the same individual state of bodies; and philosophers have not yet agreed where they should begin with applying the terms Heat and Cold; and so it may be with regard to the terms Excitement and Collapse. In saying that, in the ordinary state of waking men in health, the excitement is total with respect to the functions of the brain, but readily admits of diminution so as to produce the state of sleep, I mean," continues Dr Cullen, " to settle the matter a little more exactly. I would call those states, states of excitement, where the excitement is total with regard to the functions of the brain, where there is in every part the general exercise of sense and volition. I know that there are many cases where there is a mixture of the excitement and the collapse, or of the increased and diminished energy of the brain, but I hold that to be the state of waking and of excitement, when the whole of the functions of the brain can be exercised; and, upon the contrary, whenever these functions are for the most part suspended, that I would call, more strictly, a state of collapse; and so upwards and downwards as it may occur. A degree of collapse, or diminished energy of the brain, takes place in the case of natural sleep; we call it a state of collapse when the excitement is partial, when the collapse prevails so far as to suspend very entirely the exercise of the functions of sense and volition, so that such a collapse takes place in sleep as is sufficient to weaken the general activity of the brain."

Dr Cullen supposed that the Delirium or Reverie which appears in the state intermediate between sleeping and waking, arises from the brain being in a state of partial collapse; and that dreaming, in which the animal functions are obviously not entirely suspended, is owing to the collapse, or diminished energy of the brain, being in a less complete degree than in the state of natural sleep, and that, in proportion as the degree of collapse is less or greater, the dreaming is more or less active. "There are," he observes, "intermediate states between sleep and waking, and the sensibility is greater or less, as the tendency to either happens to prevail; and we particularly observe such intermediate states in the application of narcotics, when they produce a degree of drowsiness or stupor, and therefore the sensibility of the brain is diminished by the operation of narcotics."

In allusion, apparently, to an observation of Haller, that sleep is sometimes produced by mechanical causes occasioning compression of the brain, Dr Cullen points out in what he considers the state of coma resulting from compression, to differ from sleep.

"A certain compression of the brain can produce a state of the system resembling sleep with very great exactness, as we learn from the histories and dissections of cases in which such preternatural sleep had appeared. This has occasioned the supposition that some such state occasions sleep, and the removing it, a contrary state; but little attention is to be paid to this theory. The state produced by compression is, in some respects, different from that of ordinary sleep, and the difference chiefly consists in the state of *excitability*. We find that natural sleep can be interrupted by moderate stimuli, whereas, this artificial sleep is not to be interrupted,

unless by removing the cause. At the same time," he continues, " I must say that there are certain states of sleep produced by the causes that produce natural sleep, that are to such a degree, that persons in them are hardly more excitable than in those states depending upon compression. Thus, in a man dead drunk, where I suppose the sleep produced by a similar cause with that which produces natural sleep, the *excitability* is here hardly in a greater degree than in cases of compression."

In extending his application of the doctrine of Excitement and Collapse to the Morbid phenomena of the animal economy, Dr Cullen made choice of mania and syncope, as two affections calculated to illustrate his opinions respecting these opposite states of strength and weakness, vigour and debility, sthenia and asthenia of the nervous system; the first, Mania, being an example of a disease in which the excitement is increased above the natural standard; and the second, Syncope, of one in which the excitement is reduced below that standard, or in which a degree of the state of Collapse or diminished energy of the brain takes place.

" In the case of mania," he remarks, " it is well known that men can exert a force vastly greater than any which they could have exerted in their ordinary health, and that not from a more violent passion or emotion of mind, but in every trifling exertion; even when in a calm and placid state their strength is greater, and it continues greater when various means, such as cold, hunger, and evacuations have been employed to weaken it; and this can be referred to nothing but the state of the brain, the primary seat of the disease. In maniacs, I say, that the force, the power producing excitement, is the strongest, because the animal power, or energy of the brain, is in a condition of resisting most powerfully all the causes that diminish motion, such as the causes of

sleep; so that I conclude from this that an extraordinary degree of the power of excitement takes place, and because the uncommon degree of strength which maniacs frequently manifest, must imply an uncommon degree of excitement in the brain. Syncope is a state of collapse or diminished energy of the brain, in which not only the animal functions, but also the vital, concerned in the circulation of the blood, are suspended. Here the collapse may be very considerable, but there still remains some degree of excitability while the brain can be acted on by stimuli, which act only on vital powers, and while its usual excitability is still recoverable by stimuli; —That is to say, while life subsists there is a state of the brain, of the nervous system, that is different from the state of animal germs, and from the state we call death: the state we speak of depends upon a certain state of *excitability* still subsisting, but when that is gone, and the collapse is more complete and irrecoverable, it is a state of death." "Gaubius in his 197th section, certainly enters into an idea that life and death depend upon an excitement and collapse." "If we are right in supposing excitement to be necessary to life, if we take with Gaubius the definition of the vis vitalis, '*qua id ad contactum irritamenti se contrahit, crispat*,' if we take this that it is to be acted on by stimuli, we say that that indeed is vitality; and that, however the functions may seem to cease, there still remains some degree of *excitability* while the brain can be acted on by stimuli, which can act *only* on the vital powers." "The unavoidable death of old persons proceeds chiefly from the decay and total extinction of the excitability or vital power of the nervous system."

"Our system," Dr Cullen continues, "is not a mere automaton, supported in its duration by any powers, whether of mind or body, subsisting within itself. It appears that we have constant need of some external assistance, of the impression of external agents; and if these could be removed, we would not only certainly fall asleep, but we would very soon become dead. I have endeavoured to prove that there is not

a muscular fibre in the system which does not depend, more or less, for its power upon the sufficient vigour of the energy of the brain, and that without it the muscular actions are much more languid and weak. But though the energy of the brain is the proper vital function, it again depends upon certain other exercises and functions of the animal economy; and both together certainly depend upon the power of external impressions. The energy of the brain requires the external support of a certain degree of heat, and of the circulation of the blood, perhaps as one means of supporting the heat of the system; and it requires likewise a certain degree of tension in the vessels of the brain, by means of the blood supplied by the action of the heart. Heat seems to be the vivifying power with respect to all animals, that which first produces the vital powers, and it seems equally necessary for the subsequent support of the animal power, or energy of the brain; so that heat increases the motions that are the most peculiar and fundamental to the living animal. In whatever way animal heat may be produced, it is probable that it is connected particularly with the blood, and is distributed to the different parts by the blood; for we find, that, upon subtracting the circulation, the heat ceases as soon as it would do in a body of the same bulk that has acquired it. A determination of blood to the brain seems to be necessary to its functions; for, by withdrawing that, as happens in the deliquium animi, the functions of the brain and sensations disappear. I think it probable that a certain tension is given to the substance of the brain by the blood determined in such quantity to its arteries *."

To those who take an interest in tracing the origin and progress of medical opinions, it may be satisfactory to see, pointed out by Dr Cullen himself, the source from which the language he employed to express his

* Works, vol. i. p. 41–42.

opinions relative to the Energy of the Nervous System, or, in other words, his theory of Excitement, was derived.

" I will give you," he says, " some little illustration of what led me to the use of the terms Excitement and Collapse. There is seemingly diffused over the whole of nature, a quantity of electric matter. In the ordinary state of most bodies, this matter shews no disposition to a particular mobility or tendency to pass from one body to another. But we can, by certain artifices, accumulate this electric matter in more considerable quantities on the surface of certain bodies, in consequence of which it can be put in motion from one body to another, exhibiting the various phenomena of electricity. It is agreed upon among philosophers to call this Excitement, and to say that Electricity is excited, and that such bodies are excited Electrics; and all bodies may assume this state, either by being excited themselves, or by having such bodies as are excited applied to them. So, in our medullary or nervous system, there is a fluid,—it was present in the germ, but was not excited,—and it is in the *excited* state of this fluid that I suppose Life to consist, and when it is no longer *excitable* in any degree, we call this a state of Death. I can suppose that, as in Electricity, this fluid may be excited in different degrees; it can pass through various degrees, till it is entirely collapsed or disappears altogether, and so we can say that there is more excitability in one case than in another, and that the collapse may proceed to different degrees. These are some hints and analogies which may in some measure illustrate the matter; but there must be a concurrent view, and a consideration of many other phenomena, before we can apply this doctrine more strictly or specially to our nervous system; and though I have thus thrown out a conjecture, which every time I take a view of the nervous system seems to me to approach nearer to probability, yet I do not say that it will, in the present

state of our knowledge, apply to all the phenomena with any consistency."

The comparison of the different conditions of the animal power, or energy of the brain, to the different states of an electrical jar, was perhaps suggested to Dr Cullen by the attempt which Sauvages had made to establish an analogy or identity between the supposed nervous fluid and electricity, an opinion propounded by that author in 1749, and which seems to have been adopted by several contemporary physiologists, as by Lacaze, Laghi, and Wrisberg. Indeed, Dr Cullen himself, in assigning his reasons for believing that the motion excited in the sentient extremity of a nerve in the process of sensation, must be of the oscillatory or vibratory kind, suggests that it may somehow depend upon an electric tremor *.

The term Collapse Dr Cullen had probably derived from Boerhaave, who, in stating what he conceived to be the proximate cause of sleep, says, that in so far as the solids are concerned, it is the compression or *collapse* of the smallest vessels of the brain, in consequence of which they cannot transmit their fluids.

VI. *The effects of Custom upon our Corporeal and Mental Functions.*—The influence which Cus-

* Sauvages, Dissertation sur la Rage, 1749; Œuvres Diverses, tom. ii. p. 47–51, 12mo, Paris, 1771; Hemiplegia per Electricitatem Sanata, 1749; Elementa Physiologiæ, p. 131–2, 1755; De Suffusione, 1760. Lecaze, Idée de l'Homme, p. 78, 1755. See Fontana's Reply to Laghi, in the 3d volume of Haller's Memoires sur les Parties Sensibles, &c. Wrisberg, Note to the 379th Section of his edition of Haller's Primæ Lineæ. Cullen, Works, i. 38.

tom exercises upon the Corporeal and Mental Functions, is a subject which seems to have occupied much of Dr Cullen's attention. There are still preserved, in his own handwriting, fragments of an essay on this subject, which appear to have been written at an early period, and in which he has endeavoured to generalize the effects of custom as exerted, 1st, on the Simple Fibres; 2d, on the Nerves, as organs of sense; 3d, on the Motion of the Nervous Power; and, 4th, on the Circulation of the Blood, and functions depending upon it; and to point out some uses of this doctrine in the Practice of Medicine. Most of the materials of this essay were, at a subsequent period, given to the public by his son Dr Henry Cullen, in the Inaugural Dissertation *De Consuetudine*, which he published on graduating in 1780; and of which, as Dr Cullen mentions in his Treatise of the Materia Medica, it was his son's intention to publish another edition, in a still more complete form, in the English language.

In the view which Dr Cullen took in his Physiological Lectures, of custom and habit as frequently determining and regulating the action of the brain, he directed his attention chiefly to their effects on sensation, and on the action of the moving fibres. "By custom," he says, "every one understands the frequent and ordinary repetition of any thing that the body does or suffers. But here we shall consider only the effects of those repetitions by which the state of the body, as it is different in different persons, is determined; by which the body is more or less disposed than it would otherwise have been, to bear certain applications, or to perform certain actions; or, in short,

by which the body, in performing its various functions, is subjected to certain laws, which, without such custom, would not have obtained."

The effects of custom on sensation, Dr Cullen reduced to the following principles or laws :—1st, That a sensation proves strong or weak only as it is stronger or weaker than the sensation which had immediately preceded it, or than that degree of force to which the nerves had been immediately before accustomed. In illustration of this law, Dr Cullen refers to the difference in the sensations of temperature experienced when a part of the body is exposed to a given heat, according to the degree of temperature to which the part had previously been subjected; and to the variations in our power of discerning visible objects according to the intensity of the light that may have previously acted on the retina.

2d, That the same impression soon repeated does not produce the same strength of sensation as before; and that hence all new impressions are, *cæteris paribus*, strongest; and moderate impressions, frequently repeated, produce no sensation unless their force is considerably increased. Hence it is, also, he remarks, that many impressions, at first strong and painful, become, by frequent use, easy and tolerable, and very often pleasant, as has been observed of those of wine, coffee, tobacco, &c., and that moderate and pleasant sensations, by repetition, become insipid, and affect us very little. Hence it is, too, that such medicinal substances as when taken into the body act directly on the nerves, by repeated use lose their force, and require to be given in increased doses, in order to produce their

effects. This may be observed in the operation of some emetic, cathartic, and narcotic substances, and of several other kinds of medicines.

3d, That actions, which at first produce a sensation of consciousness, as being accompanied with volition, come, by repetition, to be performed, without any sensation; or they produce it only when they are performed with uneasiness, pain, or unusual force. When voluntary actions are seldom repeated, Dr Cullen remarks, the mind is conscious, as often as they occur, of its volition producing them; but if they are very frequently repeated, the mind will at length nearly, or altogether, lose the consciousness of the exercise of its volition; and a motion of this kind will now be excited by external agents, or by any state of the body that formerly occasioned a volition of the mind, without any consciousness of the will being concerned in producing the motion, or *at least* without any subsequent recollection of the will having been exercised.

4th, That, as sensations usually arise from impressions made upon the extremities of the nerves, and are referred to these, so when impressions are made on the nerves in their course, the sensations they produce are sometimes referred to the extremities from which they had commonly arisen. "Here," he observes, "I have rather given the fact than the explanation. But the fact is well known, that, in persons who have suffered the amputation of a leg or an arm, the sensations referred to the toes or fingers remain, so that, in injuries occasioned by explosions of guns, and such things as can affect the body strongly, the uneasy sensations are often referred to the place of the toes and fingers that

have been removed, and not to the only proper remaining feeling part."

5th, That the association of Perceptions, on which memory depends, is formed in part by the frequent repetition of these perceptions immediately after one another.

With regard to the influence of Custom on the action of the Moving fibres, Dr Cullen observed, 1st, That the contraction of muscles—within certain bounds with respect to force, frequency, and duration—by being repeated, is performed with more facility and force. "Hardly any one," he observes, "has entered upon the learning of a new exercise, who does not know that the first attempts are what we call awkward. It is only by repetition that we acquire the command over certain muscles, and are enabled to unite and accommodate various motions to one and the same action; and there is nobody who has not observed, that whether the actions are more simple or complex, we are constantly improving in the facility of performing them. Even some of the involuntary motions, by repetition, are performed with more facility, so that their organs can be excited to the same contractions with the application of perhaps a tenth part of the force of stimulus that was at first required. If the exhibition of an emetic, or of any other medicine whose effect is muscular contraction, be frequently repeated, a less and less dose is found to be necessary. We have many instances of the same kind, with regard to purgatives, of persons at first requiring a certain dose, but who, in consequence of repetition, are moved by a smaller dose. This effect will not be disputed; but it gives difficulty

when contrasted with a law of sensation formerly mentioned, (p. 320)—that, viz. of our becoming more insensible to an impression in consequence of its being frequently applied. In a person who is at first excited to vomiting by a small dose, if that is repeated, the stomach sometimes becomes less sensible, and the dose must be increased; and this event is perhaps more frequent than the other; but they are both equally true, and perhaps no rule can be made which will determine the result, as it frequently depends upon a temperament, and perhaps upon an idiosyncrasy."

2d, That custom determines the degree of tension necessary to the action of muscular fibres. Every workman, observes Dr Cullen, knows that his actions are steady only if they are performed with a certain degree of tension, and that, in the exercise of his trade, he requires tools of a given weight. So Fencers know that a difference in the weight of their foils will make a difference in the steadiness of their action, which shews that tension is necessary, and that habit regulates the degree.

3d, That custom associates motions with sensations, which are not otherwise their causes; so that the renewal of the sensation, or of its idea, renews also the motion. Thus if a man has taken a nauseous draught, and he should next day see the same or smell it, or even see the cup or glass out of which he drank it, he may be affected with a nausea or vomiting.

4th, That custom associates different motions, so that they cannot be separately performed, though not originally nor necessarily connected. The uniform motion of the two eyes is not of any necessity connect-

ed; many can avoid it, but in most people they come to be associated by frequent repetition, so that we have not the power to disunite them. The same thing is observed in the childish play of rubbing the body with one hand and striking it with the other: the motions of the hands are so intimately associated by custom, that they cannot easily be separated.

5th, That custom determines the degree of force and velocity with which motions can be performed; and 6th, That custom determines the order of succession in associated motions, and the velocity with which they shall succeed one another.

The following are stated by Dr Cullen as laws of Custom, equally applicable to sensations and to motions:—

1*st*, That custom establishes the periodical return of certain sensations and motions, not originally necessary to the animal economy; and, 2*d*, That custom fixes an exact period for the return of certain sensations and motions, which, by the laws of the animal economy, are disposed to return at intervals otherwise undetermined. "The whole of the animal economy," he remarks, "is under this power of habit with regard to its functions. This is particularly observable in the case of sleeping and waking, and in the return of our appetites, and of several excretions. Few people eat only when they are hungry, or sleep only when they are weary; but many people follow periods, and the appetite will return at the stated time; and with this singular circumstance, that if the person does not get his meal at the usual time, the appetite goes off, and he can want food very well till the next period. In the

same way, many persons who are disposed to be good companions cannot easily keep awake at the hour they have been accustomed to go to sleep, but if they get past the ordinary period, they can sit up all night long."

Of the other parts of Dr Cullen's Physiological lectures, besides those which relate to the Nervous System, it seems to be unnecessary to take any notice, as there was but little in them unconnected with his views of the automatic and universal agency of this system in the animal economy, that can be considered as peculiar to himself. In passing from the physiological to the pathological portion of his course, Dr Cullen observed, that " the study of physiology, even considered as a piece of pure speculation with regard to the vital mechanism or organization of animal bodies, must be acknowledged to be a curious and interesting subject; but, when considered as capable of a very useful application, it becomes a subject of the greatest importance. This application is to explain the nature of the diseases of the human economy, and the operation of remedies upon it, and thereby to lead to a more certain means of curing diseases than we could otherwise attain; and the soundness and value of physiology is only to be ascertained by its being capable of this application."

In conducting his lectures on PATHOLOGY, Dr Cullen professed to make use of Gaubius's Institutions of Pathology as a text-book; and, though far from following servilely these institutions as a guide, he did not suffer a single paragraph in them which he con-

ceived to contain any useful fact, or new view, to escape the notice of his students. On one occasion, some of his students having seemed to think that he spent too much time in commenting upon Gaubius, Dr Cullen took an opportunity of expressing the opinion which he entertained of the value of that author's writings.

"When you have got to the end of all your academical studies," he observed, "it will be proper for you, in your practice, to look back into the General Pathology; and you must consult especially Dr Gaubius. He is infinitely the best writer upon the subject; he is more full and comprehensive, discovers a greater erudition, and a greater knowledge of medical opinions and facts, than any other pathologist; he displays a great degree of judgment, and he has rectified the common notions, and put them upon a much better footing. If, at the same time, he is a little defective, or perhaps erroneous, his faults are to be excused, and even studied; and, as was generously said by the late Lord Bolingbroke of the Duke of Marlborough, after his death, to whom he had been an enemy during his lifetime,—when he was asked his opinion of him in a company who were speaking unfavourably of his character—that his virtues were so great that he truly forgot all his faults; so I would say, with regard to Gaubius, that his merits are too great for any body to disparage him for his faults; and, if it cannot be denied that he has some, we may say,—'*Vitiis nemo sine nascitur; optimus ille est qui minimis urgetur*;'—but I add that it is even useful to study his faults. Thus, if ever you are to converse upon medical subjects, or would wish to know the systems of others, and particularly the prevailing opinions in medicine, I would recommend to you to study Dr Boerhaave; for, though I am of opinion that his system is defective, and perhaps erroneous, yet it is still the prevailing system in Europe, and a man would be a very imperfect judge of the writings on Physic, if he did not know that sys-

tem; and, in like manner, I say Gaubius's is the best system of pathology, and if he has faults, these ought to be known as well as his excellences *."

In explaining in what respects the view of diseases taken in a Course of Lectures on the Institutions should differ from that taken in a Course on the Practice of Medicine, Dr Cullen observed, that—

"Pathology is the consideration of diseases, but as forming a part of the Institutions of Medicine it has a limited meaning; it is that branch which delivers only the General doctrines concerning diseases. To understand what I mean by *general* and *particular* doctrines concerning diseases, I must remark that I constantly suppose that every disease, as it actually occurs in individual cases, consists of a congeries, in concourse and succession, of phenomena, or, as they are called, symptoms, which certainly depend upon causes, and sometimes on the concurrence of several causes. Now, the particular symptoms of which this congeries is formed, are, many of them, common to many different diseases. Thus pain is a symptom that is one of the congeries in many instances, and therefore is common to many different diseases. In like manner diseases depend upon different causes: Pleurisy, for example, may depend upon obstruction of the arterial vessels, but it may

* It is agreeable to learn from the following extract of a letter, written to Dr Monro by Professor Gaubius in 1774, on his being elected an Honorary Fellow of the Royal College of Physicians of Edinburgh, what opinion he, on his part, entertained of Dr Cullen's writings:

"Itaque ut tibi vir humanissime, quem inter discipulos meos numerasse etiam nunc gaudeo, pro tacto nuncio gratissimum testor animum; sic illud insuper abs te peto obsecroque ut Illustri Præsidi CULLENIO quem ex scriptis ut præceptorem meum diligo venerorque, et cæteris collegis, suæ singulis doctrinæ laude florentibus, meis verbis significes, nihil mihi aut jucundius aut honorificentius unquam in vita mea contigisse quam publicam istam suæ erga me benevolentiæ testificationem; nec me defuturum omni officiorum genere quoad potero universos et singulos demereri."—*Leid. Bat. Aug.* 20.

likewise depend upon an increased impetus of these vessels. If, then, we consider the several symptoms that constitute any disease,—if we take these separately, and consider them abstractedly from the diseases in which they occur, this consideration may be said to be a general doctrine. Thus, if I consider pain in all its different modes and causes, without considering it as occurring more in one disease than in another, that is said to be a general doctrine with regard to pain. In the same way, if I consider obstruction in the abstract, in all its various modes and causes, this will constitute a general doctrine of obstruction very different from the consideration of it in a certain concurrence of symptoms. This has given occasion to the distinction of Pathology into *general* and *particular* or *special;* the first forms a part of the Institutions or Theory, the latter the whole of what we call the Practice of Physic."

In attempting to arrive at a general notion of Disease, Dr Cullen, whilst he made occasional references to the definitions of that state given by Boerhaave and Hoffmann, confined himself principally to a commentary upon the definition of Gaubius, "*Status ille corporis humani viventis, quo fit, ut actiones, homini propriæ, non possint apposite ad leges sanitatis exerceri.*" To this definition he objected, in the first place, that, by the introduction of the term "*status corporis,*" it limits the notion of disease to a state of the body only.

"Whether or not," says he, "any disease may reside in the mind entirely, I would not positively determine, though I am persuaded that no one does, and that there is no irregularity of the mind that does not depend upon certain changes in the condition of the body; but the Stahlians would not agree with me in this opinion; they refer many diseases to pure affections of the mind, and it is not enough that I entertain a contrary opinion; a general definition should not be liable to dispute, upon any system whatever."

Another objection which Dr Cullen made to Gaubius's definition of disease, is, that it does not distinguish those interruptions to the performance of the functions which are produced by external impediments, from those which depend upon the condition of the body itself. But his principal objection was, that it represents disease as the *cause* of the symptoms that occur.

" With regard to the causes of the symptoms by which we commonly discover the existence of disease," he remarked, " they are very seldom evident; different physicians are very seldom agreed about them, and we learn nothing of the form of the disease by taking the cause into the definition: every thing in nature implies a cause, and it is doing little towards defining any thing to say, it is such an event arising from such a cause. It is not proper to define any thing by circumstances that are not evident or agreed upon, and their having employed a definition of this kind, embarrasses both Gaubius and Boerhaave, in distinguishing between what we call a disease and its proper cause."—" In order to explain this a little, I must remark, that the doctrine of disease has been named sometimes Pathology and sometimes Nosology; and these terms were considered formerly as of one and the same meaning; but of late they have been applied with different meanings, and have been differently limited. When we speak of the Pathology of a disease in the Institutions or in the Practice of Medicine, the disease is considered in its symptoms, causes, and effects; whereas when we speak of a disease in Nosology, we abstract from the causes, and consider it only as evi ent from certain external appearances; and we distinguish ' seases from one another, merely by the differences in their (. ternal appearances, and by the different concurrences of tl ir symptoms. It is allowed by all, that it would be of very great service if we

could establish a nosology by considering the external appearances alone, and could determine how diseases are to be distinguished by these. But if that is to be a proper foundation of our study of the practice of physic, we must give a definition of disease that is suitable to it. It must be defined as consisting of a certain concourse of phenomena, and without any reference to the cause; we must not define it, as Boerhaave and Gaubius have done, by what is the proximate cause or supposed nature of the disease, for this is often conjectural."

"But admitting these difficulties with regard to Gaubius's definition, to be removed," continues Dr Cullen, "there are others still remaining. What are the *leges sanitatis* of which Gaubius speaks? What are the objects of practice with regard to the body as well as to the mind? We can imagine a *standard* of health, but that perfect standard exists nowhere in any one person. Physicians have been long sensible of this, and therefore they have invented a term, *latitude* of health. They suppose that health may deviate on either side from the standard, without passing to the opposite state, that of disease."—"If I were to attempt to establish a standard of health, it would consist in a certain size in the conformation of the body—in a certain vigour of action affixed to a certain time of life, &c.—and young persons not arrived at that period, or old persons who have gone from it, though they cannot rightly perform their actions, are still considered as not under the state of disease. But there is not only this latitude of health with respect to age, &c. It may be considered with respect to every individual. No man has the same degree of vigour for two hours together, yet there are variations occurring either in different men, or in the same person at different times, that are not to be considered as diseases. Gaubius has nowhere attempted to define what is the *latitudo sanitatis*, and it is only to be defined by the common sense of mankind. *Natura non fit per saltum*, as the naturalists say, but she passes by a gradual transition from

one state to another—so that it is difficult to know to what side any state of life belongs. We must often be contented with knowing things that are widely different. In the case of colours, we know blue and red perfectly well, but they may be blended together in an infinite variety in forming a purple colour, where it may be impossible to say where the red and where the blue prevails; and yet this does not hinder us from obtaining a very distinct conception of blue and red. So with regard to the latitude of health, I must, in the main, leave it in that condition, and you will seldom commit any great error though left in this uncertainty, as, till a disease is sufficiently evident, it is not an object of the physician's practice."—" I would thus point out," adds Dr Cullen, " the many difficulties that occur in attempting to give a comprehensive, and at the same time a tolerably accurate definition of disease. I imagine that most of these difficulties, though perhaps not all of them, may be obviated by defining disease somewhat in this manner: ' *Morbus est functionis læsio, permanens, molesta, a mutationibus in ipso corpore ortis.*' By *læsio* it is understood that it is a *læsio evidens;* its being *permanens* will exclude those little transitory changes that are constantly taking place in every individual; and its being *molesta* will exclude those *Turpitudines et Deformitates* that do not affect the economy, and are not, therefore, considered as diseases or objects of medical practice."

After thus endeavouring to give a general notion of disease, Dr Cullen proceeded to consider the import of the term, Causes of diseases, or Ætiology, and the divisions of these causes which medical men have established.

" Almost every event in nature," he observed, " may be considered as part of a chain or series of causes, which have, in that series, produced one another; and of which, consequently, every part may be said to be a cause of the last ef-

fect. Thus a man on board a ship of war applies a lighted match to the touch-hole of a loaded cannon; this kindles the gunpowder; this produces an explosion; this explosion pushes with great rapidity the bullet; this bullet, striking upon the timber of another vessel, shivers it into splinters; one of these splinters happens to hit, with great force, the head of a man standing by, and instantly kills him. Now this death may be easily traced from any one of the series or chain of causes; and the same is the case with regard to most other effects or events in nature. It is common to apply the term cause to each of these actions or motions, but there is often a necessity for distinguishing them as more immediate or more remote in relation to their ultimate effect. In the case supposed, the stroke of the splinter was the immediate cause of the man's death, and all the rest of the series were remote causes. This distinction is very necessary; it has been much employed in physic, and has given rise to the terms Remote and Proximate Causes."

But the term Immediate cause, or *Causa Proxima* of disease, Dr Cullen conceived, may be always defined in the way that Gaubius defines disease itself, as being the *condition* or *conditions* of the body upon which the several phenomena or symptoms that constitute the disease immediately depend. "I say condition or conditions," he remarked, "for they may be different, and in different parts of the body; but, if they all concur to produce the disease, they all make a part of the proximate cause; for physicians have not limited themselves to taking the very immediate cause as the proximate. This will be best understood by an example. Pain in a joint constitutes the disease we call Rheumatism; if we endeavour to trace the cause of this disease we shall find that the pain is owing to an over-distention of sensible fibres, to an overstretching

of the bloodvessels; we shall again find that this overstretching is owing to the increased impetus of the blood, to a stimulus applied to these vessels, often to the whole system, often to particular vessels; and that this has had for its cause the application of cold. Now the last may be said to be the only proper remote or exciting cause; while the stimulus, the increased impetus, and the over-distention, we may consider as all belonging to the proximate cause. To understand this," continues Dr Cullen, " we must observe that a scientific practice of medicine is founded upon a knowledge of the Indications, that is, of the changes that must be produced in the body, in order to cure a disease; and whatever furnishes the indication, that is to say, every condition of the body which requires to be changed, *quidquid petit ablationem vel mutationem sui*, is a part of the proximate cause." " The only good definition of a Proximate cause," he adds, " is that given by Dr Boerhaave in the 740th paragraph of his Institutions :—' *Causa proxima morbi appellatur tota illa simul, quæ totum jam præsentem directe constituit; hæc semper est integra, sufficiens, præsens, totius morbi, sive simplex fuerit, sive composita. Hujus præsentia ponit, continuat morbum; hujus absentia eum tollit.*' This is the notion that may be taken of a Proximate cause,—the whole of the internal conditions of the economy that occasion and maintain the several phenomena or symptoms of the disease, whether they are a series of conditions producing one another, or separate conditions acting in concurrence."

With regard to the Remote causes, Dr Cullen observed, that they are all those Agents or circumstances

which in succession or in concurrence produce the proximate cause, and then cease to act farther on the body. "The limits between Remote and Proximate causes, however, are very often difficult to assign; a remote cause may continue to subsist in the body, and to act as a part of the series of causes, in which case it is to be considered as a part of the proximate, though, before the production of that, it was properly a remote cause only. Thus a Plethora is commonly considered, and may be considered as the remote cause of Hæmorrhagy; but whenever the plethora continues along with and supports the hæmorrhagy, it is to be considered as a part of its proximate cause."

Dr Cullen took notice of a proposal made by some physicians, among whom he enumerated Gaubius and Sauvages, to restrict the appellation of Cause in Medicine to what has been defined as the Proximate cause, and to introduce, from the Wolffian philosophy, the term *Principium* in place of that of Remote Cause. "The term Cause, they say, is only properly applied to that circumstance upon which the existence of the effect immediately and constantly depends; and every other remote series of causes, which might have taken place without the effect immediately under consideration being produced, are not properly considered as Causes, but merely as *Principia*. Thus of the series of causes which I formerly mentioned, the blow of the splinter alone acts as the immediate cause; all the other circumstances, the kindling and explosion of the gunpowder, and the very shivering of the timber, might have taken place without producing the man's death, had not one splinter struck upon his head."

In proceeding to consider the other distinctions that have been laid down with regard to the Remote causes of diseases, Dr Cullen remarked, that they may be Simple or Compound.

" I call them Simple," says he, " when they operate upon every man, or upon those conditions of the economy that are in common to every man, because these common states are always given, and we have only to consider the agents which affect them. But the causes are very frequently Compound, as where the effect is produced by a particular agent acting upon a *condition* which in kind or degree is peculiar to one man, or exists in a greater degree in one man than in another: here the agent would have no effect if it did not meet with that peculiar condition; nor would this condition, on the other hand, produce any disease, if the agent did not happen to act upon it, so that the effect or disease is the compound result of the operation of the agent and of the peculiar condition of the body acted upon. Thus, in the kindling of gunpowder by a spark produced by the collision of two bodies, this can only be produced by the collision of two certain bodies, flint and steel; the collision of flint upon soft iron will not produce it at all; there is therefore a concurrence of causes necessary for producing the effect. In the production of disease the subject is the human body itself, and we know that this is at different times in different conditions with regard to its fitness for being acted upon by particular agents. A condition of the body rendering it peculiarly liable to be acted on by the exciting causes of disease, is called a Diathesis, Predisposition or Predisponent cause. The term Predisposition, therefore, implies some peculiarity in the constitution or condition of the body. Thus, any man whatever, if he falls from a certain height, may have an Hæmoptoe; but in a man singularly plethoric, an hæmoptoe may be produced by a slight fall, that would not have been sufficient to produce it, without the concurrence of the plethoric state; in this case, therefore, the plethora may be con-

sidered as a predisponent cause of the hæmoptoe. A term of this kind has been long admitted in physic, but Predisposition is a Latin term, and has not this signification in the Latin language, and Gaubius would therefore wish to substitute another term, that of *Seminium*, in its place."

"Whenever we consider Agents, again, as acting upon such a predisposition, as producing their effects only under certain conditions, they are called Occasional or Exciting causes. For this term, Dr Gaubius has substituted that of Noxious Powers—a term," Dr Cullen remarks, "which may with more propriety be employed to comprehend the whole of the Remote Causes of diseases, whether Simple or Compound. Predisponent and Exciting Causes, therefore, are always to be considered as relative terms, and are not otherwise to be employed. We make mention of a predisponent cause only when the disease is excited by an occasional cause; and, on the other hand, those causes of disease which operate upon the *Seminia communia*, or conditions of the economy that are in common to all mankind, are not properly termed Occasional: they are not to be so called but when they have relation to a predisposition. It is not to be supposed, however, that peculiar Predispositions cannot produce a disease without the concurrence of an Occasional cause; for a predisposition may of itself, by arising to a certain degree, produce a disease, without the concurrence of any obvious external cause. Thus, plethora may be sufficient of itself to occasion hæmoptoe. In many instances, on the other hand, there are Remote Causes which operate independently of predispositions."

With regard to the proper signification of the term Symptom, Dr Cullen observed, that,

"Every phenomenon that can be perceived as different from the natural and ordinary state of the human body is a symptom. This term should be applied only to the phenomena of diseases, and it is very improper to speak of the symptoms of health. A symptom, therefore, is a very evident deviation or alienation from the healthy state, as ascertained

by common apprehension. Now every disease nosologically considered, consists of a concurrence, in concourse and succession, of more or fewer symptoms, and every separate phenomenon so called may be considered separately."

"For a long time past, Symptoms have been divided into three kinds, *Symptomata Morbi*, *Symptomata Causæ*, and *Symptomata Symptomatum*. The *Symptomata Morbi* are those evident phenomena that constitute a disease—the immediate effects of the proximate cause. The *Symptomata Causæ* are produced by the accidental concurrence of a separate, and generally a less severe affection, arising from the agency of the same external or remote cause which produces the primary disease, and ought not to be called symptoms, which would imply that they form part of the same disease. The *Symptomata Symptomatum* are secondary effects of the other two kinds of symptoms, arising in the progress of a disease, though not necessarily forming a part of it. To illustrate these different kinds of symptoms we may observe, that in Pleurisy there occur the symptoms of pain, fever, and cough, which are all immediate consequences of inflammation of the pleura, and consequently are all *symptomata morbi*. If the inflammation has arisen from cold, this cause may at the same time have produced Coryza, either singly, or in concurrence with a Rheumatic, or several other affections; these, however, are not constant parts of the primary disease, Pleurisy; though from their arising from the same remote cause, their symptoms would be called *symptomata causæ*. Now the Pleurisy may continue to subsist by itself, or by the violence of the cough it may produce an Hæmoptoe. There would then exist two separate diseases—the Pleurisy and the Hæmoptoe; but as the same proximate cause continues to act, the hæmoptoe is considered not as a new disease, but as a part of the primary affection happening in a series, and its symptoms are termed *symptomata symptomatum*. Though the symptoms of this last class are not necessarily to be consi-

dered as parts of the primary disease, they are necessary parts in the *history* of every disease; for the history of a disease is not complete until the several series of succeeding symptoms that may happen to arise from the same proximate cause have been marked, and the *symptomata symptomatum* are useful, inasmuch as they mark the force of the proximate cause, and therefore the violence of the disease."

In apologizing for the minuteness of these preliminary details, Dr Cullen observed—"The use of such generalities will not be immediately obvious, but some accuracy and correctness in your ideas and language with regard to them, you will find on many occasions to be of consequence. It is proper that you should be sometimes exercised in forming abstract and general notions. Without these you may think of acquiring a greater number of particular facts; but till the terms in which these particular facts are expressed are accurately defined, and till the facts themselves are digested into a system, they will not be very useful, nor admit of much practical application." After having made these and other general remarks on the nature, causes, and symptoms of diseases, Dr Cullen proceeded to explain the general plan which he proposed to pursue in his lectures on Pathology.

"Former Systematics," he observes, "as Boerhaave and Gaubius, have marked out four divisions of Pathology; but there is some fallacy in their divisions, owing to their definitions of disease being definitions of the proximate cause of disease. Their first head, '*De Natura Morborum*,' treats of the various possible Proximate causes; it is an explanation of these only, and therefore is not a separate title from the head of Causes. With regard to their second head, '*De Differentiis Morborum*,' part of it falls more properly to be con-

sidered under the doctrine of Causes, and part in the Course of the Practice of Physic. The Pathology that I am to deliver, is properly to be referred to two heads, that of Causes and that of Symptoms, to the *Ætiologia* and *Symptomatologia* of Dr Boerhaave. The first head is to be considered as comprehending the Proximate and Remote causes. Contrary to the usual order, however, I shall delay the consideration of the Remote causes till I have considered the Symptoms, in order to put them next to the doctrine of Therapeutics, as they truly belong to both heads; for the same Agents that are upon one occasion the Causes of diseases, are upon another equally employed as the Remedies of it. Thus, heat and cold may be considered as Remote causes of diseases, but upon innumerable occasions we find that they are most useful Remedies; and so with regard to other agents, according to the condition of the body to which they are applied."

In conformity with the plan proposed, Dr Cullen entered, in the first place, on the consideration of the Proximate causes of diseases.

"The knowledge of proximate causes," he remarks, "is the great desideratum in, and the necessary foundation of, the Dogmatic System of Physic; but, from the imperfection of our physiology, from our ignorance of the nature and manner of operation of the various powers that can operate upon the human body, this is a knowledge which it is extremely difficult to acquire. I acknowledge that it is difficult, and that it may, therefore, in many instances be fallacious: and, indeed, if you take the whole history of Physic together, from the time of Hippocrates to the present, the doctrines with regard to Proximate causes have been more erroneous, perhaps, than any other part of medical science. But this does not affect the foundation of a Dogmatic system; its being liable to certain abuses is not sufficient to cause it to be rejected altogether. I do maintain, that a great deal tolerably

certain can be affirmed with regard to the Proximate causes, or Morbid states of the body, in particular diseases; but to put what we know of this matter into a complete system of Pathology is, I believe, by no means possible in the present state of our science. We obtain many advantages, however, from attempts to generalize any subject, and we can never hope to succeed in this but by making repeated attempts. There is a particular reason for attempting it in the present instance, in order to examine what is sound and what is fallacious and erroneous in the theories that have been advanced on this subject; for there is no man that practises physic, let him call himself a Dogmatic or an Empiric, that does not make use of more or less of a General pathology. Thus the Apothecary will practise physic, and will say, 'This man is too full of blood, and must have some drawn out of his veins;' and, on another occasion, he will say, 'That man's blood is foul, and he must be purged in order to purify it.' Whether he is right in this it may not be easy to determine; but so it is in this and in every other department of knowledge—men deceive themselves in imagining that they avoid Theory; it is only the man who knows its fallacy that can properly avoid it."

"Proximate causes, or morbid states, are generally *compound*, consisting of different conditions in different parts of the animal economy, which all concur together in the case of actual diseases as they affect individuals; and in a course of lectures on the Practice of Physic, proximate causes may be considered in this concurrence, but here, in the Pathology, we must take them separately; and such a consideration has given what pathologists have called the *Morbi simpliciores*. As the proximate cause of a disease may consist in a fault either in the solid or in the fluid parts of the body, the diseased states are divided into those of the Solids, and those of the Fluids. The diseases of the Solids are of two kinds; those of the solids as they are in common to the whole of the

body, and those of the same solids as formed into various organic parts. This gives again what may be considered as the *Morbi solidi simplicis*, and the *Morbi organici*. That the Fluids have a share in the performance of the natural functions and that their deviations have a share in diseases, is true. The *vitia fluidorum* are, therefore, agreeably to both ancients and moderns, properly considered in a view of General pathology, as proximate causes of diseases. But diseases properly consist in the *actiones læsæ*, and consequently there may be a great many deviations in the state of the fluids, from their most healthy state, without diseases being produced; it is only, as Gaubius says, when the solids are at length affected, that they form diseases. The pathologists, therefore, have been very improperly employed in considering only the affections of the fluids. Physicians have been accustomed so long to talk upon this subject, that they do not apprehend the mystery, as I would call it, in which it is involved; but it is still involved in a greal deal of obscurity, and I think I cannot do better than to consider the subject as it lies in Gaubius, under the two heads of *Morbi Humorum Absoluti* and *Morbi Humorum Relativi*, the former comprehending changes in the coherence of the fluids, their vitiated qualities, and the vitiated states of the secretions; the latter changes in the quantity, in the place, and in the motion of the fluids. In commenting on Gaubius's divisions, I shall often have more to object to than to illustrate, but I could not attempt a system for myself, and it is to be presumed that Dr Gaubius, who is a man of uncommon discernment and great erudition, has put the matter in as clear a light as is possible, and that in considering what he has written, we shall see at least the present state of our knowledge on this subject."

In his lectures on Physiology, Dr Cullen had pointed out the distinction of the common or fundamental solid parts of the body into the Simple Solid more strictly so called, and the Vital Solids comprehending

the Nervous and Muscular systems; and as it appeared to him that the pathology of the Simple Solid cannot be properly separated from its physiology, he introduced into that branch of his Course a short view of its various morbid conditions *. In his lectures on Pathology, therefore, he proceeded immediately to the consideration of the different morbid states or conditions of the Vital Solids.

"Under the Morbid affections of the Vital Solids," says Dr Cullen, "I intend to treat of what is peculiar to every part of the Nervous system—of the Medullary fibre, with the peculiar properties that it possesses in the living animal. The doctrine of the Vital Solids has been very little attempted in the Systems of Medicine, and never in its full extent. The first who attempted it was Willis. He was followed by Baglivi, with more extended views, and more accuracy in his principles. Hoffmann, too, has considered this subject; but he has done it very imperfectly, and with the admixture of a good deal of manifest error. Dr Boerhaave, one of the most valuable of our systematics, has hardly taken in the consideration of the Vital Solids. He has, indeed, treated *De Morbis Nervorum*, but not either in his Institutions or in his Aphorisms; nor has he treated of their laws in his Physiology. Gaubius is the last who has attempted the subject; but his views have been too limited. He considers, under the title of *Solidum Vivum*, only the irritable muscular fibre, and his views are, therefore, confined to the state of motion. He has had some view indeed to the state of sense; but he has confounded Sensibility and Irritability together, and he is, in several respects, not only confused, but deficient in his explanations." "Every one, who is at all conversant with the science of Medicine, begins now at length to perceive that the greatest part of the morbid affections of the human system

* Works, vol. i. p. 13.

depend on affections of the Nerves. But the laws of the Nervous system are very difficult to be perceived or properly understood, being entirely peculiar to the Animal kingdom, and not admitting of illustration from the consideration of other departments of nature. There is within us a strange mixture of the operations of the material and immaterial parts, and these are liable to very great irregularities. Hence the laws of the Nervous system are not even tolerably ascertained. We work on detached parts of it; but, with regard either to the whole or to any part of it, we speak obscurely, and take shelter under general terms."—"And here," observes Dr Cullen, "I must premise the general apology, that some parts of this subject we, I hope, see clearly, so as to be able to apply them; but to bring the whole into one complete and coherent system I do not pretend, nor can I make it tolerably perfect to my own satisfaction."

In entering upon the consideration of the General Pathology of the Nervous system, Dr Cullen conceived it necessary, in the first place, to explain what the pathology of this system properly comprehends, and to determine what are strictly called Nervous diseases.

"Dr Whytt," he remarks, "who has treated this subject *ex professo*, observes the difficulty there is in limiting the subject, as all diseases may, in a certain sense, be called affections of the Nerves. Every preternatural state, either of sense or motion, depends upon the Nervous system, so that the nerves are more or less concerned in every disease; and this title might consequently comprehend the whole of diseases. But some diseases are more strictly called Nervous, and some limits have been tacitly assigned or conceived in the minds of physicians. Those diseases, the symptoms of which appear only or chiefly in the Nervous system itself, which appear purely in the functions of Sense and Motion, and in which the Sanguiferous system is not necessarily or is only occasionally affected, such may be called more strictly Nervous

diseases; so that the whole of the Febrile diseases, however much they may be said to consist in affections of the Nervous system, are, by this limitation, excluded."

To render his definition still more precise, Dr Cullen added, that "those diseases only are to be called Nervous which arise primarily in the Nervous system, and in consequence of a change in the general states of the Sensibility and Irritability;" and that "though diseases do consist chiefly in affections of the Nervous system, yet if they have arisen from causes that affect merely the organs with which the Nervous system is connected, without any indisposition of the general Nervous system itself, none of these are to be called Nervous diseases." After making some comments on Dr Whytt's limitation of Nervous diseases, and on the illustrations by which he explains it *, Dr Cullen remarks, "I am not to limit the pathology of the Nervous system to the pathology of what may be strictly called Nervous diseases; but I am to take in the pathology of the Nervous symptoms, and therefore of the Nervous system, so far as it is concerned in producing the morbid phenomena or symptoms of the animal economy; in short, I am to consider the several deviations in the state of the Nervous system, in its different parts, from the standard of health."

In attempting to communicate to the reader some idea of the view Dr Cullen gave of the Pathology of the Nervous system, his observations may be advantageously arranged according as they relate to the three great and generally recognized functions of that system, —Sensation, Intellection, and Motion.

* Whytt's Works, 4to, p. 528-9.

1. *Morbid states of the function of Sensation.*—"Sensibility," Dr Cullen observes, "consists in a certain fitness to be acted upon by impressions—to be moved by the impulse of external bodies, so as that a motion may be propagated to the Brain, and produce sensation and its various consequences. Now, the *Force* of impression being given, the effects are different in different persons, and in different circumstances of the same person, according to the degree of susceptibility of impressions, or fitness to be acted upon by them. We must suppose, however, that there is a degree of sensibility that is suited more especially to the economy of every person,—always making allowance for the latitude of health, which is manifestly within the limits of certain degrees that are suited to the purposes of the Animal economy;—and if the sensibility be either more or less than this, it may be considered as morbid under the general title of Sensibility in *excess* or in *defect*. But besides the degree of force with which they act, impressions likewise differ in another respect, to which I have applied the new term of *Quality* of impression. Upon several occasions the qualities of impressions produce unusual and irregular effects, which must depend on some preternatural state of the organ of sense. The morbid states of Sensibility must be considered, therefore, both under the head of *Sensibility in excess or defect*, and under that of *alteration of quality*, or of *irregularity and depravity of Sense*."

"The Causes of the several morbid degrees of sensibility are to be sought for in the operation of the same causes or powers which upon different occasions affect the sensibility of persons in health, and may consist either, 1st, in particular states of the Sentient Extremities of the Nerves, or, 2d, in particular states of the Sensorium or Brain itself. To the first may be referred differences in the state of the medium through which the external impressions act upon the medullary substance of the sentient extremities,—the different states of that Medullary substance as given to it in the original sta-

mina, natural constitution, or primary organization,—the different states of vascular tension in the sentient extremities,—the different states of their temperature,—the effects produced upon them by custom and habit,—and, lastly, the causes of Excitement or Collapse directly applied to them."

Of causes of Collapse operating directly upon the sentient extremities, so as to diminish their sensibility, Dr Cullen observed, that we have examples, in the operation of sedative and narcotic substances applied to them. Thus opium diminishes the energy of every portion of the nervous system to which it is applied. With regard to causes of Excitement operating directly on the sentient extremities, so as to increase their sensibility, he acknowledged that he was uncertain if any such exist, though since there are powers of Collapse so operating, he thought it probable that there are analogous powers of Excitement.

The different states of the Brain which have an effect in increasing or diminishing sensibility, Dr Cullen conceived to consist, 1st, In that organ being more or less free; 2d, In its being more or less excited; and, 3d, In its being more or less under the influence of the causes of Attention.

1. "Sensation or Perception," Dr Cullen observes, "arises only when a motion propagated from the nervous extremities arrives at the Brain, and produces a corresponding motion or change there; but if from any cause the freedom of motion is interrupted, no sensation can take place. We have frequent instances of this where the Brain is under compression,—which is the most evident cause of interruption to the freedom of motion,—whether the compression be produced by external force, or by fluids effused into its ventricles or over its surface. It is difficult to say what degree of compression

is necessary in order to produce the effect in question. Experiments have been made upon dogs and other brute animals, from which it appears that a strong and considerable degree of compression is necessary to take away sense and motion in these animals: but in the human body, the loss of sense and motion has been seen to take place when the compression appeared to be very slight and partial. A distension of the Ventricles of the Brain with water may produce this effect; but in several instances where the effect has appeared, the cause has not been discoverable; and there are various instances of very inconsiderable tumours that have had the effect of producing palsy, apoplexy, and death. In some instances we find that partial compression produces only partial effects, as in compression of the optic nerve, where it has only the effect of producing a loss of sight; but in other instances we find that a less degree of compression in other parts of the Brain, has the appearance of producing a total insensibility. Several questions relative to this circumstance are not ascertained; as, for instance, to say what extent of compression is necessary to produce different degrees of loss of sense. We have many instances where the loss of motion is produced, and not the loss of sense; in Palsy this is more generally the case, and every pathologist has sought for the cause of it. Dr Haller has alleged that more force or a greater degree of freedom is necessary to the active than to the passive powers of the Brain, and that this organ is active with regard to motion, but passive with regard to sense; but he has not attempted to adduce any proof of this, nor said wherein the activity of the Brain consists. I have had occasion to observe, that in the production of every voluntary motion there is an increase of Excitement in the Brain; whereas in the case of sense, there is no particular exertion of the Brain necessary—no special Excitement takes place in it, so that we can understand why sense, which only requires the Brain to be capable of its usual motions, may remain when the power

of motion is entirely gone. Take these little touches of theory by the way, and with the cautions I was just now giving."

"2. I say the Brain gives more or less of Sensibility, as it is more or less *excited*. To explain myself, I need only repeat what I formerly said on the subject of Sleeping and Waking, from which I chiefly take my proofs of the Brain's being liable to different states of Excitement and Collapse, or of Sensibility and Stupor, states which are different from those of being compressed or free. But taking these principles for granted, I say there are intermediate states between sleeping and waking; and the sensibility is greater or less as the tendency to either happens to prevail. We particularly observe such intermediate states in the application of Narcotics, when they produce a degree of drowsiness or stupor; and, therefore, the Sensibility of the Brain may be diminished by the operation of Narcotics and by other causes. There are, however, cases in which our Narcotic medicines have an opposite effect—that is, produce waking and increased sensibility. This is commonly explained by a supposed combination in these substances of a stimulant and a sedative power, and perhaps there is some foundation for this supposition; but I shall shew hereafter that such a combination is not very clearly explained, and that the fact is explicable upon a principle different from that of two powers in the same medicine. I go on to mention another piece of theory that is more evident—that Sensibility is increased by an increased distention of the vessels of the Brain. Nothing indeed is more evident than that the Energy of the Brain, that is, its action in determining the Nervous power into the rest of the system, depends very much upon the fulness and tension of its bloodvessels; and it is, therefore, to me probable, that the degree of Sensibility in the Sensorium will, in some measure, depend upon the same circumstance. It may perhaps be started as an objection to this, that a certain excess of fulness in the bloodvessels of the Brain, seems to

have the effect of destroying sense altogether; and that any preternatural fulness might have, in some measure, the effect of impairing the Sensibility of the Sensorium. But still this will not destroy the opinion otherwise so well supported, that while the motion of the Nervous power remains in a certain measure free, a certain degree of fulness is necessary to the Energy of the Brain, and therefore that a certain degree of it may increase Sensibility. Thus we observe that Phrenitis or inflammation of the Brain, is constantly attended with an increase of sensibility. I need not say that the same reasoning which we use with regard to phrenitis will apply to those fevers that are attended with a remarkable increase of sensibility, for we observe that they also are attended with an increased impetus of blood into the vessels of the Brain."

"3. The degree of Sensibility depends upon the several causes which regulate the degree of Attention that we pay to particular impressions, and which are enumerated in the 47th paragraph of the Physiology *; and these are circumstances which we are not a little concerned with in Physic. The first cause I there mention, namely, the force of impression, I pass over, as not connected with the Brain; the rest of them, namely, the pleasure or pain arising from the impression, the degree of emotion or passion produced, and the emotion's being more or less related to the person feeling it, depend upon states of the Brain. Thus the matter may be viewed in general, but it is worth while to observe that emotions and passions not only give an increased sensibility to impressions in general, but to some impressions more than to others. I shall have occasion to speak of the different tones of mind with regard to courage or timidity, gaiety or sadness; and according as one or other of these tones prevails and is indulged by us, our attention is very much guided, so as to receive such impressions as are favourable to the prevailing tone, as our poet mentions with regard to jealousy—

* Works, vol. i. p. 35.

'Trifles light as air
Are, to the jealous, confirmations strong
As proofs of holy writ.' "

After having considered the different causes of morbidly increased or diminished Sensibility, Dr Cullen next proceeded to consider the Errors or Depravities of Sense, as they arise sometimes from particular states of the Sentient extremities, and sometimes from particular states of the Brain. These he refers to three classes: 1st, The Perception of things that do not exist, or, the supposing things to exist that have no real existence; 2d, The Perception of things that really exist without us, otherwise than they truly exist; and, 3dly, The Perception of the qualities of bodies in an unusual manner.

1. The first class of the Errors of Sense—the false supposition of external existence—is, says Dr Cullen, what we call more strictly Imagination. As an example of Imagination, or false perception, depending on a particular state of the sentient extremities, he related the case of a lady, who, when labouring under Hysteria, imagined that she was surrounded with goblins and spectres, which followed her from one part of the room to another, but which were found to disappear upon covering one of her eyes. As other instances of false perceptions referrible to this class, Dr Cullen enumerated the appearance of lucid stars produced by shutting the eye and rubbing it—the Muscæ volitantes which patients sometimes see floating in the air, in the shape of the darting of lightning, of lucid ribbons, &c.—the peculiar sounding in the ear, termed Tinnitus Aurium, and similar deceptions with regard to the other organs of sense.

" I will add," says he, " another illustration of the power of Imagination in exciting the same state of a part that was previously produced by an impression made upon it. If you tickle a man's upper lip with a feather, you may throw him into convulsions ; and, if you have done so once or twice, you need only, the third time, approach him with the feather —for, before you come within an inch of his lip, he feels the same tremor in it as before; so that, from the influence of Imagination, he has this false perception, that he thinks the feather touches his lip when it does not.—Another instance of Imagination or false perception, is the noted case of amputated limbs, where the sensation is referred to toes or fingers that no longer exist.—And another case that may be referred to the same class, is when sensations are referred to other parts than those on which the impression is made. Thus, we make a false judgment with regard to the place upon which the impression is made in calculous complaints; for here an impression made upon the neck of the bladder causes pain at the orifice of the urethra. These last cases, however, are different from the cases of Imagination previously mentioned in this respect, that though they are false judgments with regard to the place of impression, they are not so with regard to the place to which the sensation is referred, as we presume that there is a motion actually existing in that part and giving occasion to the sensation."

In attempting to ascertain some general principles to enable us to decide whether particular instances of Imagination or false perception depend upon the state of the Sentient extremities of the Nerves, or upon the state of the Brain, Dr Cullen remarks, that—

" Whenever the Sensation is of a very simple kind, as the starry light which arises from rubbing of the eye, or the muscæ volitantes, or when the false hearing is a simple tinnitus aurium ;—in these cases the false perception may depend upon the state of the organ, and in the case of the eye

we can sometimes explain it. But whenever the perception is of a more complex kind, where, for example, we not only discern colour but complex figures, &c., few of these false representations depend upon the organ; and, I say for certain, where the perception is complicated and takes in different organs,—as when we not only fancy that persons are speaking to us, but that we see them,—it is a false state in two or three organs at the same time, and the presumption is, that such erroneous representations or false perceptions depend upon the state of the Brain. My reason for supposing that such suppositions of false existence depend upon the state of the Brain, is that, for the most part, we can perceive an organic affection of the Brain present at the same time; thus, there are few instances of Mania but where, upon dissection, we find some organic disease of the Brain, and in other instances there is a special and singular affection of the Brain for the time. It is proper, therefore, to consider the state of the Brain as chiefly concerned in producing the supposition of false existences."

With regard to the manner in which those causes operate, which by their action on the Brain give rise to imaginations, or false perceptions, Dr Cullen conceived that it must be by producing such a state of this organ, as does not admit of a proper communication between its several parts, and therefore does not admit of the mind's recovering the several relations that are necessary to determine the identity or presence of any object.

"Our ideas," he remarks, "are for the most part complex, but we shall find that in the case of false perceptions they are commonly not accompanied with all the circumstances that usually accompany them; and that several circumstances, which should check them by determining the presence or absence of objects, are not at the same time applied. Thus, in the Berlin Memoirs, we are told of a per-

son who had received a contusion and wound on the head, by which he was rendered speechless and insensible; but he heard distinctly those around him mentioning to what depth the wound had reached, and what was supposed by the Surgeons to be the state of the Brain. As he felt no pain, he never thought of applying what they said to himself; but imagined they were talking of some other person. Here, I say, the patient, from not having a feeling of pain, had no means of establishing the full relation of what they were talking about. There are many other instances which prove that it is not enough to have a few parts of a complex idea, in order to judge properly, but that we must have the whole of its relations. We have frequent illustrations of this in the case of Delirium; a person in a fever is kept from light and noise, and therefore the imagination that arises in his mind has no circumstances to correct it. Thus, if he imagines that he is not in his own bed, he has no images in his chamber but what his imagination supplies, and as these are of another chamber, he is in the right. But upon letting in the light, he sees his own bed, &c. and he recovers himself, and I have tried the experiment upon myself. Now, there is no more in this than that the patient wants the several external circumstances that should enable him to ascertain where he actually is. This error, therefore, evidently depends upon the want of a great many parts of the complex idea—on the want of the external circumstances that serve to determine the identity of the several objects."

2. With regard to the second class of the Errors of Sense, where the supposition of the external existence is well founded, but we judge of it otherwise than it really is, Dr Cullen remarked, that—

"If we admit the philosophy of Bishop Berkeley, we say that the whole of the perception of external existence is purely ideal, is purely an operation of the mind, and that the supposition of body is altogether false; and even the Scep-

tics who do not carry the matter so far, allege that we have no certainty that in any one case we perceive external things such as they really are. We can condescend, it is true, upon many instances of our mistakes in this respect, and from these we may extend the possibility of our being in error, to the whole; but we are not to be disturbed by any subtilties of this sort; it is enough that by the constitution of our nature, we unavoidably suppose and conclude the existence of external objects, and that we do distinguish them in different cases by our different senses. This is the unavoidable practice of the human mind; and it is enough to say that our perceptions are erroneous, when we perceive at any time in a different manner from what we do at almost every other time, or when the perceptions of one sense do not correspond to those of another. Most of the False judgments which we make with regard to things really existing, probably depend upon the state of the organs of Sense, but they may also in part depend upon the state of the Brain. We consider them to depend on the Brain when there is a want of steadiness in viewing objects which makes us hesitate about their reality; or when we see more vivid colours, as when in fevers, the eyes throw a red colour upon every object; or when a well-known figure appears to be distorted. In like manner, False judgments will arise if the causes operating upon the Brain in producing false perceptions have impaired the powers of reminiscence and memory, in consequence of which persons may feel the want of several circumstances that are necessary to ascertain the identity of objects, and to give the proper complex idea. It is certain that the state of the Brain may be such as to affect the Sentient Extremities, and give occasion to Hallucinations, so that we become uncertain with regard to the number, distance, &c. of objects; and I myself, when liable to any degree of feverishness, see every object as through a telescope,—every thing appears as if it were fifty times the distance from me that it is, and as

if it was diminished in proportion, which certainly depends on a state of the Sentient Extremities communicated to them from the Brain."

3. "The third class of Errors of Sense are those in which we perceive the qualities of bodies in a very unusual manner, or in which these qualities have a very unusual effect upon us. I say the qualities of bodies affect us in a very unusual manner when our judgment of these qualities is totally changed; as, when we consider Assafœtida to have an agreeable odour, and the odour of Musk to be remarkably disagreeable; instances of both of these occur, but they are certainly contrary to the most general state of sensation among mankind. In the same way, when a small portion of the white of an egg —which is a bland, mild, substance—on being taken into the stomach of a particular individual, produces violent pain and other disorders, this may be reckoned among the unusual effects of substances applied to our nerves. Many of those singularities mentioned by physicians under the title of Idiosyncrasies, are of the same kind—are to be considered as Errors of sense, and have been called Depravities of sense. As to the nature of that peculiar condition of the Sentient Extremities, or of the Sensorium upon which idiosyncrasies depend, I can say," observes Dr Cullen, "that we know nothing of the matter. I have said that we know of no necessary connexion between Impression and Sensation, and that, for aught we know, the substances which act upon our smell, might have been made to give the idea of colour; and it will be difficult to say when the condition on which idiosyncrasies depend, is in the Nervous Extremities, and when it is in the Sensorium; we can only say, that the one is probable in one case, and the other in another case."—"You may, however, take this means of judging of it; where it is a pure and single idiosyncrasy with regard to the person and part of his body, we say it depends upon a certain modification of the Sentient Extremities in such a part; but where it

is connected with the whole of the temperament, or with a certain disease, or is the immediate consequence of a disease, I would suspect that it depends upon the state of the Brain; as when, in an hysteric woman, the odour of a rose is disagreeable and that of assafœtida is particularly agreeable; and where this change of the agreeable and disagreeable, for instance, pretty constantly takes place in a certain temperament, or when perhaps hysteric symptoms are present, I impute it to the state of the Brain."

II. *Morbid conditions of the Intellectual Faculties.*—Though Dr Cullen did not deem it necessary to dilate, in the physiological part of his course, upon the operations of the Intellectual Faculties in the sound state, but contented himself with referring his pupils for information on these subjects to the writings of Metaphysicians, and more especially to those of Mr Locke, of whom, if not an implicit follower, he was at least a great admirer, he took an opportunity, in his lectures on Pathology, to enter pretty fully into the consideration of the Deranged states of the intellectual faculties, under the two heads of Imbecility and Error, and of the several corporeal causes by which these two states seem to be produced.

"The first, Imbecility," Dr Cullen remarks, "is also called Fatuity; where the mind has not the power of retaining impressions, nor of recalling others that have a relation to them, which is the foundation of all our judgments. In Error, or Delirium, there is always some partial affection of the Brain, rendering it unfit for the communication of the various relations and associations of our thoughts." "Gaubius considers all the *Læsiones Sensuum Internorum*, or deranged states of the Intellectual Faculties, under the general title of Delirium, but with no great propriety. He is sufficiently aware of the different morbid states of the intellect as

they might be distinguished, and he does mention them in the course of his work; but it is a little incorrect to comprehend the whole under the title of Delirium. I say there are two distinct cases of derangement of the Intellectual Faculties, which I mark under the titles of Fatuity and Delirium. Fatuity is imbecility, weakness, or imperfection of thought and judgment. In this state the feelings of the mind are imperfectly excited, and still more imperfectly retained. From the first circumstance the relations of bodies are not marked, and from the second they are not recollected; so that there is but little, if any, exercise of thought, of judgment, or of will, as Judgment consists in viewing the relations of our perceptions, and the Will depends on the judgment we form of these relations: there is therefore a certain weakness in the exercise of thought that is to be distinguished from the errors or false exercise of it, which alone are properly called Delirium. With regard to this fatuity, we may observe that there are two different cases of it, the Fatuity of *Infancy* and the Fatuity of *Old Age*. We must imagine, since these do very constantly occur at particular periods of life, that they must be connected with some particular state of the cerebral organs for the time."

"In his definition of Delirium," continues Dr Cullen, "Gaubius has pointed out three modifications of this state, which correspond with False Perception, Erroneous Judgment, and Irregular Passion; the three principal operations of Thought being, Perception, Judgment, and Will, the Delirium is distinguished as it consists in error of one or other of these. The first kind of Delirium I have sometimes called *False* Imagination, in complaisance to common language; but in the Physiology I have said that it is strictly called Imagination, when sensations that usually arise from external objects arise without the presence of these."

The second kind of Delirium, Error in Judgment, Dr Cullen endeavoured to explain in the following manner:—

"Judgment depends upon memory—upon the power we have of recalling the ideas of a great number of things that are for the time not present; but Memory depends upon associations of certain kinds, and particularly in a certain order, and we must trace the relations of bodies through a great number of intermediate ones. If, therefore, when certain ideas are presented to us, we cannot follow the usual order of associations, there will be a want of intermediate ideas, and consequently there will be an erroneous judgment with regard to them; the person will, from the want of the *affines* or intermediate ideas, separate such as are nearly related, or compound those that are quite distinct, so that in this case his error of judgment will depend upon some fault of the memory chiefly. But the error of judgment will be still more considerable, if not only the ordinary train of our associations is disturbed, but at the same time the imagination so far prevails as to obtrude upon the mind ideas that do not belong to the train we are pursuing. You will, therefore, perceive wherein Erroneous Judgment chiefly consists,—in disjoining ideas that ought to be joined, or in joining those that ought not to be joined. It may, therefore, be referred to the heads of *incoherence* and *inconsistence*, with regard to the proper connexion of ideas. But there is in this respect, in the marking of relations, and in disjoining or separating ideas, a considerable difference even in men in health. The objects about which men are conversant are for the most part the same, and there is so much similarity in the operations of the human mind, that the same relations are marked; there is thus a sameness of judgment established among different men, in which they generally agree, so that we are most secure in ascertaining an error of judgment when there is a considerable deviation from the common sense of mankind; and such a deviation may then be considered as a disease. But there is a certain latitude admitted in judgment, —men may differ greatly from one another in their judgments, without any of them being reckoned delirious, so that,

to constitute delirium, we must say further that the person is also deviating from his ordinary judgment, and from his usual train of thinking. I perceive that all this might be prosecuted with some curiosity, as to inquire what is meant when men speak of a weakness of mind, without there being delirium; when they explain errors in judgment by calling them mistakes and blunders; when they distinguish between wrong-headed and wrong in the head; and when they distinguish between whim, folly, and madness. These distinctions might have some curiosity and use, but they are matters of too much subtlety, and it is enough to leave them to common apprehension."

"The third manner in which Delirium appears is an Erroneous Will—that is, when the Emotions are not suited to their objects or proportioned to them; when they arise in a manner that is either uncommon to mankind in general, or that is unusual to the person who is the present subject of them. There occurs a difficulty in settling the limits between that folly and vice which it is the business of the Moralist and Divine to correct, and the delirium and madness that is only to be cured by the art of the Physician. We may observe that emotions and passions not proportioned to their objects will readily follow the state of delirium last considered, viz. errors of judgment. Errors in judgment and in sensation frequently produce errors in emotion and passion; but the sensation and judgment being given, the emotion may be rendered different from other circumstances, as from the Tone of mind that prevails in the man for the time. I have taken pains to shew that the mind is in different tones in this respect, that there is one tone of mind which appears in gaiety, and another in sadness; one which appears in irascibility, and another in timidity. Now the causes of Madness more frequently depend upon these tones of mind than upon errors of sense or judgment, and it is sufficiently evident that irregular emotions or passions seldom occur, ex-

cept when a particular tone of mind is joined to the false perceptions and to the errors in judgment."

In inquiring into the circumstances on which the different Tones of mind depend, which exert so much influence on the degrees of the emotions and passions, Dr Cullen observed that—

"The same Vigour and Debility of the Brain, which have effects upon the whole of the body, have also effects upon the Mind in producing its opposite states, such as those of Courage and Timidity, of Joy and Sadness, of Hope and Despair, with a variety of other states that can be thus opposed and contrasted. Not only do these states occur occasionally, but there are in certain men, through the whole of life, or at least through a great part of it, manifestly dispositions on the one hand, to Courage, Joy, Gaiety, and Hope, or on the other, to Timidity, Sadness, Seriousness, and Despair; and when these dispositions subsist and make a part of a temperament, we call it a certain Tone of mind, and these tones are produced by moral causes and by physical, as by states of the body. Some late philosophers, such as the Abbé du Bos, have maintained that the different states of genius and taste that have prevailed in different ages (as for example in the reigns of Augustus and of Louis XIV.), have not depended on the state of the arts, or on the munificence and liberality of Princes, &c. but have been owing to the operation of certain physical causes in giving men more taste,—which is to be sure sufficiently extravagant. Others impute these diversities in genius and taste, partly to moral and partly to physical causes; but there is an Author,* on the Character of Nations, who has maintained that physical causes have no share in producing them, and that they depend solely on moral causes. Now it belongs to physicians to consider and determine this question, and I

* Mr Hume, Essays, Moral, Political and Literary; Works, Edin. 1825, vol. iii. p. 224.

shall endeavour to give you a sketch of it, with regard to the states of Courage and Timidity in different men."

"A state of Courage may be owing to the want of a sense of danger, in consequence of which people go on headlong, like the blind mare in the mire, as the proverb says. Another state of courage is produced by the power of habit. The courage of the sailor and soldier, for example, is to be explained in no other way, but by their having the impressions of danger so often repeated that they lose their force. We have instances of the same thing in other parts of human life. Let us observe the difference between the inhabitants of a rude and uncultivated country, and those of a civilized and polished nation, in respect to the general character of Timidity and Intrepidity. The inhabitants of the former are obliged to walk along the narrow brink of the precipice, to stem the foaming torrent, and to encounter every vicissitude of the weather and season,—whereas in the cultivated country if the inhabitants are obliged to travel, roads are prepared for them; if they pass by a precipice, it is guarded by a parapet; they cross the river by a bridge, and are guarded against the more severe attacks of the weather: they are therefore liable to a great degree of timidity, while the others become hardy and bold. The contrast is equally remarkable, if we compare Women in a state of simplicity and in a state of refinement. Among the more rude nations they are obliged to take part in all the hardships of the men, and meet with less indulgence from the men who are of less delicate feelings; while in the refined state, they are carefully protected and guarded against every thing that may hurt or even shock them. And accordingly in the one state they will shew timidity in excess, the smallest troubles will throw them into fits; while, in the other state, such appearances are unknown. A third state of Courage may arise from the fear of any particular event being counteracted by the fear of a greater evil, as the fear of death by the apprehension of infamy; and in most animals courage may be produced by strong emotions,

as by those of love and anger. Now, these are so many moral causes of courage; and it has been commonly alleged that courage, as depending upon such causes, is always artificial, and that intrepidity is in no case natural to man. I am disposed, however, to think, that intrepidity is a natural consequence of the state of vigour in the body, and that the state of debility, on the contrary, is a cause of timidity. Thus, in the Brute creation, if you compare the sexes, which differ in regard to their vigour, the courage is confined to the male. It may sometimes exist in the female, when the whole tenor of life requires an equal exertion of vigour on her part as on that of the male; but when courage appears in other females, it is of the artificial kind, —it arises in consequence of emotions of love or anger. The influence of the state of vigour on the state of courage can be the more clearly proved in the brute creation, from the courage being greater or less according to the treatment of the animal in regard to exercise and diet. Whoever understands the breeding of game-cocks, will know that diet, exercise, and practice, whilst they produce vigour, produce a proportional degree of courage; and that the exertion of a cock —his state of courage as well as his force—depends upon his state of weight; and the same animal which, when half fed, will appear timid, will, when well fed, have an insurmountable courage. But, even in the human species, there is hardly any man who has felt the vicissitudes of vigour and weakness of body, but has felt that the mental feelings of resolution and fear do accompany these corporeal states, as in the case of sickness and health. I have been in use to quote here the unusually discerning Shakespeare; who makes Cassius disparage the mind of Cæsar, by pointing him out when lying on a sick bed, and whining like a sick girl,—

'Alas!' he cried,
'Give me some drink, Titinius.'

It is, therefore, a state of the Brain which produces those states of mind on which courage and timidity depend; and

these tones of mind may therefore be considered as marks of the state of the body."

After having attempted to point out the different forms of Delirium, Dr Cullen proceeded to inquire into the causes of that state. These, he conceived, may be reduced to the three following:—1st, What may be strictly called a different state in the condition of the Brain; 2d, A different state of the circulation of the Blood in the Brain; and, 3d, The effects of various stimuli applied to the Nervous system—to the Brain, either directly, or through the intervention of some other parts of the system.

"From observations made by dissections," he remarks, "we can ascertain the action of all these different causes in the production of delirium. The Brain is certainly in different states, according as a person is awake or asleep, that is, it is in the different states of thinking fully and completely, or of not thinking at all. I know that this last proposition will be disputed by many who are fond of maintaining that the mind always thinks when we sleep, but it is not necessary to enter into that discussion here. It is enough to our purpose, that, in Dreaming, which is the state of thinking in sleep, the mind does not think with regularity—that it does not think to the purposes of life—that it approaches to delirium; so that there is a considerable difference in the thinking of the mind when the body is awake and when it is asleep. These states of the Brain, I have said, may be called the states of Excitement and Collapse, by which terms I do not point at any theory; I employ them merely to express matters of fact, that in that state of Collapse or diminished energy of the Brain, which takes place in sleep, the mind does not pursue its usual or ordinary train of thinking—such a train as is necessary to sound judgment. We have, in Dreaming, false perception, incoherence, and inconsistency of judgment, which again,

are all the marks of Delirium. In whatever way it is to be explained, we observe, in fact, that between the states of sleeping and waking, there is always delirium, and in the passage from the one state to the other, when the delirium occurs, there is an unequal state of the Brain; one part is still excited, while another part is under a Collapse. This is the foundation of Dreaming, and therefore the foundation of Delirium."—" I have thus brought the subject of Delirium to a general principle, to a want of communication between the several parts of the Brain, or at least to an unequal communication of cerebral influence, and this may arise either from organic affections, or merely from some difference in the states of Excitement and Collapse which are connected with the states of Sleeping and Waking. This opens up a particular view; it does not leave the whole of the Causes of Delirium resting upon the causes which Gaubius has assigned —some considerable change in the state of the organization of the Brain;—and it points out that very considerable degrees of Delirium, some of the most violent instances of Mania, can be relieved, and the mind restored to its former state, almost as easily as Sleeping and Dreaming can be put a stop to, and the person restored to a state of Waking."

III. *Morbid conditions of the function of Motion.*—" As it was proper," observes Dr Cullen, " to consider the healthy action of Muscular fibres, with regard to the *Force* and the *Facility* with which they contract, so it is necessary to consider their morbid states likewise, in these two points of view—that is, in regard to what I have termed their *Vigour* and their *Mobility*. These two circumstances are, indeed, variously combined, and in many cases it is extremely difficult to mark how far they are combined or separated. But we frequently observe, that the Facility with which the contraction of Muscular Fibres can be excited, is increased in the same proportion as the Force with which they are contracted is diminished; and upon that ground they ought to be distinguished by particular terms. The excess

of vigour I name Increased Tone; and the defect of vigour I call, with every body else indeed, Debility of the Muscular fibre; on the other hand, the excess of mobility I name Irritability, and the defect Torpor;"—terms which obviously correspond with the *Irritabilitas nimis acuta, irrequieta seu pruriens*, and the *Irritabilitas deficiens, stupor seu torpor* of Glisson. "I begin," continues Dr Cullen, "with considering the two opposite states of Increased and Diminished vigour. Dr Whytt has alleged, that 'a greater degree of that power in the nerves which is necessary for motion, can only give more force and steadiness to the muscles when they all possess it in an equal degree; the increase, therefore, of this power is hardly to be accounted a distemper *.' Perhaps, when the vigour is increased *equally* over the whole system, and therefore is suited to the general tenor of the economy to which it belongs, it is not to be considered as a disease, but as the most perfect state of health. But I think Dr Whytt has forgotten to observe, that if this vigour is in any respect *partial*, it certainly does constitute a disease; and I say that even when it affects the whole system equally, increased vigour is a *Seminium*, diathesis, or predisposition; and predispositions, when they amount to such a degree as to expose certain persons to be acted upon more readily than others by occasional causes, constitute, universally, parts of the proximate causes of diseases. With regard to *partial* excess of vigour, it is not easy to establish it clearly, but I think we have an instance of it in the *Diathesis Phlogistica*. From the causes and remedies of this state, and several circumstances attending it, I conclude that it consists in an increased tone of the arterial system, and there is no doubt that, in this instance, the excess of vigour is partial, for commonly there is a diminished tone of the other muscles, particularly of those of voluntary motion, and often also of the muscular coat of the alimentary canal. Here, therefore, is an instance of partial excess of vigour constituting a

* Works, 4to, p. 527.

disease; and though, when the vigour is increased proportionally over the whole system, as in the case of Mania, this circumstance does not of itself constitute the disease, it certainly makes a part of it; and in other cases, it is evidently a *seminium* or predisposition, and thereby becomes a part of the disease. Thus, vigour of the general system does dispose men to have inflammatory diseases produced in them by the action of cold and of other occasional causes." "Of the several causes which can increase or diminish the Vigour of the voluntary and involuntary muscles," Dr Cullen observed, "we know a great deal already from experience and observation. But the theory has not been attempted, and therefore there remains a number of circumstances relative to the subject which we do not understand. In attempting to elucidate this subject, I hope to give you matters of real use and application."

The different states of the Vigour of the Muscular fibres, may, it appeared to Dr Cullen, depend on the following circumstances:—1*st*, On the state of the Simple Solids; or, at least, he says, it is intimately connected with this. 2*d*, Upon the Bulk of the muscle. 3*d*, Upon the state of Tension of the muscular fibres. 4*th*, Upon causes of Excitement or Collapse, applied directly to the moving fibres themselves, including Exercise. And, 5*th*, Upon the state of the Brain.

1*st*, With regard to the dependence of the vigour of the moving fibres upon the state of the simple solids, Dr Cullen remarked—

"It is certain that in comparing animals of different sexes, and of different temperaments when of the same sex, the energy of the Brain being given, the vigour of the body corresponds in some measure to the density and rigidity of the simple solids. The simple solids are more rigid in the

male sex than in the female, in the melancholic temperament than in the sanguineous; and in wild and furious animals than in those that are domesticated."

"*2d*, In men of the same stature the strength is manifestly as the bulk of the muscles. Painters know this very well, and express their Hercules by the swelling they give to the several muscles; and nothing serves to prove better the connexion between the vigour and the bulk of the muscles, than that when exercise gives an increase of strength, it at the same time always gives an increase of bulk in the muscles. We had many years ago, in this country, a very strong man of the name of Topham, whose feats of strength were mostly performed by his arms, whilst his legs were little employed. I, along with several anatomists, examined him; and we found that while his legs were soft, and rather resembled those of a woman, the bulk of every muscle in his arm was very remarkable."

3d, With regard to the dependence of the vigour of the muscular fibres on their state of Tension, Dr Cullen remarked, that

"The principal means of Tension in our system, is that which is given to the arteries by the blood propelled into them, and this can be communicated to every fibre of the system. The Tension of the arterial system is absolutely necessary and constantly operates, and this is the foundation of that aphorism of Sanctorius, *pondus addit robur*, that the body is stronger in proportion to its fulness; and hence the difference of force in different men according to the quantity and quality of their diet. Hence it is, too, that evacuations weaken the system."

4th, In considering the different states of vigour as depending upon causes of Excitement or Collapse applied directly to the moving fibres themselves, Dr Cullen remarked that the same difficulties present themselves as in judging of the influence of these causes on

the Sensibility, when they are applied to the sentient extremities.

" The operation of Narcotics and Sedatives in *diminishing* the vigour of the moving fibres is sufficiently obvious, but we do not clearly perceive any agents operating in this way as causes of *increased* tone. With regard to Stimulants, we do not perceive any thing more than their operation in producing contraction; we do not perceive that they produce any increase of vigour or irritability, but these being given they produce contraction. But, as I said before in regard to Sensibility, it is in general probable that there are powers of Excitement as well as of Collapse; and that of the Stimulant powers, such as foods and drinks, some are powers of Excitement, especially with regard to the Moving fibres. But however it may be with Stimulants, there are other powers that seem to increase the tone of the Muscular fibres, such as those we call Astringents and Tonics."

" *5th*, The Vigour of the Moving extremities will be different according to a difference in the state of the Brain, which may be communicated to them. In the case of Voluntary motion, we, as I think very speciously, allege that in every contraction of the muscles, an action of the Animal power or Energy of the Brain constantly takes place; but in what are called the Involuntary motions—whether they are originally involuntary, or are so in consequence of habit,—it has been supposed that no such exertion of the Animal power occurs, but that the contraction is in consequence of a stimulus acting upon the Inherent power, which is agreeable to the common doctrine of Dr Haller. I have already pointed out what limits this doctrine requires. I have said that the Involuntary muscles are likewise under the influence of the Brain, and depend upon its constant Energy, and that this is proved, not only from various stimuli applied to the Brain, having a considerable influence upon the action of every Muscular fibre, but from the muscular fibres being liable to Palsy, in consequence of compression of the Nerves: so that we infer

that the Inherent power of every muscular fibre is constantly dependent for its state of vigour upon the Energy of the Brain; and, consequently, that the Inherent power, and the vigour of muscular fibres, are greater or less, according to the state of the Brain, communicating its power constantly or occasionally. Thus, in the case of Mania, where the tone is increased to a considerable degree; and in the case of Fever, where it is as remarkably diminished, we seek for the causes of both only in the present state of the Brain."

"There is no doubt, therefore, that the vigour of the body is dependent upon a state of the Brain either occasional or constant, and we consider this condition of the Brain as making at first a part of the temperament, as something constant, and we say that it depends upon the state of the Original Stamina, or primitive organization. I need not here apply the proofs of it; whatever is the tenor of the whole course of life must depend upon an original conformation, or, according to the language which I use, upon original stamina. In the next place, I say, that the state of the Brain on which the vigour of the body depends, does itself depend upon the state of the Simple Solids. In so far as I have rendered it sufficiently probable that there is a particular connexion between the state of the Medullary fibre and the state of the simple solids, this must be the case. The state of the Brain will depend, too, on Exercise; as that strengthens the simple solids, so it may strengthen the moving fibres themselves, and this increased strength may be communicated to the whole Nervous system. It also depends upon the Temperature to which men and other animals are accustomed; a certain degree of heat, approaching to excess, relaxes and weakens the system, and a certain degree of cold invigorates and strengthens it, and may, therefore, increase the vigour of the Brain. Another cause influencing the state of the Brain may be assigned, which is more exactly applied to the Brain itself, viz. that it depends upon a certain ten-

sion of the bloodvessels of the system, and upon a certain Fulness and Tension of the bloodvessels of the Brain. This appears in the case of Deliquium coming on in consequence of the erect posture, and if the influence of this cause appears in this occasional way, there can be no doubt that, to a certain degree, it may operate through the whole of life. At the same time, it is proper to remark, that we have many proofs that the tension of the system depends upon certain circumstances, which we cannot easily refer to the tension of the bloodvessels. Thus a man with a moderately full Stomach, is capable of more vigorous exertions than when it is entirely empty. The distention of the stomach has indeed some effect in extending its own vessels, and in allowing them to take in a larger quantity of blood, but this will not explain the tension of the bloodvessels of the system in general, and we may suppose this to depend on a Nervous communication and a Nervous tension. The same is the case in regard to the Genital organs. I will not deny that the differences in the state of these organs at different times, may depend upon the fulness of their bloodvessels; but allowing that this serves to produce the appetites belonging to them, it will not be easy to say why it should give a considerable degree of vigour and tension to the whole system. We see that the state of the genital organs in men is connected with more or less fulness of the Vesiculæ seminales, and it is not easy to shew how that depends upon the Sanguiferous system; and we have many observations which shew that the state of the genitals in females, produces a variety of nervous disorders: we find also, that unless the genital system is evolved, the vigour of the body is less, and eunuchs, *cæteris paribus*, are weaker than other men. There is, therefore, a balance of the Nervous system in which the state both of the Stomach and of the Genitals has a considerable share. I can trace it no farther, but undoubtedly in the whole of the Nervous system, its Energy being more vigorous or weak depends greatly on the state of

vascular tension. Lastly, I am ready to allow that the vigour of the Brain, and of the system depending upon it, may be increased by the application of certain Stimulant powers producing contraction; and we not only have contractions produced in consequence of the application of Stimulants, but these contractions are more vigorous than they otherwise would be. Thus Diet is a stimulus applied to the Nervous system; and as present in the stomach, the difference of food gives more or less stimulus; and wine as a part of diet supports the vigour of the system to a certain degree, and may be considered as a cause of increased vigour."

"The causes of Debility of the Moving fibres, that operate through the medium of the Brain, are the converse of the causes of the state of vigour already mentioned. Thus, I mentioned exercise as a cause of vigour—as improving the vigour of the inherent or contractile power of the muscular fibres, and the Energy of the Brain; so indolence is a cause of debility. Another occasional cause of debility, is the withdrawing of blood from the vessels of the Brain; and as the vigour of the Brain depends upon the state of tension in the whole of the system, debility may be produced by the want of nourishment, by evacuations, by relaxation, and by the absence of habitual stimulants. I formerly said, that, among the causes of increased vigour, the application of stimulants may be admitted; and when these are daily applied, as in the case of wine, the system comes to depend upon them for its degree of tension, and, therefore, the absence of these has a remarkable effect in inducing debility. Thus, in the case of persons who use the habitual stimulus of a dram, when that stimulus should have been renewed, what a sense of debility, what tremor occurs, till the tension is reproduced by the application of the dram to the stomach. There are causes of Collapse, which, in like manner, induce debility, as certain sedatives, or, as we may call them, Poisons. Thus, a man intoxicated with wine, or opium, has not his usual vigour; and though the quantity may not be sufficient to kill,

it leaves the body under a considerable degree of debility. In like manner, the excess of heat or of cold produces debility. We have most experience of the effect of the excess of heat in this respect; but we also observe, that there is not that extreme vigour in the colder seasons as in those of a more temperate kind.

"I may here mention likewise the causes of Fatigue, though I cannot explain them. We know that a very frequent repetition of certain actions, or an unusual exertion in regard to force, if continued for too long a time, will induce fatigue, which is manifestly a state of debility. There is one easy explanation of this that has been proposed—that a certain quantity of animal spirits, secreted in the Brain, is poured into the muscles at every time of contraction. What becomes of these spirits has not been said, but it has been supposed that they are wasted or exhausted; and, according to this hypothesis, we can understand, why a certain amount of exercise should produce debility. But you will remember that I have taken a great deal of pains to shew, that no fluid, concerned in muscular motion, is secreted in the Brain, and we must seek for some other explanation. We may, I believe, find it in this, that such is the constitution of the Nervous system, that every unusual degree of Excitement is followed by a proportional degree of Collapse,—that such is the natural course of the animal economy, so that we can see the necessity of the alternate states of action and rest, and we presume that they induce one another." "I leave you," continues Dr Cullen, "to consider this explanation at your leisure. It is with the fact that we are especially concerned, with the theory much less; but the question applies to an important consideration in Pathology, How is the debility of fevers occasioned, and how is it continued? It may, perhaps, in some instances, be considered as the effect of the contagion, and referred to the head of poisons acting as sedatives; and there are many circumstances in the doctrine of fevers which lead to that supposition; but, on the other

hand, there are inflammatory fevers, produced by the operation of cold, without any degree of contagion concurring, which, nevertheless, are accompanied or followed by debility. Indeed most purely inflammatory diseases, in proportion to the degree of fever accompanying them, leave a state of debility subsisting in the general system for a long time after ;—and in cases in which the introduction of a contagion into the system may be regarded as the first cause of the debility, when this is seemingly entirely removed, and its symptoms have passed away, but some circumstances have occasioned a repetition of the paroxysms, a debility is induced that sometimes proves fatal. If we are to admit of any such explanation in the case of the debility of fever, it must be the same as in the case of debility induced by exercise,—that the repeated excitements do at length wear out the vigour of the system. To conclude this subject I must add, that Compression of the Brain may entirely destroy the power of the muscles; and there is no sort of doubt that it operates in producing all the intermediate degrees of Palsy, Atony, and a mere degree of Debility."

After having pointed out the different causes which produce an increase or diminution of the Vigour of the moving or muscular fibres, Dr Cullen proceeded, in the next place, to consider these fibres with regard to their greater or less degree of Mobility.

"I need not say," he observed, "that this view of the moving fibres has occupied Pathologists much more than that of their vigour, but the subject has been treated imperfectly. It has been common to consider every convulsive or spasmodic affection as an instance of increased mobility; now, in many instances, the spasm or convulsion produced is the effect not of the greater mobility of the moving fibres, but of the strength of the stimulus applied to them, which may be capable of producing this effect in any person, though of

course it will operate the more readily where there exists a certain degree of mobility."

The causes of Increased Mobility, or of what he calls morbid Irritability of the Moving fibres, mentioned by Dr Cullen, are, 1*st*, Increased Sensibility; 2*d*, A state of Debility in the Moving fibres; 3*d*, Such causes as give Excitement greater in Elasticity than in Density, including Heat and Tension; 4*th*, Habit; and, 5*th*, The state of the Brain.

1. "Increased Sensibility and increased Mobility," he remarks, "are so commonly conjoined, that the state of increased mobility has been considered, though improperly, as being the same with that of increased sensibility; but it is necessary to remark, that increased sensibility is not the sole cause of increased mobility; and that they may and ought to be considered as different affections. Sensibility and Irritability are not always in the same condition in the same person. I conclude this from observing, that these two properties are often under different laws. With respect to Sensibility, it is well known that the force of impressions in exciting sensations is by repetition constantly diminished; whereas, by a like repetition of motions, the readiness with which these motions are repeated, the Mobility, or what may be called the Irritability of the parts, is generally increased. In certain cases indeed, where motions are frequently repeated in consequence of the application of the same impression, sometimes the one of the laws mentioned takes place, and sometimes the other; so that sometimes to produce a repetition of the same motion, the force of the impression employed must be gradually increased; and in other cases the motion may be repeated, though the force of the impression be gradually diminished. These are cases with which physicians are well acquainted; but in what circumstances the one or the other law takes place, I cannot with certainty determine."

2. "That the Mobility of children and women is greater than that of adult men, may depend on their having more Sensibility as well as upon their constitutions being weaker; but there are many instances of persons weakened by occasional causes, as by fatigue, evacuations, preceding diseases, &c. where the Mobility is very evident, while there is no observable change in the Sensibility, so that we infer that the state of debility in the Moving fibres is a cause of mobility."

3. The third cause of increased Mobility which Dr Cullen assigned, viz. such causes as give excitement greater in elasticity than density, he acknowledged to be a subtile and theoretical one.

"Here," says he, "I assume the hypothesis of an ætherial or electrical fluid in the Nervous system, but if it explain the phenomena you may indulge it. I mentioned an increase of heat as giving an increase of Sensibility, and it is equally probable that it is as remarkable a cause of increased mobility as it is of increased sensibility; and it certainly operates upon elastic fluids by increasing their elasticity, while it diminishes their density."

4. "A fourth cause of increased Mobility is to be found in the effects of Habit. This operates in consequence of the law mentioned in the 114th paragraph of the Physiology, which is, that the repetition of motion gives a greater facility of motion—that is, gives mobility or morbid irritability. I have once and again mentioned the illustration of this law in distinguishing Sensibility from Irritability. I have to add a matter of more curiosity—that motions frequently repeated often put on the appearance of being spontaneous; that is when they are associated with sensations or with motions which, by the laws of our system, return periodically. In explaining the subject of habit formerly, I took notice of this; and in that way particularly, I explained how the daily revolutions in our system are produced, and how various other

circumstances will continue to recur in consequence of an association with other things that do occur in a diurnal or periodical circle. With regard to the application of this law to Pathology, it serves to explain how several diseases that depend upon a state of irritability, or increased mobility, as Epileptic, Hysteric, and Asthmatic affections, become periodical. The power of habit in giving such laws, in producing periodical revolutions, is especially remarkable in diseases that are most certainly periodical, such as Intermittent fevers. It appears from the most exact observations, that Intermittents continue their disposition to recur at the usual period even when no paroxysms take place, so that though they should cease for a fortnight together, when they do recur, it is not only upon the day but at the hour they were in use to do so. I must add another observation that is of use and curiosity, that such is the apparent spontaneity with which motions that have been frequently repeated take place, that the slightest associations serve to renew them, and even the recollection of them is sometimes sufficient to bring them back. Thus, a person who has taken Ipecacuanha and vomited with it, though he cannot recal the particular taste of it, can renew the disagreeable sensation, the nausea which it occasioned, and this will, in many persons, give rise to vomiting. This may serve to explain what appears very marvellous, that persons can recal at pleasure both Epileptic and Hysteric fits. It has been questioned whether this *Epilepsia Simulata*, as it has been termed, is real. I believe the fits are as real as the original ones, and are only simulated in so far as regards their cause, as we know to be the case when they are cured by exorcisms. It might be expected that in those cases in which real associations bring on a variety of Convulsive affections, as, for instance, in the case of fear, and particularly where it is produced by surprise, these associations should have less and less effect the more frequently they are repeated, conformably with the law of Sensibili-

ty being diminished by repetition of impression; but, in some instances, they seem, on the contrary, to gain strength. This, I think, may be explained in the following manner:— It is the nature of the human mind to indulge every present emotion or passion, and this would not be prevented from going to excess if it was not counteracted by some other sensation, consideration, or motive. Now, where our emotions and passions are thus counteracted, and in some measure checked, there the law of Sensibility takes place, and the impressions have gradually less and less effect upon us. But where there is no such temptation to check these emotions, and where they are consequently freely indulged in, by every repetition they gain ground and acquire more strength. There is another case in which habit has an effect in giving more Mobility, viz. as the manner of life has been more strictly uniform; for, as new impressions are, *cæteris paribus*, always the strongest, persons who have least experience of such impressions, will be most susceptible of their influence, and will thereby be rendered most irritable, so that the *strictius regimen* gives the appearance of Mobility."

5. "Lastly, the different states of the Mobility of the Muscular fibres depend upon the state of the Brain communicated to them. And here I would repeat the same reasoning that I used with regard to Sensibility and Vigour being communicated from the Brain. As a communication exists between every part of the Nervous system, the condition of one part will be communicated to every other, and therefore the different states of Mobility in the Brain must affect the state of the Mobility of the Muscular Fibres. Now the state of Mobility in the Brain is remarkably different on different occasions. 1st, It will depend upon the degree of Sensibility, and, consequently, upon the various causes already mentioned as affecting this. 2d, It will depend very much upon the state of Reflex Sensation, or the feelings of pleasure or pain with which most of our sensations are accompanied. These form a part of Sense, but are,

in a great measure, dependent upon the Brain; and when we consider that pleasure and pain are the immediate causes of Desire and Aversion, which in their different modes produce all the different states of Emotion, Passion, and Volition, it must be sufficiently evident that these states of reflex sensation will give a greater or less degree of Mobility to all the actions depending upon them. In so far as the train of Sensations, and the Passions, &c. depend upon the tone of mind, the state of Reflex Sensation must depend upon the Brain; and the actions of the body must depend on the tone of mind. Thus a degree of Vividity, of Alacrity, and Levity, or a disposition to change,—or a state of Dulness, Slowness, and Tenacity, can only be considered as states of morbid Irritability or increased Mobility, or of Torpor in the Brain; and, in the same way, Gaiety or Sadness, Complacency or Peevishness, &c. are tones of mind merely affecting the state of Mobility, or depending upon it. Nothing, therefore, is more evident, than that the state of Reflex Sensation, as agreeable or disagreeable, arises from certain modifications of the Brain. What these are I do not pretend to explain; though they are circumstances for inquiry, and are never to be removed from the mind and attention of the physician. I must next observe, that our volitions—every instance of motion, and, therefore, of more or less mobility—depend also upon our intellectual operations more purely considered,—upon the peculiar state of Genius prevailing in different men. In some there is a certain dulness of apprehension that may save them from timidity, while in others there is a greater vivacity of apprehension that collects the view of dangers that are imaginary. This vivacity of apprehension I would call Genius; and it would be desirable to inquire, upon what states of the Brain the different states of Genius depend. It has been attempted, but with no success. We can make a few observations which shew some connexions between these states of the mind and certain bodily states, but this goes a small way only."

Such being the general view which Dr Cullen took of the different Morbid conditions of the muscular or moving fibres, as Proximate causes of diseases, and of the various circumstances by which these morbid conditions may be produced, he was led to refer the different morbid phenomena or symptoms to which they give rise in diseases to the two heads of Increase and Diminution of Motion.

"The former, the Increase of Motion," he observes, "has been treated of by Pathologists in general under the title of Spasm, and the latter, the Diminution of Motion, under that of Palsy. Under the term Spasm, Gaubius comprehends every irregular motion, but this general view is not sufficient for our purpose. There is not a term now more frequent in our Pathology than that of Spasm; it is as old as any writings in Medicine, and has been on many occasions employed with very different meanings, so that it is necessary to limit its sense. As the word was originally used to express contraction in general, every kind of contraction might be called a spasm or drawing together; but it is necessary to have different terms to express the different modifications or states of contraction."

Dr Cullen, therefore, divided the irregular motions of the muscular system into two orders, Spasm and Convulsion. Spasm, he defined to be a state of the contraction of muscles, that is not disposed spontaneously to alternate with relaxation, and in which, too, the fibres do not easily yield to extending powers applied to them. Convulsion he defined to consist in the muscles being excited to contraction by preternatural causes, and contracted with unusual velocity and force; and especially in the contractions, alternating with relaxation, being frequently and preternaturally repeated. "Where Pathologists," he observes, "have used the

term Spasm according to the ancient meaning, they have distinguished the *Spasmi* into *Tonici* and *Clonici*, which, in my opinion, is more difficult language than that of Spasm and Convulsion."

The Causes of Spasm and Convulsion may, Dr Cullen conceived, be referred to three classes:—1*st*, Whatever disturbs the ordinary measure and order in the exertions of the Animal power; 2*d*, *Direct* Stimulants; and 3*d*, *Indirect* Stimulants.—With regard to the first class of causes, he observed, that—

"In all men custom has established a measure in the velocity and force of every action, and in the order of every series of actions; and, though this measure has a certain latitude, which is different in different men and in different actions, yet in every man that latitude has its limits, and if the mind is urged beyond the established limits of velocity and order, the muscular organs commonly lose their measure, and readily run into the states of Convulsion and Spasm. In most violent exertions, we readily throw the muscles into spasms, but still this is most remarkable with regard to the exertions of the animal power in the Brain. There, all hurry in the emotion or passion produces a general effort or epilepsy; and hence the effects of surprise, and indeed of all strong impressions that are sudden, in throwing moveable systems into Convulsion and Spasm, or through them into Delirium, or irregularity in the motions of the animal power. Persons who stammer or suffer a considerable interruption from convulsive motions arising in speaking, sing quite easily without any interruption at all. This appears to be owing to the circumstance, that in singing, they have their ear to determine both the measure and order, and therefore they go on without any interruption; but when they are exerting the organs of speech by the power of their own will, without any such measure, they fall into stammering from some accidental bashfulness, and generally from some hurry.

If any person who stammered was at the same time a very exquisite mimic, I think that in his imitating the sound and voice of other people, the case would be the same as if he were singing, though in his own exertions he has no such means of governing himself. This shews that it is the loss of measure that throws us into Convulsive and Spasmodic motions." "It is a sure mark of vigour when we can give a certain duration to the contraction of muscles; but when that vigour is wanting, the relaxation comes to alternate too quickly, which is the state of Convulsion. This is illustrated particularly by Tremor, which is a frequent alternation of contractions and relaxations, both of them weak and of little duration, and which is therefore a mode of Convulsion. Tremor takes place in debility, and is justly considered as a mark of that debility, and it is produced by the passion of fear which immediately debilitates the system."

The class of *Direct* Stimulants which operate in producing Spasm and Convulsion, Dr Cullen considered under the same arrangement as Gaubius, viz. Mechanical and Chemical Acrimonies applied to the Brain, either directly or through the intervention of other parts,—Pain,—and immoderate Repletion, particularly of the vessels of the Brain. In speaking of pain, he remarked, that—

"While pain sometimes produces Convulsion and Epilepsy, it at other times produces Syncope. When it produces Convulsion, I would refer this to its stimulant powers, but when it acts by producing Syncope, it seems to operate as a very strongly sedative power. Here, however, I would not rashly conclude that the effects are opposite in these two cases, for they may be the same, only in different degrees; and it may be made a question whether pain producing Syncope is not an instance of violent Excitement producing considerable Collapse, and whether, in its power of producing opposite effects, it does not resemble

Electricity, which is capable both of exciting action and of producing Death." With regard to immoderate repletion, as a *direct* stimulant liable to produce spasm and convulsion, Dr Cullen observed, "I have endeavoured to shew that a certain fulness of the vessels of the Brain is necessary to its Excitement; and that a certain increase of that tension and fulness is a strong stimulus, which will continue the Excitement and prevent the state of Collapse from coming on. Accordingly, there is not a more frequent occasional cause of Epilepsy than a little increase of fulness in the vessels of the Brain. I need not add here, that the effect of the determination of blood to the Brain in producing Epilepsy, appears from this, that convulsions often happen where we have proof of an increased impetus, and where there is a degree of inflammation, as in Phrenitis."

Under the class of *Indirect* Stimulants, Dr Cullen mentioned as causes of Spasm and Convulsion, immoderate Evacuations, Narcotic sedatives, particular Odours, and the emotion of Fear.

"I observe," says he, "what has been but little taken notice of before, that causes of Debility are frequent causes of Convulsion; whereas our pathologists have supposed that convulsions are commonly owing to the application of a tro nger irri tation; but the irritation seems to be only the *indirect* stimulus, producing the action of the *Vis Medicatrix Naturæ*. It is evident that convulsions depend upon causes of Collapse, which operate by producing this exertion of the Animal power, or what I have been induced to call the Reaction of the Brain. The chief proof of immoderate evacuation acting as an indirect stimulant, is taken from Hæmorrhagy. Dr Hales was much occupied in experiments, which led him to bleed animals to death, and he constantly found that no animal died in this way without being affected with Epilepsy. Our butchers have evidence of the same fact every day. There is no other explanation of this fact than to consider the loss of blood as a cause of considerable Collapse of

the Brain, which produces an exertion of the Vis Medicatrix, which, however, under that debility, has no measure, and therefore produces convulsions. Another proof that Epilepsy may be brought on by causes of debility, is that nothing is more common than for Syncope, particularly from bloodletting, to be attended with Convulsion, so that these states must be imputed to one and the same cause. Many Sedatives, too—such as are most powerfully Narcotic, and therefore most capable of producing a state of Collapse, particularly opium,—do not kill without producing Convulsion. And it is very probable, since Poisons have so universally the effect of producing Epilepsy and Convulsive motions, that more of them are of the *sedative* kind than has been imagined, though, at the same time, some of them may operate by their *stimulant* power, and by the inflammation which they produce."

With regard to the effect of Odours in producing Convulsion, Dr Cullen remarked, that they probably have this effect only in so far as they give rise to a disagreeable sensation; and with regard to the emotion and passion of Fear, which is so frequently the cause of Epilepsy, he observed, that its operation is to be explained, not only by its *direct* sedative effects, but from its disturbing the measure of the Animal power.

"With regard to all these three classes of Causes of Increased motions," Dr Cullen added, "we must remember that the causes of Convulsion and Spasm are generally compounded of Predisponent and Occasional or immediately Exciting causes. Those we have been enumerating are the occasional causes, but we must take into our account the predisposition, which is generally more or less of Mobility in the system."

Dr Cullen next proceeded to consider the causes of Diminished motion under the general title of Paralysis:—

" I must observe," says he, " that it has been usual with physicians to consider Palsy as an affection of Sense as well as of Motion. Loss of sense and loss of motion are indeed very frequently conjoined, and often depend upon similar or upon the same causes; but they are not necessarily conjoined, and the one may occur without the other, the loss of motion without the loss of sense, and the loss of sense without the loss of motion. I am, therefore, of opinion, that they should be considered separately, and as accidentally combined; here we are considering motion only, and we shall therefore consider palsy as an affection of motion, which may be called complete though sense still remains."—" The most common idea of physicians with regard to Palsy, considered as an affection of motion, is, that it is a *total* loss of motion. I must own that there are no exact limits to be set between the slightest deviation from a state of Vigour to a state of Debility, and of complete Palsy, so that they may be all considered as only different degrees of the same affection; but I must here confine myself to the consideration of Palsy, strictly so called, that is, when the loss of motion is complete or nearly so. Gaubius says, that Palsy or extinction of the motive faculty, if it affects the fibres of the viscera and vessels, is called Atony. I doubt if this is conformable to the present language of physicians, who commonly apply the term Atony to debility short of palsy; but I suspect that Gaubius may have a foundation for limiting the term in this way, inasmuch as the consideration of the increase or loss of tone is most commonly taken notice of in relation to the Arterial system, and perhaps it may be proper to confine the term to that view."

" Gaubius has referred the Causes of Palsy to three general heads:—The 1st comprehends what we may call vitiations, faults, or defects in the origin of the nerves, and in the nerves strictly so called; what most people would call faults of the Nervous system, as they do not with me extend that term to the Muscular fibres. The

2d is a fault in the state of the Circulation; and the 3d, a fault, vitiation, or defect in the Muscular fibres themselves. With regard to the first of these classes, there is, I think, a distinction of some consequence. There is one case of palsy which depends upon an extinction of the moving power, or upon a Collapse of the Brain, and another case which depends upon Compression, or some such organic affection, preventing the exercise of the Nervous power, or of the excited state of the Brain. Now, to me this distinction is absolutely necessary, with a view to practice. I say that the cases of the first kind, those that are called cases of Collapse, are commonly benefited by stimulants, and are hurt by evacuations of all kinds considered merely as such. On the other hand, the cases of Compression, arising from a congestion of blood, frequently require evacuations, and may be hurt by every kind of stimulus."

With regard to the influence of faults in the state of the Circulation in producing Palsy, Dr Cullen observed, that—

" A want of the due influx of blood into the vessels of the Brain from any cause obstructing the blood, brings on a *deliquium animi*, and, with that, a state of Palsy of the whole muscular fibres of the system. But whether or no any lesser degree of that diminished influx produces Palsy, or any degree of it, without *deliquium* occurring at the same time, I do not perceive, and very much doubt. Gaubius, however, has had chiefly in view the state of the circulation in the muscles themselves,—the state of the motion of the blood in the arteries interposed between living fibres. I have formerly said, that such a motion of the blood seems to be necessary to the heat, tension, and sensibility of the muscular fibres; but some think it a part of the inherent or contractile power, and explain the state of the inherent power by the state of the circulation in the muscles. But irritability is not strictly connected with the motion of the blood. A frog,

whose heart is cut out, will continue to leap about for an hour or two afterwards; and when we tie the arteries belonging to particular muscles, we observe that their irritability continues long after the motion of the blood through them is interrupted. Nothing is more common than to allege the contrary of this,—that by tying the Aorta a palsy of the lower extremities is produced; but, supposing that experiment correct, it is doubtful whether it affects the muscles or the spinal marrow. Physiologists have tied the arteries of particular muscles, in consequence of which the muscles have appeared to become paralytic; but I would conclude, that the motion of the blood has no farther share in the contractions of the muscles, than as it keeps the parts in due condition. Thus, in the case of ligature of the brachial artery, for the cure of Aneurism, it is true that some appearance of palsy comes on, but it is slow, and sometimes does not occur at all; and, when it does, it is liable to be immediately succeeded by gangrene, which shews that the loss of power does not depend on a cause in the Nervous system, but on an impediment external to it."

Conformably with these views, Dr Cullen reduced the causes of Palsy to four heads. 1st, The state of Collapse in the Brain, analogous to the state of natural sleep; 2d, Those Organic causes that give an interruption to the communication between the origin of the nerves and the several muscles of the body; 3d, The several causes of the loss of Inherent power in the muscular fibres themselves; and, 4th, Those affections of the Brain or Muscles, by compression or otherwise, that prevent the exercise of the Inherent or Animal powers, to which head is to be referred those which influence the state of the Circulation.

There are two forms of *Irregular* motion, with regard to which it may be difficult, in particular instances, to determine whether they should be referred

to the head of Increased or to that of Diminished motion, viz. Tremor and Palpitation.

"Tremor," observes Dr Cullen, "consists in a frequent alternation of contractions and relaxations, and in so far is properly considered as a convulsive motion, and has been placed by Sauvages among his Convulsive affections; but Gaubius very properly observes, that it is to be considered rather in its cause than in the mode of action, that being of little importance. Now in its cause Tremor is to be considered as a state of Debility."—"With regard to Palpitation, Gaubius has observed that it is to be referred to Spasmodic affections. The cause of Palpitation as affecting different muscular fibres, is a matter that we do not well understand; we understand it only as it is an affection of the heart, where it is to be reckoned among the convulsive motions of that organ, and may arise from a variety of causes; as from the influx of the venous blood into the cavities of that organ being made with more velocity, and therefore, in the same time, in greater quantity than usual, as is the case in violent exercise;—from any resistance given to the free and entire evacuation of the ventricles of the heart;—from a more violent and rapid influx of the Nervous power into the muscular fibres of the heart, as in palpitation from mental emotions;—from a peculiar irritability or mobility of the heart excited by irritation in the intestinal canal or other parts of the system;—and, lastly, from causes producing a weakness in the action of the heart, by diminishing the Energy of the Brain with respect to it. Now, in this last case, when it depends upon a state of Debility, we must, as in the case of Tremor, consider the cause much more than the mode of action."

The numerous extracts which have been given from Dr Cullen's manuscript lectures, will be sufficient, it is conceived, to convey to the reader, a tolerably just idea of his general manner of lecturing on Pathology, —of the importance which he attached in this part of

his Course to the consideration of the derangements of the different functions of the Nervous System, and of the accuracy with which, in his explanation of Diseased states, as well as of the Agents producing them, he at all times distinguished between the vital properties and functions of the Nervous and of the Muscular systems.

The third part of the Institutions of Medicine, which treats of Remedies, Dr Cullen denominated THERAPEUTICS or the Methodus Medendi, and defined it to be the General consideration of the Means of preventing and curing diseases.

"The term Therapeutics has indeed," he remarked, "been hitherto almost entirely confined to the study of those General principles which should guide us in the administration of Remedies as Means for curing diseases, while the study of the Means of preventing diseases, has been supposed to constitute another branch—that of Hygiene. Now, the prevention of diseases consists either in obviating those biases and deviations of the constitution from the most perfect standard of health, which proceeding to a certain degree might prove a disease—that is, in obviating Predisponent causes or Morbid dispositions; or in avoiding the operation of those powers which induce diseases—that is, in avoiding Occasional and Exciting causes. In so far, therefore, as Predisponent causes are obviated upon the same principles, and by the use of the same Means as are employed for the cure of diseases when they are more formally established, Hygiene and Therapeutics must, it is obvious, form one and the same General doctrine."

In teaching the Therapeutical part of the Institutions, Dr Cullen, though he referred occasionally to the writings of Boerhaave and of Hoffmann, followed a plan of arrangement in many respects peculiar to himself.

The consideration of the operation and effects of Medicines had been long very familiar to him, not only from the attention he was obliged to give to these in an extensive private practice, and from his having had much occasion to treat of them in his Lectures on the Practice of Medicine at Glasgow, as well as in his Clinical Lectures at the Royal Infirmary in Edinburgh, but also from his having delivered a distinct course of Lectures on Materia Medica in Glasgow during his stay there, which he had occasion to repeat and extend in Edinburgh, as has been already mentioned, on the death of Dr Alston in the year 1761. In lecturing on the Institutions, the very small share of the course which he could devote to the consideration of Therapeutics, obliged him, in treating of this subject, to confine himself to the statement of general doctrines, rather than to enter into any minute details; but the principles which he delivered, and the general arrangement which he followed, were so nearly the same with those to be found in the different editions of his Treatise on the Materia Medica, that in giving an account of his Lectures on Therapeutics, it will often be of little importance whether the illustrations of his opinions be taken from the manuscript notes of his lectures, or from the publications just referred to.

"Under the term Means of preventing and curing diseases," Dr Cullen observes, "I intend to comprehend the consideration of every Power acting upon the human body, and capable of changing its condition. These powers or means of changing the condition of the human body may be considered either as Causes of diseases or as Remedies of them; and it is obvious that they prove the one or the other only according to the degrees of their action and the state of

the subjects upon which they operate. In consequence of the different views which Medical men have taken of the proper foundations of Medical science, these Means have been differently arranged. According to one plan of arrangement, the *Empirical*, the several preventive and curative Means are enumerated, and under each of these Means its several effects are mentioned; according to another plan, the *Dogmatical*, the several effects desired to be produced are considered, and under each of these effects the several Means are arranged by which it can be produced. It may perhaps be proper to consider to what general heads the several Therapeutical means or powers can be referred abstractedly from the effects which they are capable of producing; but here we are to consider Remedies under the several heads of their effects, and therefore as directed by *Indications*. The general plan of Dogmatic physic, as I have before explained, consists in comparing the action of the functions in the states of Health and Disease. By knowing what is the state of the body in health, and comparing that with our knowledge of its state in disease, we learn what change must be produced in the body in order to restore it from a morbid to a healthy state, and this perception of the change that is necessary to be produced for restoring health is strictly called an Indication. It is the *Indicatio Curatoria* of the Systematics, and is more particularly explained by its being the *Indicatio ad tollendam Causam Proximam*."

"Having thus explained the strict sense of the term Indication, as implying the knowledge of the change necessary for restoring the body from a morbid to the healthy state, I must observe, that writers on Therapeutics have not been precise and correct in the employment of this term, but have used it to signify what may more properly be called the consideration of some general intention. Accordingly, most Systematics have alleged, that in the cure of every disease, the general Indication is of four kinds, the *Conservatory* or *Vital*, as it is termed by Boerhaave, which is said to consist in

preserving life, and supporting its powers in general; the *Preservatory*, or *Prophylactic*, which consists in the removing or guarding against the renewal of those remote and external causes which either may have produced, or might aggravate, the disease; the *Curative*, which has been already mentioned as the proper Therapeutical indication, and as applied to removing the proximate cause of the disease; and the *Mitigatory*, or *Palliative*, which is the application of Remedies to particular symptoms, when we do not know the Proximate cause, or when we do not know the means of directly removing it; or when, from the nature of the disease, or from the circumstances of the constitution, it cannot be removed, whilst, in the mean time, one of the symptoms is extremely urgent, or threatens immediate danger to life."

With regard to the Conservatory, or Vital Indication, Dr Cullen observes, that—

"Whether we can add any thing to the powers of life, is to me very doubtful. If they are already sufficient for their purposes, all our attempts to carry them farther must be superfluous, and we can have this Indication only in cases in which these powers are for the time deficient. But if there be in any disease a deficiency of the Vital powers, this must be included as a part of the proximate cause of that disease; the Conservatory Indication, therefore, will necessarily be included under the Curative, so that there is no particular occasion for treating of it separately. The Preservative Indication, which is only another term for the Hygiene, may, also, for the reason already assigned in speaking of Hygiene, be comprehended under the Curative Indication. The Palliative Indication, in as far as it is not empirical, and may be considered in a general Methodus Medendi, will be fulfilled by applying Remedies to the proximate cause or causes of the more urgent symptoms, in the same manner as in the Curative indication. From this view of Indications, results

the plan that is to be followed in these Lectures—that it is the Curative Indications only that are to be considered, and under which the consideration of all the several Remedies is to be placed. It is attention to the Curative Indication that establishes the difference between the Empiric and Dogmatic plans of practice: the dogmatist seeks out indications, the empiric neglects them, and is entirely directed by what has been found by experience to be beneficial or hurtful in particular cases. The dogmatist always pursues a general plan, and it is this plan, so far as it can be brought under a general doctrine, that we are to pursue in the Therapeutics."

"It may be supposed that, in treating of the several kinds of Remedies, I am to speak of the different articles of the Materia Medica individually; and I allow that the enumeration and consideration of these articles may be properly subjoined to a therapeutical view of remedies, and that there is no better way of delivering it than as a part of the Methodus Medendi." "The study of the Materia Medica is truly the study of the medicinal virtues, and therefore the plan that arranges the several substances, according to their agreeing in some general virtues, will be the best adapted for acquiring the knowledge of these, and will most readily inform the practitioner what different means he can employ for his general purposes. It will also inform him how far the several similar substances may differ in their degree of power, or how far, from the particular qualities assigned to each, he may be directed or limited in his choice. As it seems proper that every practitioner ought, as far as is possible, to practise upon general Indications; so it is evident that the purpose of his study of the Materia Medica is especially to know the several means by which these indications can be best fulfilled. Such a plan, therefore, must be the most proper for giving a student instruction; and if, while medicines are arranged according as they answer general Indications, the particular substances be likewise thrown together, as far

as possible, according to their sensible qualities and botanical affinities, this plan will have the advantage of any other that has been proposed for presenting together the subjects that ought to be considered at one and the same time, and give the best means of recollecting every thing that relates to them." "But in these lectures on Therapeutics I am to take only a general view, and to point out what is in common to the several remedies, without entering on the more particular consideration of each of them regarded as articles of the Materia Medica."

As a guide to his students in this department of the Course, Dr Cullen printed a short Syllabus, containing the heads of his Lectures on Therapeutics. In this syllabus, remedies are divided into four classes, according as they act: 1st, on the Simple Solids; 2d, on the Fluids; 3d, on the Moving Powers; and, 4th, on Extraneous Matters lodged in the body. The substances which act on the Simple Solids are subdivided according as they serve to supply their matter, to consume their matter, to strengthen their cohesion, or to weaken their cohesion, under the general titles of Nutrients, Corrosives, Astringents, and Emollients. The substances which act more immediately on the Fluids are subdivided according as they serve to diminish their consistence, to increase their consistence, or to correct the state of their mixture, by correcting acidity, alkalescency, or acrimony more generally, under the titles of Resolvents, Inspissants, Ant-acids, Ant-alkalines, and Demulcents. The substances which act on the Moving powers are subdivided according as they increase tone, diminish tone, increase motion, or diminish motion, under the titles of Tonics, Atonics, Stimulants, and Sedatives. The class of Stimulants

is further subdivided according as they act on the General system, or on Particular parts of it, so as to increase the secretions and excretions from those parts; under this latter head are included Emetics, Cathartics, Diuretics, Diaphoretics, Errhines, Expectorants, Sialogogues, Emmenagogues, and Epispastics. And, lastly, to the class of substances acting on Extraneous matters lodged in the body, are referred Anthelmintics and Lithontriptics.

"I have given here," says Dr Cullen, "a Scheme of Indications. With regard to it, I would observe that any such method, sufficiently comprehensive, and, at the same time, proper, is extremely difficult to be found; I am far from thinking that I have found it, or that the method I have chosen may not lie open to several objections, and to particular corrections. But a very ingenious gentleman has gone farther, and alleged that such an arrangement and order is impracticable. For that gentleman's ingenuity and judgment I have the highest regard, but in this I cannot agree with him. Without entering into any dispute upon the point, I would observe that the objections which he has stated against this arrangement of therapeutical means are applicable to any arrangement that has been, or that ever will be, attempted. Accurate definition and division are the utmost attainments of science; they imply the most comprehensive and precise knowledge of a subject; and till the particulars of the science of remedies have made a greater progress to perfection than they have hitherto made, it is impossible to give an arrangement of the whole that will be absolutely perfect. But though method in all parts of science is imperfect, it has been highly useful, and, I will not say the worst order, but any order, though considerably imperfect, is better than none." "From the difficulty of rendering any plan of arrangement of the Materia Medica tolerably exact and perfect, some writers have deserted all of them,

and have thought it best to throw the several articles into an *alphabetical* order. If, however, there can be any advantage from bringing subjects of some affinity together, this alphabetical order is the most unfit for the purpose, as by separating substances similar in their qualities it must be perpetually distracting to the student. It can, therefore, have no advantage but that of a Dictionary, in referring readily to any particular substance that may be inquired after; but this advantage can be obtained in every plan, by means of an index, which cannot be saved even in an alphabetical work, as the different names under which the same substances are known necessarily require an index comprehending all those different names."

"In forming a Table of Medicines according to their several operations on the human body, it seemed proper to distinguish them as they operate upon the Simple Solids, which are much of the same nature, and possess similar physical qualities in the dead as in the living body; and as they operate upon the Sensible and Moving Solids, which have their qualities and powers only as they exist in a living body. This distinction will on many occasions be necessary and useful; but we cannot follow it throughout; and on these occasions where the medicines at the same time operate upon both the simple and living solids, the consideration of their operation cannot be taken separately."

But in establishing a class of medicines which act upon the simple solids, or, in other words, upon the physical properties of the different textures and organs of the body, Dr Cullen was far from conceiving, as has been erroneously represented in some recent works upon Materia Medica and Pharmacology, that any considerable effects can be produced in the body by external substances operating upon it as upon a mass of inanimate matter, and independently of any influence upon the vital powers of the animal economy.

"The state of laxity or rigidity in the Simple Solids," he remarked in his Lectures on Therapeutics, "has had a great share in our modern pathology, and we think that the consideration is not to be neglected altogether; but, at the same time, we think that the laxity or rigidity of the simple solids being given, it is not readily or occasionally changed; and that all sudden and occasional changes in debility or strength, in contractility or flaccidity, are more properly referred to the state of the moving fibres under the influence of the nervous power, *quam facile mille res turbant.*" "Most of the powers acting upon living bodies do not act in the same manner, or do not act at all, upon dead bodies, so that their effects depend upon the powers of life, upon the existence of sensibility or irritability in the whole or in the several parts of the system. It is certain that we must constantly consider the subsistence of the vital powers as modifying the effects of the several remedies, and unless these subsist, the remedies are to no purpose applied." "We adopt the proposition very well established, '*Medicamentum non agit in Cadaver;*' that is to say, that the operation of medicines does not depend on the laws of matter and motion which take place in inanimate bodies, but on a principle which subsists in living animals only." "It is to be remarked, as a principle commonly assumed, that few or no medicines act upon the living human body in the same manner, and with the same effects, as upon inanimate matter; and it is now well known that the operation and effects of substances applied to the living human body, are for the most part altogether different from the effects of the same application to the dead body. Few indeed, or none, of those substances which are considered as medicines, have any effects at all upon the dead body; and, therefore, assuming this as a principle, when hereafter I shall have occasion to speak of the action of substances upon the body, *it must be constantly understood to be their action on the living body only*—at least with very few exceptions, which shall be taken notice of when occasion requires."

In explaining still farther his opinions upon this subject, Dr Cullen remarks, that—

"The *peculiar* effects of substances in general, or of those substances in particular which we call Medicines when applied to the human body, depend upon their action upon its sentient and irritable parts; or, in other words, upon motions excited and propagated in the Nervous system. What is the nature of the matter in which these motions take place, or in what manner it is inherent in the nervous system, is not well known; but we think it may be justly held as existing, and may be spoken of under the appellation of the Nervous Power. As it appears only in the living, and disappears entirely in the dead state of the body, it may be otherwise properly enough termed the Vital Principle of Animals."

With regard to the action of medicines upon the Fluids of the human body, which has been represented as another fundamental error in Dr Cullen's classification of remedies, he observed that—

"This subject has occupied a great part of the writings on the Materia Medica; but, in my opinion, with no advantage. The doctrines have been often drawn from mistaken facts—from an imperfect view of things—and have been commonly explained upon mistaken principles." "From the earliest accounts of physic with which we are acquainted, it appears that from the most ancient times down to the present, physicians have been attached almost entirely to the study and consideration of the Fluids; and, from the supposed state of these, have endeavoured to explain the phenomena both of health and of disease. In this, however, they appear to me to have been unfortunate; for, not to mention the imperfection and falsehood of the many speculations, both of Galenists and Chemists, which have formerly prevailed upon this subject, I would venture to assert that the doctrine concerning the fluids is still the most imperfect part

of our physiology. Every thing systematical that has been delivered upon it, till within these last forty years, may be totally disregarded; for it is only within the period now mentioned that we have acquired any distinct notions of a fundamental doctrine, or, in other words, of the state of aggregation in the mass of blood; and even with respect to this there still remains much doubt and obscurity."

"The supposition of a preternatural Spissitude of the mass of blood,—or, as it may be expressed, of a Lentor,—being a frequent cause of disease, has had a great share in almost all the modern systems of pathology; but I allege that it has been for the most part hypothetical, and has hardly, so far as I know, in any case been ascertained as a fact. I am disposed to maintain, that the supposition is for the most part improbable; that there is little foundation for the supposition of a preternatural spissitude prevailing in the mass of blood, or of its proving commonly the cause of disease."—"Nothing has been more common among physicians than to suppose, that an Acrimony of the fluids is a frequent cause of disease. It is very possible that it may be so, and upon many occasions it certainly is so; but it appears to me that the supposition has been too rashly and too frequently admitted, and that it has been for the most part purely gratuitous, without any proper evidence of it in fact. The possible species of acrimony have been little understood, and several of them altogether erroneously supposed. Others of them, though possibly occurring, have not been shewn really to take place in any unusual degree; and the phenomena adduced in proof of them may be commonly explained from other causes, and are certainly often produced by causes of a different and even contrary nature. We are only well acquainted with two kinds of Acrimony, the *acid* and the *alkaline*. Upon the whole of this subject I would conclude, that the supposition of an acrimony, as the cause of diseases, has been too frequently admitted in our modern pathology, and

that it ought not to be admitted, unless when the causes and existence of it are well ascertained."

"I will not deny, that the state of the Fluids may have a share in distinguishing the different states of the body both in health and in disease; but at the same time I must maintain, that we know little of the manner in which it may have this effect; that our theory of the human fluids is still very incomplete and imperfect; that, while in this condition, it has been employed too rashly and too largely in every part of the system of physic; and that we have little temptation to do this, as it is highly probable that the state of the fluids depends very much upon other circumstances of the constitution, which are more fundamental and more powerful in determining the several conditions of it."—"Every change in the state of the fluids poured out, has been considered as depending upon the state of the fluids in the vessels, and it may be so in some cases. But as it is probable, that the secreted fluids are chiefly formed in the secretory organs, so it is certain that the state of these fluids is changed by the relaxation or constriction of the Secretory vessels; and therefore the fluids poured out are more frequently a mark of the state of the moving powers, than of the general condition of the fluids themselves. Thus the urine, which has been so long considered as a mark of the state of the circulating fluids, is, however, for the most part, no other than a mark of the state of its secretory organ."

"As it has happened that, ever since the introduction of chemical reasoning, physicians have generally considered the causes of diseases to be depending upon the state of the fluids, so they have considered the operation of medicines chiefly as changing that state; and this theory still enters, to a considerable extent, into the doctrines of the materia medica, while the state of the moving powers, and of the various means of changing them, are as yet but little attended to. I judge this to be very improper. With respect to this, Dr Hoffmann admitted the general principle, and has made this ob-

servation: "*Demum omnia quoque eximiæ virtutis medicamenta, non tam in partes fluidas, earum crasin ac intemperiem corrigendo, quam potius in solidas, et nervosas, earundem motus alterando ac moderando, suam edunt operationem; de quibus tamen omnibus, in vulgari usque eo recepta morborum doctrina, altum est silentium.*" Yet, notwithstanding the expression of this opinion, in treating of particular medicines, Dr Hoffmann has himself, for the most part, employed the Corpuscularian philosophy, or a very ill defined Chemistry, to explain the operation of medicines upon the fluids."—"The medicines which act upon the fluids do so commonly in consequence of acting on the solids. I do not say that this is the constant, but certainly it is the general case; and we every day see that writers, as well as practitioners, incline more and more to the pathology of the living solids."

Of the substances capable of altering the conditions of the solid and fluid parts of the body, Dr Cullen regarded Nutrients as the most important; and, accordingly, he devoted a portion of the therapeutical branch of his course to their consideration. In the general view which he took of these substances, he directed his attention chiefly to those Hygienic considerations which should regulate their employment for promoting the growth, or repairing the waste, of the body in the state of health; and with this he incorporated a brief account of those Therapeutical principles, which should direct the practitioner in the administration of a full or of a spare diet, and in the choice of aliments, in the different states of disease, according as he may wish to exert a stimulant or a sedative influence upon the system. He treated successively of vegetable and animal matters, as the sources from which nutrients are obtained,—of the products, or, as they are now termed, the proximate principles of vegetables, on which he

supposed their nutritious qualities to depend,—of the more general qualities which distinguish the different kinds of animal food, and which render them more or less eligible as articles of diet,—of those substances, and particularly milk, which he considered as of a nature intermediate between animal and vegetable matter, alluding here to the experiments of Beccaria, as affording some grounds for thinking that, from the properties of the gluten which they contain, some of the *Farinacea* might also be included under this head of intermediate substances,—of the different nutrient, stimulant, and sedative effects which aliments produce upon the system, and of the circumstances on which the production of one or other of these effects, and the degree in which they occur, may be supposed to depend, —of the different kinds of preparation to which nutrients are subjected previous to their assimilation, both out of the body from the art of cookery, and within it from the processes of manducation and digestion, —of the possibility of supplying food temporarily by the use of nutritive glysters,—of the number of meals that should be taken in the course of the day, the times of the day at which they are most properly taken, and the propriety of applying to business, or of sleeping after meals,—of the influence of the temperature of our food, and particularly of our drink, in modifying the effects which they produce upon the system,—and of the mischiefs which may arise from the improper use of nutrients. Under this last head he considered the effects resulting from the use of a diet, either excessive or deficient in quantity,—the diversities in the effects of aliments, according as they contain a larger or

smaller quantity of nutritious matter, and according as they are more or less accommodated to the state of the assimilating powers of different persons,—the proper proportion of vegetable and animal matter, of which the diet of different individuals should consist, according to period of life, climate, degree of exercise, &c.—and the proportion that should subsist between the quantities of food and of drink. "We are of opinion," observes Dr Cullen, in his treatise of the Materia Medica, "that every kind of food taken into the stomach, as soon as it sets this organ at work, increases the action of the heart, and occasions a frequency of pulse; and, if we mistake not, by the energy of the Brain being thus directed to the heart and stomach, a torpor of the animal functions, both of sense and motion, is produced, often amounting to a degree of sleepiness. These are the effects of food soon after it is taken into the stomach."

In considering the therapeutical means that change the conditions of the Moving powers, Dr Cullen gave, in succession, an account of the various effects of Tonic, Atonic, Stimulant, and Sedative remedies. Under the two former of these classes of remedies, he comprehended chiefly those means which appear to increase or diminish the vigour of the irritable or contractile power of the muscular fibres, without exciting contractions in them; while, under the two latter, he included those therapeutical agents that act chiefly by increasing or diminishing the vigour of the Animal power or Energy of the brain.

With regard to Tonics, Dr Cullen observes, that "Tone may be considered as a state of the Inherent

or Contractile power of the Moving fibres, but at the same time, the Inherent power is in a very constant dependence upon the Animal, and therefore Tonics increase the Inherent power, by increasing the vigour or continual Energy of the Animal power."—Some Tonics, it appeared to him, act, in their primary operation, as Astringents, others as Bitters, some as Stimulants, and others as Narcotics. With regard to Astringents, he remarks, " we can perceive that they have a tonic operation upon the Moving fibres, for they not only operate on the fibres to which they are applied, but affect the neighbouring parts in the same manner; and we have many reasons to believe that their effects are often propagated in a considerable degree over the whole System." With regard to Bitters he observes, that—

" Their most obvious operation is, that, being taken into the stomach, they increase the appetite for food, and promote the digestion of it. But we take it for granted, that the appetite and digestion depend upon the tone of the muscular fibres of the stomach; and, therefore, may suppose that their improvement depends upon an increase of tone in those fibres. And, farther, as loss of appetite and indigestion can often be distinctly perceived to occur from a loss of tone in the stomach, so Bitters, as they are often effectual in curing these disorders, may be presumed to do it by restoring the tone of this organ. But at the same time, their long continued and frequent use is liable to be followed by a very great loss of tone." " The more strict notion of Stimulants is, that they are substances exciting contraction in the Moving fibres; and so far as regards certain parts of the System that are otherwise exposed to constant stimuli, as the heart, the arteries, and the stomach, Stimulants certainly do act by exciting contractions. But as they seem to increase the Energy of the Brain, I imagine that, in a degree within that of excit-

ing contractions, they may increase the Animal, and thereby the Inherent, power of particular parts, so that a moderate use of Stimulant substances may be considered as frequently Tonic."—" Narcotics in their first operation are stimulant, and, in this view, may be considered as Tonic; but even when they proceed to their Sedative operation, they appear to act as Astringents, suppressing or restraining evacuations. This may in part be imputed to their diminishing the Sensibility and Irritability of the vessels, so as to prevent the irritation that promotes their constant excretion. But as they not only take off spasmodic affections, but are very powerful in preventing the return of spasms, it may be suspected that they likewise possess a peculiar tonic power, and produce an effect upon the Nervous power, very different from the atony which is occasioned by their Sedative power."

Besides the different kinds of Tonic remedies already mentioned, Dr Cullen conceived that there are other powers, such as Cold, Exercise, and the Passions of the Mind, by which Tonic effects may be produced in the system according to the degree and manner in which they are applied.

" With regard to the Tonic power of Cold," he observes, " there is no doubt of it; for, if it acted only as an Astringent, it could only operate upon the part to which it is immediately applied; but it is well known that when Cold is applied to a small portion of the surface of our bodies, its astringent effects extend over the whole surface: and from its power in producing the Phlogistic Diathesis throughout the whole Vascular system, it would appear that its action extends to the whole internal System. It is probable that it has a peculiar operation upon the fluid on which we suppose the Inherent power of Animal fibres to depend."—" Exercise strengthens the Moving fibre and is likewise a stimulus to the Animal power, and invigorates it; so that we can perceive how it should prove a remarkable Tonic."—" Passions of the Stimulant kind may, like other stimulants, prove tonic, but it is

passions of the Sedative kind that act more remarkably in this way,—as grief, and certain modes of fear, such as the fear of some impending evil. The former, grief, produces a degree of insensibility, and diminishes the irritability in a considerable degree. Thus persons whose minds are occupied with grief, as in attending a sick friend, if they are exposed to cold, resist its power; but as soon as their grief is over, perhaps the previous application, but especially a fresh application of cold, though trifling in degree, will affect them considerably. With regard to awe and dread, we know that they can cure Epilepsy and other Spasmodic disorders; and Charms of various kinds that are effectual in this way, can be supposed to operate on no other principle; as when a spasmodic disorder is cured by making the patient drink out of a dead man's skull; or when the Chin-cough has been cured by the application of a dead man's hand, and especially of the hand of a man who was hanged, which is said to be particularly powerful in this respect. The nature of the operation of the Passions it is certainly difficult to explain. It may be imputed to their effect in changing the direction of attention, or to their inducing a degree of insensibility, but it is probably connected with a Sedative power taking off the mobility of the Inherent and Animal powers."

The Occasions on which Tonics are indicated may, Dr Cullen conceived, be considered as of two kinds; 1*st*, When Debility, increased Sensibility and Irritability, and increased Evacuations, take place,—under which circumstances the tone may require to be restored in the whole of the System, or in Particular parts of it; and, 2*d*, When the System is to be particularly enabled to resist the Sedative powers of Cold, Contagion, &c. which are soon to be applied to it.

"Under this second head we may comprehend," he remarked, "the use of Tonics in obviating the return of Spasmodic affections, as it is upon a state of debility that their recurrence depends; as in the case of Intermittent fevers, the paroxysms

of which I call Spasmodic affections." "In the Administration of Tonics, the choice of the Matter and Means requires very often a great deal of nicety and discretion, as it is difficult to obtain the Tonic without the Stimulant effects; or, in the case of Sedatives, to obtain their Tonic without their Stimulant or their Narcotic effects."

In proceeding to the consideration of Atonic remedies, Dr Cullen remarked, that he used the term Atony, as opposed to Tone, as implying that the contractility of the moving fibres is less than it ought to be.

"There is," he observes, "great difficulty in finding a proper distinction between Atonics and Sedatives. This is a difficulty which I cannot surmount; but I have established Atonics as a separate title from Sedatives upon this view, that they are such remedies as act more upon the part to which they are applied, than upon the whole System, more upon the Inherent or Contractile power, than upon the Animal power, or Energy of the Brain; while Sedatives act upon the particular part, only in consequence of their general action; act more upon the Animal than upon the Inherent power. This distinction will not apply in all cases, but it certainly will very often."

The Matter of Atonics Dr Cullen regarded as of two kinds, Emollients and Narcotics. Under the head of Emollients he considered the practice of warm bathing more partially or more universally applied, as being, from the conjunction of humidity and heat, the most powerful Emollient, and the most considerable Relaxant; and he was at much pains in pointing out the ambiguity which attends the use of this remedy in many diseases, from the Stimulating effect produced by a degree of heat higher than that which proves relaxant.

"Narcotics," he remarks, "do not act upon the Simple Solids, but upon the Nervous power, and, so far as they act

upon the part to which they are applied, upon the Inherent power. That they may be comprehended under the title of Atonics is evident, as they have the power of diminishing the Tone, Vigour, and even Mobility of the part, and of extinguishing them altogether. But in what cases Narcotics are properly applicable as Atonics, so as to avoid their ambiguous use from their Stimulant effects, is not ascertained."

Under the head of Atonic Means, Dr Cullen entered at considerable length in his lectures into the consideration of the effects of Bloodletting, of Evacuations by increased excretions, of Heat, of Rest, of the Passions, and of the Antiphlogistic Regimen, or the avoidance of *Irritation*, in diminishing the tone of the System in general, or of particular parts of it.

From the peculiar views which Dr Cullen had adopted with regard to the influence of the Nervous System in the Animal Economy, he at all times attached great importance to the consideration of the effects of Stimulant and Sedative powers upon the various conditions of the Moving and Sentient fibres. Stimulants, he defined to be—

"All those powers which excite in the different parts of the human body the motions that are peculiar to it. By this definition," he adds, "we exclude from the class of Stimulants all external powers, which, though they may operate variously in moving the body, do not excite and increase its own peculiar motions. Stimulants are properly such powers as act upon the Vital Solids, and not upon the dead body, or at least by no means with the same effects. The operation of Stimulants, either on an extensive or more limited view, is difficult to be explained; because our knowledge of the Living Principle or Nervous power upon which they operate, and of the various modifications of the different states of its mobility, is still very imperfect. At present, it seems enough

to observe, that we know in general that the Nervous power may be in different states of Mobility, or susceptibility of external or internal impressions from the free communication of this nervous power with the brain; and that there are substances, some of which, when applied to the nerves, have a power of *increasing*, and others of *diminishing*, the mobility of the power contained in them. The former class of substances we name Stimulants, the latter Sedatives."

"Stimulants may, 1*st*, Increase the motion of the Nervous power as it occurs in the sentient nerves and sentient extremities, thereby increasing Sensibility; 2*d*, They may increase the motion of the Animal power in the brain and nerves; 3*d*, They may excite the action of the moving fibres wherever they are placed, whether in muscles or around muscular cavities or vessels; and, 4*th*, They may increase the motion of the blood, and other fluids of the body, by their operation on the heart, and on the secretory organs. But at the same time I must own, that what has been chiefly taken notice of in regard to Stimulants, is their exciting the action of the Moving or muscular fibres. Now, the action of these fibres may be excited either by powers acting directly upon them, or by powers applied to Sentient parts only, which, in consequence of a motion propagated to the brain, excite sensation, which, again, excites the animal power, and directs it into the moving fibres more directly or more powerfully on different occasions."

"Those Agents which excite the action of the Moving fibres through the medium of the Brain, may be considered as of two kinds. Some of them have a direct and manifest power of increasing motion in the System; and all such powers, though acting by the intervention of the Sensorium, are probably of the same nature with those which act immediately upon the Moving fibres; that is to say, those powers which, when applied to the Moving fibres, excite their action, can produce the same effect through the intervention of the Sensorium. These powers we include under the general denomination of *Direct* Stimulants. But there are other

powers that excite the action of the moving fibres through the intervention of the Brain, that have no such direct and manifest power of increasing motion in the system; nay, in which in many cases we can observe a tendency to diminish motion in the system, such as a sense of difficulty, of resistance, or of debility in the exercise of the functions; and which still, in consequence of a law of the animal economy, do occasion an exertion of the animal power. Thus, sighing manifestly arises from a sense of difficulty in the transmission of the blood through the vessels of the lungs. Coughing often arises from the same sensation, without any direct stimulus being applied to any particular part of the lungs. Vomiting frequently arises merely from a sense of debility, as when it accompanies a syncope, from causes which cannot be supposed to operate directly upon the stomach; and the vomiting so frequently produced by narcotics seems to me to be more properly explained by a sense of the debility induced by these substances, than by their affording any direct stimulus. We explain in the same manner the yawning and stretching which occur in persons coming out of sleep, and on some other occasions, when no other cause can be supposed than a sense of some difficulty in the exercise of voluntary motions. These phenomena seem to afford unquestionable proofs of a power in the animal economy to obviate and correct certain deviations from the standard of health; and both these, with the instances that might be mentioned of direct stimuli producing motions suited to throw off matters applied which give pain and uneasiness, or which may prove noxious to the system, concur in showing that there is in the animal economy a power to obviate and correct, in a certain degree, every thing not suited to the health of the economy, and which has properly enough been named the *Vis Naturæ Conservatrix et Medicatrix*. After so many evident instances of the exercise of this power, we can hardly doubt that the like powers operate also in the more obscure internal parts, in many cases of disease, which are spontaneously

cured by the operations of nature, or, in other words, by the inherent spontaneous powers of the animal economy; and particularly that the state of the circulation is often regulated so as to be excited to a stronger action, merely by the occurrence of resistance or debility. All this particularly applies to render it probable that the effect of sedatives, in exciting the action either of the system in general, or of particular parts, may be explained entirely by their being effects of a *vis medicatrix naturæ* obviating injuries which *threaten* the whole system, or particular parts of it. And, to finish this subject, nothing can shew better that active powers can be excited merely by a sense of debility, than this, that if a stimulus accustomed to support the activity of the system happen to be withdrawn, the sense of debility thence arising produces various actions in the system, or in particular parts of it. All these means of exciting the action of the system, or of particular parts, we name *Indirect* Stimulants."

In considering the operation of stimulants as applied to the internal parts, which, he remarks, is especially by their being taken into the stomach, Dr Cullen observed, that—

"It is needless to say how readily and constantly all impressions made upon the stomach are communicated to other parts of the system, and particularly to the origin of the nerves. It is possible that impressions made upon the stomach, may, without the intervention of the brain, be communicated to several parts of the system, and particularly to the surface of the body, or to parts under disease, and therefore in a state of uncommon irritability: but the most common and very general manner in which stimulants taken into the stomach operate, is by their stimulus being communicated to the brain, by exciting the energy of which, various effects are produced in different parts of the system. Upon these occasions, the operation of stimulants may be especially distinguished as being of different degrees of force. In some cases, it seems to amount to no more than the increasing of

the mobility of the nervous power of the brain itself, and thereby rendering the exercise of the intellectual powers more free, easy, and active. Probably at the same time, or at least with some higher degree of force, stimulants render the derivation of the nervous power into the several parts of the system, especially into the nerves of the voluntary functions, more free and full,—without, however, producing any uncommon increase of it in particular parts, to which a will or propensity is necessary. Another case of stimulants applied to the stomach, is when the stimulus applied is of a still stronger kind; and in consequence, a stronger impulse is communicated to the brain, and when, consequently, a stronger exertion of its energy is produced. This, however, without particular determination, may have no effect on the animal functions; but, as we have said before that the energy of the brain is constantly exerted in supporting the activity of the vital functions of the heart and arteries, so any unusual increase of this exertion may increase the force and frequency of these functions. Thus the effects of certain impressions on the stomach may be to increase the force of the circulation of the blood, and especially its more general determination to the surface of the body,—whence the heat and sweating which commonly ensue."

"With respect to the whole of the Stimulants applied to the organs of Sense, we have to remark, that the exercise of Sensation, is, in general, a Stimulant power, and is a chief means of supporting the mobility of the living principle in the Nervous system, more especially in what concerns the Animal functions."—"Some sensations arise from chemical and others from mechanical stimuli; but Sensation, in every shape, is a stimulus to our system in one part or other. All sensations of Impression or Impulse are *direct* stimuli; those of Consciousness, too, are sometimes direct, but more frequently *indirect* stimuli. It was proper to mention this, as it is by the application of sensation that the activity of the system is supported; for withdraw Sensation and the System

falls asleep. From this, and many other views, we can see the necessity of Sensation, and that it is the means of supporting the activity of the System."

"Narcotics are the chief of the Sedative powers, and a sedative effect is their direct tendency; sometimes it is their only effect, and they proceed to destroy life altogether. But on many occasions we find them acting as Stimulants, and it is either in consequence of their quantity, or of their further operation, that they prove sedative. The explanation of this has always been found to be difficult. It has been imagined that the same substance, as Opium, may contain both a matter that is stimulant and a matter that is sedative. We know that such a combination takes place in some Narcotics, as in Hyoscyamus; but we cannot perceive it every where, and the more general cases would require some other explanation. I have explained it before. The analogy runs through sedative matter, sedative powers, and refrigerant powers; viz. that they are *indirect* stimuli that excite sensations which produce an exertion of the Animal power, suited to oppose their effects."

With regard to Electricity, considered as a Stimulant power, Dr Cullen remarks—

"It appears that by a mechanical force, the Electrical matter can, in its rapid motion, prove a stimulus to the nervous system. I think its stimulant power is thus simply accounted for, and agreeably to all electrical experiments. But it is remarkable, that, in its operation on the nervous system, Electricity leaves a stupor and some degree of torpor upon the parts it passes along; and acting more strongly, it can extinguish the powers of life altogether. This has been supposed to be but another operation of its stimulant power—that by the violence of its impulse, it destroys the texture of the sensorium. But from collating the whole of the facts upon this subject, as detailed by Dr Priestley, we find that, on innumerable occasions, where examinations have been made, no sort of lesion of the brain has been observed, and

therefore we must seek for another explanation. I am willing to explain it in this way, which is analogous to many other phenomena in the animal economy,—that any power exciting the Nervous system in a very considerable degree, is followed by a proportional degree of Collapse, or diminution of the energy of the brain; and when the excitement is to a certain degree, the Collapse may be fatal."

The influence of different degrees of Temperature, and of changes of Temperature, upon the condition of the human economy, was a subject to which Dr Cullen paid much attention; and his speculations regarding it he constantly endeavoured to reduce to general principles. His opinions upon this subject were at different times communicated to the public in the inaugural dissertations of several of his favourite pupils,—as in that *De Catarrho*, published by Dr George Fordyce, on graduating at Edinburgh, in 1758; in that *De Frigore*, published by Dr Percival, on graduating at Leyden, in 1765; and in those *De Calore*, and *De Frigore*, published, the former in 1772, by Dr Goulding, and the latter in 1780, by his son Dr Archibald Cullen, on taking their degrees at Edinburgh.

"Heat and cold," Dr Cullen observed in his Therapeutical Lectures, "are considerable agents in the operations of Nature, and are necessarily so with regard to the human body, not only from the powers which they possess over it in common with all parts of nature, but in consequence of their producing sensations, and in consequence also of their peculiar relation to the power of generating heat that is possessed by animal bodies. Upon that account they are very important objects of our study. I have frequently said that Heat seems to be the vivifying power with respect to animals,—that which first produces the Vital powers; and it

seems equally necessary for the after support of the Animal power, or Energy of the brain; so that heat increases the motions that are the most peculiar and fundamental to the living animal, and may be considered as always a Stimulant power. But its effects are prodigiously varied, according to its different degrees and relations to the different states of the body; and the difficulty is to explain its different effects as they depend upon different circumstances. Cold is a subject the consideration of which, in this climate, is even of more importance than Heat, and which, at the same time, is of more difficulty, as the effects of Cold are more diversified, and the circumstances upon which this diversity of effects depends are much more complicated. In this climate, cold is a principal *potentia nocens;* but we are often puzzled to perceive the occasions on which it acts, and we often do not perceive them but by the effects; and, from the occasions on which it acts not being sufficiently understood, we are not in a condition to guard against its effects. There is a great deal still wanting that is necessary to clear up this matter; and I am not satisfied either with my own views, or my manner of delivering them."

"There is, I believe, a degree of temperature, the application of which to our bodies destroys all sensation; so that there is a lowest degree in which no sensation takes place, and every increase of temperature above that is felt in its first application as hot, and in some measure as a stimulus; but it is not permanently so, nor does it become a permanent stimulus till the increase is above the limit of 62° Fahr., which, in this climate, is our medium temperature. Above that, heat is a permanent stimulus, and always more and more stimulant till the surrounding temperature is 98°, that is, is equal to the ordinary temperature of the body. Within these limits, from 62° to 98°, in this climate, are the proper and durable effects of heat upon the human body exerted. It is certain, in fact, that such a degree of heat increases first our sensibility, and therefore the force that stimuli of every kind

have in producing sensation; and, next, it increases the energy of the brain in the system, at least in so far as that can depend upon a greater degree of mobility, or susceptibility of impression, and, therefore, the increased energy of the brain appears in some increase of the action of the heart and arteries, which especially depends upon the constant stimulus of the blood; but, at the same time, the continuance of such a degree of heat constantly weakens the tone of the part, either diminishing the vigour, except in so far as that is supported by the stimulus, or rendering the exertions in any part of the system less durable. From its increasing the action of the heart and arteries, heat has effects upon the distribution of the blood; and every increased action of the heart and arteries is especially exerted upon the vessels of the surface, causing an increased perspiration. Heat operates in this respect both in consequence of its influence on the distribution of the blood, and of its *direct* stimulus, applied to the surface of the body, increasing the action of the vessels there; it therefore takes off the determination to the internal parts; and hence it is that a warm climate is beneficial in diseases of the Lungs. At the same time, an increase of heat diminishes the internal secretions, and renders some of them less diluted, and consequently more acrid. Its principal effect is from the relation between the surface of the body and the alimentary canal; by supporting constantly the dilatations of the extreme vessels, it prevents the return of appetite so sharp as it is when cold is applied to the surface of the body."

"I may next proceed to consider how the effects of Cold are produced in a greater or less degree, according as the cold is more or less intense. And, first, I say that, under the ordinary circumstances of our system, if any body of a lower temperature than 98° be applied to it, a sensation of cold is immediately felt; but if the degree of this temperature does not fall below 62°, as the generating power of heat is equal to such a loss, it is very soon counteracted by communi-

cation, and the part of the system affected is immediately restored to its temperature, and the generating power still increasing the heat, the application is immediately felt hotter than before. Thus, for example, if a person plunges his body into the warm baths of Buxton, which are of the temperature of 81° or 82°, he has a sensation of cold; but if he remains a few minutes he feels them warm. I have only to add, that this change takes place in proportion to the vigour of the power generating heat within, and probably the sooner from this generating power being increased by the tonic effects of cold. The conclusion, therefore, is, that nothing is felt to be permanently cold, unless it exceeds what the generating power of heat within is able to compensate."

"Though the cold is below the medium temperature of the atmosphere—62° Fahrenheit—if the application is transitory, the heat will soon be recovered, as the cold acting *indirectly* as a stimulus will increase those motions upon which the generating power of heat depends. But if it is intense in its degree, and permanent in its application, so that it becomes an overbalance for the generating power, then more permanent effects are produced, as a constriction of all the vessels upon the surface of the body, and therefore a suppression of Perspiration. And perhaps, in the same circumstances, or in a higher degree, by producing a stronger contraction in the Moving fibres, Cold produces the Inflammatory Spasm; and if it truly lowers the temperature of the parts to which it is applied, it will produce a certain degree of Atony, a loss of Sensation and Mobility. With regard to these three effects of Cold, suppressed Perspiration, Inflammatory Spasm, and Atony, I would observe, that the last is very apt to accompany the first, and this is the reason why suppressed Perspiration is upon many occasions so difficult to restore; and I can find nothing else that will explain the necessity for warm bathing, which prevails in the coldest climates we are acquainted with. Farther, I would observe, with regard to these effects, that the suppression of Perspiration might be

especially expected from a very general exposure of the whole body to cold; but the tonic and stimulant effects of the cold applied, actually prevent the change of temperature upon the surface, so that the suppression of Perspiration is more frequently produced from the application of cold to a particular part of the body only. The Inflammatory spasm, likewise, appears to be more commonly the effect of a partial application of cold. And, further, you will observe that this application seldom produces an inflammation upon the surface, because there the contraction of the vessels is attended with a certain degree of atony; but it produces its effects in the internal parts, as in some of the viscera."

"As the changes from hot to cold continue to fall below 62°, they tend still farther to diminish the effects of the generating power of heat, and are capable of communicating a lower temperature to our bodies, and therefore they come to diminish Sensibility—to produce Torpor—the collapse of Sleep—and may proceed till they extinguish the Vital powers in the whole system, and occasion death; and these different effects, it will be readily understood, take place in the order I have mentioned. With regard to the effect of such changes in producing death, where the cold is applied to the whole body, the effects will be gradual, and no congelation of fluids will come on before there is a total extinction of the Vital powers; for so long as these powers subsist, there is such a degree of the generating power of heat as to prevent any such congelation. Gaubius explains the production of death by cold as depending *ex encephalo congelescente*, &c. but there is no appearance of its being the effect of freezing. In all those animals that pass the winter in a state of torpor resembling temporary death, the action of the Vital powers totally ceasing for a time, we observe that life is recoverable only in so far as they have avoided the freezing power by retiring to caverns, immersing themselves in masses under water, &c. Even in those affections arising from the more partial application of cold, I doubt if freezing has any share

in producing them: the vital powers are gradually diminished, sense and mobility are rendered less and less, and the fluids may stagnate in the part, but from their communication with the fluids in the rest of the body they preserve their fluidity, and in some measure their heat; and it is only from their doing so, that a state of putrefaction and gangrene can arise, which is an affection of a living body, and different from the putrefaction that takes place in the whole, or in a part of a dead one. The body, after death, is more or less disposed to putrefy, in proportion to the quantity of fluids in a fluid state which it contains; and, as the tendency of the blood in such cases continues longest towards the depending places, it is always there that the putrefaction begins; but in a dead body, exposed to a considerable degree of cold, perhaps at the freezing degree, putrefaction never takes place at all."

"The last of the Stimulant Means to be mentioned, are the Emotions and Passions of the Mind. The greater part of these are manifestly either *Direct* or *Indirect* Stimuli. The ascertaining the effects of particular passions I find a very difficult subject; I have not yet satisfied myself with regard to it, so as to enable me to deliver it as I could wish. The difficulty arises from the effects of the same passion being different according to its different degrees, and according to its duration, and the frequency of its repetition; and the effects of the passions are in general the more difficult to be ascertained, from their being always very intimately mixed with one another—even those of a contrary nature. It is this very circumstance that prevents our observing the effects of each, and it is, I think, from the most part of writers being inattentive to this intermixture of different passions, that they do not give us the information we expect. Without analyzing the whole, and considering the particulars, we learn nothing properly, so that I cannot prosecute this subject very fully, and must only give you two general principles that will guide you in judging of particulars. The first

is, that all our feelings may be referred to the Agreeable or Pleasant, and the Disagreeable or Uneasy, and that all our pleasant emotions are Stimulant, and all our uneasy emotions are Sedative, in their *direct* tendency at least, and, if they appear otherwise, it is from a sudden vicissitude to a contrary passion. The Stimulant emotions, often repeated, induce debility, and, when in excess, they can extinguish life altogether; the Sedative not only suspend action, but they induce stupor, torpor, and even death itself; most of them, however, do not produce this effect immediately, but indirectly, by disordering the functions, in consequence of which they bring on death. Secondly, our passions lead to action or restrain from it. All those that lead to action are in their nature Stimulant; all those that restrain from it are Sedative. I can go no farther than to give these rules."

Of the last class of Therapeutical Agents capable of changing the conditions of the Moving fibres, viz. Sedatives, Dr Cullen observed;

"Sedatives are powers which *directly* and without evacuation diminish motion in Animal bodies; or, considered as Remedies, they are powers which diminish the preternaturally increased motions of Animal bodies. Increased motion may be considered as of two kinds, according to the part of the System which it chiefly affects. Thus, the increased motion may appear chiefly in the Sanguiferous system, and no otherwise in the Nervous system than as it is immediately connected with the Sanguiferous. Or the increased motion may appear chiefly in other parts not necessarily connected with the Sanguiferous system, and may affect it only in a secondary way, and in a transitory manner. This distinction will be readily understood; and both cases require to be again distinguished in regard to the causes that produce them. Every action, every motion that occurs in the human body, may have a comprehended cause; and the degree of action must be as the stimulus applied, and the mobility of the System, or its susceptibility of impressions, taken together. Increased

action or motion, therefore, may be considered as arising either from the power of stimuli that are unusual, or from the usual stimuli being increased; or, the stimulus being given, it may arise from increased Tone or Vigour, or more strictly from increased Mobility. From this general view, the complication of the subject will appear, but I must endeavour to separate and arrange it as well as I can."

"*First*, then, with regard to the increased motion occurring in the Sanguiferous system, such as that in which Fever consists: this may be without, but it frequently is conjoined with increased Tone in the Arterial system. Now, in order to diminish such increased motion, we must, 1*st*, take away the stimulus producing it; 2*d*, we must avoid the concurrence of other *irritations* that would increase the power of the stimulus; and, 3*d*, we must take off the increased tone.

"1*st*, If the increased action of the Sanguiferous system arises from a matter introduced into the body from without, this matter has been supposed to act as a *direct* stimulus, and, accordingly, the means for taking it away have been those for correcting and expelling it by Concoction and Crisis. But without discussing this difficult subject, I say that we truly know nothing of the nature of the matter thus introduced; we know not so much of it as to know how it is to be either corrected or expelled; and those who have supposed it to undergo these operations, have, at the same time, allowed that they must be left to nature. Without remarking on that, I say the whole of the inquiry is superseded by this consideration, that this matter which introduces Pyrexia into the system, acts by inducing Atony or Spasm in the extreme vessels, which proves *indirectly* a stimulus, both causing and supporting the increased motion we speak of. The means of taking off the stimulus that can be employed, are truly, therefore, the means of taking off this Spasm. This may, perhaps, be accomplished suddenly by Sudorifics, but their use is somewhat ambiguous. It is commonly accomplished more slowly by Diluents, Refrigerants, Emetics, by

Warm-bathing, and by Blisters, which are the means of taking off the stimulus subsisting in the system, in the case of Fever or Pyrexia. These remedies, therefore, may be considered as Sedatives.

"2*d*, The increase of stimulus we avoid, by avoiding the concurrent *irritations* arising from stimuli that are often unavoidably applied in a greater or less degree to the body. In this consists the Antiphlogistic regimen,—the avoiding the sensations of light and noise;—the avoiding the exercise of the body, and also of the mind, so far as this can be a stimulus;—the rendering the aliment, that unavoidable source of irritation, as little irritating as possible, with respect to its quantity and quality;—the supporting the excretions, in so far as their retention is irritating, by proper dilution, which especially increases the perspiration and urine, and by glysters which increase the evacuation by stool;—and lastly, the taking off those *irritations* that are occasionally present, particularly the *irritations of the stomach* from indigestion or other causes.

"3*d*, Where the increased motion depends upon the increased tone or vigour of the system, upon the *diathesis phlogistica*, it is to be diminished by the various Atonics, chiefly by Bloodletting, Purging, and the application of Cold. With regard to the application of Cold as an Atonic, I am at a loss to speak of it decidedly. The practice with regard to small-pox, lately introduced, shews the Atonic or Sedative power of Cold, but to what other cases of the febrile state it is applicable, is not yet ascertained. Probably it is of considerable application, and our doubt with regard to it arises from our not knowing with certainty the operation of Cold —whether it acts by diminishing the heat, or *directly* as an Atonic—and consequently from our not knowing the circumstances or means by which we may avoid its stimulant or spasmodic power, when we seek only for its Atonic effects. These, then, are the Sedative means in the case of increased motion in the Sanguiferous system.

" *Second*, When the increased motion is more entirely in the Nervous system, and only occasionally affects the Sanguiferous, and that commonly in a transitory manner, the means to be employed are referred to two heads, 1st, The obviating and preventing the increased motion; and, 2d, The taking it off when it actually occurs. With regard to the first object, it is obtained by avoiding Stimulus or by correcting Mobility, from both of which the increased motion may proceed. Now we avoid Stimulus by avoiding external causes such as particular odours or other causes of disagreeable sensations, whether applied to the organs of sense or to the stomach; and under this head, too, we comprehend the avoiding of all such causes as produce emotions or passions of the mind; we avoid all hurry or excitement of the Energy of the Brain. Next, we avoid stimulus, and thereby obviate increased action, by removing stimuli inherent in the body, as teething, worms, calculi, and particularly the irritation of the stomach from indigestion and other causes; and we are especially to attend to the fulness of the system, which generally gives rise to both increased Mobility and Irritation. We obviate increased action in the Nervous system, again, by taking off the Mobility or morbid Irritability of the system, which is sometimes original, but is often acquired only by habit, and which has often a greater share in the production of Nervous affections, than the power of the stimuli applied. This Mobility is to be taken off either by interrupting the course of habit, or by the employment of Tonics, of which I have already spoken.

" With regard to the second object,—the taking off the increased motion when it actually occurs,—this is to be effected by taking away the *Irritation* that is present, when it is evident and can be removed. In several cases, when it is evident but cannot be taken away, we endeavour to prevent its effects by cutting off the communication between the part affected and the Sensorium, by cutting through nerves, or by compressing them by ligatures; and, lastly, increased motion, when it occurs, may be diminished by the power of Narcotics,

which are substances that have the power of diminishing the Excitement, and, therefore, the activity of the Nervous power, and that to the degree of diminishing the exertion of the Animal power or Energy of the Brain."

In his Treatise of the Materia Medica, Dr Cullen has described the manner in which he conceives Narcotic substances to exert their Sedative influence upon the System.

"Narcotics," he observes, "are commonly remarkable for inducing that cessation of sense and motion, in which sleep consists; and are, therefore, often named *Soporific* or *Hypnotic* medicines. As their power and operation may be extended so far as to extinguish the vital Principle altogether, they form that set of substances which are properly, and such as may be strictly, called the *Poisonous*.

"As the powers of sense and motion chiefly depend upon the state of the Brain, so it has been commonly supposed that the medicines we treat of, act primarily and especially upon that organ; but as the operation of Narcotics diminishes the motions and powers of motion, especially in the parts to which they are *immediately* applied, and this, too, in parts which are entirely removed from all connexion with the Brain, we must conclude that their operation is upon a matter in common to the whole of the Nervous System—that they diminish the Mobility of the Nervous power, and when in a certain quantity can destroy that Mobility altogether.

"Although the operation of Narcotics is first and especially on the Nerves to which they are immediately applied, it is very constantly at the same time communicated to other communicating nerves, more or less, according to the number and *peculiar sensibility* of the nerves to which they are first applied. The most remarkable and frequent instance of this is in the application of Narcotics to the internal surface of the stomach, where both the number and peculiar sensibility of the nerves give occasion to a very extensive and widely diffused operation; so that it is especially and most

commonly from the operation of Narcotics on the stomach and alimentary canal, communicated to the Brain, that general effects so readily appear in the whole System. It is accordingly these effects, extending from the Sensorium to the whole System, that have been chiefly attended to in the operation of Narcotics.

" But it is proper to be more particular, and therefore to observe, that the effects of Narcotics, commonly and especially, first appear in those functions in which the mobility of the nervous fluid or power admits the most readily of a change; that is, in the *animal* functions, in the cessation of which sleep consists : and therefore it is that this state is so commonly induced by them. At the same time, the effects appear also in the *vital* functions, so far, that the motions of these are weakened, and the frequency of their action rendered less; and although this, from considerations to be mentioned afterwards, may not constantly appear, there are, however, innumerable experiments which prove that a diminution in the action of the vital functions is frequently, and even commonly, the effect of narcotics. The power of narcotics in diminishing the mobility of the nervous power appears still more considerably, and without ambiguity, in the *natural* functions. Thus, the activity of the alimentary canal, that chief organ of the natural functions, is always diminished by narcotics, any how thrown into the body. Another effect of narcotics, relative to the natural functions, is the diminution and suspension of all secretions, and of every excretion except that of sweat."

" From all this it appears, that the operation of Narcotics extends to every function depending upon the Energy of the Brain; with respect to which they show a sedative power; and though this be various in its degree, and variously modified, both by the different kinds and quantities of the narcotic, and by the different conditions of the body to which it is applied, yet the effects are universally and *directly* sedative. There occurs, here, however, a considerable difficulty,

as it is to be particularly remarked, that narcotics, instead of always proving sedative, or diminishing the action of the heart, often seem to be powerfully stimulant with respect to this organ, and in their first operation often increase the force and frequency of its action. How this may be explained, in consistency with our general doctrine, it is difficult to say. Some have imagined that in the same narcotic substance there is a stimulant as well as a sedative matter; and that they have some foundation for this opinion appears from this, that the substance of the narcotic is acrid to the taste, and when applied to the skin readily inflames it. That in wine, or other ardent spirits, which commonly act as narcotics, the stimulant matter is in large proportion, may be readily admitted. But, on the other hand, the *direct* stimulant power is doubtful; as in many substances the sedative power appears in masses of so very small bulk, and in that bulk the stimulant matter can hardly be in such proportion as to stimulate the heart very powerfully; as we know no instance of a pure stimulant that in the same bulk will have that effect, even when applied to the stomach or in any other way to the body. Another consideration may be also offered here. There is no ground to suppose that, were a stimulant and sedative power combined in the same matter, the stimulant power should commonly act before the sedative.

" To explain therefore the stimulant effects that often appear from the exhibition of Narcotics, it seems necessary to assign some other cause than the *directly* stimulant power of the substance applied; and this cause appears to be that resistance and consequent activity which the animal economy is suited to oppose to every application that has a tendency to hurt it. This power, as we have said before, is well known in the schools of physic under the title of the *Vis Conservatrix et Medicatrix Naturæ;* which, however difficult to explain, must, as a general law of the animal economy, be admitted as a matter of fact, as we have already, when treat-

ing of Stimulants, endeavoured to prove very fully. We have no doubt that it might be fairly employed here to account for the stimulant effects which so often appear upon the exhibition of narcotics, which are, indeed, very often evident and considerable; but they do not imply any directly stimulant power in the narcotic substance, as they can be so well accounted for by considering them as *indirect* stimulants, in the manner we have both here and above explained.

" I will only add one other illustration on this subject. It is frequently the effect of narcotics to excite that delirium which is well known under the name of Ebriety or Drunkenness. This often appearing with the same circumstances that are ascribed to a stimulant power, has been frequently supposed to arise from a stimulus applied to the brain; and it is true that it does in some measure depend upon the stimulant operation which here takes place in the manner we have explained; but, were it proper here, it might be shewn by the laws of the animal economy, that a stimulus is commonly insufficient; and that it is only by the concurrence of a sedative power that the symptoms of ebriety, produced by the exhibition of narcotics can be accounted for.

" We conclude, therefore, upon the whole, that the operation of Narcotics is always *directly* Sedative. But before we proceed to illustrate this in particulars, it will be proper to take notice of a circumstance that relates to the operation of Sedatives in general. With respect to them, it is to be remarked, that when Sedatives thrown into the body do not prove absolutely mortal, their operation is of a certain duration only; and, therefore, after a certain time, or at least sooner or later, according to circumstances, it entirely ceases, or at least its effects are greatly diminished. It is in consequence of this that when, for the purposes of medicine, it is necessary that the effects of Narcotics be continued, this can only be done by a repetition in due time of the Sedative; and, upon such occasions, it is found that the law of the economy, by which all impressions which do not excite to action

become weaker by repetition, here takes place; and, therefore, that, in the repetition of Narcotics, the impression, that is, the dose, must be made stronger than before. This happens very constantly in the repetition of Narcotics; and to persons acquainted with the powers of Custom, affords an argument, that in most instances those Narcotics act by their sedative rather than by their stimulant powers. This ceasing of the effects of Narcotics is difficult to explain; and, to do it, we must take notice that it is connected with a question occurring with respect to natural Sleep, which is—Whether this, after it has taken place for some time, always ceases from stimuli applied? or, if it ceases spontaneously upon the System being restored to the state in which it was before the causes of sleep were applied? The latter account will be adopted by those who suppose the Nervous fluid to be a secretion that may be exhausted and again restored and supplied. But this is a supposition so improbable, that I believe few maintain it at present; and, if we reject it, the question returns to say—How the state of the Nervous or Animal power, either when natural sleep has subsisted for some time, or when it has been artificially induced by Narcotics, returns or is restored to its waking condition?

"Although it may be difficult to explain in what physical or mechanical condition of the Brain the different states of sleep and waking consist, it is highly probable that these two states do truly alternate with each other. That the state of waking does necessarily induce the state of sleep, will be readily admitted; and it is equally probable that a certain duration of sleep does not only take off the state of sleep, but does also induce the condition necessary to the waking state. If this is admitted with regard to natural sleep, it will be readily allowed that the same means will also operate on the state induced by Narcotics, and will therefore at length naturally put an end to it."

I cannot, I believe, conclude the account of Dr Cullen's course of lectures on the Institutions of Medi-

cine better than by introducing the notes from which he seems to have delivered his valedictory address to his students at the termination of the session 1770–71, of which I find a copy in his own handwriting, headed "Epilogue, 15th May 1771."

"With regard to both Pathology and Therapeutics, the best assistances are to be got from writers on the practice; and we are to conclude with this general advice, that a man, moderately imbued with a system of institutions, may read medical writings of all kinds, but what he is especially to search for is facts, and wherever he finds these he ought immediately to think of applying them to the institutions; that is, to establishing, limiting, or correcting his General principles, and a plan of this kind ought to be constantly in view.

"I have now told you how you may prosecute your studies, and have only to add a short apology for all my own deficiencies and imperfections. I know they are too many to admit of a full excuse, but some apologies may be admitted. The multiplicity of our subjects occasioned some to be omitted, and many to be discussed too briefly. Some wonder at my continuing my lectures beyond the usual period, but there is more reason to wonder that I have been able to touch so many subjects, and conclude so soon.

"Of the subjects touched, many of them are imperfect, but I plead that the present state of science does not admit of our going farther. I have endeavoured always to point out the most important inquiries. I have attempted to throw new lights upon them, but always, I hope, with due diffidence. I have dictated no opinions, but have always led you to the exercise of your own judgment. It is only by this exercise you will be led to inquiry, or can succeed in it. I shall be vain of having erected you into critics upon myself. If this be upon a full view of the subject, it may lead you to discover my faults; but I expect it will at the same time dispose you to excuse them, and I shall thus be happy in connecting your knowledge with my own credit."

In the sketch that has been given in extracts from his manuscript lectures on the Institutions of Medicine, of the view which Dr Cullen took of the different functions of the Nervous System in the animal economy in the state of health,—of the universal and constant influence of this system in the production and modification of the phenomena of diseases,—and of the various effects produced upon it by external agents,—exhibiting the general outlines of his Theory of Excitement, —I have had it chiefly for my object to bring under the eye of the reader some of those more original attempts at the generalization of facts,— or approximations to truth, as he was accustomed to term them,—by which, in relation to the influence of the Nervous System on the different functions of the animal economy, Dr Cullen endeavoured to promote the study of the science, and to improve the practice of medicine.

Some traces of the recognition of the existence of such an Energizing influence of the Nervous System as that which Dr Cullen has designated under the appellation of the Animal power or Energy of the Brain, are unquestionably to be found in the writings of some of his predecessors ; but Dr Cullen seems to have been the first medical teacher who pointed out the general and automatic agency of this power in all the motions of the animal economy, voluntary, involuntary, and mixed ; and who endeavoured to collect and arrange the principal facts regarding it under distinct heads. " The subject of the Nervous System," he himself observed, in his lectures on the Institutions, " has been but slightly touched on by any physiologist, and very imperfectly handled ; and I flatter myself that I have brought it more into view than has hitherto been done.

You will be greatly at a loss to find much assistance in studying it, in the writings of physiologists." "As it appears to me certain, that the human system can only be viewed in these three respects, that is, as a chemical mixt, as a hydraulic machine, and as an animated nervous frame, I consider our system of physic to be now complete as to the parts which it ought to comprehend."* How extensively and deeply Dr Cullen's physiological, pathological, and therapeutical speculations with regard to the Nervous System have influenced the opinions of medical philosophers and practitioners from the time they were first delivered in his lectures on the Institutions of Medicine in the University of Edinburgh to the present, will be most readily perceived by those who are best acquainted with the past history, and with the recent progress of discoveries and improvements in Medical Science,—with the important share which has been allotted to the doctrine of Innervation or the Influence of the Nervous System, in the physiological and pathological writings that have lately issued from the press, both in this country and on the Continent of Europe,—with the different changes in language and statement which the Excitement Theory of Dr Cullen has undergone, and with its various modifications as it is at present taught in the Medical schools in different parts of the world. Dr Cullen has himself informed us of the use which he wished to be made of these speculations, in the improvement of a science, which,—from its being the result of observation, experience and critical analysis, and from its dependence on the state of other collateral arts and sciences,—he regarded as being necessarily but

* Works, vol. i. p. 409.

slowly progressive in its advancement, in the following passage with which he was accustomed to terminate his account of the functions of the Nervous System :— "I do not say that this is the Theory of the Nervous System : I say that these are the chief facts and laws relative to it; for in this and every other part of the Physiology I am more anxious to state what happens than to explain how it does happen; and wherever, in some instances, you may think I have approached to theory, I would wish you to receive every particular I have delivered with great diffidence, till you are persuaded that the conclusion is established as a matter of fact. If you do so, there will be no danger of misapplication; you will be constantly enlarging the number of facts, and be much more fit for the application of them*."

The learned Professor Sprengel, in observing that "Dr Cullen appears to have been the first who, in combining the opinions of Hoffmann with the new doctrines of the forces of the body, founded a true system of the living solid," has by some oversight alleged that it was late before Dr Cullen perceived the errors of the Boerhaavian system, and has referred to the introduction to the First Lines of the Practice of Physic for Dr Cullen's express corroboration of this assertion †. The passage referred to, however, obviously relates to the progress of the opinions of the other Professors in the Edinburgh School of Medicine, and not to that of Dr Cullen's own mind, in discovering and pointing out the imperfections and deficiencies of the Boerhaavian

* Works, vol. i. p. 157.

† Hist. de la Medecine, tom. v. p. 359. Paris 1815.

system. "When I first applied," says he, "to the study of physic, I learned only the system of Boerhaave; and even when I came to take a Professor's chair in this University, I found that system here in its entire and full force." That Dr Cullen had at a very early period adopted the views of Hoffmann respecting the Nervous System, is sufficiently established by the testimony of his pupil Dr Wallace; and Dr Cullen himself, in the introductory lecture already quoted, explains, in a distinct and ingenuous manner, the circumstances that led him to abandon the medical opinions which he had received from his instructors *.

The opinions which Dr Cullen delivered in his Lectures on the Institutions of Medicine, respecting the functions of the different parts of the Nervous System, and particularly his doctrines respecting the Animal power, or Energy of the Brain, and the states of Excitement and Collapse, attracted, by their ingenuity and novelty, the particular attention of his students,—became the subjects of daily discussion in their societies for mutual improvement,—were applied, in many of the inaugural dissertations issuing from the University to the explanation of diseases,—and gradually found their way into the Medical writings published, both in this country and on the continents of Europe and America, by several of those who had been educated at the University of Edinburgh. In 1772, Dr Cullen himself communicated to the public a view of his opinions respecting the Nervous System, and particularly respecting its functions in the state of health, by the

* See pp. 24 and 118 of this volume.

publication of his Outlines of Physiology, a work admirably adapted to serve as a model for that form of Text-book which it is so desirable that every Professor should put into the hands of his pupils as a guide in the study of the branch of philosophy or science which it is his duty to teach,—as consisting not of a bare enumeration of the topics to be discussed, but of a concise summary of the general principles of the science, in distinct propositions of which it was the purpose of his course of lectures to afford the illustrations and proofs. A second edition of the Outlines was published in 1777, in compliance with the wishes of Dr James Gregory, who had by that time succeeded Dr Cullen in the chair of the Institutions of Medicine. A third edition appeared in 1785, and in the following year a German translation was published at Leipzig under the title of "Anfangsgründe der Theoretischen Arzneiwissenschaft."

The fullest and most circumstantial account, however, that was given, or indeed that has ever been given, to the public of Dr Cullen's opinions relative to the functions of the Nervous System, is that which is to be found in a work entitled, "Analyse des Fonctions du Système Nerveux," by Dr De la Roche of Geneva, printed at that place in 1778. The following letter addressed to Dr Cullen in 1772, explains the circumstances which gave rise to that publication:—

"Geneva, 11*th July* 1772.

"Sir,—Nothing could be more agreeable to me than receiving, as I did some time ago, by Dr Moore, a letter from you, and seeing that I still hold a place in your remembrance and good will. The pleasure this gave me can only be felt by those who, like me, have been so happy as to be person-

ally acquainted with you, and have conceived that love and regard for you,—I had almost said that enthusiasm,—which is so general among your pupils. But I am afraid lest you should repent of that favour when you see that it has emboldened me to importune you. * * * * *

" Among other means of making myself known to the public as a physician, I have pitched upon one which will be an arduous task:—it is writing a book. I see what credit some physicians have acquired by publishing very *mediocre* performances, and mostly because they were written in their vernacular tongue. I have, therefore, thought on a subject; and the one I find to suit best my intention is that of Nervous Disorders, as it is so rich, and so little has been published upon it that is any way systematic. But I cannot give any thing interesting on that matter but what I have learned from you, and as it is not my opinions but yours that I wish to offer to the public, I will not undertake any thing towards it till I have your leave of doing it. You must not doubt but that if any honour accrues to me from it, I will, as just, refer it to its legitimate owner, and inform my readers of the source whence I have taken my ideas on the subject. If, notwithstanding that I have set my heart upon this, you should seem to take it amiss, you may depend upon it I shall not think any more of it. But if, on the contrary, you grant the permission I ask, would you be so good as to give me some advice upon a general plan; I have none well formed as yet.—I remain, with the utmost regard, Sir, your most humble, obedient, and most obliged servant,

" De La Roche."

I have not been able to find any copy or notes of an answer to Dr De la Roche's letter among Dr Cullen's papers, and it is therefore impossible to say whether, in publishing his account of Dr Cullen's opinions, he had obtained his permission to do so; though, from his not making any mention of such a permission in

the preface to his book, it seems probable that he had not obtained it. In that preface, Dr De la Roche certainly acknowledges his obligations to Dr Cullen's lectures, but in a manner calculated at least, if not intended, to impress the reader with the belief that the account which he published of the functions of the Nervous System, was intended for the purpose of explaining his own views, rather than the opinions of Dr Cullen.

"With regard to the plan which I have adopted," he observes, "I should be essentially wanting in what I owe to Dr Cullen, if I did not acknowledge that I have followed the syllabus of his lectures, published at Edinburgh in 1772, under the title of Institutions of Medicine, part I. Physiology. This little work, in which the subject of the Nervous System occupies from p. 24 to p. 115, has served me as a text; but I have taken the liberty to make some changes on it, in regard both to form and matter. I have omitted some propositions that have appeared to me too hypothetical. I have added others, with the truth and importance of which I was more struck. In general, in the development of all those which I have preserved, and which form by much the greatest number, I have modified them in my own way, because I have had it less in view to state the opinions of Dr Cullen, than to explain my own."

Notwithstanding this declaration on the part of Dr De la Roche, I feel it to be incumbent on me to state, that a careful and repeated comparison of his "Analysis," with the manuscript notes of Dr Cullen's lectures, has left me no room to doubt that that work, instead of containing opinions peculiar to its author, consists in most parts of a literal, and in some parts of a free, translation of Dr Cullen's account of the functions of the Nervous System, as given in his lectures on Phy-

siology, Pathology, and Therapeutics, to which Dr De la Roche has occasionally added illustrations taken from other sources. That this was the impression which the work produced, at the time of its publication, on the minds of Dr Cullen's pupils, is manifest from the following passage of a letter addressed to him, in May 1779, by Dr Beerenbroek: "Dr De la Roche of Geneva, who some years ago was in Edinburgh, has published your Physiology under the name of Analyse des Fonctions du Système Nerveux. I am told that he has enlarged it, but can give no full account of it, as I have not been able to find it in Paris."

The "Analyse des Fonctions du Système Nerveux" was translated into the German language, sixteen years after its publication, by Dr Merzdorf of Halle; and has usually, since that time, been referred to by French and by German medical writers, as containing the opinions of Dr De la Roche. Under this impression, the late distinguished Professor Reil, than whom there could be few better judges of the value of these opinions, states in a review of them, which he gave in the first number of his Archives for Physiology (1795), that "Dr De la Roche has the merit of having been the first who has described properly the peculiar functions of the Brain, and who has entered upon the true path of investigation in the Physiology of this the noblest organ of the animal economy." In addition, he observes, that, "in his work, equally important to the progress of Medicine and of Psychology, the author, with the greatest sagacity and the finest spirit of observation, has shewn that the different affections of the mind are dependent upon corporeal changes; has

introduced simplicity and order into the consideration of the phenomena of the animal economy, and has reduced these phenomena to general laws." But whatever degree of praise is justly bestowed on the Analysis of the Functions of the Nervous System, in the character given of that work by Professor Reil, must be transferred to "its legitimate owner," Dr Cullen, as his Lectures were the source from which the whole of Dr De la Roche's ideas on the functions of the Nervous System, and more particularly those which relate to the operation of the Animal power, or Energy of the Brain, were immediately and undeniably derived.

During the period that Dr Cullen was occupied as a public teacher in Glasgow and Edinburgh in the study and explanation of the functions of the Animal economy, and in endeavouring to extend and to improve the views which had been taken by Hoffmann, Haller, Whytt, and others, of the influence of the Nervous system upon all the functions of that economy, considerable changes were gradually taking place in the doctrines taught in the other medical schools of Europe, though in most of these schools the systems of Boerhaave and of Stahl continued for a long time to predominate. In the school of Montpellier, in particular, which had existed for upwards of 500 years, and had produced a succession of celebrated physicians and surgeons*, SAUVAGES†, who was thoroughly imbued

* Mémoires pour servir à l'Histoire de la Faculté de Médecine de Montpellier. Par M. Astruc. 4to. Paris, 1767.

† Sauvages was born in 1706, appointed Professor at Montpellier in 1734; died in 1767.

with all the medical learning of his own and of former times, adopted, and retained till the time of his death, a system of the animal economy which combined more or less of the doctrines of the mechanical physiologists with the Animism of Stahl. Instead, however, of referring with Stahl, all the motions of the animal economy to the operation of the *rational* soul alone, Sauvages ascribed to this power only those motions which are performed from the dictates of reason. The evident muscular motions that are concerned in the exercise of the vital functions of circulation and respiration, and, in some of the natural functions, he referred with Whytt to the agency of the *sensitive* faculty of the soul, which he denominated Nature; while he imagined that the functions of nutrition, secretion, digestion, and generation, which are common to animals with plants, might, except in so far as they are in the animal creation connected with or dependent upon muscular motion, be accounted for by the agency of the properties possessed by inanimate matter, such as gravity, cohesion, or attraction, elasticity, &c., which he denominated inanimate forces *.

But in employing the word Nature to signify the sensitive or animal soul, instead of using it as had been done by Galen and by Hoffmann, as a general term significative of the common cause of organic life, and corresponding with the vegetative soul of Aristotle, it is obvious that Sauvages made a great, unnecessary, and therefore inadmissible, change on the language of preceding physiologists. His supposition that the Or-

* Prolegomena in Nosologiam, sect. 152, 154, 208, 210, 235, 242, 261.

ganic functions of digestion, secretion, nutrition, and generation, common to plants with animals, are, except in so far as they depend on muscular motion, produced by the operation not of animate but of inanimate forces,—whether, in the progress of natural knowledge, it shall prove to be well or ill founded,—shews how little foundation there is for the assertion of the historian of the school of Montpellier, M. Berard, that Sauvages was the first physiologist in Europe who set aside mechanism in explaining the functions of the animal economy *. It is singular that Sauvages should not, in any part of his writings, have made any reference to the peculiar views which his contemporaries Hoffmann, Haller, and Whytt, had respectively taken of the functions of the Nervous System.

The first medical writer of the school of Montpellier, who seems to have taken an extensive view of the organic influence of the Nervous system on the functions of the animal economy, is M. BORDEU †. This author explained and illustrated his peculiar opinions with much ingenuity, vivacity, and elegance, in a succession of publications which appeared from 1742 to 1776.

In his thesis *De Sensu generice considerato* ‡, M. Bordeu denied the existence of animal spirits, and maintained the hypothesis that it is the peculiar arrangement of the fibrils of the nerves, acting by vibratile motions, which fits these organs for the performance

* Bérard, Doctrine Médicale de l'Ecole de Montpellier, p. 45. & 52.

† M. Bordeu was born in 1722; settled in Paris in 1752; died in 1776.

‡ Œuvres Compl. de Bordeu, tom. i. p. 1. Paris 1818.

of the corporeal part of sensation. Like Perrault and Whytt, he considered the soul to be united with the whole of the Nervous system, but the different parts of the body to be possessed of sensibilities that are peculiar to themselves; and, in a scholium annexed to this thesis, he threw out the opinion that the nerves contribute to the function of secretion *. This last opinion, however, he afterwards more fully explained and illustrated in his treatise, entitled Récherches Anatomiques sur les Glandes, published in 1752, in which he endeavoured to prove that the action of the nerves distributed on the bloodvessels is the cause of the increased flow of blood which takes place in these vessels to glands in a state of secretion, and that on them also depends the selection which particular secreting glands make of the humours contained in the general mass of blood †. Several physiologists previously to M. Bordeu, as Cole, Brunner, Nuck, and Berger, in treating either of the function of secretion in general, or of particular secretions, had attributed some influence in this process to the nerves, or to the fluid of which they supposed the nerves to be the conductors. Although the opinion of the dependence of secretion in animals upon the influence of the nervous system cannot, therefore, be regarded as having originated with M. Bordeu, and although it must be confessed that the proofs which he has adduced to establish this dependence are of a somewhat indefinite and unsatisfactory character, still he must be allowed to have had the merit of pointing it out to the attention of medical men in a much more

* Sect. 45. and 48. and Schol.

† Œuvres Compl. tom. i. p. 45; sect. 107, 108.

particular manner than had been done by any preceding physiologist.

M. Le Caze, the uncle of M. Bordeu, had maintained, in various publications *, that there are three great centres of action in the animal economy,—the Cerebral, Cardiac, and Epigastric or Phrenic centres, an arrangement obviously corresponding with the old division of the functions into Animal, Vital, and Natural. This view of the animal economy was adopted by M. Bordeu, to whom, indeed, the composition of M. Le Caze's treatises has, though without any very satisfactory proof being given, been ascribed. The action of the Phrenic or Epigastric centre, in particular, or of what has more recently been termed the Ganglionic system of nerves which are distributed upon the organs subservient to the Natural functions, appears to have occupied much of the attention of these authors; and, in adopting many of the views originally suggested by Van Helmont, with regard to the influence of the epigastric region in the economy, they gave to them more precision, by substituting the action of the great sympathetic nerve as a physical cause, in place of his Archeus, which they regarded as an occult quality †.

In his Researches on the Pulse, and in those on the History of Medicine, published in 1754 and 1764, M. Bordeu endeavoured to set aside the distinction which had been made by Haller between the properties of Sensibility and Irritability, erroneously alleging that

* Specimen Novi Medicinæ Conspectus, 1749. Institutiones Medicæ ex novo Medicinæ Conspectu; and Idée de l'Homme, 1755.

† Le Caze, Idée de l'Homme, p. 15. Bordeu, Œuvres, t. i. pp. 174, 193, 198; t. ii. p. 671–3.

Sensibility may very well serve as a basis for the explanation of all the phenomena of life, whether in the state of health or of disease *. These views were adopted, and the opinion still more explicitly maintained, that Mobility, Contractility, or Irritability, is merely one of the two powers comprised in the property of Sensibility, by M. Fouquet, Professor of Medicine at Montpellier, in the article Sensibilité, inserted by him into the 15th volume of the Encyclopédie, which was published in 1766.

The shortest and most correct view of the peculiar opinions of M. Bordeu, is that which he has himself given in the first part of his Researches on Chronic Diseases, published in 1775 †. In his desire to be regarded as an original author, this ingenious physician does not, either in this or in any of his preceding writings, take any notice of the views of the functions of the Nervous System which had been given by Hoffmann, Rega, Kaau Boerhaave, Gaubius, Whytt, or Cullen; nor indeed, it deserves to be remarked, have the Neurological inquiries and speculations of these authors been adverted to by his biographers, or by any of the historians of the school of Montpellier, in the accounts which they have given of M. Bordeu's doctrines.

A third distinguished physiologist of the school of Montpellier, contemporary with Dr Cullen, was M. Barthez ‡, author of the "Nouveaux Elémens de la Science de l'Homme," a work which exhibits many

* Œuvres, t. i. p. 420; t. ii. p. 668.

† Œuvr. Compl. ii. 829—847.

‡ M. Barthez was born in 1734; appointed Professor at Montpellier in 1761; died in 1806.

convincing proofs of the intellectual acuteness, the classical and scientific attainments, and the great theoretical and practical knowledge of its author. This physiologist regarded it as essential to the philosophical investigation of the phenomena of living beings, that these phenomena should not be considered as having their origin either in the powers of the mind, as had been conceived by the Stahlians, or in the properties of the organized body, as had been believed by the mechanical school, and still more recently maintained by the Neurologists, whom he terms the Solidists; but he thought it desirable that a general term should be adopted, expressive of the cause that produces the phenomena of life, which should not imply that these phenomena are either of a mental or of a corporeal nature, and the term which he selected for this purpose, was the Vital Principle. "The utility of abstract conceptions with respect to this unknown principle," he observes, "is to secure us against those too limited views which all the sectarians have taken, and against the errors into which they have fallen, in wishing to define the vital principle by more determinate notions *."

M. Barthez did not think it necessary to limit the operation of his Vital Principle to the production of the organic functions of nutrition and generation, the only functions which Aristotle and Galen had assigned to the Vegetative Soul, or Nature; but, instead of the exercise of these functions, he assumed, as its characteristic marks, the being possessed of sensitive and motive forces,—properties which Aristotle in-

* Nouv. Elém. Disc. Prelim. p. 18–22, 27.

forms us, as has already been remarked, had been regarded by all the ancient philosophers as the distinguishing characters of Mind, and which he, under the general appellation of the Sentient Soul, had pointed out as the characters that distinguish living animals from plants. The peculiar sensations and motions which M. Barthez conceived to be produced in living beings, by his Vital principle, he denominated blind feelings and unreflected volitions, in order to distinguish them, as he himself states, from those intelligent feelings and rational volitions which result in the human economy from the operation of the thinking soul. Conformably with these conceptions of the Vital Principle, it was necessary for M. Barthez either to deny life to plants, as had been done by Hoffmann from other considerations, or to affirm that they are possessed of sensitive and motive powers, analogous to those possessed by animals. In adopting the latter of these alternatives, he endeavoured to support his opinion by an elaborate enumeration of those phenomena exhibited by plants, which have been supposed to prove that they are possessed of sensibility and irritability, although he acknowledges that the perceptions which plants have of the objects of their appetites, are, to all appearance, entirely different from those which man or brute animals receive by their senses *.

M. Barthez does not seem to have been able to determine whether his Vital Principle should be regarded as a substance different in its nature from Body and Soul, and existing by itself, or merely as an abstract term expressive of a simple vital faculty of the human

* Nouv. Elém. sect. 44, 7, 8; vol. i. Notes, p. 35–51.

body, unknown to us in its essence, but which is endowed with motive and sensitive powers. "We are ignorant," he says, "if it be a substance, or only a mode of the living human body."

"It is undoubtedly possible," he again remarks, "that, conformably with a general law established by the Author of nature, a faculty possessed of motive and sensitive powers should necessarily be attached to the combination of matter, of which each animal body is formed, and that this faculty should contain the sufficient reason of the series of motions that are necessary to the life of the animal throughout its whole duration. But it is possible also," he adds, "that God has united to the combination of matter which is arranged for the formation of each animal, a principle of life subsisting by itself, and which differs in man from the thinking soul *."

From various passages of his writings, it is obvious that M. Barthez inclined most to the latter of these opinions, but at the same time he frequently complains of having been very much misunderstood or misrepresented in being supposed to imagine that the name of Vital Principle introduced into the Science of Man affords a sufficient explanation of the functions of Life; and professes to employ it merely as an abstract term for an unknown power, the use of which he conceived would facilitate the investigation of the phenomena of living beings, just as in Algebra letters are employed to denote unknown quantities †. "It is of no importance

* Nouv. Elém. sect. 43, 11, and 36.

† M. Barthez insinuates (Vol. I. Notes, p. 15.) that M. Dumas has done him injustice in employing, without attributing to him, the comparison of the advantage which may be derived from the use of the term Vital Principle in the science of Physiology, to

to me," he says, "whether we attribute or refuse a particular and proper existence to the being which I call Vital Principle;" but, "in investigating the secondary laws of an experimental cause or faculty, it is useful to employ the name of that faculty, as if that element were known *."

Before admitting, however, the existence of a Vital Principle in the economy of organized beings, as a substance distinct from Mind and Matter, and characterized like them by peculiar qualities of its own, it would be necessary in the first place, to establish that the properties attributed to this third substance, do not belong either to the sentient mind, or to the corporeal organization. But is it possible to affirm this of either of the properties, Sensibility and Irritability, by which M. Barthez has characterized his Vital Principle? It surely cannot be doubted that sensibility is a property of the principle we denominate Mind, which is excited by the agency of external bo-

that which is derived in Algebra from the use of letters to denote unknown quantities. This comparison, however, had previously been suggested by Sauvages in the Prolegomena to his Nosology. (§ 209.) But how these physiologists could suppose that any resemblance can be traced between the employment, in Algebra, of symbols to denote quantities unknown in their value, but always of the same nature with the quantities the value of which is known, and the employment, in Physiology, of an abstract term expressive of an unknown cause, in investigating vital phenomena which are alleged to be different from the properties both of organized matter and of mind—the only known elements of the animal economy—they have not explained, nor is it easy to conjecture.

* Nouv. Elém. sect. 37–42; sect. 45; vol. i. Notes, p. 3; sect. 43; Disc. Prelim. p. 15.

dies acting through the intervention of the nervous system; or that irritability is a property of that form of organized matter which we term muscular fibre, the contractions and relaxations of which can, by the application of various stimuli, be made as apparent to the eye as any other motions occurring in the physical world. If, instead of the properties by which M. Barthez has chosen to characterize his Vital Principle, and which are obviously the marks not of Vegetable but of Animal life, Life be considered, as it was by Aristotle, as consisting simply in the exercise of the functions of nutrition and generation, there is surely nothing known respecting these functions, much as they differ from any phenomena observable in unorganized matter, to prevent us from believing that they arise from the action of the external agents by which living bodies are surrounded, upon the germs in which they originate, and the material organs of which they come ultimately to be composed; and there can, consequently, be no occasion whatever for admitting the agency, in the production of these functions, of a Principle different from organized matter. Indeed motions or changes of matter, arising from the agencies of external powers, seem to give rise to all the vital phenomena and functions which we are able to discover, by observation or experiment, in the economy of living Vegetables. In the Animal economy, the existence of Muscular fibres and of a Nervous system, as well as the union of a Sentient and conscious principle with the material organization, not only modify in a very considerable degree the manner in which the functions essential to the preservation of the individual and of

the species, and which are common to animals with plants, are performed, but give rise to additional functions which are peculiar to animals, and furnish the characteristic marks by which they are distinguished from vegetables. The Functions of the human economy may perhaps admit of a fourfold division; 1st, into functions which seem to be wholly independent of the sentient principle, and to arise solely from the action of external agents on the organic properties with which the different parts of the organized body concerned in their performance are endowed—such as those functions that are usually included under the term Natural; 2d, into the function of Perception, in which impressions made and changes produced in some part of the Nervous System are succeeded by those affections of the mind denominated Sensations; 3d, into functions which appear to be wholly of a mental nature, and unaccompanied in their exercise by the perception or consciousness of any corporeal change, such as those of Memory and Judgment; and, 4th, into functions in which affections of the mind, such as emotions, propensities, desires, and volitions, are followed by motions or changes in some part of the body. These four divisions of the phenomena, or functions of the Animal economy, the *Organical*, the *Organico-psychical*, the *Psychical*, or strictly Mental, and the *Psychico-organical*, seem to comprehend all the operations of that economy of which observation or reflection have afforded us any knowledge, and to leave none which can render it necessary for us to resort to any third principle, besides Mind and organized Matter, for their explanation.

The other supposition respecting the Vital principle suggested by M. Barthez,—that it may be regarded as a peculiar faculty of the living human body,—is liable to this obvious objection, that the phenomena proposed to be comprehended under this term, are so widely different in their nature from one another, as not to admit of being referred to one common principle, nor, consequently, of being designated by one common name. By the definition of M. Barthez, the Vital principle was made to comprehend all those phenomena which, as Haller had shewn, ought to be referred to the two distinct properties of Irritability and of Sensibility. The laws of Sensibility, of Irritability, and of the Animal power, or of any other peculiar and distinct property of living animals, which we may yet be able to detect from an analysis of the various and complicated operations of which the functions of these beings consist, are undoubtedly as proper objects of investigation to the Physiologist, both separately and in their relations to one another, as the laws of gravity, of heat, of electricity, or of chemical affinity, are to the natural philosopher; but there does not seem to be such an analogy or close connection between Sensibility and Irritability, as that any advantage should be derived from including them under a common term. To employ the term Life, or Vital Principle, to designate a single cause, power or property of the organized body, upon which the whole of the phenomena of living beings depend, seems, indeed, as unphilosophical as it would be, if, after inventing a term to denote all the various operations on which the motion of a complicated piece of machinery depends, we should fancy to ourselves

that the general term so employed denoted a common power from which all these various operations proceeded. The term Vital principle, considered simply as a general expression for the whole phenomena which constitute the functions of living bodies, the only legitimate meaning that can be attached to it, may, if rigorously defined,—that is, if the phenomena comprehended under it be accurately stated,—prove a convenient, because a short, expression for these phenomena in medical writings; but the use of this term certainly can never lead, as M. Barthez and others seem to have expected, to any improvements or discoveries in the science of living beings.

M. Barthez, though he considered Sensibility and Irritability as properties of one common principle, seems to have perceived somewhat more clearly than MM. Bordeu and Fouquet had done, that they must be regarded as two distinct powers, and not merely as modifications of one another. "It is necessary," he remarks, "to distinguish in the Vital principle, the sensitive forces from the moving forces, because these two sorts of forces produce effects entirely dissimilar; just as in the mind, which is one, metaphysicians distinguish the understanding and the will, because the operations of these faculties are evidently different." Yet the extreme jealousy which he, in common with most of the other members of the Montpellier School, seems to have entertained of the ascendency which the doctrines of Haller had acquired, rendered him anxious to demonstrate that Irritability is not altogether independent of Sensibility, as Haller had maintained*. A

* Nouv. Elém. sect. 88; sect. 101, seqq.

slight examination, however, of the proofs by which he has illustrated this opinion, is sufficient to shew, that when Sensibility is said to be the immediate cause of the contractility of muscular fibre, the term is employed in precisely the same signification in which that of Natural Perception had been used by Glisson—both terms implying merely susceptibility of the impression of external agents as manifested by organized beings, and admitting therefore of application to plants though they are destitute of a nervous system, as well as to animals which are possessed of one.

In adopting the common division of Corporeal Sympathies into General and Particular, M. Barthez endeavoured to improve our knowledge of particular sympathies, *first*, by establishing a more precise and correct limitation of the phenomena which ought to be included under that term, than had been proposed by preceding physiologists; and, *secondly*, by instituting a more accurate classification of these phenomena under distinct heads, according to the analogies which they bear to one another. A particular sympathy, he conceived, is shewn to exist between two organs, whenever an affection of the one occasions, sensibly and frequently, a corresponding affection of the other, without its being possible to refer this succession of affections to casual coincidence,—to the mechanical action of the one organ upon the other,—or to the synergia or cooperation of several organs, in the performance of some particular function, or in the production of some particular disease*.

* Nouv. Elém. sect. 156. M. Barthez alleges, that the authors who had written before his time on the Sympathies of the organs,

The phenomena of Sympathy, like those of Life in general, M. Barthez thought it improper to attribute either to the physical organization of the body or to the agency of the mind. Even in those cases in which nervous communications can be traced between the parts which sympathise, he alleged that these communications must be regarded as sensible conditions, but not as necessary causes of the particular sympathies, which cannot be explained on so mechanical a principle. With regard to the hypothesis maintained by Whytt, that all the sympathies of the nerves depend on an intermediate affection of the Sensorium, which hypothesis he states to have been entertained also by Astruc and Kaau Boerhaave, M. Barthez conceived that this pretended necessary intervention of affections of the Sen-

had confounded them with the Synergiæ. "There are, however," he adds, "some enlightened physicians who seem to have had a general idea of this distinction, though they have not expressed it." (Sect. 161.) And his biographer, M. Lordat, in repeating and confirming Barthez' claim to the merit of this distinction, observes, that "though Bausner seems to have indistinctly perceived the necessity of separating these two classes of phenomena, when he divided the sympathies into the *Consensus actionum* and the *Consensus passionum*, (*De Consensu Partium.* Amstelod. 1656), he was not successful in fixing the line of demarcation between them, and he deduced no general conclusions from the view which he adopted." (Expos. de la Doctr. Med. de Barthez, p. 182.) It is singular that neither Barthez nor Lordat should have referred to Rega, who, though he includes both classes of sympathetic phenomena under the term of Consent or Sympathy, has distinguished them very accurately from one another. How correctly Dr Cullen distinguished the Synergiæ, or, as he termed them, "the concurrent actions of several parts necessary to produce particular effects," from sympathies, has been already shewn.—*Vide supra*, pp. 251, 252, & 308.

sorium commune may make us lose sight of, but cannot diminish the difficulty of ascertaining, the real causes of the sympathies of the nerves, which require, he says, to be sought for in a primary law of the principle of Life *. But to attribute any phenomenon, with Barthez, to the operation of an unknown Vital principle, is obviously to say that we are not able to explain it by the known laws of organized matter or of mind; and it seems still farther to imply, that all attempts to trace it to the known, or to other hitherto undiscovered, laws of these two substances will be fruitless. If the hypotheses of Nervous connection or of the intervention of the common Sensory, are singly or conjointly insufficient to explain all the phenomena of Sympathy, it is certainly proper that their application should not be pushed beyond its just extent; but to say that these phenomena depend on a law of an unknown Vital principle, is at once to throw aside the most probable explanations hitherto received, and to discourage all attempts to arrive at others of a more satisfactory nature.

M. Barthez, conceiving that the small progress which he alleges to have been made in the Science of Man, proportionally to what has been made in other useful sciences, is to be attributed to a neglect on the part of its cultivators, of the fundamental rules of the true method of philosophizing, judged it necessary that the whole body of doctrine in this science should be renewed, conformably with the true principles of philosophic method; and accordingly he professes the object of his "Nouveaux Elémens de la Science de l'Homme"

* Nouv. Elém. sect. 192, 196.

to be, to give a general idea of the new form which Physiology ought to assume, from the introduction of his Vital Principle *. To those who have been accustomed to trace the slow progress with which Medical knowledge, as well as all other kinds of knowledge that are dependent on observation and experience, has been accumulated, it is amusing to find from time to time authors gravely asserting, that the advances made previously to their own times in Medical science,—in an acquaintance with the laws of the animal economy, and of the numerous and complicated relations of its different parts to one another, and to the multifarious physical agents which are capable of influencing its conditions in the states of health and disease,—have been of no importance, or only calculated to mislead; and that they themselves have been the first to introduce into this department of knowledge, those views and principles, by the observance of which medical men will in future be enabled to rear up a body of science which may perhaps admit of addition but can never be supplanted,—which never can experience that fate to which they so remorselessly condemn the labours of those who have preceded them. How strangely the pretensions of such innovators contrast with the just views taken of the progress of the natural sciences by the philosophic Seneca! "Non est res unius ævi contemplatio Naturæ, nec nititur fragili unius ingenii fulcro, venerabile Verum." "Multum egerunt qui ante nos fuerunt, sed non peregerunt; multum adhuc restat operis, multumque restabit. Neque ulli nato post mille secula præcidetur occasio aliquid adhuc adjiciendi."

* Nouv. Elém. Disc. Prem. p. 2-4.

A critical analysis of those "New Medical Doctrines," which from time to time are obtruded on public attention, with high pretensions to originality and strictness of philosophical induction, is in general sufficient to shew, that they are merely the re-statements, under new forms of expression, of opinions long ago proposed, and probably long ago exploded,—in which, for the most part, from a hasty and imperfect generalization of facts, some of them true in themselves, but bearing no strict analogy with one another, two or more of those general principles or facts under which the particular facts of the science have been arranged, are comprehended under some common appellation, such as those of Anima, Archeus, Vital Principle, Excitability, or Irritation, and these verbal abstractions of the mind are preposterously represented as the expression of an universal law applicable to all the phenomena of the animal economy, and to the whole facts of Medical Science *.

The pursuits of the school of Montpellier, and the opinions to which they gave rise, seem for a long time to have produced little effect on the doctrines and on the practice of the medical Teachers of Paris. M. Richerand, in an account of M. Bordeu, prefixed to a late edition of his works, has remarked, that "Bordeu did not live to witness the triumph of his opinions; though admitted by some men of vigorous understanding, they could not find their way into the schools; and the mechanical theories of Boerhaave prevailed in these till the period of the Revolution. It was reserved for the present School of Medicine of Paris,"

* See Appendix, Note S.

he adds, " to *consummate* what our author had so happily commenced. From the first years of its formation, the doctrines of Boerhaave retained there only one single defender, M. Corvisart."

Among those individuals of superior understanding who outstripped the progress of the teachers in the Medical School of Paris, we must include M. SENAC, the distinguished author of the Traité du Cœur. In endeavouring to arrive at some principle which might account for the motions of the heart, this author rejected all those explanations of animal motions which physiologists had founded on the properties of unorganized matter, such as those of fermentation, effervescence, elasticity, &c.; and he rejected also, as still less worthy of attention, the supposition of a spiritual agent presiding over all the automatic motions of animated bodies. " The action of the Soul or Sentient principle," he remarks, " throws no light on the action of bodies. To seek in such an agent for the causes of the vital motions, is it not to seek these causes in an obscure and unknown agent; is it not to declare that the springs of mechanism cannot be the principle of our involuntary motions? Had Stahl sufficiently established the insufficiency of mechanism to account for the automatic motions of living beings? Before rejecting it, it would be necessary to know all its extent and all its limits; but can the greatest genius, possessed of the highest attainments, flatter himself that he knows all the motions which can result from mechanism? Stahl therefore was not entitled to deny that the automatic actions of animals may depend upon mechani al principles, and the agent which he supposed is merely a

name, with which he was contented, in order to avoid the necessity of laborious investigations." The mechanism of which Senac speaks in the foregoing quotation was the mechanism of organization, for he fully recognised the existence in the animal economy of a re-active force, corresponding in its properties with the description which Glisson had given of Irritability, and he very properly distinguished this power from Sensibility. In explaining why the theory of irritability had been so little attended to by physiologists subsequent to Glisson, he observes, " What has deceived so many writers is a false idea which still prevails with regard to the meaning of irritability, which is an equivocal term. It has been supposed that every principle of irritation is a principle of sensation or of pain ; and it has not been adverted to, that a part may be very susceptible of motion or of agitation without being susceptible of sensation or feeling. The heart, for example, appears insensible ; animals in which we tear it remain tranquil ; and still there is in this organ a secret force which obeys every impulsion and all irritating bodies." Contrary to the opinion which Haller had advanced, but without naming him, M. Senac espoused the hypothesis that the force which animates the heart is not inherent in the fibres of this organ, and independent of the other parts, but that it is contained in the nerves disseminated through its substance. At the same time, he was quite aware that the integrity of the nervous system is not necessary to maintain the heart in a state of action ; and he entered into an able and elaborate investigation of the influence which the Brain, the Spinal Marrow, and the

Nerves, respectively exercise upon the motions of this organ *.

Dr CULLEN delivered his course of lectures on the Institutions of Medicine in the successive sessions of 1766–67, 1767–68, and 1768–69, and alternately with his course on the Practice of Physic in the sessions of 1770-71 and 1772–73. That in changing the scene of his exertions from the Chair of Chemistry to that of the Theory of Physic, he lost none of the popularity which he had so justly acquired with the students, may be inferred, amongst other circumstances, from the following address presented to him by a number of his pupils, in the beginning of 1768:—

"SIR, EDINBURGH, *6th February* 1768.

"There is a portrait-painter now in town, who is reckoned eminent in his profession. As we, and many more of your pupils, are soon to leave the University, and so perhaps shall never have an opportunity of seeing you again, we entreat that you would take the trouble of sitting to him for your picture, so that by this means an engraving may be got, of which each of us may retain an impression.

"The regard which you have often shewn to our more important concerns, inclines us to hope that this request may be honoured with your compliance; and although no circumstance in future life can ever efface the memory of the benefits which you have conferred on us and on the science which we cultivate, yet, by having it thus in our power to transfer your presence wherever we go, our affection will not only be quickened, but you will still continue to excite in us that diligence and ardour which the desire of imitating so inestimable a pattern must undoubtedly inspire. We are, Sir, your most humble and obedient servants."

* Traité du Cœur, tom. ii. p. 100–153; livre iv. chap. vii. viii. ix. See Appendix, Note T.

Dr Cullen having complied with the request of his pupils, his portrait was painted by the artist alluded to, Mr Cochrane. The further intention of the students to have a print made of this portrait does not seem to have been fulfilled, no engraving, so far as I am aware, having ever been made from it. The print of Dr Cullen in circulation was taken from a portrait of him by Martin, executed for the Medical Society in 1777, and which still adorns their hall.

The list of pupils who enjoyed the benefit of Dr Cullen's medical instruction, from the time of his commencing to teach Clinical Medicine in Edinburgh, to the time of his quitting the chair of the Theory of Physic, contains the names of many individuals who afterwards acquired great reputation as cultivators and as practitioners of the healing art, and most of whom manifested upon all occasions the strongest disposition to testify their obligations to Dr Cullen, both as a teacher and as a friend. Besides the letters of Dr George Fordyce and Dr Balfour Russell, which have already been noticed, there are various others preserved from pupils of this period, which shew how eager those who had in this capacity enjoyed his instructions and friendly intercourse, were, subsequently to their leaving the University, to avail themselves of his advice and to procure his approbation, before submitting to the public eye the results of their observations or speculations on medical subjects; and his answers to these communications shew that he did not treat applications of this kind as mere matters of ceremony, nor fear to offend the *amour-propre* of those who reposed this confidence in him, when he hoped, that, by his point-

ing out imperfections in their literary productions, either in point of matter or of form, they might be enabled to render these more worthy of public approbation.

Amongst the Medical students of this period, who seem to have enjoyed a large share of Dr Cullen's notice and favour, may be mentioned Dr Matthew Dobson, who has been already alluded to, as having assisted Dr Cullen in his experiments on the Cold produced by Evaporation, and who, on leaving Edinburgh, settled at Liverpool, from whence he subsequently removed to Bath; besides an Essay on the Urine in Diabetes, which he contributed to the fifth volume of the Medical Observations and Inquiries, Dr Dobson was the author of a work on Fixed Air;—Dr Balfour Russell, and Dr G. Fordyce, of whom mention has already been made;—Dr Thomas Smith, who acquired great reputation by the inaugural dissertation on the Motion of the Muscles, which he published on graduating at Edinburgh in 1767, and who settled as a practitioner at Birmingham;—Sir Walter Farquhar, who rose to much eminence in London, first as a general practitioner, and afterwards as a physician;—Dr A. Monro Drummond, who was nominated Dr Cullen's successor in the Chair of the Institutions of Medicine, on the death of Dr John Gregory, an appointment, however, of which he never availed himself;—Sir James Carmichael Smyth, who, amongst his different writings, is perhaps most widely known by his observations on the Influence of Structure in modifying the phenomena of Inflammation, and by his recommendation of Acid Gases for the purpose of destroy-

ing Contagions;—Dr William Saunders, who settled in London, where he was appointed physician to Guy's Hospital, and who was the author of well-known works on the Liver, and on Mineral Waters;—Dr Thomas Percival of Manchester, who acquired much reputation by various literary and professional publications;—Dr Withering, afterwards of Birmingham, well known by his botanical works, as well as by some essays on practical medicine;—Dr Haygarth, who particularly distinguished himself by his philanthropic inquiries into the means for preventing contagions, and his plan for exterminating small-pox;—Dr John Rogerson, who became physician to the Empress Katharine of Russia;—Dr Francis Balfour, the author of a well known and ingenious work on sol-lunar influence;—Dr John Bostock, father of the present distinguished physiologist, whose premature death disappointed the sanguine hopes which Dr Cullen and his other friends had been led to form of his maturer exertions in the promotion of medical science, from the early manifestation of his talents, and the assiduity with which he cultivated them;—Dr John Fleming, who has been already alluded to as having acted as Dr Cullen's clinical clerk in 1765, and as having been long a member of the Medical Board of Calcutta;—Sir Charles Blagden, afterwards Secretary to the Royal Society, to whose Transactions he contributed several curious and important papers;—Dr William Falconer, who settled at Bath, and who, besides his treatise on the waters of that place, was the author of several medical treatises of considerable merit;—Dr Benjamin Rush, who, by his writings and lectures, as Professor of the

Theory and Practice of Medicine in the University of Pennsylvania, materially contributed to the promotion of medical science in the United States of America; —Dr Odier of Geneva;—Mr Benjamin Bell, who rose to the head of his own department of the healing art in Edinburgh;—Dr Nathaniel Dimsdale, the son of Baron Dimsdale;—Sir Gilbert Blane, whose contributions to Naval Medicine justly entitle him to the esteem and gratitude of a country so much dependent on her maritime strength, and whose writings everywhere evince how much he had imbibed of the philosophic spirit of his master;—and Dr James Johnstone, who published, during the time of his residence in Edinburgh, an essay on the use of the Ganglia, which procured him considerable reputation as a physiologist, both at home and on the Continent of Europe. I have inserted into the Appendix a number of letters written by the more distinguished of these pupils to Dr Cullen, at later periods of their lives, partly because the names of the writers are themselves sufficient to warrant the propriety of their preservation; and partly because they reflect light on the character and dispositions of the subject of this narrative, as manifesting the strength and permanence of the impressions produced on their youthful minds by the variety and accuracy of his knowledge, the activity and vigour of his understanding, and the kind interest he took in their particular studies and prospects *.

Amongst the different subjects to which Dr Cullen's attention was occasionally requested by former pupils, were complaints on the part of those who had

* Appendix, Note U.

settled as medical practitioners in England, of the prejudices with which they had to contend as graduates of a Scotch University. The abuse which had undoubtedly occurred in several instances, of the practice pursued at that time by some of the Scotch Universities, of conferring degrees in absence, had afforded some pretext for insinuating, that but little consideration was due to a Scotch degree; and the graduates of the English Universities had,—by their being entitled, in virtue of their degrees, to practise physic in every part of England, except London and its vicinity, without obtaining any licence from the College of Physicians in London, as the graduates of other universities were obliged to do, and by their being exclusively eligible as Fellows of this College,—been led to foster the notion of their own superiority over the graduates even of those Scotch Universities where degrees were not conferred without an accurate investigation of the candidates' qualifications.

Unfortunately for the University of Edinburgh, though in general free from the reproach of conferring its medical degrees on unworthy objects, an occurrence took place in 1766, which exposed it to suspicion and censure. In that year, as has been observed by Dr Fothergill's biographer, "one Samuel Leeds, an illiterate person, who had been brought up to the trade of a brush-maker, was admitted by the University of Edinburgh to the degree of Doctor of Physic; and on his coming to London soon afterwards, being espoused by several leading persons among the Quakers, was chosen physician to the London Hospital. Soon after his election, one of his colleagues, in a conversation with

Dr Fothergill, happening to mention Leeds's success, the Doctor replied, 'Take care he does no mischief.' It was not long before Leeds gave sufficient marks of his ignorance to alarm the Governors, and what had dropped from so respectable a person as Dr Fothergill, might perhaps not a little tend to convince them of their precipitancy in electing a physician to their hospital, so unqualified for the duties of it. They therefore made a resolution, 'that no physician should continue to officiate in that hospital, who had not passed an examination at the College of Physicians.'" Dr Leeds, on presenting himself to the College, was examined and rejected. The expression that had escaped Dr Fothergill with regard to Leeds, having come to his knowledge, he made it the foundation of an accusation which was brought before his own Society, by whom it was referred to five arbitrators, three of whom awarded L. 500 damages to Dr Leeds, refusing to hear Dr Fothergill's principal evidence. Dr Fothergill having been advised by his friends to refuse to abide by the decision of the arbitrators, sent his brother to Edinburgh, to collect proofs of Dr Leeds's ignorance, and of his having obtained his degree by undue means; and on this occasion he wrote two letters to Dr Cullen, who had been his fellow-student at Edinburgh in 1734–5, which, as they afford some explanation of the manner in which this unlucky blunder on the part of the University of Edinburgh had arisen, I have inserted into the Appendix *. After much altercation in the Society of Quakers, the case was brought by Dr Leeds before the Court of

* Note V. Appendix.

King's Bench. "Lord Mansfield, on hearing the evidence and counsel on the part of Dr Leeds, refused to hear Dr Fothergill's counsel, because, he observed, the evidence on the part of Dr Leeds's arbitrators was sufficient to prove the illegality and injustice of their own award. The learned Judge further added, that Dr Fothergill did no more than his duty in saying what he was charged with; and that he would not have acted as an honest man had he said less."

The attention of the College of Physicians of London having been directed by this occurrence to their regulations respecting the admission of licentiates, it appears that their President, Sir William Browne, took occasion to assert that the graduates of Edinburgh had no right to be taken on trial for admission into that body, as Edinburgh was not a University, and consequently not capable of conferring degrees,—a declaration calculated to excite no small degree of alarm in the minds of those who had received their degrees from that University, after having passed in it through a regular course of medical instruction, and after having undergone strict examinations into their qualifications. Among Dr Cullen's papers I find a letter addressed to him at this time by Dr William Cumming, who had studied along with him and Dr Fothergill at Edinburgh, and had subsequently settled as a physician at Dorchester, of which the following is an extract.

"An event has lately happened which affects me and three other gentlemen in this county, who have had the honour of receiving our medical degrees from the University of Edinburgh. I need not tell you, as I believe you already know it, that, upon a late occasion, Sir W. Browne, Presi-

dent of the College of Physicians in London, has *in office* asserted that Edinburgh is no University, and therefore has no power to confer degrees. * * * I was informed from London, that a report prevailed there that one of the Elects, or of the Fellows, of the London College had written a letter to Dr Robertson, your worthy Principal, assuring him that Sir W. Browne's behaviour in this matter was contrary to the sentiments of the College, and that the young gentleman who had been refused the appellation of Doctor, was now to be examined by the College as an extra-licentiate, and was to be allowed that title. The Graduates of the English Universities have, on many occasions, thrown out hints, as if they looked on the Scotch doctors as of a class inferior to themselves, and sometimes refused to consult with them, as once happened to myself many years ago, in the case of a gentleman of fortune in this neighbourhood, when the insolent Oxonian declared, *totidem verbis*, that he had no objection to my person, parts, or skill in my profession, but that he thought it a duty which he owed to that University of which he was a member, not to consult with any one who had not been educated at an English University. The consequence in this particular case was, that I was employed in the gentleman's family for fifteen years after (so long as he or any of his family lived), and that the Oxonian was instantly dismissed. That many of the Fellows of the London College entertain sentiments very different from those of their President, I am well convinced; but, at the same time, I believe the President is not single in his opinion, and that there are some amongst them who are of the same sentiments with him. This act of his cannot be looked on as merely declaratory of his *private* opinion, but must undoubtedly be construed as an act of the *whole body*; as it was an assertion *in office* of the *President* of the College who is (if I am not mistaken, and I am pretty clear I am not), the only person who, by the Act of Parliament, is to be applied to in the case of an extra-licentiate. The Professors of Edinburgh

have, therefore, an undoubted right to demand and insist on a public and ample renunciation and disavowal of this insult, and an acknowledgment of their rights and privileges under the Seal of the College."

With a view to remove all just cause of reproach against Scotch graduates, Dr Cullen became extremely desirous that the practice of conferring medical degrees on unqualified persons should be effectually checked, by the Universities being prevented from granting these to any persons who had not gone through a regular course of study, and submitted themselves to a strict examination. A favourable opportunity for procuring the attention of Government to this subject seemed to present itself on the election of the Duke of Buccleugh as an Honorary Fellow of the Edinburgh College of Physicians, during the time Dr Cullen was President of that body in 1774. I find the following letter, indicative of the Duke's wishes, addressed to Dr Cullen by his Grace's law-agent Mr Davidson:

"Dear Doctor,

"I am ordered by the Duke of Buccleugh to tell you he is most sensible of the honour the College of Physicians have done him, and that he wishes most heartily it may be in his power to show it. He regrets greatly he had not the pleasure of seeing you to express all this to you. He desires me to ask you if it is true that, in any of the Scotch Universities, degrees in physic are given on paying certain fees, without due examination and previous attendance at the Universities; because, if it is so, his Grace thinks it is truly a matter of reproach to the country, and of most pernicious example, and what he would wish to have remedied, if he had information how that can be done. All this I should have told you personally, but I thought a letter would reach you

sooner and more certainly than I could; and you know, if you choose it, I would wait on you at any time you pleased, though I am not willing to break in upon you unseasonably. If you pass this way I will be happy to see you on this matter, and I am, with the compliments of the season, most perfectly, Dear Doctor, your humble servant,

"JOHN DAVIDSON."

"CASTLE HILL, EDINBURGH,
January 15. 1774."

In order to facilitate the Duke of Buccleugh's application to Government with regard to Scotch medical degrees given in absence, Dr Cullen drew up the following Memorial, setting forth the grievances complained of, and suggesting the different means by which they might be remedied.

"MEMORIAL.

"It has been long complained of, that some of the Universities of Scotland make a shameful traffick of degrees in physic.

"The complaint is founded on the following circumstances of their practice.

"The Universities of St Andrew's and Aberdeen have frequently given degrees in absence; that is, have conferred degrees by diploma on persons who never appeared before them to undergo any examination; and they confer the degree merely upon a certificate of two physicians, declaring that they judge the candidate or person recommended to be worthy of a degree in physic. Very often the physicians thus certifying are obscure persons, and as little known to the University as the person they recommend; and sometimes it may be presumed that the certifying physicians are very little qualified to judge of medical knowledge and abilities. In short, it is alleged, and commonly believed, that little else

than the payment of the usual fees is necessary to obtain such a degree.

" Farther, the same Universities of St Andrew's and Aberdeen, when conferring degrees upon persons present and undergoing examination, do, however, admit persons to such examination without any evidence of the candidate's having applied regularly to the study of physic, or of his having made any residence for that purpose in any University where the several branches of medical study are properly taught. How the examinations, in such cases, are conducted, it is not possible to say, in general; but, from some instances of persons who have received such degrees, it is strongly suspected that the examinations are not strict or rigorous.

" It is to be observed, that the University of Glasgow, though they do not commonly give degrees in absence, yet they often give degrees, without requiring any certificate of the candidate's previous study.

" These practices are considered as an abuse. A degree in physic is, or should be, considered as the testimony of proper judges, in favour of the medical knowledge and abilities of the person who holds the degree; and, at least, it should be considered as a certificate of a person's having regularly, and for a due length of time, applied to the study of physic; but, as a degree, conferred as above, cannot be viewed in any of these lights, the practice of conferring such degrees must be considered as an abuse of the institution, an imposition upon the public, a great discouragement to the proper study of physic, and a matter of constant reproach speciously thrown upon all Scotch degrees.

" The abuses complained of are particularly a hardship upon the Royal College of Physicians of Edinburgh, who are obliged by their charter to grant licences, without examination, to any person who has obtained a degree from any of the Universities of Scotland.

" It is presumed, that every man who has a regard to the public good, and to the honour of Scotch literature, will be

zealous to have the above mentioned abuses corrected, and the practice reformed; but the means of obtaining this appears to be somewhat difficult.

"There is no very certain and effectual means of correcting the irregular practices of the Universities of Scotland, but by a Royal Visitation; that is, by his Majesty's granting a Commission to certain persons to inquire and to correct, as they shall see cause.

"Such a Commission, however, is to be passed under the Great Seal, and is to be directed to some persons of high rank and distinction, who must necessarily repair to the several Universities, and in this and other respects take a great deal of trouble. It is doubted if his Majesty's ministers will readily engage in such expense and trouble; and we would therefore wish to find some other method that might be more easily and readily executed.

"But if it shall be found improper to have his Majesty's power exercised, or name employed, in any other way than by a Commission, it is supposed, that any person in high office, and particularly the Secretary of State for the Northern Department, might, even in a private capacity, give an advice or admonition to the administrators of the Universities, to have certain notorious abuses corrected.

"It is presumed that such an admonition would be attended to, as it might be understood as a threatening that, if the admonition was neglected, it might be followed by a Visitation, which, we know well, has the power of distressing disobedient members; and, as the benefit arising from the conferring of degrees in the manner above mentioned is only considerable with respect to a single member of the University, that is, the Professor of Physic, and very small with respect to the whole community, it is therefore supposed that a slight injunction will be sufficient to engage most of the members of the University to join in correcting an abuse which truly dishonours the Society.

"If it shall be thought proper to give any injunction to

the Universities, it is humbly suggested that it should be simple, and with respect to the two following particulars only.

" First, That they be enjoined to give no degrees in absence, but such as are honorary, and given without fees, to persons of rank and distinction, or to persons who had given some public specimen of their literature and medical knowledge.

" Secondly, That, in giving degrees to persons present, and willing to undergo a proper examination, they shall not admit any person as a candidate, who does not bring a proper certificate of his having resided for two years at least in an University wherein physic is regularly taught, and of his having applied to all the branches of medical study.

" It may be justly doubted if the two years' residence here proposed is enough for acquiring a tolerable knowledge in physic; but it is taken from a late regulation of the Royal College of Physicians at London, who have made it a condition with respect to the licentiates they are to admit.

" It is not doubted that, in every University, the University Senate has full power to make the regulations proposed, and has also an opportunity of securing the observation of them, as no degree is at any time conferred without the consent of the Senate, or diploma granted that is not subscribed by a majority of the members.

" In directing any injunction to the Universities, it may be considered how far it is proper and necessary to address it to the several Chancellors, from whose authority every academical degree is supposed to proceed; or if it may be addressed to the more immediate administrators of the University; that is, to the Rector, Principal, and other members of the University Senate, without whose concurrence it is believed that no new regulations in the University can be made.

" The present Chancellor of the University of St Andrew's, is the Earl of Kinnoul.

"The Chancellor of Glasgow is the Duke of Montrose.

"The Chancellor of Aberdeen is the Earl of Bute.

"The Marischall College, Aberdeen, which acts as a separate University, has at present no Chancellor.

"The University of Edinburgh has the Magistrates and Town Council of Edinburgh, as Patrons in the place of Chancellor and Rector.

"It may be observed, that the Chancellors of the several Universities, though they may act as members of the University Senate, have not for a long time past taken any part in the administration of the Universities."

This Memorial, at the desire of the Duke of Buccleugh, was submitted to the consideration of Dr Adam Smith, who had some years before accompanied his Grace in his travels on the Continent of Europe, and who was at the time engaged in the composition of his work on the Wealth of Nations, in which, as is well known, in treating "of the expense of the institutions for the education of youth," he has very ingeniously, though perhaps somewhat incautiously, applied to the business of Education those principles of unfettered and unassisted competition which it was the great object of that work to inculcate in regard to Trade and Commerce. After a careful consideration of the Memorial, Dr Smith transmitted to Dr Cullen a very detailed statement of his opinions on the subject to which it relates, in the following letter, in which he has, with much ability, playful ingenuity, and an unusual liveliness of manner, endeavoured to palliate in some degree the practices complained of, and to apply his general principles of free competition to the teaching and to the practice of the Medical profession.

"My Dear Doctor,

"I have been very much in the wrong both to you and to the Duke of Buccleugh, to whom I certainly promised to write you in a post or two, for having delayed so long to fulfil my promise. The truth is, some occurrences which interested me a good deal, and which happened here immediately after the Duke's departure, made me forget altogether a business which I do acknowledge interested me very little.

"In the present state of the Scotch Universities, I do most sincerely look upon them as, in spite of all their faults, without exception the best seminaries of learning that are to be found any where in Europe. They are, perhaps, upon the whole, as unexceptionable as any public institutions of that kind, which all contain in their very nature the seeds and causes of negligence and corruption, have ever been, or are ever likely to be. That, however, they are still capable of amendment, and even of considerable amendment, I know very well, and a Visitation is, I believe, the only proper means of procuring them this amendment. Before any wise man, however, would apply for the appointment of so arbitrary a tribunal, in order to improve what is already, upon the whole, very *well*, he ought certainly to know with some degree of certainty, first, who are likely to be appointed visitors; and, secondly, what plan of reformation those visitors are likely to follow. But, in the present multiplicity of pretenders to some share in the provincial management of Scotch affairs, these are two points, which I apprehend neither you nor I, nor the Solicitor-General, nor the Duke of Buccleugh, can possibly know any thing about. In the present state of our affairs, therefore, to apply for a Visitation in order to remedy an abuse, which is not perhaps of great consequence to the public, would appear to me to be extremely unwise.

Hereafter, perhaps an opportunity may present itself for making such an application with more safety.

"With regard to any admonition or threatening, or any other method of interfering in the affairs of a body corporate, which is not perfectly and strictly regular and legal, these are expedients which I am convinced neither his Majesty nor any of his present Ministers would choose to employ either now or at any time hereafter, in order to obtain an object even of much greater consequence than this reformation of Scotch degrees.

"You propose, I observe, that no person should be admitted to examination for his degrees, unless he brought a certificate of his having studied at least two years in some University. Would not such a regulation be oppressive upon all private teachers, such as the Hunters, Hewson, Fordyce, &c.? The scholars of such teachers surely merit whatever honour or advantage a degree can confer, much more than the greater part of those who have spent many years in some Universities, where the different branches of medical knowledge are either not taught at all, or are taught so superficially that they had as well not be taught at all. When a man has learned his lesson very well, it surely can be of little importance where or from whom he has learnt it.

"The monopoly of medical education which this regulation would establish in favour of Universities would, I apprehend, be hurtful to the lasting prosperity of such bodies-corporate. Monopolists very seldom make good work, and a lecture which a certain number of students must attend, whether they profit by it or no, is certainly not very likely to be a good one. I have thought a great deal upon this subject, and have inquired very carefully into the constitution and history of several of the principal Universities of Europe. I have satisfied myself, that the present state of degradation and contempt into which the greater part of those societies have fallen in almost every part of Europe, arises principally, first,

from the large salaries which in some Universities are given to professors, and which render them altogether independent of their diligence and success in their professions; and, secondly, from the great number of students, who, in order to get degrees, or to be admitted to exercise certain professions, or who, for the sake of bursaries, exhibitions, scholarships, fellowships, &c., are obliged to resort to certain societies of this kind, whether the instructions which they are likely to receive there are or are not worth the receiving. All those different causes of negligence and corruption, no doubt take place in some degree in all our Scotch Universities. In the best of them, however, those causes take place in a much less degree than in the greater part of other considerable societies of the same kind; and I look upon this circumstance as the real cause of their present excellence. In the medical College of Edinburgh in particular, the salaries of the Professors are insignificant. There are few or no bursaries or exhibitions, and their monopoly of degrees is broken in upon by all other Universities, foreign and domestic. I require no other explication of its present acknowledged superiority over every other society of the same kind in Europe.

"To sign a certificate in favour of any man whom we know little or nothing about, is most certainly a practice which cannot be strictly vindicated. It is a practice, however, which, from mere good nature, and without interest of any kind, the most scrupulous men in the world are sometimes guilty of. I certainly do not mean to defend it. Bating the unhandsomeness of the practice, however, I would ask in what manner does the public suffer by it? The title of Doctor, such as it is, you will say, gives some credit and authority to the man upon whom it is bestowed; it extends his practice, and consequently his field for doing mischief; it is not improbable, too, that it may increase his presumption, and consequently his disposition to do mischief. That a degree injudiciously conferred may sometimes have some

little effect of this kind, it would surely be absurd to deny; but that this effect should be very considerable, I cannot bring myself to believe. That Doctors are sometimes fools as well as other people, is not, in the present times, one of those profound secrets which is known only to the learned. The title is not so very imposing, and it very seldom happens that a man trusts his health to another merely because that other is a doctor. The person so trusted has almost always either some knowledge or some craft which would procure him nearly the same trust, though he was not decorated with any such title. In fact, the persons who apply for degrees in the irregular manner complained of, are, the greater part of them, surgeons or apothecaries, who are in the custom of advising and prescribing, that is, of practising as physicians; but who, being only surgeons and apothecaries, are not fee-ed as physicians. It is not so much to extend their practice as to increase their fees, that they are desirous of being made doctors. Degrees conferred, even undeservedly, upon such persons, can surely do very little harm to the public. When the University of St Andrew's, very rashly and imprudently, conferred a degree upon one Green, who happened to be a stage-doctor, they no doubt brought much ridicule and discredit upon themselves; but in what respect did they hurt the public? Green still continued to be what he was before, a stage-doctor, and probably never poisoned a single man more than he would have done though the honours of graduation had never been conferred upon him. Stage-doctors, I must observe, do not much excite the indignation of the faculty; more reputable quacks do. The former are too contemptible to be considered as rivals: They only poison the poor people; and the copper pence which are thrown up to them in handkerchiefs, could never find their way to the pocket of a regular physician. It is otherwise with the latter: They sometimes intercept a part of what perhaps would have been better bestowed in another

place. Do not all the old women in the country practise physic without exciting murmur or complaint? And if here and there a graduated doctor should be as ignorant as an old woman, where can be the great harm? The beardless old woman, indeed, takes no fees; the bearded one does, and it is this circumstance I strongly suspect, which exasperates his brethren so much against him.

"There never was, and, I will venture to say, there never will be, a University from which a degree could give any tolerable security, that the person upon whom it had been conferred, was fit to practise physic. The strictest Universities confer degrees only upon students of a certain standing. Their real motive for requiring this standing is, that the student may spend more money among them, and that they may make more profit by him. When he has attained this standing, therefore, though he still undergoes what they call an examination, it scarce ever happens that he is refused his degree. Your examination at Edinburgh, I have all reason to believe, is as serious, and perhaps more so than that of any other University in Europe. But when a student has resided a few years among you, has behaved dutifully to all his Professors, and has attended regularly all their lectures, when he comes to his examination, I suspect you are disposed to be as good-natured as other people. Several of your graduates, upon applying for a licence to the College of Physicians here, have had it recommended to them to continue their studies. From a particular knowledge of some of the cases, I am satisfied that the decision of the College, in refusing them their licence, was perfectly just; that is, was perfectly agreeable to the principles which ought to regulate all such decisions, and that the candidates were really very ignorant of their profession.

"A degree can pretend to give security for nothing but the science of the graduate; and even for that it can give but a very slender security. For his good sense and discre-

tion, qualities not discoverable by an academical examination, it can give no security at all. But without these, the presumption which commonly attends science must render it, in the practice of physic, ten times more dangerous than the grossest ignorance, when accompanied, as it sometimes is, with some degree of modesty and diffidence.

" If a degree, in short, always has been, and, in spite of all the regulations which can be made, always must be, a mere piece of quackery, it is certainly for the advantage of the public that it should be understood to be so. It is in a particular manner for the advantage of the Universities that, for the resort of students, they should be obliged to depend, not upon their privileges, but upon their merit,—upon their abilities to teach, and their diligence in teaching; and that they should not have it in their power to use any of those quackish arts which have disgraced and degraded the half of them.

" A degree which can be conferred only upon students of a certain standing, is a statute of apprenticeship which is likely to contribute to the advancement of science, just as other statutes of apprenticeship have contributed to that of arts and manufactures. Those statutes of apprenticeship, assisted by other corporation laws, have banished arts and manufactures from the greater part of towns-corporate. Such degrees, assisted by some other regulations of a similar tendency, have banished almost all useful and solid education from the greater part of Universities. Bad work and high price have been the effects of the monopoly introduced by the former. Quackery, imposture, and exorbitant fees, have been the consequences of that established by the latter. The industry of manufacturing villages has remedied in part the inconveniences which the monopolies established by towns-corporate had occasioned. The private interest of some poor Professors of Physic in some poor Universities, inconveniently situated for the resort of students, has in part re-

medied the inconveniences which would certainly have resulted from that sort of monopoly which the great and rich Universities had attempted to establish. The great and rich Universities seldom graduated any body but their own students, and not even them till after a long and tedious standing; five and seven years for a Master of Arts; eleven and sixteen for a Doctor of Law, Physic, or Divinity. The poor Universities, on account of the inconveniency of their situation, not being able to get many students, endeavoured to turn the penny in the only way in which they could turn it, and sold their Degrees to whoever would buy them, generally without requiring any residence or standing, and frequently without subjecting the candidate even to a decent examination. The less trouble they gave the more money they got, and I certainly do not pretend to vindicate so dirty a practice. All Universities being ecclesiastical establishments, under the immediate protection of the Pope, a degree from any one of them gave, all over Christendom, very nearly the same privileges which a degree from any other could have given; and the respect which is at this day paid to foreign degrees, even in Protestant countries, must be considered as a remnant of Popery. The facility of obtaining degrees, particularly in physic, from those poor Universities, had two effects, both extremely advantageous to the public, but extremely disagreeable to the graduates of other Universities, whose degrees had cost them much time and expense. *First*, It multiplied very much the number of doctors, and thereby no doubt sunk their fees, or at least hindered them from rising so very high as they otherwise would have done. Had the Universities of Oxford and Cambridge been able to maintain themselves in the exclusive privilege of graduating all the doctors who could practise in England, the price of feeling a pulse might by this time have risen from two and three guineas, the price which it has now happily arrived at, to double or triple that sum; and English

physicians might, and probably would, have been at the same time the most ignorant and quackish in the world. *Secondly*, It reduced a good deal the rank and dignity of a doctor. But if the physician was a man of sense and science, it would not surely prevent his being respected and employed as a man of sense and science. If he was neither the one nor the other, indeed, his doctorship would no doubt avail him the less. But ought it in this case to avail him at all? Had the hopeful project of the rich and great Universities succeeded, there would have been no occasion for sense or science. To have been a doctor would alone have been sufficient to give any man rank, dignity, and fortune enough. That in every profession the fortune of every individual should depend as much as possible upon his merit, and as little as possible upon his privilege, is certainly for the interest of the public. It is even for the interest of every particular profession, which can never so effectually support the general merit and real honour of the greater part of those who exercise it, as by resting upon such liberal principles. Those principles are even most effectual for procuring them all the employment which the country can afford. The great success of quacks in England has been altogether owing to the real quackery of the regular physicians. Our regular physicians in Scotland have little quackery, and no quack accordingly has ever made his fortune among us.

"After all, this trade in degrees I acknowledge to be a most disgraceful trade to those who exercise it; and I am extremely sorry that it should be exercised by such respectable bodies as any of our Scotch Universities. But as it serves as a corrective to what would otherwise soon grow up to be an intolerable nuisance, the exclusive and corporation spirit of all thriving professions and of all great Universities, I deny that it is hurtful to the public.

"What the physicians of Edinburgh at present feel as a hardship is, perhaps, the real cause of their acknowledged

superiority over the greater part of other physicians. The Royal College of Physicians there, you say, are obliged by their charter to grant a licence, without examination, to all the graduates of Scotch Universities. You are all obliged, I suppose, in consequence of this, to consult sometimes with very unworthy brethren. You are all made to feel that you must rest no part of your dignity upon your degree, a distinction which you share with the men in the world, perhaps, whom you despise the most, but that you must found the whole of it upon your merit. Not being able to derive much consequence from the character of Doctor, you are obliged, perhaps, to attend more to your characters as men, as gentlemen, and as men of letters. The unworthiness of some of your brethren may, perhaps, in this manner be in part the cause of the very eminent and superior worth of many of the rest. The very abuse which you complain of may in this manner, perhaps, be the real source of your present excellence. You are at present well, wonderfully well, and when you are so, be assured there is always some danger in attempting to be better.

"Adieu, my dear Doctor; after having delayed so long to write to you, I am afraid I shall *get my lug* (ear) *in my lufe* (hand), as we say, for what I have written. But I ever am most affectionately yours,

"ADAM SMITH.

"LONDON, 20*th September* 1774."

Whether the unfavourable reply of Dr Smith had discouraged the Duke of Buccleugh from persevering in the proposed application to Government, or whether other circumstances had occurred to withdraw the attention of those more particularly interested in it, does not appear, but no farther progress seems ever to have been made in this matter, and it was left to the Scotch Universities themselves to remove the abuses com-

plained of, as they have now done, by prescribing a course of study which all applying to them for a medical degree must have previously passed through, and by subjecting candidates to a rigorous examination of their literary, scientific, and professional acquirements.

I have not found among Dr Cullen's papers any direct reply to the arguments of his friend Dr Smith; but in a Latin discourse, which he pronounced about two years afterwards in the University, previously to the ceremony of graduation, he took occasion to state in what respects he conceived that the principles of free competition, though applicable to the mechanical trades, do not admit of being extended to the exercise of the profession of Medicine, and why it is proper that every one who proposes to engage in this profession, should be obliged previously to pass through a regular course of education, and to submit to a strict examination of his qualifications. As this is a very important subject, and one on which a considerable degree of misconception seems still to prevail *, I shall introduce here a translation of Dr Cullen's observations :—

" As the life and health of their fellow-creatures are so often intrusted to those practising medicine, and depend so much upon their skill, it seems a matter of no small importance for the public interest, that care should be taken to prevent any uneducated or unskilful persons from practising this art. There are some, however, who doubt, whether it is necessary for the interests of society, or in any way proper, to make laws or regulations for preventing persons of this descrip-

* See Speech of Sir C. Monck in the House of Commons, on the second reading of Mr Courtenay's bill for the better regulation of Surgery. " Exposition of the State of the Medical Profession," &c. 8vo, London, 1826, p. 234.

tion from engaging in the practice of Medicine; and it is very obvious, that neither in this nor in most other countries are effectual measures adopted for that purpose. Those who approve of this neglect, seem to me to trust to certain principles too generally applied. They, with justice, indeed observe, that many regulations which seem adapted for putting down unskilful workmen, have not only been fruitless and useless, but even extremely injurious in their operation. For there is no need, say they, that the skill of a carpenter should be determined by the opinion of other carpenters only, as every one receiving work from a carpenter can judge well enough of its execution, and of the skill of the workman; and certainly if an opportunity is afforded of comparing the workmanship of several artificers with one another, he who makes this comparison will always be able, with sufficient ease, to determine which is best. An unskilful carpenter, therefore, will be intrusted with work only proportioned to his skill, and if he should, perhaps, pass himself off on some incautious individuals, as a person of skill, no very great nor permanent mischief will result from this. They likewise urge, that many regulations which have been established against the unskilfulness of workmen, have not only been little required, but have often been hurtful, by affording a pretext for driving away the most skilful and by favouring those of least merit. What has been said of the carpenter, may be said of several other artificers,—indeed of all those whose want of skill is not calculated to cause much mischief, and whose degree of skill all are able easily to ascertain. But in the practice of Medicine none of these reasons for unfettered competition are of any force. The physician employs remedies, which, unless they be employed cautiously and skilfully, may do much harm, and those ignorant of the art cannot judge of his skill merely by the consideration of the effects that result from the employment of his art. Many diseases being utterly incurable by any art, physicians have a ready

excuse when at any time they treat diseases unskilfully; and as death is to all at last inevitable, fate, and not the ignorance of the physician, is generally blamed. Success in the cure of diseases does indeed afford some means of judging of the skill of the physician, but he who knows that many diseases are cured, not only without the aid of the physician, but even in defiance of his measures, will not always regard a return to health as sufficient proof of medical skill.

"*Lastly*, We contend, that the bulk of the community are scarcely able to judge, and, in fact, scarcely ever judge correctly, of the merits of medical men. Those who are well acquainted with the medical art, and they alone, perhaps, have any proper notion how many things are required to form a skilful physician,—are aware how much experience is necessary for the proper discrimination, and accurate distinction of diseases,—with how much labour almost all the works of nature must be examined, in order that the animal economy may be thoroughly understood, by the knowledge of which alone the various characters and modifications of diseases can be understood,—and, lastly, how much labour is necessary to acquire an accurate knowledge, not only of the medicines, but also of all the agents which are capable of acting on the human body, by which kind of knowledge alone we can be enabled to adapt appropriate and efficacious remedies to the various forms of disease. For obtaining a knowledge of all these things, how many branches of information must previously be acquired; how much acuteness of intellect should the student of medicine be possessed of, to prevent him from being deluded by hypothesis; how much sagacity, to prevent him from being the dupe even of experience, which has justly been said to be most deceitful! It would be easy to prove to any one of moderate capacity and attentiveness, that all these things are absolutely necessary for the proper education of a medical man; but the greater

part of mankind, either from their ignorance of medicine are unable to understand these things, or from negligence do not chuse to understand them; and hence, almost all form either no judgment at all on the qualifications necessary for medical men, or an erroneous one. At the present time, we see men of a very dull intellect, and of no learning, received and treated as physicians. Do not we every day observe men not altogether destitute of sense, swallowing the frivolous drugs of some old woman or other, and shamefully deluded by the tricks of quacks and ignorant pretenders? From this circumstance in particular, we may infer how erroneously the public judge of this matter, that so many persons who have received no education, rashly engage in the practice of medicine, or at least in the administration of drugs. In these countries, indeed, any one can set up for a physician, and nothing is more obvious than that the public are in the use of making scarcely any choice amongst those who profess medicine. The thing has gone so far, that the life and health of a great portion of mankind are in the hands of ignorant people, or of rogues; and to me it appears probable, that from the practice of medicine, as it is at present exercised, there arises much more mischief than advantage, and that it would perhaps be better for mankind if no medical practice existed at all. But it is impossible wholly to suppress this art, for as long as men shall be liable to pain and sickness, so long some art of medicine will be required, and will exist amongst them. Nothing therefore remains, but that the Legislature should take especial care that this necessary art should, as far as is possible, be rendered both safe and useful to society. Something seems to have been attempted towards the accomplishment of this object in this and other countries of Europe, by the institution of Universities, in which students of medicine may be instructed in medical science, and by the appointment of fit judges to license those who are thoroughly instructed in medicine. The functions of these individuals seem well calculated to promote the object

in view. But in some Universities, unfortunately, these excellent regulations are ill observed, and the title of Doctor of Medicine does not always prove that the bearer possesses either learning or skill. This unfortunate abuse cannot in all instances be easily prevented or corrected. But, in the mean time, we hope that all are sufficiently aware, that, in this University, the best regulations are faithfully observed, and that the title of Doctor acquired here, more certainly than in several other Universities, proves its possessor to be both learned and skilful."

A system of medical legislation, the chief recommendation of which appeared to Dr Adam Smith to be, that one kind of abuse served in it as a corrective for another, and under the governance of which the practice of the medical profession had become, in the eyes of Dr Cullen, rather a misfortune than a blessing to mankind, cannot be supposed to have any high pretensions to perfection. A slight inquiry into the progressive formation of this system as it at present exists in this country, would probably shew that many of its imperfections are to be attributed partly to its being grounded on opinions and usages that prevailed a long time ago, but which are now become obsolete, and partly to the Legislature having never considered more than detached portions of this system at a time, nor attempted to give to it the consistency and uniformity of a whole.

As one of the erroneous principles on which the system of medical legislation existing in this country has been founded, and perhaps as the chief of these, may be mentioned the opinion which has very generally prevailed, that the extent and nature of the education, and the amount of qualifications, required of Medical students, should vary according to the designation under which

they are to practise their profession, whether that of physician, of surgeon, or of general practitioner, or apothecary. In the history of ancient times, we find nothing which bears any resemblance to those divisions of the medical profession which have prevailed in modern Europe. Medical practitioners, in those times, seem either to have practised indiscriminately all the branches of the healing art, or to have undertaken only the cure of particular diseases. It was during the dark period which intervened between the overthrow of the Roman Empire and the revival of literature in the twelfth and thirteenth centuries, that a separation was effected between the practice of Physic and the practice of Surgery; the former having passed almost entirely into the hands of the Clergy, who, by involving it in superstition, rendered it a profitable employment, and in many instances an introduction to ecclesiastical preferment; whilst Surgery was in a very low and degraded condition, a principal part of it, the care of the wounded and lame, being commonly intrusted to women and ignorant pretenders. Anatomy was nowhere taught, and the knowledge of surgical operations, even imperfect as it had been among the Greeks and Romans, was no longer to be found in papal Europe.

The greater part of the Universities first established in Europe, were originally, as Dr Smith has remarked in his letter to Dr Cullen, Ecclesiastical corporations, instituted chiefly, if not solely, for the education of churchmen. In these institutions the study of medicine was conjoined with that of divinity, and the priests continued to retain, in a great measure, in their own hands the practice of physic, whilst for the per-

formance of such manual operations as were necessary, they usually employed their own servants who acted at the same time as their barbers. The difficulty, however, of preventing the clergy from intermeddling with the practice of surgery, notwithstanding repeated papal edicts, gave rise to two decrees at the close of the thirteenth and beginning of the fourteenth centuries, by which surgery was formally separated from physic. The priests were absolutely forbidden to practise it under pain of excommunication; and the University of Paris, in consequence of these decrees, refused to admit any student into the Faculty of Medicine who did not abjure surgery. It is from the separation produced by these two decrees, it is believed, that we ought to date the true origin of the distinction between physician and surgeon, such as it has existed in modern times.

When separate colleges of physicians and surgeons began to be established in the different countries of Europe, attempts were made to ascertain and to establish by law the particular limits of these two professions. In making these attempts, however, Governments had undertaken a task far beyond their powers, the object of them being quite irreconcileable with the laws which nature observes in the production, course, complications, and cure, of local and constitutional diseases. Hence it happened, that, in the charters granted in different countries, or even in the same country at different periods, to the respective colleges of physic and surgery, the limits between these two parts of the medical art were perpetually varying, according to the prejudices of the legislators who vainly endeavoured to fix them; or according to the temporary, and but too

often selfish, interests of those by whom such charters were obtained. The records of the medical art in modern times, afford ample proofs that the colleges of physic and surgery, instead of regarding their charters as barriers wisely placed by the Legislature between the Public and those who pretended to practise medicine, without having had the advantages of a liberal and professional education, speedily converted them into instruments of mutual warfare, and made them the subjects of endless and most disgraceful disputes.

If at the present day we inquire into the occupations in which those are engaged who have obtained licences to practise the healing art, under any of the different designations conferred on medical practitioners, it will be found that, in regard to a great proportion of these practitioners, there is no such difference in the nature of their employments, as the provisions of their collegiate charters and the difference of their titles might seem to imply; and that, in fact, almost all medical men act, particularly at the commencement of their professional career, in the capacity of general practitioners of almost all the branches of the profession. In small towns, and particularly in country villages, subdivision of labour in the medical profession is totally inadmissible. In these situations, the customs, necessities, and convenience of their patients, do not admit of any distinctions among medical practitioners which are not immediately derived from a real or fancied superiority of professional skill. But how extensive the practical information required to qualify the village or country practitioner for the duties which he has to perform, will be obvious, when we reflect that he must

undertake the cure of all the accidents and diseases to which men, women, and children, are liable in the district in which he is to practise. It is in great cities only that the distinction in the exercise of the medical profession, between physician, surgeon, and general practitioner, either has been or ever can be attempted to be observed; and even in those cities in which this distinction has prevailed in the highest degree, how small a portion of the community, it may be asked, have enjoyed the advantages supposed to be derived from this division of the healing art. The rich, it is true, may in all cases of danger, whether real or imaginary, add the attendance of the physician or surgeon to that of their ordinary medical attendant; but the poor, who form the greatest body in every community, must either be contented to live and to die without the advice or assistance of those who practise physic and surgery as distinct professions, or betake themselves to some of the asylums or hospitals that are maintained at the public expense. Indeed, the most superficial acquaintance with the symptoms, progress, and termination, of the various morbid, acute or chronic, febrile or inflammatory, affections to which the human body is liable, must be sufficient to convince every unprejudiced inquirer, that there is but a slight foundation, if indeed there be any, for the distinction between physicians, surgeons, and general practitioners, in the nature of the diseases which these practitioners are required to treat, or in the mode of treatment by which the diseases themselves may be cured or relieved. Experience has long shewn that, on the one hand, the use of internal remedies is required in a large proportion of the diseases which are regarded as strictly

surgical; and, on the other, that there are few diseases which come under the care of the physician in which morbid affections, requiring the manual aid or practical skill of the surgeon, do not frequently occur. Each of these classes of diseases, however, must, in the ordinary practice of the art, be treated indiscriminately by the general practitioner.

Since, therefore, in the ordinary and general practice of medicine, Physic and Surgery never have been, and never can be, separated from one another, it seems but reasonable that those who, from the nature of their profession, and from the circumstances of the situations in which they may be placed, must practise them conjointly, should all of them learn equally the rudiments of both arts. And considerable as are the advantages which may be derived, in the improvement of individual skill, from a certain degree of subdivision of labour in the medical profession, in large towns especially in which the state of society is such as to admit of this subdivision, it is only by all those who are destined for the exercise of the medical profession receiving the same general and professional education, that their mutual cooperation can be ensured, and that they can be enabled to discharge, with full utility to the public, the duties of any particular branch of medical practice, to which, from inclination or interest, they may afterwards be induced to devote their more particular attention. Such a general and equal system of Education, appears to me to be the only proper barrier that can ever be raised between the medical profession and the practice of dangerous quacks and ignorant pretenders. It is in the view to promote this improvement in the literary, scientific, and professional, education of medical men, that the hono-

rary degrees conferred by Universities, and the certificates of qualification granted by Colleges of Physic and Surgery, should have their foundation, and not in that exclusive and illiberal spirit of monopoly, which, by enjoining a distinct course of instruction, vainly endeavours to separate branches of knowledge which, in their attainment and practical application, ever have been and ever must be united. "Illud ante omnia scire convenit," says Celsus, "quod omnes medicinæ partes ita innexæ sunt, ut ex toto separari non possint, sed ab eo nomen trahunt, a quo plurimum petunt."

In confirmation of some of the foregoing remarks, I have much pleasure in being able to subjoin the following passage from a discourse of one of the most acute and intelligent medical authors of the present day:

"So far as education is concerned, I take the liberty of remarking, that the artificial distinctions in the profession, so much insisted on and valued by some, ought to be entirely disregarded; and of asserting, in the broadest way, the unity of the medical profession as an object of study. The several component parts of our frame are so closely connected,—their relations and mutual influences, in health and disease, are so numerous and powerful,—the causes of disease, and the modes of remedying it, are of so general a nature, that no part can be understood without a knowledge of the whole. No particular class of affections can be comprehended or properly treated, except by the aid of principles derived from the whole range of medicine: the diseases and treatment of each part, on the one hand, are governed by these principles, and, on the other, they reciprocally furnish data capable of illustrating general pathology. Therefore, whether the student means to be a physician, or surgeon, and *a fortiori*, if he be intended for a general practitioner, he must learn Anatomy, Physiology, General Pathology, Therapeutics, Materia Medica, Chemistry, and Pharmacy; and, inasmuch

as he is unacquainted with any of these, he is so far unfit to practise his profession."

"After receiving a general medical education, different persons may be led by taste, convenience, or various other considerations, to select one part of the profession rather than another; this is a matter of subsequent choice, and ought not to regulate scientific education. In large communities, it may be advantageous to patients and to the progress of science, that the established distinctions in the profession should be maintained, provided there is no anxiety to draw the line very narrowly. Indeed, the latter attempt, which is in itself absurd, is very little likely to be encouraged by the public, on whom it could only operate as a limitation of choice, preventing them, on many occasions, from having recourse to that professional assistance in which they might be disposed to confide *."

But distinctions between the members of the medical profession have also been recognised by the legislature, under the titles of Graduates of Universities, Extra-licenciates, Licenciates, and Fellows of Colleges, founded not on the different departments of practice to which they profess more particularly to direct their attention, but on the constitution of the different corporate bodies from which they derive their licences to practise, and on the different titles which these bodies are pleased to bestow upon them. By means of such distinctions, the practitioners of the healing art in these kingdoms, instead of enjoying equal and reciprocal privileges as members of one great community, are, as it were, divided into a number of petty companies, each possessing a monopoly in some particular branch of the trade, which, in

* Speeches delivered by Mr Lawrence at two meetings of Members of the Royal College of Surgeons, &c. London, 1826.—See Appendix, Note W.

too many instances, it watches with the utmost jealousy, and defends with the utmost keenness, against the encroachments of its rivals. From a variety of circumstances, easy to be conceived, it is in England that this system of monopoly has been carried to the farthest extent, and most pertinaciously adhered to. The graduate of a Scotch University may attain the highest medical rank in the Military or Naval service of his country, but his diploma will not entitle him to practise in the smallest town in England. If he attempt to do so, he may be placed under very humiliating circumstances; he is there only a Doctor by courtesy, and therefore cannot claim rank, nor sue for redress in courts of law, even though he may have been grossly abused in his professional capacity *. The graduate of an English University, or the extra-licenciate of the College of Physicians of London, may practise physic in any part of England, provided he keeps without that magic circle, the circumference of which is drawn, with a radius of seven miles around the metropolis †; and even the licenciate of the College of Physicians, who is admitted to the ordinary exercise of the profession within this sacred precinct, cannot obtain a right to participate in those *spolia opima*, which the College reserves for its own Fellows, but by the especial favour of the Brotherhood ‡.

Of distinctions and privileges such as those which have been mentioned, it is enough to ask whether they

* Medical Jurisprudence by Paris and Fonblanque, vol. i. p. 17, Note.

† Ibid. p. 24, note.

‡ Ibid. p. 51.—See also p. 52 for an enumeration of the "legal classes of Physicians."—See Appendix, Note X.

really indicate, even in the remotest degree, the respective qualifications of those who possess them, for exercising with security and advantage to the public, the responsible duties of their profession; or whether, supposing they did indicate such differences in the qualifications of their possessors, any thing can be conceived more absurd than for a nation to fix a different standard of acquirements for its military, for its provincial, and for its metropolitan Physicians. If it be desirable, for the interests of society, that there should exist a separate class of medical practitioners, under the title of Physicians, the Legislature would surely confer a greater benefit on the public by fixing a course of preliminary and professional education, and providing for the strict examination of those who desire to be licensed to practise in this capacity, and then leaving them at liberty to exercise their profession where they please, than by indulging particular corporations in the exercise of a narrow and exclusive system of monopoly, the only conceivable operation of which is to engender arrogance and presumption, and consequently ignorance and rashness, in the minds of those who are admitted within its pale, and jealousy and rancour in those who are kept without it. It has been justly remarked, that "our physicians have heretofore been derived from various sources, with very unequal qualifications. It would undoubtedly be desirable so to assimilate the courses of education pursued in our medical seminaries, as to remove all disparity, but such as natural inequality of talent must ever impart *."

* Dr Barlow, Edinburgh Med. and Surg. Journ. xxviii. 347.

The only species of defence which it has been attempted to set up for distinctions of the kind that have been mentioned, is the importance, as regards the dignity of the profession, of "encouraging a liberal education amongst those who are destined to move in the higher ranks of physic." This defence is obviously founded on the imperfect literary and scientific education of the great bulk of medical practitioners, which, however, it makes no provision for correcting; and if, by moving in the higher ranks of physic, be meant the practising physic among the higher classes of society, it involves the farther absurdity of providing for the rich, by legislative enactments, what, if congenial to their tastes, or necessary for their comfort, they are perfectly able to secure for themselves, without any assistance from the legislature. If, again, by those who are destined to move in the higher ranks of physic, be meant those whom the profession and the public may select, from a belief in their superior sagacity or practical skill, to assist and advise their professional brethren in cases of difficulty and danger, it is a most erroneous idea to suppose, that it is possible to ensure the public's being provided with individuals qualified for those duties, merely by subjecting a few of the members of the medical profession to an extended course of University education. The qualifications necessary for those who are to act as Consulting practitioners must be the result of extensive and long continued opportunities of practical observation and experience, and of the industry and talent necessary for turning these opportunities to the best account. Neither the most extended course of University education that could be

prescribed, nor the most rigid examinations that could be enforced, can ensure the acquisition and possession of such attainments, and certainly no University or Corporation privileges can confer them. It is in the proper education of the whole body of the members of the medical profession, and in the experience which particular individuals may acquire by extensive private practice, or by attendance in dispensaries or hospitals, that the public have their best, and indeed their only, securities, for an adequate supply of intelligent and experienced Consulting practitioners. The Legislature, therefore, will have performed its duty, when it shall have taken care that no one can be licenced to enter on the practice of medicine without such preparation as a thorough preliminary and professional education can afford. Whatever particular degrees or titles Universities or Corporate Societies may be pleased to confer on individual members of the profession,—in addition to the common licence which all medical practitioners should be obliged to obtain from Boards specially appointed for granting them,—these degrees or titles ought to derive their importance not from any exclusive legal privileges being attached to them, but from the opinion the public may entertain of the discretion and impartiality with which they are conferred. The Public must be left at liberty, with the benefit of such assistance as it may chuse to receive from the medical profession itself, to select from among the general mass of medical practitioners, those whom they consider to be best qualified, by their attainments and experience, for the duties of the higher ranks of the profession. Any attempt to limit its freedom of choice in

this particular cannot fail to be followed by the ludicrous result, which has uniformly attended all such legislative restrictions, that those who, in the eyes of the law, are the sole proprietors of a particular branch of medical practice, are very far from being so in reality.

There are obviously two ways in which attempts may be made by the Legislature to secure the fitness of those who obtain licences to practise medicine; 1st, By laying down a regular course of general and medical Education, through which they must previously pass; and, 2d, By obliging them to submit themselves for Examination to some Board or public body, intrusted with the duty of judging of their qualifications. In most instances in which the Legislature has hitherto interfered for the regulation of medical practice, it has contented itself with the latter of these precautions, and has confided to the Bodies so intrusted the further duty of prescribing a course of study to those whom they receive on examination. The statutes and bye-laws of the different public Bodies in these kingdoms intrusted with the power of licensing medical practitioners, under the different designations of physicians, surgeons, and apothecaries, with regard to the conditions on which they grant these licences, form at present a strange medley of incongruous regulations, framed, it is to be suspected, with but little, if any, reference to the interests of the public, and in many respects extremely ill calculated to provide for the actual wants of the community. Each of these bodies requires a different, and, in most instances, a very insufficient course of study, from those to whom it grants its licence to practise medicine; each of them confers on its licentiates a monopoly of some branch of medical practice, or an

exclusive right of practising a particular branch in a particular district of the country; and, unfortunately, it does not in all instances happen, that the extent of privilege which each of them has it in its power to confer on its licentiates, by any means corresponds with the amount of qualifications which it requires of them. It may be doubted whether the circumstance of the Legislature having in a great measure confided to these corporate Bodies the power of regulating the education of medical students, has not been a principal source of the very low condition of a large portion of the medical profession in point of general instruction and professional attainments. It would not, it is believed, be difficult to shew, that the introduction of improvements into the Courses of study prescribed by these Bodies, has been in a great degree retarded by the apprehension which each of them has entertained, that to raise the standard of qualifications required of its licentiates, might, without being productive of any public benefit, operate prejudicially to its own interests, and only promote those of its rivals,—by inducing those who wished to be licensed, to seek that privilege from some other Corporation which did not require so extensive a course of education, nor subject its licentiates to so strict an examination. It is in a great measure from the paralysing influence of this apprehension, that there seems to arise a necessity for the interference of the Legislature itself, to prescribe such a course of preliminary and professional education for all those who may be desirous to obtain licences to practise medicine, and to provide for their being subjected to such an examination, as may afford to the public a reasonable se-

curity of their being properly qualified for the responsible duties which they are to undertake in the exercise of their profession.

If the views be correct, that have been now taken respecting the propriety of the Legislature laying down a *common* Course of preliminary and professional education, through which all desirous to obtain licences to practise medicine shall previously pass, it is obvious that it must be a matter of great importance to determine what the course of study prescribed ought to be;—what it ought to comprehend,—in what order the different branches which it embraces should be studied,—and what ought to be its extent and duration. At a time when the subject of Medical Education is about to be brought so prominently before the attention of the public, by the inquiries and by the report of the late Commissioners for Visiting the Universities and Colleges of Scotland, and when it may be anticipated, that it will ere long engage the attention of the Legislature, I am glad to be able to adduce the opinions of one who had so long occasion to turn his attention to this subject as Dr Cullen had, and who was so well qualified by his own acquirements,—by his having passed through the various ranks of the profession,—and by his intercourse with medical men in all stages of their advancement, to judge what is the course of education best calculated to prepare students to enter on the practice of medicine. In the notes of one of his introductory lectures to his course on Chemistry, I find the following heads written down for commentary, which, though they afford mere hints of his opinions, will, I am convinced, be found by those

who have paid attention to the subject, not to be hasty and superficial suggestions, but the results of mature consideration.

"OF THE STUDY OF PHYSIC."

"The study of Physic is seldom properly conducted. The Universities prescribe no rules to direct students. Students come to them uninformed, and their various circumstances occasion great irregularities. I have often been asked to give my opinion, and I take this opportunity. If it were possible, a genius should be chosen. A knowledge of languages; a familiar acquaintance with Latin, and at least a moderate with Greek. French at present necessary, and the other modern languages useful.

Study of Belles Lettres, Logic and Metaphysics, Mathematics and Natural Philosophy. Disputes about their utility. Natural Philosophy was at one time pushed too far in Medicine, and has had little success; but though it does not go so far as was once imagined, still it is necessary in many respects. The question is connected with that about the use of Theory in Medicine, and is then to be discussed. The study of it (Natural Philosophy) is necessary, and time should be allowed for it as a preliminary.

Natural Philosophy consists of two branches, the Mechanical and the Chemical. The first is taught in the schools of Philosophy. The second unhappily has been transferred to those of Medicine. It is my own branch, and I should have no improvements to make in it, but there are many. It is a part of Philosophy, a preliminary study, and should be treated as such.

When Chemistry is finished, another preliminary study, that of Natural History, to be entered on. Chemistry is necessary to a knowledge of minerals. Botany may be taken at any time, but the sooner the better. Zoology may be entered upon by itself, as the Insects, but the most of it will

fitly succeed the Anatomy. It affords the Comparative Anatomy.

Instructed in Natural Philosophy, Chemistry, and Anatomy, a student is to proceed to the Institutions, the General Doctrines, the Theory of Physic. False notions upon this subject. Disputes about the use of Theory. It has been abused, it still is, and much of it is hurtful; but some parts of it are useful, and a full discussion of it is necessary. It is suitable to the young, and at least by them to be indulged in, but always with diffidence.

The Institutions consist of three parts, Health, Disease, and Means of Cure. The first occupies our students; the second is repeated in the Practice, but should be taken more generally, and our Professor thinks so. The third part but slightly touched on in the Institutions. It is supplied by the Materia Medica. The Materia Medica has been improperly placed, as it requires a knowledge of Natural History, Chemistry, and the Theory of Medicine. It has been improperly taught, but now it is taught more properly as a Methodus Medendi.

Instructed in the preliminary studies, and in the general doctrines of Physic, the student proceeds to particulars, or the study of the Practice."

On another occasion, Dr Cullen expressed himself in regard to medical education, in the following terms:—

"The Study of Physic comprehends that of a great number of auxiliary sciences, as Natural Philosophy, Chemistry, Anatomy, and Natural History, which require a great deal of time and labour, that many would willingly avoid. The necessity of these studies proceeds entirely on the supposition of the usefulness and necessity of a Theory of Physic. But this supposition is not universally admitted. Old practitioners commonly pretend to despise it, and even many students seem not fully convinced of the importance or necessi-

ty of it. This leads to indifference and inattention, not only in the study of the Theory of Physic itself, but also of the several parts of science that lead to it. Accordingly I find that few of our students apply to the study of the Mechanical Philosophy, or are tolerably acquainted with it. Few, also, have patience enough to attend fully to the Chemical Philosophy. This indifference to the study of Philosophy and the Theory of Physic, has manifestly a very bad influence upon the conduct of our studies, particularly it leads to an irregular course, and almost always to a very hurried course of study.

After shewing the necessity of Physic being studied upon a regular and dogmatical, rather than upon an empirical, plan, Dr Cullen proceeded—

"In the present state of physic, we must unavoidably meet with the theory of others, and in any state of physic it will be impossible to avoid our own. But both the one and the other are fallacious and uncertain, and it is of the utmost consequence to guard against the errors they expose to. For this, we may in part depend upon the rules of logic, or rather what these are founded on, the principles of good sense; but neither the acutest genius nor the soundest judgment will avail in judging of a particular science in regard to which they have not been exercised. I have been obliged to please my patients sometimes with reasons, and I have found that any will pass even with able divines and acute lawyers; the same will pass with the husbands as with the wives. No person is qualified to judge of the soundness of a theory, unless he has been much exercised in reasoning upon the same subject. It certainly requires the utmost effort of the most acute genius, as well as much exercise in metaphysical reasoning, to arrive at genuine scepticism, that is, to discern everywhere the weakness of human reasonings. In like manner, in the Theory of Physic, no man can be a proper judge unless he who has examined it very thoroughly,—has examined the foundations, and has applied the plummet and

square to every part of the superstructure. It is he only that can determine what parts may stand, and what must be pulled down again. The errors of theory require a full contemplation; and it is he only who has a thorough acquaintance with theory, and has been frequently exercised in it, that can find the real use, or avoid the abuse, of the Theory of Physic. My arguments in favour of the study of this branch of medicine are drawn from the considerations employed against it. I might add some arguments in direct proof of its utility and importance. My time, however, does not allow of this; but from what has been said, we conclude, that the Theory of Physic is necessary, that much time ought to be bestowed upon its study, and every one ought to be well prepared in the *auxiliary* sciences that are the foundation of it. This I do not say with a view to recommend chemistry, the auxiliary science in which I am more immediately concerned, for a knowledge of chemistry is equally necessary upon the empirical as on the dogmatic plan. I wish to recommend the study of the Theory of Medicine as being necessary to make a skilful as well as an ornate physician. I wish chiefly to shew you how much time and pains is necessary to the study of physic. I shall add but one other remark. The rude and inaccurate reasonings, the crude and rash conjectures, of young physicians, are liable to excite disgust, and certainly are an abuse which may be considered as dangerous. It is true that, by the study of theory, some are made coxcombs that Nature meant only for fools, and the abuse of every part of science may happen to produce pedantry and heighten folly. Yet it is probable that, in Physic, the abuse of theory is not so often owing to want of genius, as to want of time, application, and exercise, in the study of it; for here it is essentially true, that 'a little learning is a dangerous thing*.'"

There are two opinions regarding Medical education which these hints prove Dr Cullen to have entertained,

* See Appendix, Note Y.

that appear to be particularly deserving of attention. The first is, that those Bodies which possess the right of licensing medical practitioners, should not only demand of candidates for licences that they shall have received a certain amount of professional education, but should prescribe the order in which their studies shall be pursued. The propriety of this measure obviously arises from the connection and dependency which exist between the different branches of medical science, and which render it necessary for the student, in order to be able to study some of these branches with advantage, to have previously acquired a knowledge of one or more of the other branches. He who begins to study the functions of the human body, before having acquired a knowledge of its structure, or to study the diseases to which the body is liable, before he has acquired a knowledge both of its structure and of its functions, does little better than waste his time; but instances of blunders of this kind on the part of medical students are far from being rare; and so long as no fixed course of study shall be laid down for them by the Legislature or by the Schools of Medicine, Dr Cullen's observation will continue to be true, that "the study of physic is seldom properly conducted*."

The second opinion relative to Medical education which these hints prove Dr Cullen to have entertained, is, that students of medicine should receive an extended course of literary and general scientific instruction, previously to entering on, or conjointly with, their medical studies. There is, perhaps, no point connected with the education of medical men more deserving of

* See Appendix, Note Z.

the care of the Legislature, than the improvement of the literary and scientific education of those on whom licences to practise the medical profession are conferred. It seems indeed absurd, that any one should be allowed to enter on the study of Medicine without having acquired a competent knowledge of the Latin and Greek languages, and of the elementary parts of Mathematics, or should be allowed to prosecute his medical studies without conjoining with them the study of Natural Philosophy, Logic, and Moral Philosophy. These are branches of education, the study of which is of the utmost importance to students of medicine, not only from the tendency which their cultivation has to open, enlarge, and strengthen the understanding, and to engender habits of attention and of accurate reasoning, but also from a competent knowledge of them being absolutely necessary, to enable students to comprehend readily those lectures and books through which their professional information must be obtained. It is not to be expected that all who obtain licences to practise the healing art should be profound scholars, or deeply versed in the physical and metaphysical sciences; but there can be little doubt, that they who have gone through the mental discipline necessary for acquiring a competent knowledge of these branches of education, must enter on their medical studies, and pursue them, with their minds infinitely better prepared for acquiring professional knowledge, than those who have not received such a preparatory education.

It has been urged as an argument against making any considerable increase in the extent and duration of the education prescribed for medical students, and

consequently in its expense, that many individuals of superior talents, but in humble circumstances of life, would thereby be excluded from the profession, and that the total number of those who engage in it would be diminished below what is requisite for the interests of the public. These apprehensions seem to receive a sufficient practical refutation, in the course of preparatory education which has long been required of those who are permitted to enter upon the study of divinity in the different Universities of Scotland; for though the students of divinity neither are from a more wealthy class of society, nor have the prospect of greater emoluments, than students of medicine, there is, I believe, more reason to lament the excess than the deficiency of their numbers. On the possibility as well as the expediency of improving the general education of those who are intended for the learned professions in this country, I am happy to be able to quote the opinions of one who has made the system of education pursued in the Scotch Universities the subject of very careful consideration.

"It is not for the purpose of reducing the number of competitors, that we would beset the entry to the learned professions with additional difficulties. But the present excess of those competitors, proves with what safety such difficulties may be imposed. Nor would we impose them for their own sake. The toil and the cost of a more lengthened and laborious attendance at our Universities are not desirable in themselves, but the more comprehensive and profounder scholarship to which they are subservient is highly desirable; and to the attainment of this object, the present state of matters is convertible. As formerly, we were obliged to let down the system of education for the purpose of draw-

ing more students to college; so now that we can afford to have greatly fewer students, we may raise that system. The standard of preparation was lowered, when the circumstances of the country required such a facility for the due supply of the learned professions; but now that the facility is such as vastly to have overdone the supply, this is the intimation to us how much the standard of preparation admits of being elevated *."

Besides fixing a course of Study for those who are desirous to receive licences to practise the medical profession, there are two points connected with medical education which seem to be also extremely deserving of the most careful consideration of the Legislature; the first to determine who should be considered as qualified Teachers of the different branches of study prescribed; and the second, who ought to be intrusted with conducting those Examinations to which medical students should be subjected previously to their receiving licences to engage in the practice of their profession†.

There are at present in this country two classes of medical Teachers, who, in mercantile language, may be said to enjoy a bounty on teaching, by having certain privileges attached to attendance on their instructions, viz. the medical Professors in the Universities, and the Members of the different medical Incorporations. The rules laid down by the different Universities, with regard to the Teachers, attendance on whose instructions they recognise as qualifying medical students for graduation, are very various. Some Universities confine this privilege to their own Professors, and to those of certain other Universities; others extend it

* Dr Chalmers on Endowments, p. 68.

† See Appendix, Note A A.

to the Professors of all legally constituted Universities; and others, again, extend it to all Teachers, who are members of the different chartered medical Incorporations. The other Corporate bodies which possess the power of conferring licences to practise medicine, as the Colleges of Physicians and Surgeons, the Societies of Apothecaries, &c., receive from their candidates evidence of their having attended the lectures either of University Professors, or of the Members of their own or of other medical Corporations.

Is it necessary for the Legislature to make any provision for insuring that those private Teachers, attendance on whose instructions is received as qualifying medical students to be examined for a licence to practise, shall be competent to the duty of teaching the particular branches of the science which they profess? If so, should the simple circumstance of being a Member of one of the medical Incorporations,—admission into which, it may be remarked, has hitherto had no reference to the supposed qualifications of the candidate for the business of teaching,—be held as sufficient proof of the possession of the requisite qualifications for a Teacher, or should intending lecturers be subjected by the Incorporations to which they belong, to some particular probation of their fitness? Ought privileged Teachers in the private schools of medicine to be allowed to engage in teaching as many of the branches of Medical science as they may please, or be obliged, like the Professors in Universities, to confine their attention to one particular department of instruction? And what security should be required of them, that the young men who enter as their pupils, actually merit those certificates of regular

attendance on their instructions, which candidates for medical licences must produce, before being admitted to examination by the different public Boards?

Dr Smith, in his letter to Dr Cullen, besides defending the expediency of allowing all to engage in medical practice who chuse to do so,—and that even under the sanction of a degree, if they can procure it,—has also asserted the opinion, afterwards maintained by him in his Theory of the Wealth of Nations, that there should exist a free, unfettered, and unassisted competition among the instructors of youth; and that the selection of teachers may, in all cases, be most safely intrusted to students and their friends. The arguments which he uses to demonstrate the inexpediency of favouring particular teachers, by endowing their Chairs with salaries, or by enforcing attendance on their instructions, seem to reduce themselves to the following:—1st, The tendency of such artificial helps to diminish the diligence of these teachers; 2d, Their oppressive and unjust operation upon private and unprivileged teachers; and, 3d, Their directing the course of education towards objects less useful than those to which it would naturally have gone.

It would lead into too wide a field of discussion, and one too remote from the proper object of this work, to inquire whether the evils so forcibly pointed out by Dr Smith, as resulting from the endowment of Seminaries of education, by salaries, or by enforced attendance, might not be in a great measure corrected by the Legislature subjecting the proceedings of these Seminaries to occasional examination; or whether, even in their present magnitude, these evils are not more than coun-

terbalanced by the concomitant advantages of these endowments. It will at least be allowed to the advocates of Endowment, that in Medical, if in any species of lay education, it is for the advantage of the community that the highest talents should be engaged in the business of teaching, even though the cost of obtaining the proper exercise of these talents should considerably exceed, by the erection of Museums, Hospitals, &c., what those who are to receive instruction shall be able to defray; and it might not perhaps be difficult to shew, that there are some circumstances in a great degree peculiar to Teachers of medical science, which render a selection of the persons who are to act in this capacity, by competent and unprejudiced judges, peculiarly desirable.

But, whatever arguments may be advanced in favour of endowing or conferring peculiar privileges on particular medical Teachers, it is impossible to deny the justness of Dr Smith's general principles,—that what the Public seems to be principally interested in, is the possession of a sufficient stock of preparatory and of professional knowledge on the part of those who receive licences to practise medicine, it being of very little consequence to it in what Schools this knowledge has been obtained; and—that in the business of teaching, as in every other profession, the exertions of the greater part of those who exercise it, will be in proportion to the necessity they are under of making that exertion. If, therefore, it be deemed expedient by the Legislature to limit competition in the business of teaching medicine, it is obviously its duty to provide, as far as is possible, in the first place, for the privileges it grants being conferred upon competent persons; and, in the second

place, for the diligence of the persons selected being steadily maintained. The first object can only be effected by placing the choice of medical Teachers in the hands of persons interested and qualified to make a good selection ; and the second, by the establishment, throughout the different parts of the country, of such a number of medical Schools, on a fair footing of equality with one another in regard to privilege, as will leave to students a considerable latitude of choice amongst different teachers ; and render the teachers, in a proportional degree, dependent on their success and reputation in their particular departments To be members, or not to be members, of a particular University, or of a particular College in a University, may be a proper enough ground for admission into, or exclusion from, a convivial club or other voluntary association ; but that, in the present day, when the opportunities of acquiring medical knowledge are so extensively diffused over these kingdoms, the right of practising physic, in any part of England, should depend on a person's having pursued his studies in one or other of two Schools, which are in many respects most inconveniently circumstanced for those who are educating to the medical profession, and confessedly but little capable of affording the information necessary to qualify them for its exercise, is an abuse well calculated to create and to keep up a general disgust and dissatisfaction with the system of privileged Universities.

With regard to the question, who should be intrusted with the Examination of candidates for licenses to practise medicine, it seems very obvious that the examinators should not have any motive for ingratiating themselves with those who are to become candidates for licences,

nor have any direct influence either in granting or in withholding licences. Whether these considerations should exclude from the office of Examinators those who are engaged in the business of teaching, who may naturally be supposed to be prejudiced in favour of their own pupils, is a matter deserving the attention of the Legislature. They at least shew the propriety of the remuneration of the examinators not depending on the number of individuals on whom they confer licenses; and they suggest likewise the inexpediency of intrusting those who are practising in a particular place or district, with the exclusive power of deciding on the qualifications of persons who may propose to become their rivals*.

Whether it be possible to determine by Examinations the fitness of candidates for licences to practise medicine, to receive these licences, is a question on which there seems to have been a tendency to exaggeration of opinion on both sides; some, with Dr Smith, representing examinations as altogether unworthy of being relied upon as a test of this fitness, and others considering them as so complete a test as even to supersede the necessity of prescribing any particular course of professional education to medical students. The proper object of examinations seems to be, to insure a due degree of attention being paid by the student to those instructions attendance on which is prescribed; and in so far as these instructions are calculated to prepare a student for entering on the practice of the medical profession, examinations must increase the security of the public that those who successfully

* See Appendix, Note B B.

undergo them are worthy of being licensed to practise Medicine. It is much to be wished, indeed, that medical students, whether educated in Universities, or in particular Schools of Medicine, were subjected to repeated examinations at different stages of the progress of their Course of education. Their attention to their studies would thus be ensured during the earlier as well as during the later years of that course; those who from idleness did not avail themselves of the opportunities of acquiring knowledge which they enjoyed, would speedily be warned of the necessity of increasing their exertions; and those whose inability to undergo their examinations seemed to depend on want of capacity rather than of application, would receive timely notice of the expediency of turning their thoughts to some other line of employment. An effectual stop would in this way be put to the practice of students who have idled away the greater portion of the time that ought to have been devoted to their studies, being hurriedly got ready in the course of a few months,—by methods which are well known to be practised, but which it is not necessary here to particularize,—for undergoing those examinations before the different public Boards, on the issue of which their future prospects in life so materially depend: While at the same time the examinators would be relieved from the painful alternative of sacrificing the interests of the public to compassion for an individual, as Dr Smith seems to have suspected may, on the present system, not unfrequently be done; or of pronouncing a young man who had passed through the whole of the prescribed course of study, unfit for exercising a profession, in attempting to acquire a know-

ledge of which he had spent some of the most important years of his life.

One of the few provisions for the education of Medical men that have been made by the Legislature, is the obligation of serving a long Apprenticeship, which it has imposed on that class of practitioners who at present enjoy in England a monopoly of the general practice of medicine. That it may be highly advantageous for young men, entering upon the study of medicine at a regular school, to be placed under the immediate superintendence of individuals able and willing to guide them in their studies, will not be denied. But it does not seem essential towards obtaining the advantages of this superintendence, that the student should be condemned to pass a large portion of every day of his apprenticeship in performing the menial drudgery of a general practitioner's shop, whilst all the practical skill in pharmacy or surgery which he can there acquire might be more advantageously obtained in the course of a few months, by the study of Practical Chemistry, or by attendance at a druggist's shop, and by officiating as dresser in an hospital, or as pupil at a dispensary. An Apprenticeship may unhesitatingly be pronounced pernicious, which absorbs either the means or the time that ought to be devoted to the acquisition of preliminary literary and scientific knowledge. And when we remember the circumstances under which a large proportion of medical apprenticeships are at present passed, at a distance from any School where either preparatory or professional knowledge can be acquired, and in the performance of a perpetual routine of menial services, which could be performed with equal advantage

to the public by the most uneducated ; such apprenticeships cannot but be considered as an arrangement in which the interests of those who are training to the medical profession are sacrificed to the interests of those who are already engaged in its practice. For it is in vain to expect that even the most conscientious master can compensate to his apprentices by his own instructions, for a want of those opportunities of acquiring a knowledge of the different branches of medicine that are afforded by attendance on the instructions and lectures of professed teachers. And when we remember how small a portion of provincial practitioners can have the time, even if they had the inclination and the ability necessary, for guiding the studies of their apprentices, we cannot be surprised that apprenticeships of this kind should be, as they are now very generally considered by the liberal and enlightened part of the profession, more frequently the nurseries of idleness and ignorance than of industry and knowledge.

The period at which an apprenticeship to a well employed practitioner, may be served with advantage, is when the student has acquired as much professional information as can be obtained from attendance on the instructions of professed teachers, and is thus prepared for availing himself of those practical lessons which are to be derived from witnessing and assisting in the treatment of actual diseases, by one older and more experienced than himself. Without the preparation afforded by this mode of medical instruction, the young practitioner, when he first enters upon the practice of his profession, can supply the want of personal experience, only by the lessons which he obtains from the errors

and mistakes he commits, at the hazard or even to the detriment of his patients. It is in this stage of a young medical man's education that attendance at the provincial hospitals and public dispensaries, now very generally distributed over this country, is calculated to be of immense benefit to him as a preparation for entering on the exercise of his profession. "The provincial hospitals of England," as has been observed by Mr Lawrence, "many of which equal in the number of their patients the smaller hospitals of London, afford every opportunity of acquiring that most valuable kind of knowledge which is derived from experience. The practical study of the profession is here conducted with peculiar advantage, from the comparatively small number of students, and the circumstance of their attention not being distracted by a multiplicity of other objects. The number of persons to whom instruction can be imparted at the bedside of the patient, is circumscribed within narrow limits. However great the ability and knowledge of the practitioner, and his desire to communicate information, however zealous and attentive the students, the examination and explanation of a case, and its principles of treatment, can be made useful only to the small number who are able to see the patient and hear the practitioner."

The doubts that have till within a short period been entertained with regard to the legal rights of General medical practitioners in England, to claim remuneration for their professional attendance and advice, and the encouragement thus held out to them, to make their professional emoluments depend on the quantity of useless, and too often pernicious, drugs with which

they may be able to drench their patients, have been equally prejudicial to the interests of the Public, and degrading to the Medical profession; and, so long as the English public shall chuse to foster this most absurd and disgraceful trade in drugs, now very generally abandoned in this part of the kingdom, it is in vain to expect that medicine can be practised amongst them, upon scientific, rational, or honourable principles *.

The contrariety of opinion expressed by Dr Cullen and Dr Smith, in the documents which I have inserted, with regard to the necessity or propriety of interference on the part of the Legislature, for securing the fitness of those who engage in the practice of medicine, has led me farther, perhaps, than the occasion may seem to justify, in an endeavour to sketch some of the leading principles which, I conceive, ought to be kept in view, in any attempt to reform the system of Medical Legislation which at present exists in these kingdoms.

"Expositions of this nature and extent," it has been justly remarked, "are the more necessary, from there being no superintending association fitted for taking those comprehensive views which the occasion requires; for, while each department is represented by Corporate Bodies, to whom the care of its interests is confided, none exists for regulating the Collective Profession, adjusting the conflicting interests of its several branches, and adapting the energies of the whole for the public good The departments, in asserting their respective rights and interests, are necessarily limited by partial views. They conceive with sufficient truth and accuracy, perhaps, what would be required to perfect each branch

* See Appendix, Note C C.

as an isolated establishment, but they fail to recognise the natural connexion which unites the whole, and are thus, even in their collective labours, incompetent to display the comprehensive views, or establish the just conclusions, by which alone the Legislature can be safely guided*."

The rapidly increasing disposition, in this country, to make our different National institutions harmonize with the advanced and advancing state of knowledge, renders it highly probable that an attempt at Medical reform will ere long be made, and that the different medical Incorporations of the country will eventually be placed on a footing more suited than their present to the wants of the Public, and to the liberal spirit of the times. Whatever differences of opinion may exist as to the particular measures by which that object may be best effected, which all attempts at Medical Legislation should have in view, viz. the insuring the public's being provided with an ample supply of skilful practitioners, all, it is hoped, will be disposed to concur in the justness of the observation, that "it is quite impossible to legislate, unless for all departments of the profession at once, and for all the three kingdoms together †."

* Dr Barlow, Edin. Med. and Surg. Journ. xxviii. 333.

† See article in the Edin. Med. and Surg. Journ. vol. xxv. p. 407, on "The English Apothecaries' Act."



APPENDIX.

APPENDIX.

MANCHESTER ROYAL INFIRMARY

NOTE A. p. 9.

DR FOTHERGILL'S ACCOUNT OF THE EDINBURGH SCHOOL OF MEDICINE.

In speaking of Dr Alexander Russell, Dr Fothergill observes, "In the years 1732–3–4, Dr Russell continued his medical studies under the Professors, who at that time so ably filled the several Chairs of Physic in the University of Edinburgh, and laid the foundation of that character which ranks it in the public esteem superior to most others in Europe. Though there had long been Professorships for Medicine in that place, and several attempts had been made to introduce a general course of medical instruction, it was not till about the year 1720 that this University distinguished itself. Several gentlemen who had studied under Boerhaave, with a view to revive the study of medicine in their native country where it had formerly flourished, qualified themselves for the purpose of giving courses of public lectures on every branch of their profession. The celebrated Monro taught Anatomy, after having studied it under the ablest masters then in Europe; the *Theory* of Physic was assigned to the amiable, the humane Dr St Clair; Drs Rutherford and Innes chose the *Practice*; Chemistry was allotted to Dr Plummer; and the teaching of Materia Medica, together with Botany (of which last he was appointed King's Professor), devolved upon the learned and indefatigable Alston. The City of Edinburgh favoured the generous design, added to the salaries allotted from the Crown, and provided as suitable conveniences as the place then afforded. They had no sooner opened their respective professorships than

many students of their own nation, some from England, some from Ireland, and not long after some from the plantations, flocked thither. This stimulated the Professors to exert their great talents with the utmost energy. Dr Monro's class soon became numerous, and the anatomy of the bones, of the nerves, and his other works, will long remain as testimonies of his great abilities, when the grateful regard of the multitude who studied under him, and were witnesses of his singular attention to instruct and encourage his pupils, as well as to act the part of a parent to every stranger, fails of expression. With what grace and elegance, with what minuteness and precision, would the humane, the inimitable St Clair explain the Institutes of the master (Boerhaave) whose nervous simplicity he studied to exemplify, though not with servile imitation! Where he differed in opinion from that great man, with what diffidence would he offer his own! Ever the students' friend, and their example in a noble simplicity of manners, and a conduct becoming a gentleman and a physician. Dr Rutherford still enjoys his country's praise and the lasting esteem of all his pupils. Plummer is no more: he knew Chemistry well; laborious, attentive, and exact, had not a native diffidence veiled his talents as a praelector, he would have been among the foremost in the pupils' esteem. Such was the gentleness of his nature, such his universal knowledge, that in any disputed point in science, the great Maclaurin always appealed to him as to a living library; and yet so great was his modesty, that he spoke to young audiences upon a point he was perfectly master of, not without hesitation. Alston, the laborious Alston, will live for ages. What benefit his pupils had the means of reaping will best be known when his lectures, now in the press, are published. What care to separate truth from falsehood, how cautious in advancing speculations, how laborious in experiments, and chaste in forming his conclusions! The numerous MS. copies that were taken by his pupils of his lectures are sufficient proofs of their opinion of his abilities. But while I am thus attempting to pay my tribute of gratitude to the memories of those eminent Professors who laid the foundation of that seminary of physic, whose reputation has drawn students from every nation in Europe, let me not forget the learned, the able, the laborious

Innes. Though I was not so happy as to be of the number of his pupils, yet I can well remember the deep regret expressed by many of the students, my contemporaries, for his early and untimely fate; often have I heard them tell with what dignity, with what clearness and precision, in what a nervous masculine style he used to explain the aphorisms of his great master: his colleagues, too, most deeply lamented the loss which they and the public sustained by the death of so able a coadjutor; they regretted the *man* whom they loved, the physician and professor whom they esteemed."

NOTE B. p. 12.

ACCOUNT OF DR JOHN CLERK.

"UNDER the first impressions, that the death of the late worthy and learned Dr John Clerk made upon me, I conceived the design of bringing this company together. Full of respect for a person I had so long admired; warm with affection and gratitude towards the person I had so often found my friend; I wished to be, I fancied I could be, eloquent in his praise. But upon consulting more coolly my own talents, I found it was not proper for one who had been so long confined to the exercise of the mute arts, to aim at eloquence; I found it necessary to abandon every attempt of that kind: but I could not desist from my purpose of doing what honour I am able to the memory of Dr Clerk. The great merit he had with the public, entitles his memory to every mark of public respect; and the regard that every physician owes to his profession, is certainly due to a man who enjoyed so much eminence in it. By the presence of so many honourable and learned gentlemen, I can now perceive that my purpose is approved of; and I hope that even a plain narrative of the life of Dr Clerk will do him honour; I hope that even my imperfect account of his character and merit, will justify the regard the public have long entertained for him, and will, in some measure, soothe the regret they now feel for his loss.

The eminent person I am to speak of, Dr John Clerk, was born on the 11th day of September 1689. He was descended of a family which, by great honour and worth, had been for some time distinguished among the gentry of this kingdom. By a father who belonged to the medical profession, and was eminent in his part of it, Mr Clerk was early destined to physic. He was first properly imbued with all the literature that is necessary to qualify for the study, or may adorn the practice of the art. He was afterwards led through every part of the study of physic; he was led through every branch of the profession; he began as an Apothecary, and thereby, with regard to many particulars, he acquired an accurate knowledge that is sometimes neglected by the student, but the want of which is always deeply regretted by the practitioner. Our student had indeed, throughout, a singular education: he was little in colleges (public schools) of Physic, but he was constantly among diseases and medicines. The University of Edinburgh was not then so happy as to enjoy those Colleges of Physic which have since furnished so many physicians to Britain and her wide extended colonies. The genius of Clerk was at that time in danger of missing its proper cultivation; but he was singularly happy in receiving a very ample compensation for the want of public colleges. Dr Pitcairn *, whose fame in physic does so much honour to this kingdom, after he had adorned one of the most famous Seminaries of Medicine in Europe as a Professor, had then returned to bless his native country with his skill as a practitioner. This consummate judge soon discerned the rising genius of Clerk. Dr Pitcairn was pleased to find in him that taste for classical studies, by which he himself was so much distinguished: he was happy to discern in Clerk an acuteness in those sciences which he was then endeavouring, not without difficulty and opposition, to introduce into this country. He was particularly pleased in discovering in that young man, the symptoms of that

* Dr Archibald Pitcairn, who was born at Edinburgh in 1652, was appointed Professor of Physic in the University of Leyden in 1692; but returned to Scotland in 1693, and died in 1713.

sagacity, which, by proper cultivation, might, and which alone indeed can, furnish an able physician. Full of these views, Dr Pitcairn took young Clerk under his tuition. He became his Professor, or what is perhaps more valuable to the student, he became his private instructor. Such an instruction is in most cases in danger of being superficial, but such a genius, under such a master, could not fail to make great proficiency. Mr Clerk soon became very knowing in every part of physic, and of the arts relating to it; and became at length fit to go from under the immediate care of his master. He desired to visit the seats of learning abroad, but Dr Pitcairn, who had a just title to direct his course, being jealous lest any one else should have a share in rearing a production which promised so much honour to himself, would not consent that his pupil should formally study under any other master. Mr Clerk, however, did visit the seats of learning in Holland, France, and England, not indeed as a student, but on a better footing, as one desirous to receive knowledge in exchange for what of the same kind he could communicate. After travelling some time in this way, he returned to Scotland, took a degree in Physic, and appeared as a practitioner about the time when Dr Pitcairn, in the decline of life, was ready to resign his practice in favour of the person he had so well instructed and fitted to succeed him. Dr Pitcairn indeed died soon after he had, in some measure, formally bequeathed that young Physician, as a valuable gift to his friends and the public; and the public, directed by so good a judge, soon rested their hopes and put their confidence in Dr Clerk. He became immediately an eminent practitioner, at an age when most others must be employed obscurely in acquiring the experience that may entitle them to general trust and confidence. Experience is certainly necessary to a physician, and it belonged to the genius of Clerk alone, to anticipate that skill which is commonly acquired by others, only in the course of many years. Such was the progress of this physician, till he at length arrived to be in the first rank of the practitioners of this city, which has long been very happy in the most able physicians. Dr Clerk very early attained to this rank, and what is perhaps as unusual,

he preserved it through a long life to the hour of his death. I might have said more of the means by which he arrived at so much eminence; I may say more of the manner in which he supported it. It is well known that many artifices have been employed by physicians to get into practice, and there are means that may be employed by old practitioners to check the rise of competitors. A detail of these would be invidious to the profession; I cannot impute them to the eminent practitioners of this city; but I must be allowed to say, that no physician was ever more absolutely free from them than the late Dr Clerk. His great abilities rendered all artifices unnecessary to him; his undeviating probity made him incapable of employing them. The great trust he received from the public, was founded in the opinion which the members of the profession, always the best judges, had of his real skill. His credit would have been supported in spite of them, by those frequent good effects of his practice, which the most part of Scotland had soon occasion to observe. There are certain manners which may be called artifices, but which a practitioner can hardly be blamed for employing. These are, a studied attention to every circumstance of the case, and a show of sympathy with the pain and distress of the patient. Such appearances as they seem to express a proper concern, and promise always an earnest endeavour towards a cure, will commonly prove agreeable, and can hardly be hurtful in a physician; but such was the inflexible integrity of our eminent practitioner, that he could not stoop even to the most innocent artifice; he could not make a show of studious attention when his quick penetration rendered it unnecessary; he would not affect passions, which he could not really feel, for the innumerable objects of distress, that were so familiar to him.

I would here willingly consider more fully Dr Clerk in that conspicuous point of view in which he appeared so long before the eyes of the public, that is, as one of the ablest physicians that perhaps any age or country has produced. But how shall I do this? I cannot properly explain to this company, how accurate and profound his general knowledge was, or how great his penetration and sagacity in particular cases. I can only appeal to his learned

colleagues and fellow-practitioners, who, I presume, are ready to testify, how often they have admired his knowledge in solving their doubts, how often his discernment in relieving from the perplexities of the art; how often his large experience and great sagacity enabled him to determine with certainty, what in many others would have been only loose conjectures! Were it necessary, I might join here the testimony of some of the most eminent practitioners of London, from whom I have myself heard the strongest expressions of esteem for Dr Clerk. I would willingly here, too, give the surest proof of his exquisite skill, by enumerating many striking instances of his success; but such a detail would at present be improper, and I presume it is quite unnecessary, when the whole of Scotland, and this city in particular, are so well acquainted with it; while so many are on this occasion, busy in expressing their regret, and full in mentioning the particular causes of it.

I must now observe, that however great the fame of a practitioner of physic is, we may always suspect the foundation of it, unless we know him to be possessed of that learning and knowledge which is the only sure foundation of medical skill. Was Dr Clerk a learned man? It is true the occupations of great practice hardly leave the leisure that is necessary to a proper display of literary merit, and our great practitioner has therefore published no writings in his own name; but he was well known to his friends, and in some measure to the public, as an excellent scholar. He cultivated and delighted in polite literature: he was singularly happy in being able to join the accuracy of the philologist to the taste of the gentleman; and some editions of the Classics lately published here have been much obliged to his labours. He particularly aimed at joining the knowledge of the physician to the judgment of the critic, in giving an edition of the elegant Roman physician Celsus; but the want of some collations that were necessary, unluckily prevented his making the progress he wished in that work. He was especially learned in every thing that related more immediately to the practice of physic; and, what is perhaps unusual in a person of his rank in practice, he was to the last hour of his life attentive

to every improvement of it offered by others. A few days before his death I found him going over a late literary Journal of Medicine with all the avidity of a young professor: and very lately he went through a laborious task, that of reading over carefully the whole of the late edition of the *Methodus Studii Medici* of Dr Boerhaave, as if he had been anxious to find if there were any medical writings which he had yet left unconsulted. Such indeed was his love of medical erudition, that nothing but the avocations of practice, could have prevented his shewing a great deal of literary merit with regard to physic. He has left in the hands of his family many valuable materials which a long experience and judicious observation had enabled him to accumulate; he had even begun some time ago to digest these, and has left behind him a considerable part of a work, somewhat after the manner of Dr Sydenham's *Processus integri in morbis fere omnibus curandis.*

It is proper here to give some account of this work, but I must beg leave to address it especially to the learned gentlemen of the profession. It is upon a plan, as I judge, more extensively useful than the empiric system of the great Sydenham could admit of. Dr Clerk has given the indications of cure upon a dogmatic plan. In this he has shewn a great deal of knowledge in the Theory of Physic, but, at the same time, he shews that he knew well the proper limits of that part of our system, for under the head of cure he very often prescribes remedies which have not always a manifest connection with the indications before laid down. Under the same head he has given what is not very common, though much to be desired in medical writings,—an enumeration of the medicines which had disappointed his expectations, as well as of those which had proved successful; and he is careful in ranking the several medicines recommended in diseases according to the degree of utility he had experienced from them. On this plan he proposed to treat of most of the diseases of the human body; and with respect to several of them, as the Rheumatism, the different species of Gout, Diarrhœa, Dysentery, Cholera, and some others, his treatise is pretty complete; but under some of the titles marked out, what he proposed to have

done is not quite finished; and as the whole seems to have been intended to communicate chiefly what he had learned from his own experience, no part of it could have been properly finished but with his life. It appears that about the time of his death he had been making additions to some parts of it. When those parts that are finished shall be given, as I hope they shall be, to the public, their excellence will certainly make the want of the whole be very deeply regretted by all who either know the value of such a work, or knew the peculiar fitness of Dr Clerk to execute it. Besides this, he has left behind him also another work,—some account of the progress of Physic, and of the several sects of physicians. It is full of curious erudition, but it does not appear to be quite finished.

With all his abilities, Dr Clerk was remarkably modest; and this quality, joined to a very correct judgment, rendered him, as it has done many others, very shy in aiming at literary glory; but he ought to enjoy it, for he has made several discoveries in his art, some of which have been published; and he was always willing to share with his Fellows of the College of Physicians some honours which he in a great measure might have assumed to himself. A physician may be judged of by his prescriptions, and a College of Physicians will always be judged by those prescriptions they publish under the title of Pharmacopœias. I should not, perhaps, before this promiscuous company, publish the errors, the weakness of our profession; but I must speak freely,—I must say, that not long ago, the pharmacopœias of the several Colleges of Europe were a scandal to physic; contained many things even shocking to common sense; many of them do so still; and a very few of them only have received any considerable improvement. I hope it will always be acknowledged, always remembered to the honour of the College of Physicians of Edinburgh, that they were among the first in attempting and executing this important reformation; they were the first in aiming at that desirable simplicity, that exquisite choice and judgment, and that pharmaceutical accuracy, which now distinguish the Pharmacopœias of Great Britain. That of Edinburgh has been for some time in request over Europe. Within these thirty-five

years past the College has given four different Editions of their work, and always with improvements and additions, as further time allowed better of the conquering of old prejudices, or furnished new advances in practice. I would not willingly defraud any member of the College of their share in the honour of this work. Many of them have a title to it; but all of them will allow me to say, that as the chief labour of that work was always devolved upon Dr Clerk, so the chief merit of it always belonged to him. So far indeed as I can trace the progress of this affair, it appears to me evident that Dr Clerk was the person who chiefly introduced into this country, judgment, accuracy, and elegance in private prescription, and this very justly promoted him to the chief direction of the public labours of the same kind.

It must further be observed here, that amidst the hurry of practice, and the frequent interruptions of a valetudinary state, Dr Clerk persisted resolutely in those labours which he judged might contribute to the improvement of Physic, or to the honour of the College of Physicians. It was proper that the Pharmacopœia for the use of the Royal Infirmary, published some years ago, should be accommodated to the Public Pharmacopœia, published last year, and receive also such farther corrections as time and experience had pointed out. In doing this, Dr Clerk spent the intervals that this last winter and spring allowed him; and very lately he did me the honour to shew me a copy of that Pharmacopœia, with all the alterations and corrections which he proposed it should receive. It is now in the hands of a son who inherits his father's virtue and master's skill, and will, I hope, succeed also to the proper reward of both. When this son, by the advice of the College, shall give that work to the public, it will abundantly show, that the father preserved to the last his attention to the improvement of physic, and his abilities to contribute largely towards it.

Such were the literary and professional merits of Dr Clerk. I shall now add to the account of these some instances of his generous desire to promote the study of physic in others. This kingdom and this city for these thirty years past have received some honour from the labours of the gentlemen employed in

teaching the several parts of medicine in the University of Edinburgh. By the recess of one gentleman and the death of another (Drs St Clair and Plummer), that society indeed has lately suffered a considerable loss; but the other gentlemen who have contributed very much to obtain that honour, still continue, and shall I hope long remain, to support it. In the individual merit of these labours Dr Clerk could have no share; but he had the merit of favouring their beginnings, and to his utmost he endeavoured to encourage their progress. The labours of some of these gentlemen had been undertaken gratuitously from a zeal to promote physic; Dr Clerk endeavoured to procure them a public encouragement. There was a salary which had formerly been annexed to a private office, as a candidate for which Dr Clerk would have been, in point of favour and merit, without a rival; but he himself solicited that that salary might be applied to the encouragement of the teaching of Medicine in the University of Edinburgh. For this purpose he made application to a Noble Lord who has always been the zealous patron of all the arts of his country, and who needs only to find the means by which he may promote them. Accordingly, as soon as the vacancy happened which could give the proper opportunity, that Noble Lord obtained the application of the salary to be made as Dr Clerk had proposed; and when a nobleman, zealous to serve the Doctor, but without his participation, applied on that occasion to His Grace the Duke of Argyll, desiring that the salary then vacant might be conferred on Dr Clerk; the Duke was pleased to tell him he had prevented his solicitation, and had granted the salary to the Doctor. His Grace had indeed really done so. He had applied it at the Doctor's desire, and, though not to his private benefit, yet to a purpose he valued as much—the encouragement of physic.

This was one instance of Dr Clerk's regard to physic, and I can now mention another, which was accompanied with a very humane and generous purpose, viz. the great share he had in the establishment of the house we are now in (the Royal Infirmary), and its application to the excellent purposes it now serves,—the relief of the sick poor, and the promoting the study

of physic. That donation which has entitled him to have his name enrolled here among the generous benefactors to this establishment, even with the addition lately made to it, is still a very inconsiderable part of what it owes to him. His more important generosity was his promoting the first attempt, his warmly soliciting contributions to carry it on, his care and fidelity in directing the application of them, and his great assiduity and industry in conducting a public work, which, though all approve of, there are few who will bestow upon it the necessary time and pains it requires. There were indeed others that shared with him in these labours, and some who had such a share in them, as must always do them great honour; but I have had the particular testimony of the worthy and public-spirited gentleman (Provost Drummond), whom all acknowledge to have had the chief hand in this business, that he had on every occasion consulted with Dr Clerk, that he had been often assisted by his advice, that he had been always obliged to his assiduity, and indeed constantly supported by his joint labours in the conduct of this noble establishment. Certainly nothing could more strongly evince the desire of this eminent physician to promote the study of his art,—nothing could better show his generous inclinations to render the effects of it as extensively useful as possible.

These particulars of Dr Clerk's merit with regard to his profession must do him honour; but he deserves still more for his general character, which shewed an uniform disposition to extend the knowledge and promote the effects of physic. He was always communicative of his knowledge and experience to his brethren of the profession; and he was always ready to give unbought advice to every patient that applied to him for that purpose. I have myself, and all his acquaintances have, had many instances of his generosity in this respect. There is one entire very respectable body of men (the Clergy), to whom he often gave advice, but from whom he never would receive any fee.

I have hitherto endeavoured to shew that great respect is due to the memory of Dr Clerk as an able physician; but I own it might take much from that respect if I could not also shew that it is due to him as a very worthy man. This last in-

deed, I hope, will always be in some measure inseparable from the character of a great physician; and many of the particulars I have already mentioned prove that these characters were very strictly united in Dr Clerk, and every part of his private life served to confirm the same truth. He was a sincere friend, and he had almost as many to cultivate his friendship as to consult his skill. His relations were pleased with the honour that his reputation reflected upon them; but they loved him no less for virtues that they more especially had occasion to find in him. He was married to a woman in whom he found as much virtue as others might discern of beauty: good sense and gentle manners composed a happy family. The virtuous and worthy offspring they have left behind them are the surest evidence of the example and conduct of the parents. Our eminent physician abounded indeed in useful, and he was no less remarkable for his agreeable, qualities. His conversation was lively, full of wit and humour; it was cheerful even to mirth, but conducted with so much good sense as to be always curious and instructive. In his manner, a very rigid honesty would not indulge him in any general or fulsome complaisance, but he never offended by moroseness or ill manners. To sum up his character, I could produce the most ample and satisfactory proofs of his fine parts and great probity. I could produce, were it proper here, the long list of his more particular friends: some of these, in spite of skill and friendship, died before him; many of them still live to regret him;—all of them an honour to their age and country, and the surest pledges of the value of the person they held as a friend.

I have now gone through most of the particulars necessary to be mentioned with regard to this eminent person. There is one, however, still remaining, which cannot be omitted in an account of a physician, that is, the conduct of his own health. In the business of a physician there is nowhere an occasion for a more nice exercise of skill, than in that most valuable branch of the profession, the art of avoiding diseases. Dr Clerk was originally of a delicate habit of body, and very early threatened with several diseases. Though subsequent diseases had made violent attacks upon him, and even threatened imme-

diate danger, he had the art to protract his life for many years longer to a good old age. This required a constant and skillful attention, and was conducted with singular judgment: his regimen might indeed sometimes encroach upon his own pleasures, but it hardly ever interfered with the duties of life. Nothing less than his own exquisite skill could have managed this; and even this could not have done it, if his virtue at the same time had not enabled him to moderate every gratification, and even to decline those joys to which he was drawn by a strong motive, the pleasure he might enjoy in the company of men of wit and humour, who courted his talents of the same kind. By such virtues, and the constant exercise of a nice skill, his life was long preserved; but the fatal stroke at last came, though even it perhaps would not have proved fatal, if it had not been so remarkably sudden, and made its chief attack, too, upon his intellectual faculties. He died of half an hour's illness. The first feelings of it did not alarm him so much as to make him close the book he had been reading. It was Horace, and till after his death it lay open, where there is this passage:

> 'Sit mihi, quod nunc est, etiam minus: et mihi vivam
> Quod superest ævi, si quid superesse volunt Di:
> Sit bona librorum et provisæ frugis in annum
> Copia; neu fluitem dubiæ spe pendulus horæ.
> Sed satis est orare Jovem quæ donat et aufert,
> Det vitam, det opes; æquum mi animum ipse parabo.'
>
> (Epistol. Lib. i. Epist. 18.)

Death indeed came upon him unawares, but it did not come upon him unprepared. He had long foreseen, and often foretold, that his death would be in such a manner; and he had long been resigned to the will of Heaven, with all the firmness that conscious virtue can inspire, and with all the cheerfulness that a freedom from superstition can alone give. He had well nigh filled up the measure of life; and he parted from it, as he wished, without having ever ceased from the duties of it. Now he is gone, he is regretted by every lover of worth and knowledge; and all who are of the opinion of the Son of Sirach, that 'health and a good state of body are above all gold,' will honour our eminent physician with the honour that is due unto him, for the uses we have had of him, for the Lord had created him."

Dr David Clerk to Dr Cullen.

"Dear Cullen, Edinburgh, *March* 28. 1749.

I am not sure if, when I wrote you, I mentioned that we had got here a book that may be of service to Mr Robert Simson *, if he has it not already, which may perhaps not be the case as it is very rare. It is the only edition of the Second Book of Pappus, published along with Aristarchus by Wallis at Oxford, in 8vo.

I had a letter from Gasthart, with an account of some experiments on the Bath waters. They seemed to be far from being complete; yet, as I could not say any thing that would even satisfy myself, either upon that part of the scheme, or upon the whole in general, I did not choose to meddle with it, but set him upon you to advise him. Though the analysis of the waters be the least useful part of the scheme, yet, as it is curious, it is certainly worth the prosecuting as far as it will go. I would be glad you would write me an account of the scheme you would put him upon, both with respect to the different heads you would have him consider in writing on that subject, and what experiments you would have him make, in order to discover the constituent principles of the water.

The best thing I have seen with respect to the investigating the principles of bodies, by the simple mixing of other things with them, is the Preface of Tournefort's Treatise 'Sur les Plantes qui naissent aux environs de Paris,' though it, too, is not complete. I had a bottle of water sent me from Moffat, of a Steel spring just now found out there. It appeared by the taste and the mixing of galls with it, to be stronger of the steel than even the German Spa; but it tasted, I thought, pretty much of alum at the same time. It reddened the Succus Heliotropii, though that does not indicate a perfect acid, as that both vitriol and alum will do too. If I go to Moffat this summer, I may possibly make some few experiments, for my amusement, upon that and the other waters; so, if any thing occurs to you on that subject be so good as to communicate it to me.

* The celebrated Professor of Mathematics in the University of Glasgow.

Though I had sworn against chemistry, I am going to undertake a very long and tedious process just now. It is said to be the Lisbon preparation of mercury that cures the Lues Venerea without salivating. If it were not too long I would transcribe it for you. I am resolved to go through the whole process punctually, and give it a fair trial. It is a receipt Mr Rattray brought with him from Lisbon.

Though I write you, I can scarce expect an answer from you while you continue under the oppression of your colleges; but, when you begin to breathe again and have leisure, a line from you will be extremely agreeable. Remember me to all friends with you, and believe me to be ever sincerely yours,

DAV. CLERK."

Dr DAVID CLERK to Dr CULLEN.

" DEAR CULLEN, EDINBURGH, *Nov.* 19. 1751.

* * * *

I received the Sal sedativum, for which I thank you. I suppose it is the first ever was in Scotland. I propose trying it in the Infirmary to-morrow, if the patient is no better, in a case of Mania supervening on Epilepsy, for which the patient is getting a gentle course of mercury. He used formerly to have slight fits of this mania, or delirium, and insensibility; but this one, whether owing to the mercury or not, has continued for four or five days, notwithstanding physic and blisters. One would not expect a great deal from the Sal; but, at the same time, as it contains an acid we know so little about, there is no answering for its effects. The other part of its composition should be nothing very active or powerful. The Oleum animale I won't trouble you about. Hutton* will prepare me some of it, if I should have occasion for it. He is so enthusiastical that I cannot get him to engage in any thing useful.

* * * *

The post makes me conclude. Adieu,

DAV. CLERK."

* Dr James Hutton, the celebrated author of the 'Theory of the Earth,' &c.

Note C, page 14.

Dr Hunter to Dr Cullen.

"My Dear Old Friend, London, 20*th Sept.* 1748.

I have had a delightful ride this summer, through Holland and Flanders, on my way to Paris. My business was to visit Albinus, and in general all eminent medical people, to pick up some old scarce books, and particularly to get some German books for Dr Cullen. The list I had from Professor Moor *, and got all of them but three, viz. 'Kunckel, Philosophia Chemica,' 'Reaumur, l'Art de convertir le Fer forgé en Acier,' and 'Stahl de Elogiis Vitrioli,' which I shall endeavour to find here at medical sales, though the last probably cannot be met with on this side of Hamburgh. Two of those I got are here, and shall be sent by the first opportunity; the rest were shipped from Rotterdam, directed to the care of Mr Wilson, Edinburgh, to whom, by this post, I have sent the bill of lading. I must come down next summer and see you, for I want to chat about fifty things, and hate to write of them. I condole sincerely with you; but that is in vain, and serves only to renew grief. Pray remember me kindly to Mrs Cullen, Professor Hamilton, Mr Carrick, &c., and to our old good friends (who, I hope, have not suffered by the fire) at Hamilton. Dear Doctor, I am always yours,

William Hunter."

Dr Cullen to Dr Hunter.

"My dear Willie, Glasgow, 29*th May* 1749.

It is with pain I write to you, but it is to save your mother more pain in doing it herself. Your sister Mrs Buchanan died on Friday night last. It was after being valetudinary for some years; and from your mother's last letter to you, I suppose you know that this event was to be expected soon.

* Professor of Greek in the University of Glasgow.

I have nothing to say upon this subject but what would be superfluous, perhaps impertinent, to you. Your mother and Dorothy *, bating what they feel on this occasion, are otherwise well. I hope you will be very careful of your own health, that you may long be—what you will be while you live—a great comfort to them and to all your friends. I am, from the bottom of my heart, my dear Willie, yours,

WILLIAM CULLEN."

Dr CULLEN to Dr HUNTER.

" DEAR WILLIE, GLASGOW, 12*th July* 1751.

I have hardly time to tell you that I have been so busy of late that I had no time to write you, but I found time to be often with your mother. Her ailments continue very obstinate * * * I have endeavoured to engage her in some exercise. She has tried going on horseback, and going in a cart and in a car, but every kind of motion raises her pain so much, that we cannot persuade her to repeat the trials. Upon the whole, I am persuaded some scirrhosity is forming in the stomach, which gives me a very disagreeable prospect with regard to her. She says nothing now about Johnie's coming down; but I know, in her present temper, it would have pleased her much if he had. I shall from time to time endeavour to let you know how she goes on. Dear Willie, yours,

WILLIAM CULLEN."

" P. S. I see a book of Dr Battie's advertised †; I wish you could send it me by some of our Glasgow merchants now at London, or, as I guess at the size of it, in two franks."

Dr HUNTER to Dr CULLEN.

" DEAR SIR, LONDON, *August* 1. 1751.

I am particularly obliged to you for the care you have taken of my poor mother, and must beg of you to continue it.

* Afterwards Mrs Baillie.

† De Principiis Animalibus.

The trouble you give yourself of informing me so particularly, not only of her health, but of her inclinations, is likewise very friendly, and I thank you for it. But I cannot consent this season to her request*, for my brother's sake, for my own sake, and even for my mother's sake. It would be every way a bad scheme. I have wrote of it to her, and I hope she will consider better of it, and find that it is really a whim begot of sickness and low spirits. I have sent Battie's *Principia* to Johnson, in the city, who will send it to you by the first hand. I wish you would come up to London for a week or two. I have as much to say to you as would take up that time. I want to tell you many things about colleges, hospitals, professorships, chariots, wives, &c. &c.; and cannot write of these things. I'm busy in forming a plan for being an author. In short, my head is full of——a thousand things. * * * I received first the English and then the Latin proposals for Plato; and surely Messrs Foulis must think me an odd fellow. I have not wrote them one word. But I have been puffing for them, and I have picked up some hints for them, which I reserved till I should give them the names of two or three subscribers. Dr Duncan has kept every thing from me for more than a month, under pretence of shewing the proposals to some people of quality, who, I suppose, will do very little after all. My compliments to them all, and to all friends.

Pray take an airing now and then on Kilbryde Hills. I am, dear Sir, sincerely yours,

WILLIAM HUNTER."

Dr CULLEN to Dr HUNTER.

"MY DEAR WILLIE, GLASGOW, *9th October* 1751.

I have intended to write you every post this fortnight past, but some accident has always prevented me. I regret my neglect chiefly on this account, that it may have put off your writing to your mother, which she complains of. Her ailments have continued much in one way for some weeks past, only she

* That Mr John Hunter should be sent down to Scotland.

has by degrees grown weaker and weaker, but by such slow degrees, that it was not easy to say how soon her ailments would come to a period. I have been with her this day, and find her weakness so very remarkable, that I cannot depend now on her subsisting for any length of time. If you have not written to her or Dorothy before this comes to your hand, I would have you delay it no longer. It gives some relief to Dorothy, that now your mother seems to be declining fast, it is with very little of those pains which formerly afflicted her. I visit her often, and shall continue to do so; and, upon Dorothy's account, I shall take care to be present in your mother's last moments. As these are not far off, if you have any directions to give with regard to her funeral, they shall be punctually observed. If your directions come too late, I shall take upon me to order these matters in the manner I think will be most agreeable to you, and withal, my chief care shall be bestowed on Dorothy. As you have no objection to my plan with regard to her, I shall do as I proposed, that is, as soon as your mother is interred, take Dorothy to my house, where I am sure she will find a very hearty welcome. I am, my dear friend, yours,

WILLIAM CULLEN."

Dr HUNTER to Dr CULLEN.

" DEAR SIR, LONDON, *Nov.* 14*th*, 1751.

You cannot imagine what satisfaction you have given me, by your kind attendance upon my poor mother during such a long sickness. I was very unhappy when I thought of the pleasure it would have been to her could I have been with her, or could I have let my brother be with her; but as that was impossible, the only comfort I could procure myself upon such a melancholy occasion, was recommending her to my friends, and particularly to you. I take the first opportunity of thanking you from my heart.

The past is unavoidable. Our next care must be about my sister; who has recommended herself strongly to me by her indefatigable tenderness. Poor girl, I pity her from my soul. Your offer with regard to her is generous and friendly. I leave

her to be determined by you and her friends. In the mean time, I would propose that she should stay in Scotland till the spring, both for the sake of good weather, and that my great hurry may be over, and that we may have some time to consider how our little affairs are to be managed in Scotland after she leaves the place. Then I should propose that she come up to me, or perhaps I may then send my brother to bring her.

I had a very kind letter the other day from Mr Moor, and I have not time to answer it. I beg you will do it for me, by letting him know how sensible I am of his friendship. I have been up all night, and hurried all day; so that I am half dead, and, as you may suppose, somewhat low in spirits. My compliments to Mrs Cullen, Miss Johnson, your boys, and all friends. I am, dear Sir, affectionately yours,

WILLIAM HUNTER."

Dr HUNTER to Dr CULLEN.

" DEAR SIR, LONDON, *February* 22. 1752.

You see I have taken some time to answer your letter, but be assured I lost very little time in executing your commission, so far as depended upon myself. I employed, what do you call him?—the King's glassman, in the Strand. He told me that he could get them done for me, but said they would at least take six weeks, that it was only upon particular occasions they could blow such bottles, that they must be in white glass, and that they would be pretty expensive. I wrote in his book your directions about them, and left with him your own draughts of their figures. As soon as they are made, I will write to you, and get your instructions about sending them down.

I shall transmit Dr Battie's book with Captain Johnson, or the first hand. Your advice about the houses I intend to follow; and will beg the favour of Captain Johnson, when he goes down, to assist me in that and such affairs. I thank you for your kind attention to my sister. I would have her be preparing to set out by April or May.

In two or three weeks I shall shew one plate finished, as a

specimen of the figures of the Gravid Uterus. As a piece of painting, I believe it will be found the finest anatomical figure that ever was done. So it may; it will cost me a devilish deal of money. I may be allowed to speak well of the drawing and graving, you know; no merit thence in me. Physic is in a strange ferment here. The practitioners in midwifery have been virulently attacked, but by a madman; and in that scuffle I have had a blow too, obliquely;—the reason is, we get money, our antagonists none. May the dispute, therefore, long continue. * * * I am, dear Sir, yours always,

WILLIAM HUNTER."

Dr HUNTER to Dr CULLEN.

" DEAR SIR, LONDON, 26*th May* 1753.

For three months past, I have had too substantial reasons for not writing to you. I have been confined to my room, or country lodging, with a strange sort of eruptive fever, and am still so weak that walking across the room tires me. I propose going out to-morrow; indeed, I went abroad above a month ago, by which I had a relapse. Just in the beginning of my illness I had written the greatest part of a long letter to you, when the fever came so full upon me that I could not finish it. It contained apologies for not having sooner thanked you for your very friendly eare of my sister; and, I assure you, whatever you might think from my unaccountably neglectful behaviour, that I was uneasy in my mind to think that I might possibly die without ever having once thanked you; but then I comforted myself that I should leave behind me the half finished letter, which would be some little mitigation. If a longer life is destined me, which I have now no reason to despair of, I shall be glad in every part of it to have any opportunity of doing you a pleasure. * * * My sister sees Mrs Almack often. She is the same good girl as ever. I hope she will be very happy.

When I had your commission about the chemical glasses, I employed Mr Maydwell in the Strand (the most reputed), and

explained every thing to him from your letter and figures. He said he should be obliged to make some alterations in some of them, both because it was hardly possible to make them as you desired, and that the working chemists have found the more simple answer best. If you still would have them in that way, or whatever you want that I can procure, pray let me know. If they were intended for yourself, they are yours; if commissioned by the College, they are theirs with my respects. However it stands, Maydwell and I are both paid.

Pray let me be excused to friends with you for not writing, and by yourself for giving you the trouble of the enclosed.

My sister and brother desire their compliments with mine to your family and self.

WILLIAM HUNTER."

Dr HUNTER to Dr CULLEN.

" DEAR DOCTOR, LONDON, 3*d August* 1754.

You know how natural it is for us all to imitate those we have a good opinion of. At present I am going to tell you something I should be ashamed to mention, if it was not perfectly in your own style. Some months ago I had a long chat with Dr Stewart on the subject of your situation at Glasgow, and the scheme proposed by your friends of your going to Edinburgh. I knew he had a friendship for you, though not personally acquainted. He promised that he would take the first opportunity of letting the Duke of Argyll know it, and of urging him to exert himself in serving you, whenever he might have an opportunity. He did so. The Duke expressed a great regard for you, said he was heartily sorry to find that your encouragement at Glasgow was so narrow; that he should always be against any body's having leave to sell a professorship, but that, in your particular case, that indulgence should be granted, &c. &c., *i. e.* he declared himself to be very much your friend. Now, I know that you will swear at me for intermeddling at all, or for not letting you know this sooner, as, I suppose, you have seen the Duke. All I can say is, *better late than never*, and *valeat*

quantum it now may. If you have any application, you have now an opportunity I suppose, and if you should need that at any time in London, it may be done through Dr Stewart.

There is a Danish physician now making the tour of Scotland. His name is Chriström (I cannot swear to the spelling), a most worthy man. He was to have carried letters for Glasgow, but was disappointed. If you meet him in your walks or rides pray shew him some of the civilities that such people require, and you will very much oblige some of us here.

What say you to Mead's auction of books? I am afraid I shall ruin myself in the winter with it.

Some doctors here have attempted a Medical Society for collecting and publishing medical things, and they have made me a member. I shall give a paper in their first volume, if I can get time. If any of your friends—I was going to say they might send us what they had to publish, but I had forgotten the Edinburgh Society, which, I presume, engrosses you all.

Some very late occasions have increased the number of plates of the Gravid Uterus; but though that will retard the publication, the work will be much more complete for them. God bless you! is always the prayer of your sincere friend,

WILLIAM HUNTER."

Dr HUNTER to Dr CULLEN.

"DEAR DOCTOR, *6th August* 1754.

I very lately had the pleasure of seeing Dr Dick *, and thereby of hearing of you and some other friends. I wrote to you by the very last post, which indeed I ought to have done some months before that. I know you'll say, *another letter from Hunter! what the devil is he about now?* Why, I am going to buy *land:* the first time this has ever entered into my head. In short, Mr Fleming of Nook, in Kilbryde, writes me that Mr Borland is going forthwith to sell his land in Longcalderwood. It lies contiguous, you know, to what was my father's, and the

* Professor of Natural Philosophy in the University of Glasgow.

two together make a vote in the county. If I could get it, both would be as easily managed as one, and should it afterwards be expedient, both might be sold with the same trouble as one. Besides, I cannot get it out of my head that I shall one time or other live there, and, in that case, I should like to possess both, that I might have a little bit all round me that I could call my own, and to give you a vote, or any of your friends that should stand for the county member. I have accordingly written to Mr Fleming to buy that for me; and have told him that you and Mr Hamilton will do me the favour to procure him some Law-Doctor to assist him. I have written Mr Hamilton to the same purpose, and mentioned to him Messrs Barclay, Wilson, or Cunison, whichever you think fittest. Now, I must beg the favour that you would meet Mr Hamilton; he will shew you both my letter to him and to Mr Fleming; then you will be master of the subject; and, I make no doubt, will immediately take the proper steps in this affair. You see I am desirous of having this thing, but to no high degree of folly.

I believe in my last I forgot to mention your sister Mrs Almack. We see her often. She is well, and, I hope, very likely to do well.

Pray do not serve me now as you did when I was in Edinburgh, and put this letter in your greatcoat-pocket unopened till we meet. However, I must tell you, though I mean not to make you negligent, that if you do so, I could not *even then* be angry with you, so much am I perfectly yours,

WILLIAM HUNTER."

Dr HUNTER to Dr CULLEN.

" DEAR DOCTOR, JERMYN STREET, 17*th May* 1757.

I have nothing to say to you even after so long a silence, but that I love you, and God knows that I speak truth. I hate writing, and you will be so good as to excuse me. Mr Barclay has made me very happy in letting me know your situation and success. I make no doubt but that you will have more reason every day for being satisfied with the change you have made;

and I assure you, however negligent I may have appeared, I was very assiduous with Dr Stewart; and the good old man was so hearty and strenuous in your cause with the Duke of Argyll, that I have often wished you could take some method of thanking him. At that time I could no more go into the city to wait upon your Edinburgh Provost than go to Edinburgh itself. * * * Believe me to be always, dear Doctor, affectionately yours, WILLIAM HUNTER."

Dr HUNTER to Dr CULLEN.

"DEAR SIR, JERMYN STREET, 29*th July* 1758.

I was so confoundedly busied and confined last post-day, that, if it would have saved your life, I could not have written two lines; and if I had been able, it would have come too late for your meeting the first of the month. I have indeed but very little time at present, and therefore will only thank you once for a thousand times for the kind exordium to your last. I am persuaded you will find me as constant in my friendship, as some others will find me implacable in resentment and determined in contempt. I am always the same, yet just what the object makes me; good or bad as I am treated, and so I think every man should be.

In the college list here, in voting, &c. the members stand in seniority according to the time of their admission into the college. No other regard being paid to the date of their doctor's degree than that, if two present themselves and are admitted at the same time, he who is the oldest doctor (which appears by his degree) has precedency of the other. This you may depend upon.

I would by no means wish you to have any shyness with your brother professors, nor indeed with any body else upon my account. If I had opportunity I should be glad to procure you friends, or any other advantage, and accordingly never let any occasion of doing you justice slip me.—Pitcairn desires me to remember his best respects to you. He is, I think, most affectionately your friend. Armstrong, too, remembers you always

kindly, and I am sure he esteems you. I have seen them both since I had your letter. Smollett I know not what to say of. He has great virtues, and has a turn for the warmest friendships. I have seen him very little for some years. He is easily hurt, and is very ready to take prejudices. There had been a great shyness between him and me, which his very kind behaviour to me when I was attacked by Douglass, Pott, and Monro, has as yet scarcely conquered, so that I cannot well say how you stand with him; but if I can make you friends I will most certainly.

I meet with so many *drowsy heads* that I cannot guess which of them you mean. Your brother Robert is come to London, and will see you in a little time. I will write to you of your book at leisure. I am always yours,

WILLIAM HUNTER."

Dr HUNTER to Dr CULLEN.

" DEAR SIR, JERMYN STREET, *9th October* 1759.

I know from experience how little you must have it in your power to give up your time to private friendships and amusements; and, therefore, whenever I can, I avoid giving letters of recommendation. In the present case, I must beg the favour of you to believe that I am interested. This gentleman, Dr Wren of Oxford, is my particular friend, and lived a considerable time in the house with my brother. He is very fond of chemistry, but that is not his principal reason for wishing to be of your acquaintance. If he returns with the same encomiums of you and of Scotland as Mr Charles Townshend, I shall be very glad, and believe that you are a set of something supernatural.

There is another gentleman now at Edinburgh that I was to have introduced to you. I am sorry I had not the opportunity. Will you then do me the favour, when you have a little time, to give my compliments to Mr Syscombe, and tell him that I think myself much obliged by his letter.—Your sister is extremely well, and is now my near neighbour.

I beg leave to present my respects to Mrs Cullen, and my love to all your children. I expect no accounts of yourself from

yourself, but as soon as Lady Tweeddale comes up I shall have it all. I am, dear Doctor, always your faithful friend, &c.

WILLIAM HUNTER."

Dr HUNTER to Dr CULLEN.

" DEAR SIR, JERMYN STREET, 10*th February* 1761.

Your friend Mr Wilison came to London at a very proper time. He has passed his examination at the Hall, and goes an hospital mate (five shillings a-day) in the grand expedition. My brother goes first surgeon, and I am sure will be glad of any opportunity of shewing his regard for you. Where they are going we cannot learn.

I give you joy of Lord Bute's *immense* acquisition (about L.46,000 per annum), it is great joy to your good friend Dr Stewart. He has now, besides his Prince's favour beyond any man, a fortune superior to most; and by what is going on, I should hope that he would in time be a favourite with most of the King's subjects. He cannot be so with all of them. It is impossible.

The *Cicuta* has done nothing in London, yet the accounts from Vienna, particularly from Van Swieten, rise higher every day. It is suspected that our extract was made too late in the season, or that the plant in this climate is not rich or powerful, but my own suspicions are quite of another kind. We shall in time know how it is, but I am afraid we shall have no cure for a Cancer. Dr Fordyce reads here, and professes to take his plan and most of his matter from you, and I find it does you credit.

Our second volume of Essays will come out about May. The half is now printed. We are very desirous of giving the public the present state of inoculation; and I am desired by the Society to beg the favour of your answer to the following queries. It will take but a very little time. Do pray write to me in two or three weeks, and with as little ceremony as you please, only give me the facts.

Query 1. Whether inoculation continues to be practised in

Edinburgh and the neighbouring towns with the same success as in London, and if the practice gains ground daily or not?

2. Whether the fear of spreading contagion has not prevented inoculation becoming so common as it would have been, especially in market towns?

3. Whether any cases have been certainly observed of taking the true smallpox a second time, either after the natural or inoculated smallpox?

4. Whether the success of the inoculation at the breast has been in general equal to that at three or four years of age? in what proportion have the numbers been? at what month of their age generally have the little creatures been inoculated? and do you know any objection to this practice from good authority, I mean *observation?*

Your Sister is pretty well, though she has had a cold. Mr Almack and the children are quite well. They are all in good spirits; and you know I am always, dear Sir, your faithful

WILLIAM HUNTER.

P. S.—Who is to be your Botany Professor? I hear there is one from Paris much recommended, but I cannot learn his name or country. For God's sake take the best, if he should come from the moon."

Dr HUNTER to Dr CULLEN.

" DEAR SIR, 12*th August* 1762.

I have just time to congratulate you on the Queen's happy delivery of a fine boy, the Duke of Cornwall, this morning at half an hour after seven o'clock. She was taken ill only at four. There was every circumstance that could give joy, which indeed was expressed in every face, and I believe felt in every heart at Court. She has had composing sleeps through the day, and is now as well as I could wish. She was attended by a midwife. I am very happy, and have been so for some time. I owe it to you, and thank you from my heart for the great honour I now have, and have had for some time, though very few know any thing of it,—I mean having the sole direction of

her Majesty's health as a child-bearing lady. You may suppose I say so much only from gratitude and friendship between ourselves. I am, dear Sir, your most faithful and most devoted

WILLIAM HUNTER."

Dr HUNTER to Dr CULLEN.

" DEAR SIR, 1763.

The enclosed was given to me by Mr Hawkins from Mons. Morand of Paris. I know not who is your secretary, and therefore must recommend the letter to your care. Pray do not put it into your great-coat pocket, as you once did when I wrote from Edinburgh to you at Hamilton.

I have a thousand questions to ask you about yourself, your family, and other subjects, and for that reason dare not, and know not, where to begin. I have a desire, too, of writing a thousand things to you of myself and your friends in this place, but especially (because it would be the most diverting of all) relating to the characters and awkward situations of those who are and wish to be in power.

I am busy at present about a thing of public utility, but it is too long for a letter ; and besides, I hate writing so much that I am several months in debt (I mean for a letter) to Mr Wilson, Dr Clerk, and Dr Whytt. Be so good in the mean time to present my respects and thanks to these gentlemen ; you see them often, and could easily make them believe, what is certainly most true, that I value their friendship. Dr Whytt was kind enough to write me a letter upon the *Commentaries,* though the subject distressed him ; for he was obliged to say, " *both honourable men,*" which, however it may affect me, is certainly an impossibility.

If my *old master* would answer a very short and civil letter which I sent him some time ago, under cover to Mr Kincaid, I would steal some half hours from my hurry for the sake of amusing him in his *calm retreat.* I think I could be a comfort to him in his old age, and I am much mistaken if he does not want a comforter. The paternal care and affection he has al-

ways shewn to me has, I think, bewitched me, for it makes me laugh always when I should cry.

WILLIAM HUNTER."

DR HUNTER to DR CULLEN.

"DEAR SIR, *26th February* 1765.

For these three months past it has so happened that I have been hurried beyond measure, else I should have written to you often. * * * *

I thank the young advocate *, and give him joy and all good wishes: but I cannot commend the composition, because I do not understand one word of it. I understood by Mr Wood, that I was to have a letter to present with it to Lord Greville, which I have still waited for; but lest there should have been some mistake, I told the case to Lord Warwick one morning soon after, and he invited me to come to breakfast with them upon the occasion, which I have never yet done. * * *

Since I began this letter, I have been informed by Mr Wood that poor Dr David Millar is dead. I went yesterday to his lodgings, at his relations, because I was shocked that I had not seen him. Pitcairn told me on Sunday, at Almack's, that he could not live, and I had intended every day to see him, but was always prevented. He did indeed appear to me to be an excellent young man. I grieve for the loss of him, and for what you and his mother will feel. How he judged of my behaviour to him, I know not. I could not give him much of my time; but I hope he believed that I wished him well, both for his own sake and for yours.

This evening after my lecture, Mr Stark † gave me your letter. I am always glad to know your friends or young men of genius and good dispositions. He and Mr Hewson (who adores you) and I had a long chat upon the affairs of Edinburgh and Glasgow.

* Dr Cullen's eldest son, Robert.

† Dr William Stark, author of Clinical and Anatomical Observations, &c. Lond. 1788.

This letter has lain by me several nights without my having time to finish it, and this night I am determined it shall go, though I have not time to read it over to see what I have said. Pray give my best respects to Mrs Cullen and all your family, and remember me to Mr Russel and all friends.

Mr Strange has brought me proofs of four plates of my Gravid Uterus, which have been engraved at Paris so well, that I believe I shall send some more there; five are now in hand in London. They will now make above thirty folio plates, and upon the whole I think a more complete work of the kind than could have been expected. I am, Dear Sir, most affectionately yours, W. H."

Dr Hunter to Dr Cullen.

1768.

"As I have no children to absorb my attention, my affection to the public utility, whatever tends to make mankind happier, whatever raises them from ignorance and barbarity, and teaches them to enjoy the higher pleasures of life, I think should be the great objects of men who have been blessed with education. Judge, then, what pleasure I have in hearing of the good works going on with you. The flourishing state of your University gives me a very sincere pleasure indeed; and the more, as the cause is so much in yourself. Tell me honestly if you are not much happier to be so instrumental in doing so much good, and with so deserved a reputation for it, than you could possibly be by any pecuniary income that attention to worldly affairs could bring in. Go on, and in the remaining part of the short life of man, enjoy that longer life of fame which death will not be able to take away. I heard lately at Mr Strange's, with infinite pleasure indeed, that your pupils had asked your picture. I am pretty much acquainted with most of our best artists here, and live in friendship with them. If I can be of any use in getting a single line of the engraving better done, command me.

"My own affairs go on well. I am, I believe, one of the happiest of all men, though my hurry is somewhat greater than I

could wish. At present I am sinking money so fast, that I am rather embarrassed. I am now collecting in the largest sense of the word, and I have already paid above L. 6000 for my habitation in Wind-mill Street, which will cost me at least two more. I shall go into it in June, and hope to print off my plates of the Gravid Uterus there, in the course of this summer. I shall have a printing press of my own. The engraving is now finished, only the letters of reference and the inscription at the bottom of the plates to be put in. They make thirty-four large plates. It will be a very considerable work for expense and show. Perhaps it will be the most considerable in that way that will ever be published, so few men can have the same opportunities or better than I have had. After that, if I live in health, I shall publish many other things. I only want time, and in less than two years I hope to be doing *only a little* business."

Dr Cullen to Dr Hunter.

"Edinburgh, 11*th February* 1769.
(The day of my relief, being the last day of my Clinical lectures.)

"My Dear Friend,

Your last letter, as all your others, was very kind, and puts me quite out of countenance. Your commission was delivered in due time to Mr Kincaid, and he has purchased a good many books for you. Some he has missed, because the University or College of Physicians' Library were resolved to have them at any price. Some of the rest I can supply; but I have not your list at hand, and cannot therefore be exact in saying which. I cannot myself afford to make any shew, and am only anxious to have every book in Physic that contains matter of fact. A collection of that kind I am intent upon, and can readily part with matters of curiosity. I take the opportunity of writing this by my friend Dr Blagden *, who has been several years here, and is now impatient to receive your instructions. I have had a particular intimacy with him, and he has been very much in my family. I therefore know him well to be a man of very

* Afterwards Sir Charles Blagden, and Secretary to the Royal Society.

great worth, which must always be the case of one I would recommend to you. Dr Blagden is, besides, of excellent parts, and of more knowledge than most of his standing. I dare say you will excuse me for asking your civilities to him, and I do it very earnestly.

I thank you kindly for recommending Mr Williams to me; he is a fine lad, and will do us credit; and I shall have pleasure in taking all the pains upon him I can. But if he were an angel I would not take so much pains upon him as I will do to shew how much I am in every thing, my dear friend, your affectionate and obedient servant,

WILLIAM CULLEN."

Dr HUNTER to Dr CULLEN.

" DEAR SIR, *7th December* 1774.

Four copies of the very first impressions of the Gravid Uterus will come to your hand about the time you will receive this, sealed up and directed by my own hand to you. I beg of you to honour me by putting one of them in your own library, and one, with my best respects, in the University library. The other two you will please to dispose of, as the enclosed directs, one for Dr Baillie, and one for the University library at Glasgow, which you will be so good as to deliver to them with my affection.

If the examination of this work were to give you a little pleasure, I think it would be a very affecting pleasure to you, who know that but for you, I should never, in all probability, have known any thing of the matter. If the King's *graciousness* and *goodness* to me had not superseded all other obligations, I had intended, in a dedication, to tell the world how kindly you had treated me in the earlier part of my life. In the mean time accept the continuation of my thanks. I am ever, dear Sir, with much affection and gratitude yours,

WILLIAM HUNTER."

Dr Cullen to Dr Hunter.

"My Dear Friend, Edinburgh, *5th February* 1776.

Your silence has appeared to me very long, and I have often thought of soliciting you to speak, but having myself nothing particular to say, my many occupations have constantly diverted me from it. Now you have begun, I cannot fail to cultivate the correspondence, and write you in course.

I am infinitely obliged to you and Dr Baker for his paper, which I shall certainly improve by, but have not yet had time to be in the least particular with regard to it; it is with regard to the other particular relating to him that I am impatient to write, as I would not have you think I could neglect for a moment any such hint from you or concerning him. You must know, then, that I do not know an instance of our College granting a gratuitous fellowship, except it is an honorary one. * *

[After explaining how ordinary fellowships are conferred by the College of Physicians of Edinburgh, Dr Cullen proceeds]:—

"Such is the state of matters with regard to ordinary fellowships, which I suspect will effectually prevent your friend from applying; and with respect to honorary fellowships, truly a post of honour with us, though it is not as in the London College, I am afraid the difficulty is as great. We have lately made some regulations in this matter, and particularly have resolved that the number shall be limited; that, of the men of rank, there shall not be above three, and of men of literature, not above ten. With respect to the last, it is a resolution that they shall be foreigners. It is observed that the admission of British might, from several causes, be liable to abuse; and not long ago, when it was proposed to move Sir John Pringle from the list of ordinaries to that of honoraries, he positively refused it, as he thought it might be a precedent for what he thought should be very rarely or never done.

Though I esteem Dr Baker, have an affection for one who associates so much with my particular friends, and think his name would do honour to our list, I am uncertain how the proposal would be received, and I could not urge a business against

which I have so lately declared my opinion. I have troubled you with a very long letter, but you must take it as a proof that I think a long apology necessary when I cannot do whatever you wish. However, I will not forget the business, if a manner of executing it more agreeable can be contrived. I shall conclude by mentioning a subject somewhat a-kin. I should not dislike being a fellow of the Royal Society, but have little anxiety about it, and therefore have never mentioned it to any person. Please give me your opinion how far there is any probability of success in case I should apply for it. Sir John Pringle and I were for a good while in a rather cold way, but for some years past I have paid him a great deal of attention, and I believe he is pleased with it. I would wish to write to him, or have him spoken to, before any motion was otherwise made in the affair. You may, however, consult with your brother John, who is of the Council. I give him joy on his late promotion, and assure you that nobody rejoices more sincerely than I do in every good thing that befalls him.

The whole of this letter is written in much hurry, but I expect to hear from you soon, and then, or perhaps sooner, I shall endeavour to supply all omissions. Believe me always to be affectionately yours,

WILLIAM CULLEN."

Dr HUNTER to Dr CULLEN.

" DEAR SIR, *9th September* 1776.

The bearer Mr Halhed will be able to deliver this into your hand as soon as the post; I therefore send it by him to beg your civilities in his favour. He was one of my very first pupils, and a pleasant, respectable one; bred in London,—has been long in Jamaica,—thinks now of spending three years with you, and of taking his degree,—and afterwards to do business or not as may be agreeable. He is a worthy man; I need say no more.

After you consulted me about the Royal Society, I was so much hurried by the addition of attending the Queen, that I delayed

writing to you from day to day till it was so late, that I could not have had your answer before the Society's vacation; else I should have told you that Pringle, Pitcairn, and Baker, told me (what I very well knew) that there could not be a doubt of your being chosen if you were put up. Tell me in your next letter that you still wish to be of the Society, and I will put you up at our first meeting in November, when, by the rules, you must hang up some time, and then be put to the ballot. A few of *us* must have the honour of putting our names to your *recommendation*, but the greater number shall be *Englishmen*. Be assured that your character will make the election honourable.

You have done me a pleasure and honour in submitting the enclosed pages* to my opinion. I have read them with care, and shall speak with freedom. I cannot so well judge of this part without seeing also that upon Phthisis. I am sensible of no material objection to your doctrine, and through the whole your meaning is clearly expressed. Indeed I have not yet made up my mind about many of the phenomena, particularly about inflammatory crusts, exsudation, pus, expectoration, &c., and, therefore, dare not in some of those questions decide with you or against you.

To convince you, however, that I do not want inclination, I will propose a few alterations in the words only, which otherwise hardly deserved attention. * * * * *

Farewell. I pity the whole Almack family from my heart. I ever shall be, dear Sir, most affectionately yours,

WILLIAM HUNTER.

P. S.—Pray, if you should have as much time send me Hume's last days †."

DR HUNTER to DR CULLEN.

"DEAR SIR, *7th February* 1777.

I have been as much hurried as any good Christian needs to be in order to prove his patience. Many little affairs of my own

* Of Dr Cullen's First Lines of the Practice of Physic.

† See Appendix, Note N.

have been neglected, but yours were not, nor ever shall. I was with your bookseller before your books arrived. He sent them round as you had directed. I had prepared Sir J. Pringle, Sir G. Baker, and Dr Pitcairn. Sir John desired me to send a *bound* copy to the Royal Society, which he presented. I should have carried one in your name to Dr Heberden, whom we all esteem, but the occasion was lost before I certainly knew that there were none to be had on the best paper. It was of no consequence. I should have given my own copy either to him or to the Royal Society, but I had the very first night I was possessed of it made some marginal notes for myself in the first pages. I had charged myself with taking the trouble from Sir J. Pringle and Sir G. Baker of writing a letter of thanks to you, because they seemed to think it an unpleasant piece of formality, so indeed I thought, but Sir John after all thought it better to write himself. It is a rule with him indeed, and he recommends it to us, to write to an author before he has read the book, which makes it much easier.

Most of us are so much employed at this time that we have not yet got through much of your book, and it requires being studied as we read, so that it takes time. You know what must be said of it,—clear, well expressed, with a great deal of ingenuity, and a great deal of good practice. What recommends it most to your students will be what most people here think the worst part. In my fit of the gout I was much entertained with my own character. I was going to refer to two sections, that you might see how I must have been diverted, but the hour strikes for the post. I will write you hereafter, and give you any remarks I can from your friends for a second edition. God bless you.

WILLIAM HUNTER.

Your recommendation is hung up at the Royal Society with a respectable list of names. There is no doubt of your being chosen."

Dr Cullen to Dr Hunter.

" My Dear Friend,

It will give me the utmost pain to find upon any occasion that it is not in my power to do you any service you may desire of me. Happily this is not the case at present. About the beginning of this winter, Dr Monro junior laid before the Philosophical Society a great number of anatomical preparations and drawings. I was present at the beginning of the meeting, but finding it impossible for me, during the short time of our meetings, and amidst a number of other persons, to take any satisfactory view of these preparations, I left the meeting before he began his demonstration; and when I received your letter yesterday I was nowise in a condition to give it an answer. But I found no difficulty in applying to Dr Monro himself, and, after talking upon the subject, I even let him know the reason of my inquiry. The result of all is this: Among other preparations, he produced to the Society a preparation of a gravid uterus, and a drawing from it. The purpose of both was to determine the nature of the communication between the uterus and placenta. He says that, when he heard you upon that subject, you were of opinion that certain arteries of the uterus pass the external surface of the placenta, and pour out their blood into the cellular substance of it, to be taken up from thence by the proper vessels of the placenta. He says he was then, and has always been, of a different opinion; but, to determine the matter more certainly, he made this preparation. He had injected the vessels of the uterus with coloured wax, the arteries with red, and veins with green, and left those of the placenta without any injection. This injected uterus he cut longitudinally, so as to obtain a section and distinct view, for some length, of the uterus and placenta as applied to each other; and from this view, he thinks, it is evident that no sensible vessels pass from the one to the other. The vessels of the uterus appear to be perfectly well filled, and in the extremities of the veins the coloured matter is found mixed, but not a particle of it in the placenta. Many ar-

teries of the uterus appear upon its inner surface, forming many convolutions there, and swelling out on that inner surface, but still manifestly confined to it, without any appearance of their entering the placenta. He observes, that, as so many arteries reach the inner surface of the uterus, in separating the placenta several of these arteries may be torn, and give some occasion to the opinion you had formed; but that such a preparation as the present is best fitted to determine this fact, and very strongly supports his opinion of it."

Dr Hunter to Dr Cullen.

"Dear Sir,

I have but two minutes to tell you that the bearer* sets out in two hours for Edinburgh. He is a young gentleman that I have the greatest regard and affection for; both because he is the son and the very image of a man to whom I owe most and love most of all men in the world, and because, in the short time that I have known him, I have taken a great fancy to him for his own sake. Shew him all the civilities you can. It will oblige me exceedingly.

I have written without spectacles, and therefore cannot read what I have written. You will understand my meaning. God bless you and your family. Yours affectionately,

William Hunter."

Dr Cullen to Dr Hunter.

"My Dear Friend,

Ever since my son Harry returned to this place, I have intended every day to write to you; but the difficulty of saying what I wished to say, amidst my constant occupations, has altogether put off my writing any thing. You know well how to gratify your own disposition to oblige me. Your behaviour to my sons has been the kindest possible, and has quite over-

* Dr Cullen's son, Dr Henry Cullen.

whelmed me. I value your friendship in the highest degree, and owe so much to it that I think you have the best title to advise me—to direct me with regard to my sons. I sent them to London in high expectation of the benefits to be received from your friendship and instruction, and I had some confidence in their conduct, else I had not trusted them in London; but in this last article I have the highest satisfaction in finding that they have got your approbation, who are so good a judge, who never did a foolish thing yourself, and therefore may be a little rigorous with respect to others. I hope Archie* shall go on to deserve your good opinion.

Though I am very sensible of the benefits to be received with you, and did not myself think of interrupting Harry's enjoying them for some time, I could not disapprove of the step taken by my family in bringing him down here, as I must acknowledge it was for the benefit of my health, in danger of being hurt by a great deal of fatigue, which Harry relieves me from.

With respect to Archie, I had several reasons for proposing to bring him here as soon as your first course was finished; but I shall not trouble you with any of my reasons but one, and that is, my own declining age. There are some schemes of life which I may think of for Archie, which could not be executed but in my lifetime, and this, I think, is on a very precarious footing. Not only my years render my duration doubtful, but I actually feel many symptoms of my decline; and, particularly, apprehend pretty certainly, that, sooner or later, I shall have an attack of Palsy. In this situation, I think that nothing which may depend upon my life should be delayed, and therefore you need not be surprised at my hurry with respect to Archie. However, I am so sensible of the benefit he may receive by remaining a little longer at London, I value so much the opportunity of his confirming your friendship, I have so much regard for your advice, and am so greatly obliged to you for the pains you have taken in giving it, that I shall hazard every

* Dr Cullen's son Archibald, who, after graduating as M. D., studied Law, and became an eminent barrister in London.

thing to follow it. Your very kind offer, of bearing a part of his expense, is beyond all bounds. I shall have so much other use for your friendship, that while I live I hope Archie shall put you to no expense in money; but if I should die before next May-day, I shall certainly leave him to you as a legacy.

I have not time to say more, nor to say a word to Archie to-night. Please to communicate this letter to him, and bid him make use of his time and opportunity. You may bid him, command him, for I have that opinion of his sensibility to your kindness that your commands will go further than mine. I hope all my children shall always be, as I am, most gratefully and affectionately yours,

WILLIAM CULLEN."

DR CULLEN to DR HUNTER.

" MY DEAR FRIEND, 21*st August* 1778.

I was favoured in course with yours of the 7th, and delivered the two enclosed in an hour after. Your letter made me blush to the bone when I thought how often Archie had reminded me, and how long I had forgot to write to you. I can plead incessant occupation in excuse, but the true reason was, that I could not resolve upon what I was to say, or how to express the sense of obligation I lay under, and I find, at length, that it is to no purpose to attempt it. My two sons are full and warm upon every occasion they have of mentioning your favours. They will certainly feel the benefit, and, I hope, the sense, of the obligation for life. They both say it is intolerable that I do not write to you, and I have at length set about it.

I am happy to find that you have time for such a variety of agreeable amusements. You say you are in a whirlwind, but I say it must be agreeable to ride in such a whirlwind and " direct the storm." I am, however, a little vexed that you require fifty years to publish something; I believe, it may require a long time to make your something very complete and perfect, but, for Godsake, do not think of what is beyond the reach of human genius; and let something slip from you, though not quite per-

fect. You say, you will allow the public to be sometimes diverted with you; I do not think that any thing you can publish will have that effect, and I am certain your friends will be much delighted by it; and if, too, you should lose a little money for this purpose, I dare say it will give you no concern.

With regard to your present employments, I say that every man that has acquired wealth is entitled to have his *hobby*, nay, his *hobby-horses;* though some may laugh, it is of no consequence if they cannot at the same time blame, and, truly, I think your employment liable to neither.

I say all this to tell you that I, though late, have got my *hobby* too. I have got a farm, and, if the public would not laugh, I would call it a villa. It is truly a scheme of pleasure, not of profit. I hope, indeed, to make two stalks of corn grow where one grew before; but I believe this will be of more benefit to the public than to myself, and my purpose is purely the beauty of strong corns and fine grass. I have a brook, a wood, and very fine prospects; and I shall bring out more pleasure ground than any body expects. I hope, in short, in a few years to shew a Leasowes in Scotland. Whether I succeed or no, this language will sufficiently shew you that I am at present happy. I find but one drawback, that is, that I can hardly hope for the pleasure of shewing you how much I have done. Two or three years hence I would willingly come to London to fetch you. Do you never think of looking this way? I have heard that John speaks of it. If I did not think his health required it, I should be very happy in his visit. I am sure my sons would be glad to entertain him. I believe their hearts are much set on London, but they cannot possibly be there next winter.

Please tell your brother, though I do not write to him, as my sons say I ought, I am not the less sensible of what I owe to him. I would have you and him both believe that I am most affectionately yours,

WILLIAM CULLEN."

Note D, page 34.

DE DISPOSITIONE HERBARUM FLORE COMPOSITO OBSERVATIONES.

Quantum scientiæ plantarum promovendæ contulerit methodus botanica ostendit optime Botanices progressus ab eo tempore quo Morisonus nostras, faustis admodum auspiciis, botanophilos ad usum methodi allexit. In hoc opere tamen, licet jam per centum fere annos summo cum profectu laboraverint quam plurimi egregii viri, restat adhuc multum agendum ut Botanice gradum illum perfectionis quem animo metimur attingat, quod ex observationibus sequentibus de dispositione Herbarum Flore Composito donatarum satis clare patebit.

1. Herbarum flore composito Classis, naturalis adeo ut locum invenisset in omne systemate botanico. In variis tamen systematibus pro floris compositi idea varia, quædam herbæ minime cognatæ cum his conjunguntur, quædam maxime affines inde separantur.

2. Cæsalpino inventori et Morisono primo cultori in classibus minus accuratis utpote in hocce studio primis, facile ignoscendum.

3. Raius, vestigiis naturæ plerumque insistens, de herbis flore composito melius egit. Inter eas tamen admisit Scabiosam, Dipsacum et Globulariam, et eodem jure plurimæ aliæ conjungendæ sunt quæ certe ab herbis flore composito in multis differunt.

α. In illis enim Flosculorum Limbi fere semper quadrifidi. In compositis semper quinque vel sex-fidi.

β. In illis cuique flosculo adsunt stamina tantum quatuor cum antheris totidem ab se invicem distinctis. In compositis vero stamina ubique quinque, cum antheris totidem in cylindrum coalitis et vaginam circa pistillum formantibus.

γ. In illis præter calycem communem adest perianthium cuique flosculo proprium, quod in compositis certe

rarum, et nulli mihi noto competit præter Echinopum, herbam inter Capitatas Raii et Dipsaceas Vaillantii ambigentem.

∂. In illis demum totius floris aggregati habitus neque Corymbiferis neque Capitatis similis, et hæc omnia Raius ipse suspicatus est cum illas in nulla compositorum classe sed tantum ut hisce affines recensuit.

4. Raii coætaneus Rivinus minus bene adhuc de compositis egit, cum non solum Scabiosam &c. cum Raio sed Nymphæam etiam Nigellam et alias plurimas floribus duplicatis gaudentes inter herbas flore composito admisit, quanto errore tamen cuique facile patebit. Ruppius quidem Rivinianæ methodi cultor hæc omnia emendavit, sed definitione floris compositi apta non adhibita, hinc est quod Ludivigius et ædem methodo post eum insistens Scabiosam et congeneres iterum admiserit.

5. Tournefortius, systematicus certe eximius, qui tam multos sequaces habuit, structuram floris flosculosi (ut loqui amat) bene annotavit, definitioni tamen a sæipso datæ non stetit, cum non tantum Scabiosam, &c. sed Xanthium quoque et Ambrosiam, multo magis adhuc differentes, inter compositos collocavit.

6. Nec minus a legibus methodi et principiis propriis recessit, in aliis sagacissimus, Boerhaavius, dum inter herbas flore composito non solum Scabiosam, &c. sed Cuminoidem etiam et Staticem, ad Gymnomonospermas, et Eryngium ad Umbelliferas pertinentes, enumeravit.

7. Eo errasse videntur Botanici omnes supra memorati, quod antheras in cylindrum coalitas ex notis essentialibus floris compositi esse non annotaverant.

8. Hoc probe observavit oculatissimus Linnæus, et ideo Scabiosam, &c. a compositis separavit; sed dum hanc notam solam classis essentialem esse posuit, in eadem classe cum compositis, Corymbium, Lobeliam, Violam, &c. ordinavit, minime dictitante natura, ut ipse vir optimus fatetur, neque minus forte erravit Parthenium a cæteris compositis separando.

9. Vaillantius, Botanices castioris callentissimus, solus classes Herbarum flore composito pure naturales exhibuit, dum indus-

tria et experientia summa, genera omnia convenientia simul collocavit, differentia vero ad alias classes ablegavit, ut videre est in Actis Academiæ Scientiarum, Annor. 1718, 1719, 1720, 1721, 1722.

10. In distributione florum compositorum in Tribus errasse etiam videntur plurimi Botanici egregii. Planipetalæ et Capitatæ Raii tribus bene distinctas et maxime naturales constituunt; qui has igitur non distinxit vel cum aliis immiscuit, contra leges bonæ methodi peccavit.

11. Peccavere ideo Tournefortius, Rivinus et horum asseclæ, Capitatas cum Corymbiferis et Discoideis nudis ordinantes.

12. Nec minus erravit Linnæus dum sexui, notæ variabili et in plerisque minus conspicuæ astrictus, ordines compositorum naturales permiscuit.

13. Melius certe Raius, Boerhaavius et Vaillantius Planipetalas et Capitatas a cæteris omnibus distinxere.

14. In cæteris tamen neque scopum attigere Raius et Boerhaavius, ille a seminibus papposis vel nudis, hic a corollis radiatis vel nudis, differentiam petens. Sunt enim genera quædam quorum species aliquæ seminibus papposis, aliæ seminibus nudis gaudent, ut Othonna, et sunt genera quorum species aliquæ corollis radiatis, aliæ corollis nudis gaudent, ut Jacobæa.

15. Bene igitur Vaillantius herbas flore composito præter Planipetalas et Capitatas reliquas omnes in unam classem sub titulo Corymbiferarum conjecit, et optandum sit quod iisdem differentiis (14) in ulteriori subdivisione etiam non adhibuisset.

16. In Generibus herbarum flore composito constituendis plurimum dissentiunt Botanici. Morisonus, Raius et Rivinus, paucis observationibus instructi, genera minus accurate condidere.

17. Tournefortius et Dillenius ut in toto systemate sic in hac parte accuratiores; nec illi tamen genera ubique certa et naturalia construxere, forte quod partibus fructificationis justo paucioribus usi essent.

18. Super hanc rem in diversa abierunt Linnæus et Vaillan-

tius. Hic genera multiplicavit, ille e contrario ex generibus tum aliorum tum Vaillantii plura in unum collegit, et pauciora in totum constituit; sic ex Planipetalis cum genera viginti sex constituerat Vaillantius, ex iisdem non nisi septemdecem Linnæus. Quisnam rectius, judicent periti. Est utraque via errori locus. Genera multiplicata laborem nimium facessunt; genera nimis pauca et characteristicis essentialibus haud bene insignita, difficultates frequentes objicit.

19. In generibus plantarum constituendis optima nobis videtur Linnæi methodus, quæ non ex partibus fructificationis quibusdam tantum characteres artificiales, sed ex partibus fructificationis omnibus simul sumptis naturales et certiores obtinet.

20. Cum genera plantarum vix bene stabilita, minime mirandum quod Species ad genera propria non relatæ fuerint, et vix ullus quidem ex Botanicis est qui in hac re errores plurimos non commiserit.

21. Qui varietates plantarum pro speciebus vere distinctis tradunt, sine fine laborem inutilem Botanicis facessunt; catalogum tamen plantarum vix invenies ubi hoc non sæpissime factum sit. Raius quidem methodum qua varietates a speciebus distingui possint primus indicavit, a paucis tamen adhibita fuit. Cæteris cautiores plerumque Vaillantius et Linnæus. Verendum tamen ne sæpius hic nimis temere de varietatibus et forte inconsulta experientia pronunciavit.

22. Observationes hæ 16–21 applicari rite possunt ad Raii Catalogum plantarum flore composito Brittanniæ indigenarum.

α. Lactucæ tres primo recensitæ sunt tantum varietates ejusdem speciei.

β. Lactuca sylvestris murorum flore luteo J. Bauhinii, ad Lactucæ genus non pertinet, sed ad aliud Prænanthis nomine a Linnæo inscriptum.

γ. Sonchi species 1ma, 2da, 3tia, 4ta, et 10ma, est una eademque.

δ. Sub Hieracii nomine plura et diversa genera recensuit Raius. ex. gr. Hieracium longius radicatum, Gerard. 298, ad Hypochærin Linnæi pertinet. Hieracium castorei odore Monspeliensium ad Crepin Linnæi; Hiera-

cium Echioides capitulis Cardui Benedicti, ad Picrin Lin.

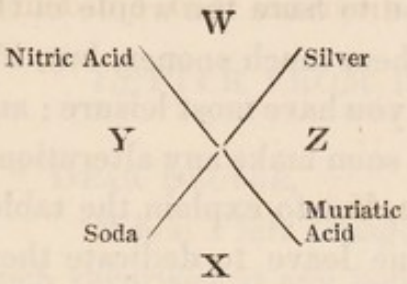
ε. Pilosellam genus ab Hieracio diversum fecit, ejusdem tamen sunt.

Note E, page 45.

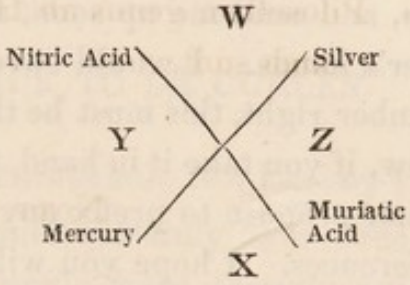
The following passages from a letter written by Dr Cullen to his friend and former pupil Dr George Fordyce of London, in October 1759, contain his own statement of his views with regard to Double Elective Attractions.

——"I must give you the manner of considering the subject, which I fell upon last session, and shall continue to employ as the most easy and simple. I begin with your third and fourth cases, and to these one general rule applies, viz.—That, when two mixts are applied to each other, if in each mixt there is a substance that, from the table of electrive attractions, is by itself capable of decomposing the other mixt, the attractions between these substances and the substances they attract in the opposite mixt must always be greater than the attractions subsisting in the mixts applied to each other; and therefore, &c. Thus, if nitrum argenti and common salt are applied to each other, as, by the table of elective attractions, the nitric acid in nitrum argenti is by itself capable of decomposing the other mixt, common salt; and the muriatic acid in common salt is capable of decomposing nitrum argenti: the attraction between the nitric acid and the soda, with the attraction of the muriatic acid and the silver, must be always greater than the attractions subsisting in the mixts, nitrum argenti and common salt, that were applied to each other. This I illustrate by the diagram adjoined. Let there be two rods intersecting one another, and moveable on a common axis in the point of intersection. At the extremities of each let there be placed substances that have an attraction for each of the substances on the extremities immediately contiguous to them, and let the attractions be expressed

by the letters W, X, Y, Z. The rest of the illustration will readily appear from the diagrams.



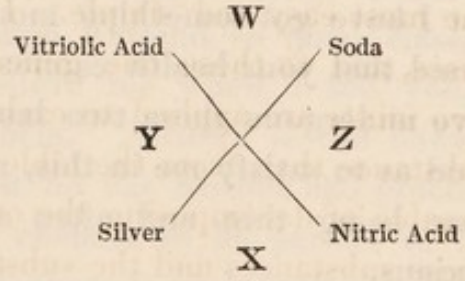
$Y > X$ and $Z > W$ by table
Ergo $Y + Z > X + W$.



$Y > X$ and $Z > W$ by table
Ergo $Y + Z > X + W$.

You see that the prevailing attractions are here determined from the table of single elective attractions. * * *

We are now come to the only difficulty in the affair of double elective attractions in instance first. To this our general rule does not apply.



See how it comes out when my new scheme is applied to it. Y and Z are, by the table of elective attractions, each of them less than W, but greater than X. If therefore Y and Z are exactly as much greater than X, as they are less than W, the four attractions would be exactly balanced; but if Y and Z exceed X in any degree more than they fall short of W, then $Y + Z$ must be greater than $W + X$.

Dr G. Fordyce to Dr Cullen.

"Dear Sir, Suffolk Street, *Sep.* 4. 1762.

I have enclosed two tables which I propose, provided you give me leave, to prefix to my tables of chemical attraction. I find it impossible to go on without something of a syllabus, and therefore I have determined to publish tables of simple attraction, which will serve in some measure as a syllabus to the Chemical History till yours is published. I have gone through all the experiments myself that I have put down, leaving those that I have not had time to satisfy myself in fully, to be filled up in a future edition. They will be quite imperfect unless you

give me leave to prefix the two tables I have sent you, which I hope you will be so good as correct or alter in any manner you please, and send me up soon, as I hope to have the whole in the printer's hands. I would have sent them much sooner, but, if I remember right, this must be the time you have most leisure; and I know, if you take it in hand, you can soon make any alterations. I do not propose to prefix any preface, but to explain the tables in references. I hope you will give me leave to dedicate them to you, as indeed any merit that may be in them is entirely yours, and as there is no knowledge I am possessed of but what I entirely owe to you.

Considering the scarcity of pupils the war and Dr Hunter's giving over lecturing have occasioned, I have had tolerable success. The first course this year I had nineteen, and the summer course fifteen; with this and my practice since Christmas last I have got something more than 200 guineas. I am much afraid that your health cannot but suffer from the fatigue you have undergone these two last years. I hope you will be so good as to satisfy me in this, as some accounts I have heard of your being thin and often ailing have made me extremely anxious.

There is nothing new here either in physic or chemistry: in physic we are all quacks; and some particular treatment of our own, which nothing would make us alter, does every thing in every case (warm water and bleeding for ever). You cannot imagine the number of alchemists we have in London. One Garden, a silversmith, whose business was worth to him £ 1500 a-year, has lately been ruined by it, and a great many others. I believe there is a scheme of making a Medical School in London after the peace. I have some reason to fear that Dr Hunter would recommend Morris. I should be much obliged to you if you would take an opportunity of mentioning this to him.

I flatter myself you will be so good as to answer this soon, and am, with compliments to Mrs Cullen, Robert, and all the rest of your family, dear Sir, your affectionate and humble servant,

G. Fordyce.

Do you want any thing from London?"

NOTE F, p. 49.

LETTER FROM DR BLACK TO DR CULLEN.

" DEAR DOCTOR, EDINBURGH, 10*th February* 1753.

When I left Glasgow I would certainly have been very much surprised at any body who would have thought me capable of writing to you only once till this time; but, indeed, my time is so taken up with five or six hours' attendance at the colleges, and the shop, and the patients, that really I can very seldom indulge myself in the pleasure of reflecting on my friends in Glasgow, which, I assure you, is a very great one, but that the thought of my having now lost their company and conversation, or at least any prospect of enjoying it long, makes it end something melancholy. Though I attend so many classes, I suppose you will venture to believe me when I tell you, I have little satisfaction in any of them. Alston's is the newest to me, and pleases me most; but he has neither had practice enough, nor is he a good enough chemist to be perfect in his profession; and, by-the-by, I think there is no branch should be more cultivated in a medical college. You have no doubt seen his Index Triplex: it seems to me rather calculated for the use of practitioners who are already acquainted with the virtues of medicines than for students; and I have resolved, when I have time, to make a list of my own, where I will not meddle with the Indications, but attempt to arrange medicines by their more general effects on the body, as Aromatics, Bitters, Bitter Aromatics, &c. This, I imagine, would be much more methodical and easier for beginners than the jumble he has made of Hepatics, Splenetics, Stomachics, &c. in the Index Secundum Vires: at least I am persuaded the attempt will be of very great service to myself. I see no science that stands so much in need of chemistry as Materia Medica: every article offers some experiments. One of the simples of little use in Medicine, the Radix Anchusæ, I find, from some experiments, will be of use to Mr

Wilson * in tinging spirit of wine thermometers; the bark of it is red, and this colour resides in an oily or resinous substance, that gives a very pretty colour to spirit of wine, oleum terebinthinæ, melted tallow, &c., and not to water, but is even precipitated from spirit of wine by water, if the tincture is moderately strong. I proposed it to Mr Monro's son, to tinge anatomical injections; he could not be persuaded that it was better than other vegetable tinctures, so I intend to try it myself. If there is none to be got in Glasgow, I shall send Mr Wilson some of it.

Medicine has put on quite a new face to me since I came here, and I can now see an operation with as great ease as before I did a dissection. I go among patients groaning or gasping without any uneasiness, so that objection is quite vanished. I believe this is owing to the cheerful easy faces of the company on these occasions, and, perhaps, partly to the cold bath which I have plied pretty close all winter. I have found no time to read on the pathology, nor one word on lime-water, so that I must, if possible, spend another winter here, which, I believe, is worth my while, on account of the shop, infirmary, and private patients. These, I find, make me read with much more keenness and to better purpose.

* * * * * *

I waited on Dr Clerk, and mentioned the liquid shell, &c. but had only a very short conversation with him as yet; he expected mineral waters. I believe he has a pretty good collection of chemical books which are very scarce here.

The dulcification of acid spirits (*i. e.* the preparation of Ethers) is a curious and a very necessary subject of inquiry, considering the stupid manner in which it is ordered in the Dispensatories. I have compared them for the other subjects, and some remarks have occurred to me which you have, perhaps, made already; but, however, I shall mention them. I consider all the tinctures or elixirs into which the aromata enter, to be very injudiciously mixed with any of the fossil acids (or with any

* Afterwards Professor of Astronomy in Glasgow.

of these dulcified by the ordinary processes, since they all contain a naked acid), and not at all to answer the intention they were designed for. Thus, I suppose, they expected the acids would remain inactive in the composition, and leave the properties of the other ingredients the same, but it is quite otherwise; for they alter entirely the nature of the essential oils and aromatic resins, giving them a bituminous smell; and they perhaps bring them so near the nature of the bitumens as to destroy at least their easy solubility in spirit of wine; and, if the ardent spirit be taken highly rectified, and the acid very strong, which I see Pott recommends with a view to a dulcification, this change in the oils will only happen sooner, for these acids act much more powerfully on the aromata than on spirit of wine. These reflexions made me examine the elixir proprietatis (seu aloes) and elixir vitrioli in the shops. The first had been made by the New Edinburgh Dispensatory, and therefore with a moderate proportion of a very weak acid, and yet the smell and taste of the myrrh was mostly destroyed, and there was a very great quantity of a slimy sediment formed in the phial. The elixir vitrioli had been prepared by the old pharmacopœia by adding oleum vitrioli to a tincture of the species, but it had no taste except of the acid, and its smell was so far from that of the aromata, or of spiritus vitrioli dulcis, that it evidently resembled that of the bituminous stuff left after deflagrating the essential oils with nitrous acid, or rather a smell something like it, which I remember to have had in Glasgow from a solution of oleum terebinthinæ in spirit of wine, with some spirit of salt added to it, which, after standing I believe some months, gave exactly this smell. A solution of cupri cornua (muriate of copper?) in spirit of wine, with some drops of oleum pimento, also gave the same smell. The elixir vitrioli had likewise a sediment. I filtrated it, washed and dried the sediment, but I did not find it so oily and inflammable as I expected; I do not know what it is. I remember very well that the above mentioned solution of cupri cornua and oleum pimento in spirit of wine, which was at first clear and green, after standing cold some months, became of a dark brown colour, got the bituminous smell, and deposited on

the surface of the liquor, and on the sides and bottom of the glass, little globules of an inspissated oil, of a deep red or black colour, which had the bituminous smell very strong. This, and the sediments in the elixirs, are my reasons for saying that the fossil acids destroy the solubility of the essential oils.

This is probably all very tedious to you, who have thought on the subject already, but you know how keen a chemist I am, and you must forgive me. I am vastly pleased with the hopes of your experiments on the solution of calculi; I want to know something certain on the subject, and would wish they were tried in a heat pretty near the animal. I will not, however, trust your experiments with the cold solutions of the absorbents, unless you bring them either to give no colour with the tinctura rosarum, or a green one. The London Dispensatory evaporates the solution of iron for the tincture to a half. I think it were better to evaporate to dryness in an iron pot. You expect a dulcification; I believe that was not intended, but I should much sooner expect it from the martial tincture of Wilson's alchymical experiments mixed with alcohol. I do not comprehend the tincture with spirit of nitre, as iron will not easily dissolve in that acid; but I believe it is owing to my own ignorance, for I do not remember Du Hamel's paper on that subject.

I am told they are not very nice about evaporating pans here. They use metallic ones wherever they can, or, if forced by corrosive salts, glass ones, made of old retorts, &c. I hear there is a new stone-ware work just set up at Prestonpans; I imagine you would fit yourself best there. I had no particular use for the Irish limestone, but that I thought it the purest of any I had seen, and intended to compare it with lime-shell; however, I am in no hurry about it. Mr Russell has put me on an excellent way of comparing their specific gravities by glass-bubbles.

I wish to know how you are pleased with the new furnaces. It would give me pleasure to hear they answered entirely your expectation, but if you have any fault to them I want to know it, for my own improvement. I have met with nothing in that way here. Dr Plummer's are slovenly and ill contrived, and the laboratory

ones are built so as that the utmost fire almost you can raise in them will be just sufficient for the process they are appropriated to, so that they have no dampers, nor do not need them. As for myself, I am kept so busy that I have almost forgot all my Glasgow projects; and as medicine is grown quite agreeable to me now, I have no reason to be quite so anxious about them as I was. Mr Russell has promised to introduce me to Dr Roebuck when he comes to town, a great dealer in the trading chemistry, who will be very ready and capable, as he says, to inform me of many particulars on that subject.

I had lately a letter from my father, by which I see he expects I will take my degree here this spring, and is very keen for it; but I must persuade him soon of the impossibility of it; or, if I cannot, will be obliged to take it in Glasgow if I can get it without a thesis. I can hear nothing of Dr Hutton's new sudorific, either from Dr Clerk or from Mr Russell, so must know something more particular about it before I can give you an answer.

Assure all your family of my most sincere respect and affection; and, particularly, return my thanks to Robert * for copying over the list of authors so neatly for me. I am much ashamed of my carelessness about Mrs Cullen's commission, in not letting her know something about it. I am most sincerely and affectionately yours,

JOS. BLACK."

Dr BLACK to Dr CULLEN.

" DEAR DOCTOR, EDINBURGH, 15*th January* 1754.

I sent you some time ago an unfinished letter, in which I promised a few observations on your paper on Salts. I assure you I think it very good upon the subject, and were I less acquainted with you, should blush to offer my observations; but I know well that you have patience to hear me say any thing, and had always the good nature to set me right. I shall there-

* Dr Cullen's eldest son, afterwards one of the Senators of the College of Justice.

fore give them as they occur to me, for I have not the paper beside me at present.

On the fossil alkali, you say that the common Glauber's salt is made from bittern, and composed entirely of the vitriolic acid and soda. I am not acquainted with the manufacture of it, but I had occasion to examine some bittern from Prestonpans, and it seemed to contain some Epsom salt, but more of a salt of magnesia with muriatic acid, which would not crystallize; now I imagine the common Glauber's though very impure, is yet partly perfect, which probably may be determined by its fusibility, for a pure Epsom salt of magnesia and the sulphuric acid will not melt. This town is chiefly provided with genuine Glauber's salt, made at Prestonpans,—one way of disposing of their oil of vitriol.

I see no reason for your saying that the alkalies may acquire different properties from having been previously combined with different acids, except from one instance of the acid of ants.

I think you should certainly have made tartar a distinct variety of the vegetable acid, as it is so different from the rest, and at the same time always regular. You must look upon it as a mixture or debasement, for you cannot, by any means, divide it, except by burning and destroying it altogether, so as never to be able to compose it again; and to me it appears that the different substances it is analyzed into by distillation, are as different from it as charcoal, oil, acid, &c., are from the wood from which they are obtained. A salt of tartar and the volatile alkali committed to distillation, give out the greatest part of their alkali pure before the tartar begins to be burnt.

Is it from experiments of the Royal Academy that you place the acid of amber among the varieties of the muriatic? I tried a few experiments on some of it that seemed to be pure, for it readily dissolved in a moderate quantity of cold water, and it appears to be a very different substance; for, 1st, made into a neutral salt with the vegetable alkali, it will by no means crystallize, and this compound has a bitter, nauseous, antiphlogistic or cold taste. Evaporated to dryness it burns like regenerated tartar, or rather appears more oily. 2d, The salt com-

posed of it and the volatile alkali is also bitter, pungent, readily crystallizes, but appears very different from sal ammoniac; and, I am persuaded, the pure acid would not precipitate a solution of silver; this, however, I never tried.

I long vastly to hear what improvement you have made on bleaching ashes. Do you make a cheaper ley from pearl-ashes and lime than that commonly employed; and have you calculated this matter exactly? Do you know whether or not the price of pearl-ashes would naturally rise or be raised on purpose, if they were to be generally employed in this way? This and any other chemical or medical news, I should be glad to hear from you at your leisure. I am, Dear Doctor, yours,

J. Black.

Have you determined any thing about your class in spring?"

Dr Black to Dr Cullen.

"Dear Doctor, Edinburgh, *7th August* 1755.

I have endeavoured to get an experiment made here with the air-pump to no purpose. Dr Stewart's is not in order, and his operator is so slow and surly that I am quite tired of dunning him. Mr Wilson told me he had sent one to Glasgow, which he was to set up immediately. If it be in order, I beg you will desire Mr Torbet to try the following experiment, and to let me know the result of it as soon as he can conveniently do it.

Make four or five ounces of ordinary or shell lime-water as strong as possible, and put it into a phial, which, when corked, shall be quite full, so as not to contain the smallest quantity of air. When the air-pump is ready, draw the cork of the phial, and place it immediately under the receiver, together with another phial, containing nearly the same quantity of common water, and see if the lime water upon exhausting yields any air, or if it seem to yield as much as common water.

N. B. Neither of the waters must be heated. And the lime-water may be made of fresh and strong quicklime slaked with boiling water, which reduces it to a subtile powder much more perfectly and suddenly than cold water; and it may be made in

a close vessel, to prevent its turning weak at the surface by throwing up a crust.

I am sorry Mr Torbet got so much trouble in transcribing Pott's Tables, when a few lines would have served the purpose. My paper on Lime has been read to the Society; and Lord Kames, who was not present, desiring afterwards to see it, I sent him the only legible copy I had, which is the reason I have not shewn it to you before now. Yours,

J. Black."

NOTE G, page 54.

SOME FURTHER REMARKS ON THE GENERATION OF HEAT AND COLD BY MIXTURE.

"What I have formerly had the honour to read to the Society on this subject, was drawn up in so much haste, that I could not then treat it in so simple and clear a manner as I wished. I beg leave, therefore, now to repeat the substance of it, though somewhat in a different light, that it may be better understood by every body.

Glauber-salt is a neutral, consisting therefore properly of an acid and an alkali only. But to get this salt in its crystalline state, a certain quantity of water must be united with the acid and alkali. In this state the concretions of the salt are of a determinate form, and it has a certain degree of firmness and transparency. But if, on the contrary, the quantity of water necessary is wanting, the salt, though it still contains the usual proportion of acid and alkali, does not concrete at all, or if it does, concretes in no determinate form, absolutely without any transparency, and with very little firmness of cohesion. As the salt in this state readily absorbs moisture from the air, and becomes in some measure fluid with it, I named it the deliquescent state. Now, according as this salt is in its crystalline or deliquescent state, it has, as I said formerly, different effects when thrown into water. In the deliquescent it generates heat, in

the crystalline state it generates cold. As in both experiments it is plain that the same materials, water, acid, and alkali, and these only, are employed; so, I think, it is plain that the effects of heat or cold cannot be imputed to the quality of the matter, that is, either to calorific particles in the one case, or to frigorific in the other. In my former paper, I alleged that the effects we speak of were to be imputed to the state of aggregation, which is manifestly different in the two cases. I think my reasoning was conclusive, but even then I had some different view of the matter, which I shall now lay before the Society.

In the combination of bodies, I think we can observe two different kinds of it, one of which is properly mixture, the other solution only; and that, in the experiments of the generation of heat and cold by the combination of salts and water, it is possible that, when a deliquescent salt is added to water, a part of this is united with it in the way of mixture, but that when a crystalline salt is added to the same, it is united with it by solution only. I am sensible here, that to persons not formerly engaged in this train of thinking, what I am delivering would require some further explanation; but I cannot at present afford that, and must go on to say, that, admitting the distinction between mixture and solution, it is pretty plain that, in the cases of the salts we have mentioned, the mixture is attended with heat and the solution with cold. In my former paper I went still farther in the same way. I hinted there, that it was probable that in every instance of mixture heat was generated, and in every instance of solution cold. If we take a view of the several combinations generating heat or cold, we shall find my conjecture generally founded; but I durst not maintain it as absolutely a general position, as there were seemingly some very obvious exceptions. One especially was the combination of acids and volatile alkalis, which, though undoubtedly an instance of mixture, is, however, in all the accounts of experiments of this kind, constantly mentioned as a mixture generating cold. However, from former experience, I had reason to suspect some fallacy in the ordinary experiments on this subject, and was resolved to examine them anew. The result of my late trials has

been this: The volatile alkali, as we commonly have it, is usually in a dry and crystalline state, and, added to water, it generates cold. If to the same crystalline volatile alkali I add a concentrated acid, I find that heat is constantly generated during the mixture; but if the acid, before it is added to the alkali, be diluted largely with water, I find upon the addition of the alkali that cold is generated. It was commonly in this way, that is with very dilute acids, and particularly with vinegar, that the experiment was formerly made; but in such experiments a double effect seems to have occurred, that is, the generation of heat by the mixture of the acid and alkali, and the generation of cold by the solution of the alkali in a watery fluid. The last, however, being the most considerable, appears as the result of the whole experiment. This, I think, accounts very well for the ordinary supposition of the volatile alkali generating cold with acids, though its real tendency be the contrary; and at the same time, I cannot think of any explication that can be given of the generation of heat by the volatile alkali, added to concentrated acids, if its real tendency should be contrary to this. I could here illustrate this, by shewing that we have been exposed to a like fallacy in other instances, but I choose rather to go on to give another direct proof of what I am advancing.

Among the experiments of the generation of heat and cold, it has been commonly observed that an acid added to a volatile alkali previously dissolved in water always generates heat, but as the nature of the mixture was supposed to have a contrary tendency, the heat has been imputed to the mixture of the acid and water. The explanation was very specious, but I find it, however, fallacious. Acids, indeed, added to water, generate heat, but it is only when the acids and water are united together in certain proportions. Thus, when to the concentrated acids you add an equal part of water, heat is generated, and the same happens when you add successively a second, third, fourth, fifth, or perhaps a sixth, portion of water of the same quantity, but after this, whatever water is added, no sensible degree of heat is generated. Now, to a quantity of acid I added successively portions of water, till I perceived that no more heat was

generated by the additions. I then poured this diluted acid on a quantity of fluid volatile alkali, by which I found that a sensible degree of heat was produced. It is plain this could not be imputed to the mixture of the acid with the water accompanying the volatile alkali, but must be imputed to the mixture of the acid with the volatile alkali itself. From this, and the other experiments above mentioned, I may conclude that the real tendency of acids with volatile alkalis is to generate heat, and that thereby I have removed seemingly the most considerable objection against the general position, that *every mixture generates heat, and that every solution generates cold.*"

NOTE H, page 59.

LETTERS FROM GLASGOW PUPILS TO DR CULLEN.

Dr F. Hutcheson * to Dr Cullen.

"Dear Sir, Edinburgh, *8th December* 1748.

I would have been extremely glad to have heard from you ere this how all was going on in Glasgow. No one has written me any thing particularly of the classes. I judged, indeed, from a letter of Mr Yates, that the chemistry was going on well, as it was so stuffed with chemical terms, mixts, compounds, &c. that I thought I should have been obliged to call a consultation to unravel its textures, and extract the quintessence of it. Jo. Black might have answered me.

Our Colleges here go on in the ordinary way. I cannot recollect any thing peculiar from any of my Professors except Young, and indeed it is impossible to tell his peculiarities without transcribing his lectures. Monro has finished the bones of the head this day. We had some theory upon ossification from mere pressure, and an account of some diseases not peculiar to

* Son of the celebrated Professor of Moral Philosophy in the University of Glasgow.

himself indeed. Whytt has now brought the chyle into the subclavian vein; he is not very long upon these points. Rutherford gives us no more than is in Van Swieten. He has just finished the section De Vulnere; upon this, indeed, he was short, but in general he is intolerably tedious. Haliday and I often lay our heads together and grumble at him. We are admitted members of the Medical Society here. I take it to be a very useful institution, and under the best regulations imaginable; indeed, its continuing so long as it has done shews that. I am just now busy writing for it, otherwise you would have got a letter rather more correct; I hope, however, you will excuse it. I long much to hear how all goes on with you. Pray make my kindest compliments to Mrs Cullen. She had a pretty long letter from me; Mr Moore had two. Mr Haliday desires his compliments to you. I am, dear Sir, your most obedient humble servant, FRANCIS HUTCHESON."

Extract of a Letter from Dr ALEXANDER HENRY HALIDAY to Dr CULLEN.

" SIR, BELFAST, 3*d May* 1751.

I have a request to make, and I hardly know how to address you,—like the publican in sacred writ, conscious of my own unworthiness, I approach with awe and petition with trembling.

I was in Paris when the news of your establishment in the University of Glasgow reached me. My joy was too great to be silent, and I immediately wrote to you; but, uncertain whether my letter was ever received, I must again beg leave to assure you that this event was in the highest degree pleasing to me, and it must have been more or less so to all your old pupils. Your constant treatment of us not only demanded that respect and esteem every faithful and good instructor is entitled to, but there was gratitude due for uncommon cares, and the warmest affection for repeated offices of friendship. I would be cautious of writing in this style, was I not sure you would distinguish the voice of gratitude from that of flattery: this last, as it is al-

ways an affront to whom it is addressed, so it must argue excessive vanity in the offerer, or his contempt of the patron he worships. One must have a very good opinion of himself who thinks his applause capable of pleasing a wise man.

Yet, not on your account alone did I rejoice. Ever affectionate to Alma Mater, I have been long ambitious of her making that figure in the medical that she has already done in almost every other science. An acquisition, then, which so evidently tended towards it could not fail of gratifying me.

Is it vanity that makes me think so, or was I really distinguished from the greatest number of your pupils by something of a particular attention? I hope it is not vanity, for the pleasure this reflection is attended with, comes dashed with the conviction that I neither merited nor improved, as I might have done, this happy circumstance, if it was indeed real. Be it as it will, true or only imaginary, it has had its full effect on my sentiments with regard to you, my first and most honoured preceptor in medicine.

But whither have I wandered? I have wrote much, and not one word to the purpose; yet my conversing with you, if I may call it so, and viewing you so long more as the kind friend than as the eminent professor, has taken off something of that fearful awe I sat down with; and I can now proceed with less uneasiness, &c.

The following letter to Dr Cullen is from Dr John Moore, who afterwards became so distinguished in the literary world.

" Dear Doctor, Paris, 1749.

As your long, and (as I thought) unkind silence, gave me a great deal of uneasiness, your late epistle imparted to me an equal proportion of pleasure. I assure you I was as industrious as yourself in finding excuses for you, and was fond to impute it to any thing rather than your indifference about me, the thoughts of which I could never bear, and than which nothing could more effectually humble my vanity.

I was extremely glad to find that you particularly recommend these very two branches of our business which I have made my principal study since my arrival at Paris, as the most likely to succeed in Glasgow; viz. performing Surgical operations and practising Midwifery. With regard to the first, I have not contented myself with attending the hospitals and seeing them again and again performed there, but have also taken the courses of two celebrated surgeons, where, under their inspection, I have performed every operation, several times over, upon dead subjects, particularly the stone and some others, which I have never performed upon living persons. I would fain have had my *coup d'essai* of this operation over here, but find it, to my great sorrow, impracticable—I mean upon a living person.

As to midwifery, I have attended one course, seen a good many births, and performed some myself; have also read upon this subject Mauriceau and La Motte with tolerable diligence, and shall give the finishing stroke under Smellie, whom I design to attend at London on my return. My time at present is occupied in dissecting, and attending the lectures of the famous Astruc upon the diseases of women and children,—a branch of my business which, I freely own, I have great need to study. There is an experiment in the surgical way trying at the Hospital de la Charité, which attracts the attention of all the students, but the success is not yet known; I shall delay acquainting you with it till my next. Such of your books upon chemistry as could be found in Paris I bought, and they will be delivered you by your good friend Mr Ritchie; his going to Scotland prevents me sending the news you expect, as I am very sensible he can give you them much more distinctly than I can. He will also give you an account of my present situation and views; and, as there is no man's advice which I esteem more than yours, I shall expect it with impatience.

Dear Doctor, I believe I need not tell you that I will execute all your commissions, or any other piece of service I can do you, with pleasure. This is what every man who is acquainted with your character would do, far more one who has been

your pupil ; nay, allow my vanity to add, and is joined to you by a nearer tie, circumstances which will witness against me if I am ignorant in physic.

Make my compliments to Mrs Cullen and all other friends. And believe me to be, in the sincerest manner, your wellwisher, and most obedient servant,

JOHN MOORE."

The two following letters are from Mr Thomas Melvill, whose premature death prevented the entire development of those remarkable talents of which an early promise was given in his ingenious paper on Light and Colours, and in his essay on Dreaming.

" DEAR SIR, LEYDEN, 18*th July* 1750.

According to my promise when I had last the pleasure of seeing you at Glasgow, I have taken the opportunity of Mr Robertson's return to drop you a line. I was a good deal satisfied that the boy's friends changed their first intentions of sending us to Groningen. Here, in Leyden, we have all the same advantages for literary improvement; and, besides, we have the pleasure of enjoying the company of some few of our countrymen. I attended, along with Messrs Anderson and Robertson, Professor Muschenbrock's lectures for the two or three weeks that remained of his college after our arrival here: he seems to understand his subject thoroughly, and speaks the Latin with great correctness and facility. It is incredible what a quantity of labour that gentleman has gone through in the prosecution of experimental philosophy. One may hear him mention in one lecture so many experiments that he has tried, as would require upon every article many days to perform. He seems to be entirely fitted for his business, as he is delighted with the theory, indefatigable in the practice, and of no despicable abilities. You will observe he has made several alterations and additions in the last edition of his System, particularly on the head of Electricity. If you can afford to throw away as much time, I shall be greatly indebted to you for the favour of a line, with

your opinion of these additions, and any other thing that is curious in the way of philosophy. I shall be obliged to you, likewise, for whatever news is a-going at the time: and if you want any book or pamphlet from this country, or if there be any other commission in which I can serve you, you have only to give me your commands. Please direct your letter for me as if I were in Dr Whytt's at Edinburgh, he will take care to have it transmitted to me. The circle of my acquaintance here is still very narrow, as there are but few English in the place, and I have not yet got enough of French or Dutch for conversing easily with foreigners; for that reason, I cannot pretend to know any thing of the state of the literary world here. Perhaps I may be able to know more of it afterwards. The famous pamphlet *Sur l'Electricité Medicale* you may have from Mr Carrick. Make my compliments to my good friend Mr Wilson, and tell him from me, that we have done more in the affair of kites than all the wits of Holland; for though one may see some hundreds of them always flying about, it has never once entered a Dutch brain to put one above another. I will trouble you further with my humble compliments to Professor Leechman and his lady, Mr Simson, Mr Ross, &c. If any of the Professors want any thing from this side the water, I shall have great pleasure in serving them. With my sincere respects to Mrs Cullen and Miss Johnstone, I am, Dear Sir, yours most affectionately,

THOM. MELVILL."

" DEAR SIR, EDINBURGH, 21*st February* 1752.

You will receive along with this my paper on Light and Colours. I shall be glad to know your sentiments on some articles of it, either by writing, or when I have next the pleasure of seeing you. I remember some years ago you had thoughts of making some inquiries concerning the heating and cooling of bodies. From Art. 12. of the paper I have sent you, it appears that, in measuring by experiment the facility with which bodies of different densities admit heat, the bodies made use of ought to be either both opaque or both transparent; and, for any thing I know, the reason why mercury is much faster warmed by fire-

light (in Dr Martyn's experiment) than it ought to be in respect of water, considering the difference of their densities, may be owing to its opacity. But, for this reason, the method I think you used to propose of warming bodies by the contact of a surrounding fluid is less exceptionable. At the same time, it would be well worth while to examine whether the article of opacity, or pellucidity has any thing to do in the matter when the heat is not communicated by light, but by the immediate contact of hot bodies. My notions in theory would incline me to think there must be a difference from this circumstance. I should think that the transparent body is, *cæteris paribus*, most susceptible of heat by contact. I have been very busy since I saw you in considering Lord Kames's paper (on the Laws of Motion, &c.), and drawing up some remarks upon it. I see more of the author's subtilty and acuteness after weighing it more thoroughly. There are, no doubt, some things in it which are mere mistakes, easily pardonable in one who philosophizes in the midst of such different occupations. But, upon the whole, I think his notions have arisen rather from thinking too much than too little on the subject. I have not attempted so much to confute what he advances, as to shew that it leads to mystery and scepticism, or at least terminates naturally in the system of Boscovich, the Roman professor, who denies the existence of atoms, and reduces all matter to active and moveable points of no magnitude.

Dr Simson's book is in the press and nearly finished; it is dedicated to his brother Mr Robert. I am afraid the doctrine will not satisfy the mathematician's accuracy. Blackwell's Memoirs of Augustus' Court is likewise in the press, and Wallace's disquisition concerning the populousness of ancient nations against Hume. With my sincerest respects to Mrs Cullen, I am, dear sir, your most affectionate and obliged,

T. MELVILL.

P. S.—When you are making no use of my paper you may give it to Dr Dick."

The Rev. Dr ARCHIBALD DAVIDSON, afterwards Principal of the University of Glasgow, to Dr CULLEN.

" DEAR SIR, GOETTINGEN, 19*th Nov.* 1752.

" ABOUT a fortnight ago I arrived here, and now, in compliance with your desire, do myself the pleasure of giving you the best account of the University that I have been hitherto able to learn.

" There are in all thirty Professors, ordinary and extraordinary. Several of them are very famous and reckoned men of ability ; but all of them, excepting the Professors in Physic, teach in German. The students are about six hundred, made up of Danes, Swedes, Russians, and Germans, of all the different sovereignties of the empire, but chiefly Hanoverians, Saxons and Prussians. The British are exactly equal to the muses in number, but are not reckoned their most assiduous votaries ; five of us are Scots, viz. Lord Sutherland, and his companion Captain Grant, Lord Fincastle, Mr Murray, nephew to the Duke of Atholl, Mr Macrae, and myself; and four English, all commoners. We have here a very magnificent library, greatly augmented by the bounty of the Duke of Newcastle, of Mr Pelham, and of Baron Munchausen, the Prime Minister for Hanover, who is the Mæcenas of the University. We were recommended to Professor Haller by Dr Fleming, physician at London. The professor seems to be a very agreeable worthy gentleman, but so immersed in study, that he has no time to spare for company, or even for practice, so that even when any of his own family are indisposed, he employs another physician.

I remember that you gave me a commission to inquire for a critical Dictionary of the German language. I spoke to Dr Haller about it. He told me that it was hard to find such a one, but that there was a critical work done entirely in German by one Steinbach, where the words are explained according to their etymology. If you choose to have that work, or any other thing that I can procure for you here, believe me when I assure

you that I will execute your commissions as soon as I receive them with great pleasure. The Hartz forest and mines are but about thirty English miles from us; I will probably visit them in the spring, so, if you want any thing that is to be got there, I will endeavour to serve you to the utmost of my power. Expecting to have the pleasure of hearing from you, I am, Dear Sir, with the greatest respect, your most obedient humble servant,

ARCH. DAVIDSON."

NOTE I, page 62.

DR CULLEN'S CORRESPONDENCE WITH LORD KAMES.

DR CULLEN to LORD KAMES.

"MY LORD,

I have just now remembered that I am in your Lordship's debt for a letter. I find indeed that I have been too long so, and would now make amends; but, what is worse, I have very much forgot the subject of your last. I only remember that it made some demands upon me which I could not answer, and mentioned some subjects which I would then, and must still, beg to be excused from entering upon. I write now for the sake of writing, and that I may, if possible, by at least a letter once every session, keep your Lordship in remembrance of me. I have passed the winter as usual, always very busy, but seldom employed as I would wish—projecting a great deal, but executing, at least finishing, nothing. I suppose your Society are preparing their second volume for the press. I should be proud to have a share in it, but I find I cannot. The paper I sent you last year is now destined to another work, which I hope will appear soon, and I have nothing else at present fit for you. That this letter may not be quite without philosophy, which you always require, I shall give you somewhat that has occurred to me lately, and which is, as far as I know, entirely new."

Dr Cullen proceeds afterwards to give Lord Kames an account of his experiments on the cold produced by evaporation.

LORD KAMES to DR CULLEN.

"SIR, EDINBURGH, 23*d December* 1749.

* * * *

I have put your Essay in Martin's hands, who is quite a knight-errant upon the nitre scheme. We propose many experiments. One may easily be followed out, which is, to make six divisions in a border of the same sort of earth. Manure or impregnate five of them with nitre, in different quantities. Sow the whole six with the same grain, to see what effects are produced by different quantities of nitre in the five beds, and none at all in the sixth. I trust so much more to you than myself in making experiments, that I cannot help requesting of you to try this.

* * *

HENRY HOME."

LORD KAMES to DR CULLEN.

"SIR, EDINBURGH, 5*th January* 1750.

I am a little afraid of becoming a troublesome correspondent; but, to make you easy, I do not insist upon a regular correspondence. Let me only hear from you when you find yourself disposed.

I wrote the other day to you, proposing an experiment about nitre, which I think deserves well to be tried. There is another point I am extremely curious about, and which came from yourself, and that is, to set about a course of experiments, in order to determine what proportion of their food plants draw from the air, and what are the plants which draw most nourishment from the air. If this were found out, husbandry would be much advanced by it; for, all things else being equal, it would be best to chuse such plants as live most upon what they draw from the air, without exhausting the soil. By an experiment we all

know, I should take this to be the case with cabbage, which, being hung up within a house in the winter time, will keep plump and green a very long while; and this can be owing to nothing else but to the nourishment which is imbibed from the air. I have often thought of planting an acre with cabbage purposely to make dung of it. It is a succulent plant, having a great tendency to putrefaction; and, if I were sure it lives much upon the air, and little upon the soil, I would certainly put my project in execution, and by that means augment the quantity of my dung considerably.

The enclosed is a new theory of Lime, which I thought worth your attention; how far well founded you will judge: and, supposing it well founded, it does not in the least contradict your theory, but will be a proper addition to it.

Mr Wedderburn*, who carries this, brings you two volumes from Arthur Martine. He is highly pleased with your Essay, which I put into his hand; and the other enclosed letter is from him. Yours most affectionately,

HENRY HOME."

DR CULLEN to LORD KAMES.

"SIR, GLASGOW, 17*th January* 1750.

I was favoured with yours by a private hand on Sunday night last. Before that time I hope you had received one from me, precisely, as I remember, of the same date with this I have from you.

Your correspondence will never be troublesome to me; my only fear is lest it may not be in my power to take so much trouble in answering you as I would wish to do; but bear with me a little, and at the long run I hope that, by perseverance in prosecuting this subject of agriculture, we shall, by joining our reasonings and experiments, get most of our doubts cleared up, and opinions and practice both settled on a better footing than heretofore.

The experiments fit to determine which plants draw most

* Afterwards Earl of Rosslyn.

from the air, and least from the soil, are such as Mr Hales and others have made on the perspiration and absorption of plants. Those already made are but a specimen. To answer our purpose, experiments must be made on a great variety of plants, and must be made on many of them during the whole time of their growth. A course of such experiments, therefore, would occupy a person for a summer season, require a garden in the country, assistance, and a pretty close attention. Such circumstances I cannot command at present. In the mean time, in the papers I gave you, I think I have gone some length in ascertaining this point. 1*st*, All plants, by their leaves, absorb moisture from the air, and communicate it to their roots and to the earth about them. 2*d*, All plants, before they shoot to seed, take little from the soil, and therefore probably absorb much from the air. 3*d*, Succulent plants subsist long without the assistance of roots, and therefore absorb much. 4*th*, Plants that subsist during the winter perspire little, and consequently take little from the soil but what will be found in their substance afterwards. These general principles apply readily to cabbage. It is a plant that does not stand to shoot to seed. It is a succulent plant, and of a genus that subsists during winter. I have always, therefore, considered it as a meliorating plant; but whether your project with regard to it will be successful I doubt, as I do with regard to the raising of any crop with a view to its being directly turned into manure.

Cabbages require a good mellow soil recently manured; though they take little, yet perhaps they take somewhat, and I doubt if what they take and what is lost by the perspiration of the soil, with the rent and labour, will be compensated by the crop recommitted to the soil again. I take it the best way of converting any crop into manure is by passing it through the body of an animal, as I believe the dung got this way, and the profit of the fed cattle, will exceed the value of the dung got from the vegetable putrefied any other way; and I am told that cabbage, in particular, employed in the way I propose, turns to very good account; but I am perhaps out of my depth.

I have read your paper on Lime, but have not had time enough

to consider it. I heard of Dr Alston's experiment when I was last at Edinburgh. I believe that lime may continue long to give out its virtue, and that is enough for your purpose; but that the lime-water continues so long as the Doctor alleges of the same degree of strength, I must have other proof than the Doctor's taste. Your reasoning from this experiment seems to me probable and just; but, before we rest upon it, we must have it determined whether lime directly by itself goes to the nourishment of plants, or if it operates only in preparing the nourishment, or if both are true: on these points you fluctuate a little. Upon any of the three suppositions, we may account for lime being in time exhausted; but it is only on the second that I can account for exhausted limed ground being worse than before. Mr Mure of Caldwall has engaged to procure me specimens of the two kinds of lime from the shire of Ayr you informed me of; when I have got them I shall enter upon experiments that I hope will throw light upon the whole affair of liming. Be so good, at your leisure, as to answer a demand I made you formerly, that is, let me know what are the ten crops Mr Simson takes after liming.

So far I had proceeded in sketching a letter to you yesterday, and perhaps would have delayed sending it, till I had got time to say more on this and other subjects; but a piece of news I got to-day has determined me to write to-night, and obliged me for dispatch, too, to employ an amanuensis. Hercules Lindsay has got advice to-day that his presentation came to Mr M'Millan's hand at Edinburgh last post, and at the same time I am certainly informed there is none for me: I expected they would have come together, but I am much afraid you have considered it too much as an affair that would pass of course, and that Baron Maule has been very cool on the subject. At any rate, I think this will allow him to remind the Duke of it; and I shall beg soon to hear from you upon the subject. I am not so anxious about getting this professorship, that I pay pretty dear for, as I am to be determined, whether I get it or not; the getting it sooner or later will make a considerable odds in its value, as matters stand at present in the college. One professor turned

of eighty-six, and another ill of a bad fever, both their places in the gift of the Society, and still the main point with me is to be determined. I have lived too long without a settled plan, which has always kept me distracted, and unsteady in my application to study.

I am afraid I presume too far in troubling you with so much about myself. Be so good as make my apology to Mr Martine for not answering his letter, which I got along with yours; I shall do it soon, and in the mean time thank him kindly for the two manuscripts he sent me. I am, Sir, your most obedient servant, WILLIAM CULLEN."

DR CULLEN to LORD KAMES.

" SIR, GLASGOW, 1749.

I now take the liberty you were pleased to allow me of consulting you about my plan for prosecuting the study, or, if you please, the improvement of agriculture. Your recommendation of this subject has very much strengthened my own inclinations this way, and this would have appeared sooner, if my practice and the teaching a course of chemistry had not entirely occupied me during the winter; but now the last is over, I resolve, during the summer, to apply to some experiments that relate to agriculture; but lest I lose my labour in random experiments, I wish to have your advice, and to know from you what experiments are likely to prove decisive and fittest to ascertain the principles of agriculture.

That you may do this with the more ease to yourself, and with more advantage to me, I shall now venture to lay before you my own plan on this subject. I hope, therefore, you will bear with the detail I am now to enter upon. It is the substance of a few lectures I gave in my course of chemistry. As the course was intended to teach the elements of a chemistry applicable to arts in general, agriculture claimed a place; and though I was not well prepared on that subject, yet I thought it was proper to make a beginning, and at least to open young

gentlemen's views on the subject. To that purpose I entered into some details, which it would not be proper to trouble you with, and I must even here observe many things unnecessary to be mentioned to you, but unavoidable, as serving to shew the connexion of the whole. To begin, then,—the whole of the principles of agriculture may be referred to two heads, that is to say, they are either those which relate to the nourishment of plants, or those which relate to the application of that nourishment. With respect to the nourishment of plants, the first question that arises seems to be this, Whether soils contain different juices fitted to nourish different plants, and that roots are endued with an elective attraction, whereby they take up only what is peculiarly fitted for their nourishment, and reject or exclude every thing else? Or, if the nourishment of plants, as lodged in the soil, is the same with respect to all different plants, and taken in by all different roots under one and the same form, but that by certain powers in the vessels of each respective plant, this common nourishment is converted into the juices peculiar to each? I am inclined to be of this latter opinion," &c.

Lord Kames to Dr Cullen.

"Edinburgh, 14*th January* 1751.

"Though you seem quite to have forgot me, I cannot allow matters to rest upon that footing, but must endeavour to refresh your memory, by taking the opportunity of Mr Smith to transmit my essay to you about the Laws of Motion. Among many promises you made me, if you have not forgot them all, one was to give me a sight of your inaugural oration, which I long to see, and to know if any new discoveries are made in my favourite science of husbandry.

You may shew the papers to Mr ———, and send them back when you have done with them, but not without remarks. Yours affectionately,

Henry Home."

LORD KAMES to DR CULLEN.

" SIR, EDINBURGH, 14*th July* 1752.

I came yesterday from the Whim, where I passed a day or two with his Grace of Argyle. As I have always your interest at heart, or rather that of the public, I took occasion to talk to him of your project for purifying salt. He told me of a letter he had had from you on that subject, but said frankly, he wanted to have an account of your method to judge whether it would do. I gathered his reason to be, that if he were once satisfied of the practicability of the plan, he would be extremely keen to put you upon the most advantageous footing with regard to a premium. He said he hoped you would think yourself in no danger in communicating your secret to him, and that you would find him a trusty confident. * *

I find Lord Deskfoord has not yet got your lucubrations upon bleaching and upon ashes. If I do not get this summer some of your experiments about husbandry, I will abandon you altogether as an utter bankrupt.

I informed the Duke you proposed to bestow some of your thoughts upon Moss, for the benefit of the Duke and of his farm at the Whim. He shewed me a treatise of one Dignerus, upon that subject, which I read, and think shallow enough. I find by Mr Lind, who was there, that he has written a little essay upon moss. He gave me one hint, which I should wish to know your opinion of. He reckons moss a composition of wood, digested and broke into small parts, but by no means putrefied. Its being combustible, shews, in his opinion, that there is no putrefaction. Therefore, to make moss a fit manure for land, he proposes to mix it in a dunghill, in order to putrefy it thoroughly, after which it will be good manure. This touches me. For I have in my farm a good quantity of shell-marl covered two or three feet with moss. If I can turn all this moss into good manure by putrefaction, I shall have store of dung, and my shell-marl laid open to be carried to my land, without trouble or expense. Yours,

HENRY HOME."

Dr Cullen to Lord Kames.

" My Lord, Glasgow, *July* 1752.

Your letter of the 14th is very kind. Your Lordship would blame me very justly if I were to use any reserve with his Grace the Duke of Argyle; but I have never dealt in secrets, and I never shall have any with his Grace.

When I had the honour of writing to his Grace on the subject of sea-salt, I described my process for purifying it, as I have myself practised it. I was writing at the same time on other subjects, in return to a letter I had from his Grace, and being averse to making a long letter, I was perhaps shorter on the subject of salt than I wished to be. In case you have another opportunity of talking to his Grace on this subject, here is my process. Make a saturated solution of common table-salt in water. To this add by degrees a dilute lixivium of potashes. The lixivium added to the solution makes it milky, and occasions a precipitation. When the solution becomes clear, again add a little more of the lixivium, and repeat this till the milkiness and precipitation become inconsiderable. The quantity of potashes necessary will be different according to the greater or less purity of the salt employed. I find a drachm of potashes dissolved in two ounces of water is enough for a pound of the salt I used. When the precipitation is finished, let the solution stand till it becomes quite clear, and by a syphon draw it from the sediment. Set this purified solution to evaporate in a heat not exceeding 100° in Fahrenheit's thermometer, and when two-thirds are evaporated draw out the crystals. This is the whole of my process; the theory of it is shortly, that the lixivium serves to precipitate the calcareous salts, which renders our boiled salt unfit for curing provisions. To explain this farther, or shew the insufficiency of the methods of purifying proposed by others, would exceed the bounds of a letter; I think it enough now to add, that the salt prepared by the above process has all the marks required in a perfect salt. By the bearer I send you a specimen of it, which has been evaporated by the heat of the sun. In this practice I have been very much disappointed,

finding it much slower than I expected, insomuch that the salt now sent you had been neglected in despair for a long time past, and the water has only been poured from it since I received your letter.

On the subject of moss I am not ripe to say much; I cannot quite agree with the opinion you mention, but that it is vegetable matter, and that it is not putrefied, seems pretty certain. The spongy friable moss that lies upon the surface, I take to be good for nothing. What lies deeper is more firm and unctuous, and you will find it very difficult to putrefy, or to be converted into manure; but of this farther when I can speak from more experience.

I think it is hardly fair for your Lordship to insist so peremptorily on my husbandry experiments, when it is not yet three months since I entered to the possession of my farm."

Lord Kames to Dr Cullen.

"Kames, 27*th March* 1758.

* * * I flatter myself with the hopes of having you here after your colleges are up, to relax your mind gradually from your present fatigue, and to recruit your spirits. You cannot be better fitted any where: criticism, agriculture, exercise, all shall be employed to set you on your feet again. Think you owe this relaxation to your family, if not to yourself; for the way to be rich is to live long. I took a touch at Simson's Euclid before I left the town, and am much dissatisfied with it. There is no genius displayed in it, though he had a fair opportunity. Euclid is rendered intricate and uncomfortable by his attempting to demonstrate propositions that are self-evident, and this, a capital error, is never once rectified. I read several demonstrations, not one step of which is more evident than the very proposition itself, which is the subject of the demonstration. This is a cruel oppression upon novices, and which by all means ought to be avoided. In short, the old man is fond of money, and wants to pick pockets. I dislike such an attempt, and therefore wish to be rid of the copy you gave me,

which I left at Edinburgh. You need not, however, give yourself much trouble. If you cannot easily dispose of it, let it remain where it is. Yours affectionately,

HENRY HOME."

LORD KAMES to DR CULLEN.

" SIR, KAMES, *8th May* 1758.

The time approaches of your expected, because promised, visit, which I am anxious about chiefly on your account, and that is saying a great deal. Let me assume the physician so far as to say that you require a recruit, nay, that it is essential to your health. I foresee how you may be entangled, but you must put on a resolution and break at once through all obstructions; for if you give way to every patient that may have use for you, you will never stir from Edinburgh as long as you live, which in that case I prophecy will not be many years. I give you the preference, but now a word for myself. I shall shew you a farm in fine order, which is not a common sight. And, in particular, I will shew you a new and an excellent method of rearing and training hedges, less expensive and more effectual than any other practised. I am ambitious you should see them now, because at present I have of them in all the stages, from the planting of the thorns almost to their utmost perfection. It will make a capital article in a book of husbandry. If you should be even circumscribed to three days, better this short time than none at all, for yourself as well as for me. Yours, H. HOME."

LORD KAMES to DR CULLEN.

" MY DEAR SIR, BLAIR-DRUMMOND, 22*d May* 1775.

I do not know but I ought to blush for the contents of this letter; and yet I cannot think of abandoning to oblivion any thing that is curious. There is nothing but miracles in this corner of natural philosophy,—Turkey cocks laying eggs, a number

of them about the size of a large musk plumb; and what is more, a Turkey hen bringing forth a chicken even with feathers on it like a vivipara. I do not affirm that I have legal evidence for these facts, because it did not lie in my way to put any person upon oath about them, but I believe them. At the same time, I would not have you think me so far out of my senses as to erect any prophecy upon them, such as that Wilkes will be hanged, or that Hancock will be elected king of New England. I write this to keep the thing in memory, for law in Edinburgh puts every other thing out of my head. And I write without apology, because I forbid an answer. Had you answered the first, you should not have had this. Yours affectionately,

HENRY HOME."

NOTE K, page 67.

DR CULLEN to M. DUHAMEL.

" SIR,

Your letter, which I have just now received, does me a great deal of honour, and gives me very great pleasure. From the perusal of many excellent memoirs which appear among those of the Academy of Sciences, I have long ago conceived a very high esteem for M. Duhamel de Monceau, and I expect that every subject to which you shall apply your genius, will receive a considerable improvement. It is with impatience I shall wait for the Treatises on Agriculture which you intend for me. The correspondence you do me the honour to propose to me I have long wished for, and shall take pains to cultivate. I am at present intent on some experiments for the improvement of Agriculture; and what observations or experiments I have made, or shall make, on this subject, I shall be glad to communicate to you, but beg leave to reserve them till I shall have perused your Treatise. Probably I shall thereby avoid troubling you with observations which are already well known to you. I write this only to acknowledge the honour of your letter; but

that it may not be quite empty, I shall mention a chemical experiment which I have just now made.

The chemists have thought it impossible to obtain an ammoniacal salt, made with the vegetable acid in a solid form; but I have just now succeeded in making such. I have obtained it by subliming it from a mixture of equal parts of regenerated tartar and of the vitriolic ammoniac, or, as it is commonly called, the Secret Sal-Ammoniac of Glauber.

You will readily perceive the reason of the choice of the matters I employed, and the exchange I expected, and which I find does truly take place. I need not trouble you with the conduct of the process. The ammoniac sublimes, at first, in fine, tender, and seemingly dry, flowers into the neck of the retort. Afterwards a liquor arises, which washes down part of these flowers into the receiver. This liquor, however, is in small quantity, and does not wash down the whole; but at length the neck of the retort becoming hot, the flowers are melted down, and run into the receiver half fluid. Upon the cooling of the vessels, it concretes again into a crystalline tender mass, but moistened by the liquor that is likewise found in the receiver. Both the salt and the fluid appear to be perfectly neutral. The salt has a pretty strong smell; but now, after some days, this has become much milder; it is an odour *sui generis*, very different from that either of the volatile alkali or distilled vinegar, though I think it now approaches nearer to that of the latter. It is, however, as well as the fluid that accompanied it, perfectly neutral, and makes no change on syrup of violets. Its taste resembles that of the regenerated tartar. I have examined this vegetable ammoniac in many different ways, but have not had time to make these experiments so exactly as to offer you an account of them. If such accounts are agreeable to you, I hope I shall be able to afford you some others of more importance."

* * * *

M. Duhamel to Dr Cullen.

" Ce. 22. *Mars*, 1763.

Je suis bien sensible, Monsieur, à l'honneur de votre souvenir, dont M. Dalrymple me donne des témoignages dans la lettre qu'il m'a addressée. Je prie M. Johnston de vous présenter, de ma part, un petit ouvrage que j'ay fait sur la Conservation de la Santé des Equipages ; et je vous prie de faire agréer à la Société d'Edimbourg la seconde édition de mon traité d'Architecture Navale, un tom. en 4o ; ma Physique des Arbres, 2 tom. en 4o ; mon traité des Semis et Plantations, 1 tom. en 4o. J'envoye à M. Dalrymple un petit ouvrage que j'ay fait sur un insecte qui devore les grains ; à M. Monro, un sur la Formation de Os des Animaux ; et j'ay donné à M. Johnston mes Elémens d'Agriculture. Voilà a peu près tout ce que j'ay fait depuis le commencement des troubles qui ont régné en Europe, si l'on en excepte l'Histoire des Arts de l'Académie, à laquelle j'ay beaucoup travaillé.

Je suis maintenant occupé a faire un traité sur l'Exploitation des Forêts. Mais l'age vient et les forces diminuent. Je me vois revivre dans un neveu M. Fougeroux de Bondarvis, qui a fait l'ouvrage sur les Os que j'envoye à M. Monro, et plusieurs arts pour l'Histoire Générale des Arts que l'Académie a entrepris. Je vous avoue que ce seroit pour moi une satisfaction que le neveu, qui est de l'Académie des Sciences, fut associé à celle d'Edimbourg ; il feroit son possible pour n'être pas un membre inutil. Il fait actuellement un voyage en Italie, ce qui le prive du plaisir qu'il auroit eu de présenter ses ouvrages à la Société d'Edimbourg.

J'ai l'honneur d'être, avec une très sincère considération, Monsieur, votre très humble et très obéissant serviteur,

Duhamel du Monceau."

NOTE L, p. 71.

THE following letter to Dr Cullen from Lord Deskfoord, afterwards the Earl of Findlater, is so highly creditable to the writer that I cannot deny myself the pleasure of inserting it.

" SIR, CASTLE OF BANFF, 22*d January* 1764.

I have just heard of the loss sustained by the University of Glasgow by Mr Smith's leaving it. It is of consequence to the University, and to the education of Scotland, that he should be succeeded by the fittest person. I do imagine that your acquaintance Dr Thomas Reid, of the King's College at Aberdeen, is the fittest man in the kingdom for that profession. If you are of the same opinion, it will be doing a service to the public to let your friends who have interest in the University of Glasgow know your opinion. Mr Reid is quite ignorant of my writing; and, I suppose, has no thoughts of the thing. I am, with perfect esteem, Sir, your most obedient and most humble servant, DESKFOORD."

NOTE M, p. 72.

Dr ADAM SMITH to Dr CULLEN.

" DEAR SIR, EDINBURGH, 3*d September* 1751.

I received yours this moment. I am very glad that Mr Craigie has at last resolved to go to Lisbon. I make no doubt but he will soon receive all the benefit he expects, or can wish, from a warmer climate. I shall, with great pleasure, do what I can to relieve him of the burthen of his class. You mention Natural Jurisprudence and Politics as the parts of his lectures, which it would be most agreeable for me to take upon me to teach. I shall very willingly undertake both. I should be glad to know when he sets out for Lisbon, because, if it is not before the 1st of October, I would endeavour to see him before he

goes, that I might receive his advice about the plan I ought to follow. I would pay great deference to it in every thing, and would follow it implicitly in this, as I shall consider myself as standing in his place and representing him. If he goes before that time, I wish he would leave some directions for me, either with you or with Mr Leechman, were it only by word of mouth. I am, dear Doctor, most faithfully yours,

A. Smith."

"Dear Sir, Edin. *Tuesday, November* 1751.

I did not write to you on Saturday, as I promised, because I was every moment expecting Mr Home to town. He is not, however, yet come.

I should prefer David Hume to any man for a colleague; but I am afraid the public would not be of my opinion; and the interest of the society will oblige us to have some regard to the opinion of the public. If the event, however, we are afraid of should happen, we can see how the public receives it. From the particular knowledge I have of Mr Elliot's sentiments, I am pretty certain Mr Lindsay must have proposed it to him, not he to Lindsay. I am for ever obliged to you for your concern for my interest in that affair.

When I saw you at Edinburgh, you talked to me of the Principal's proposing to retire. I gave little attention to it at that time, but, upon further consideration, should be glad to listen to any proposal of that kind. The reasons of my changing my opinion I shall tell you at meeting. I need not recommend secrecy to you upon this head. Be so good as to thank the Principal in my name for his kindness in mentioning me to the Duke (of Argyll). I waited on him at his levee at Edinburgh, where I was introduced to him by Mr Lind; but it seems he had forgot.

I can tell you nothing particular about your own affair, more than what I wrote you last, till I see Mr Home, whom I expect every moment. I am, most dear Sir, ever yours,

A. Smith."

NOTE N, p. 74.

DR CULLEN TO DR HUNTER, ON THE DEATH OF MR DAVID HUME.

" My dear Friend, Edinburgh, 17*th September* 1776.

I was favoured with yours, by Mr Halhed, on Sunday last, and have answered some part of it by a gentleman whom I was otherwise obliged to write by; but as I was not certain how soon that might come to your hand, I did not answer your postscript; in doing which, if I can oblige, a part of the merit must be that of the information being early, and I therefore give it you as soon as I possibly could. You desire an account of Mr Hume's last days, and I give it you with some pleasure, for, though I could not look upon him in his illness without much concern, yet the tranquillity and pleasantry which he constantly discovered, did even then give me satisfaction, and, now that the curtain is dropped, allows me to indulge the less alloyed reflection. It was truly an example " des grands hommes qui sont morts en plaisantant*;" and to me, who have been so often shocked with the horrors of the superstitious on such occasions, the reflection on such a death is truly agreeable. For many weeks before his death he was very sensible of his gradual decay, and his answer to inquiries after his health, was, several times, that he was going as fast as his enemies could wish, and as easily as his friends could desire. He was not, however, without a frequent recurrence of pain and uneasiness, but he passed most part of the day in his drawing-room, admitted the visits of his friends, and, with his usual spirit, conversed with them upon literature, politics, or whatever else was accidentally started. In conversation he seemed to be perfectly at ease, and to the last abounded with that pleasantry, and those curious and entertaining anecdotes, which ever distinguished him. This, however, I always considered rather as an effort to be agreeable, and he at length acknowledged that it became too much for his

* In reference to a work so entitled, published at Amsterdam, in 1732.

strength. For a few days before his death he became more averse to receive visits; speaking became more and more difficult for him; and, for twelve hours before his death, his speech failed altogether. His senses and judgment did not fail till the last hour of his life. He constantly discovered a strong sensibility to the attention and care of his friends, and, amidst great uneasiness and languor, never betrayed any peevishness or impatience. This is a general account of his last days, but a particular fact or two may perhaps convey to you a still better idea of them.

Not many days before his death a friend found him reading, and, upon inquiring what was the book, Mr Hume told him it was Lucian, and that he had just been reading the dialogue entitled Kataplous, in which Megapentes, arriving on the banks of the Styx, urges many pleas for being allowed to return for some time to the world. Mr Hume said the fancy had struck him to think what pleas he himself might offer upon such an occasion. He thought he might say that he had been very busily employed in making his countrymen wiser, and particularly in delivering them from superstition, but that he had not yet completed that great work. This he at first thought might be sufficiently specious; but he soon reflected that Mercury would tell him it was idle to think of remaining now for that purpose, as it would be time enough to return for it two or three hundred years hence. In short, the excuse would not be sustained, and he must pass the river.

About a fortnight before his death, he added a codicil to his will, in which he fully discovered his attention to his friends, as well as his own pleasantry. What little wine he himself drank was generally port, a wine to which his friend the poet* had ever declared the strongest aversion. David bequeaths to his friend John one bottle of port; and, upon condition of his drinking this, even at two downsittings, bestows upon him twelve dozen of his best claret. He pleasantly adds, that this subject of wine was the only one upon which they had ever differed. In the codicil there are several other strokes of raillery and pleasantry,

* Mr John Home, author of the tragedy of Douglas, &c.

highly expressive of the cheerfulness which he then enjoyed. He even turned his attention to some of the simple amusements with which he had been formerly pleased. In the neighbourhood of his brother's house in Berwickshire is a brook, by which the access in time of floods is frequently interrupted. Mr Hume bequeaths £ 100 for building a bridge over this brook, but upon the express condition that none of the stones for that purpose shall be taken from a quarry in the neighbourhood, which forms part of a romantic scene in which in his earlier days Mr Hume took particular delight; otherwise the money to go to the poor of the parish.

These are a few particulars, which may perhaps appear trifling, but to me no particulars seem trifling that relate to so great a man. It is perhaps from trifles that we can best distinguish the tranquillity and cheerfulness of the philosopher, at a time when the most part of mankind are under disquiet, anxiety, and sometimes even horror. I consider the sacrifice of the Cock as a more certain evidence of the tranquillity of Socrates, than his Discourse on Immortality.

I had gone so far when I was called to the country, and I have returned only so long before the post as to say that I am most affectionately yours, WILLIAM CULLEN."

NOTE O, page 79.

Dr CULLEN to Dr DAVID MILLAR.

" DEAR DAVID, EDINBURGH, *5th May* 1764.

The moment I have finished my college I sit down to write to you. I should have done it sooner, and I might give a reason why I did not, but can hardly offer it to you as an excuse. The only excuse I could think of is your standing on ceremony, and never writing but one letter to me; but, as I do not understand this, I shall make no use of it. I was much disappointed in your not telling me plainly how you were received by the two Hunters and Dr Pitcairn, which was very necessary to directing my conduct, and has, I assure you, interrupted my

writing. However, now I have time to tell you so, let all that pass, and write me more frequently, and with that trust and confidence you would repose in one who earnestly wishes you well. I suppose you think of going to Paris, and, was it but for a month or two, I judge it very proper before you settle in any place. * * * *

* I was much obliged to you for Dr Fordyce's Syllabus. I think he is a little rash in some things, and incorrect in many others; but there are few critics in chemistry. Pray find out if Fordyce has any acquaintance with Lewis, and by that or otherwise let me know if the Commercium Philosophico-technicum is to go on, and in what shape. Let me know if you have made any progress in the knowledge of fossils, or have made any acquaintance with fossilists. Have you been in the museum to any purpose? Let me know if any body at London has read Sauvages' Nosologia Methodica, or if any body enters into such a plan, or approves of others doing it. Dr Aikenside I dare say does not, but I thank you for his book. I think Baker has many fewer faults, but gives little instruction. I am sorry I must now put you to the expense of postage, but, that it may not run too high, must conclude with being, as always, dear David, &c.

Extract of a Letter from Dr David Millar to Dr Cullen.

London, 13*th November* 1764.

"Many of the French are great lovers and encouragers of Chemistry; even several of their nobility do not disdain to cultivate it, and make experiments in it themselves. Hence one might reasonably expect many teachers of the science in Paris, as there are; but the most remarkable are Bourdelin and his demonstrator Rouelle, Macquer and his demonstrator Baumé. There is also Geveland, assisted by Rassan, who, though not very remarkable, must not be passed over without notice. I went to Paris at a most unfavourable time for Chemistry, for, except a few public lectures by Bourdelin and Geveland, I heard none else, and I had no introduction to any of the che-

mists. Baumé I have most to say of, and shall introduce him first. He published last year what he entitles, "Manuel de Chemie ou Exposition des operations et des produits d'un cours de Chemie," designed for the use of students, to recal to their memories the preparation of the different substances treated of," &c. &c.

NOTE P, page 99.

The unexpected length to which this volume has extended, compels me reluctantly to abandon the intention of laying before the reader portions of Dr Cullen's lectures on the History of Chemistry.

NOTE Q, page 144.

From the PHYSICIAN who furnished the Manuscript of Dr CULLEN'S Lectures on Materia Medica.

"DEAR SIR, *January* 19. 1772.

You will doubtless be much surprised to hear from me after so long an absence, and still more on the present occasion. But though I acknowledge myself guilty of great imprudence, I do not entirely despair of your pardon, since I can, with the utmost truth and sincerity, assure you, that not the least injury was intended.

Not to keep you longer in suspense, I must own that I have been in a great measure the cause of the publication of your Lectures on the Materia Medica. I am now, and have been, greatly concerned to hear that it gave you so much uneasiness, which has determined me to lay the whole truth freely open to you. Dr B., about a year and a half ago, was at ——— and one day mentioned to me that it was a great pity so valuable a work as your Lectures on that subject should be lost to the public, and that the bringing them to light would be a most desirable thing. He had a very good copy, as indeed I had, though not the same. This he offered to give me if I would correct it, and give it to a bookseller to publish, which I impru-

dently consented to. But, in some palliation, I can truly say that we, neither of us, had the smallest idea of hurting your interest or reputation. I told Lowndes that I had no right to any gain from them, as I believe he has informed you, and that if you chose to claim the copy, it must be yours, and that my only intent was to prevent its being lost to the world. I had no view of the smallest advantage, but an infinite deal of trouble in correcting it and forming the index. I have now two or three copies, which is all I got, and Dr B. had some few likewise. I am truly sorry to have given Dr Cullen the smallest offence, and would wish to do all in my power to make satisfaction. Lowndes tells me you heard 750 copies were printed. I believe you may depend on it that only 500 were struck off. He will, I believe, make you any satisfaction he can, and I hope you will be kind enough to accept it. I might have been concealed had I chosen it, but I chose rather to lay the whole before you. I hope the work is tolerably correct, and I am sure it would do honour to any man in Europe.

I hope you will be kind enough to accept this acknowledgment, as I trust no injury can accrue to you, and to restore me to the place I once flattered myself I had in your regard. I may have acted imprudently, but I had no low or lucrative motives, nor could receive the smallest advantage. Favour me yet farther to conceal this letter. To divulge it can be of no service, and may injure me. I am, with great truth, yours faithfully."

"DEAR SIR, *February* 10. 1772.

I this day received yours, and not without great concern on many accounts, but on none so much as to observe the trouble and uneasiness which my ill-judged, though not ill-intended step, has given you. I acquiesce entirely in the justice of what you say concerning the impropriety of the publication at all, and though I cannot exculpate myself for being concerned in it, I can with the strictest truth repeat, what I before asserted, that no lucrative views in the smallest degree possible were the motives. I am scarce less concerned than yourself to see so many inaccuracies in the work, but the book was not printed

here, and the errors are more chargeable on the printer than on the editor. Lest any mistakes of the editor's should be imputed to you, I wrote a preface to take off that imputation. This was not the same with that published, but more strongly expressed. To convince you of this I here send it to you. As to correcting the style, the preface apologizes for that not being done, for fear of altering the sense affixed by the author, and the same reason holds concerning many of the obscure passages, which I thought it better to leave than reform at the expense of the true meaning, by affixing one possibly not intended. As to your interest, I protest I had no idea of your subjoining any part of the substance of the work now published to your Lectures on the Institutions of Medicine. I remember you studiously avoided it when you lectured on Chemistry, except in a few instances more immediately connected with that branch. I do not recollect Dr Whytt's giving any thing of the kind except in the most general terms possible; and though I do not mean to hint it as necessary for you to follow his steps in this respect, I confess the work appeared to me to be relative almost altogether to the Materia Medica only. The general indications are indeed common to both, but I did not imagine that these could be more than generally touched upon in the Institutions. As it is, I am extremely grieved to think of its affecting your literary fame or interest. I am not a little sorry to hear of your Lectures on the Practice of Physic being likely to be surreptitiously printed. I truly wish to see any of your productions, but I shall not be so far misled a second time as to desire it at the expense of the least possible uneasiness to yourself. But though I have been (though not criminally) guilty of an imprudent and improper action, I hope you will not impute it to an ill interest, and forgive me, when, in the public name, I request you not to envy the world your studies or the fruits of them. They will be the most acceptable present possible, and will perpetuate to ages yet unborn that estimation (perhaps in a greater degree) in which you have been always held. A time may come more proper for such a purpose than the present; but I entreat you to collect your sentiments while it is in your power."

Statement by Dr A. Monro Drummond respecting the Lectures on Materia Medica.

London, 1771.

"A work has just appeared, entitled 'Lectures on the Materia Medica, as delivered by William Cullen, M. D. Professor of Medicine in the University of Edinburgh, and now printed from a correct copy, which has been compared with others by the editors.' It is printed for T. Lowndes, Fleet Street.

This course was given in winter 1761–2, in which year I attended the University of Edinburgh for the first time as a student of medicine. Among other branches of physic I applied to materia medica, and took notes in the class, which I afterwards attempted to correct; but being young in the science, and hurried with different occupations of a similar kind, what I then wrote can be considered in no other light than that of rude sketches of doctrines imperfectly understood, inaccurately noted, and erroneously transcribed. By comparing the lectures published with my manuscript, I find they so exactly correspond, that I am convinced the latter must have been printed from one of two or three copies of the former, which, several years ago, I allowed to be taken for the use of some fellow-students; or rather, from the copy of some other person to whom they or their friends have, with equal imprudence, indulged a liberty which has been thus grossly abused. It may appear needless to add more to shew how little deserving the work in question is of the credit of the public.

It seems not, however, improper to subjoin farther, that, after having continued at the University of Edinburgh nine years from the year 1761, and had, during that period, the best opportunities, both public and private, of becoming acquainted with Dr Cullen's medical sentiments, I can with truth say they are very seldom genuinely represented in these pretended lectures. The practical facts are in a very mutilated condition; the philosophical principles, detached parts merely of a more regular and comprehensive system, which he has since evolved in his course of Institutions; and even the plan of the whole, which

was so suddenly drawn up, and gave so much satisfaction, in many respects inferior to that amended form into which he has lately moulded it for the benefit of his disciples.

Thus much, perhaps, I was called upon to declare, in order to undeceive the public and vindicate myself. I must nevertheless frankly own, that I should scarce have taken this step, but with a view at the same time of preventing any part of the censure which may be bestowed on this spurious publication from being inadvertently drawn upon Dr Cullen, a man of my particular obligations to whom, both as a friend and a preceptor, I retain too grateful a sense not to be led by inclination, as well as by duty, to embrace every occasion which may appear to me a proper one of doing justice to his character.

ALEXANDER MONRO DRUMMOND."

Dr W. HUNTER to Dr CULLEN.

" DEAR SIR, LONDON, 29*th December* 1771.

Last night Dr Pitcairn, Mr Seton, and I, had a meeting with Mr Lowndes, and talked the affair over after he was gone, and we were all of the same opinion.

It appeared pretty evidently that there had been no ill intention towards you, either with respect to interest or reputation, but rather the contrary. The first and principal actors thought the book would add to your reputation, and be of service to the public. With that view they planned this business and gave the MS. *gratis* to the printer, thinking that probably hereafter they were to merit and receive your thanks. They thought it could not hurt your interest, as you no longer read lectures on the Materia Medica. The printer understood this to be the case, and therefore put your name to the title-pages. The young men were impertinently forward, and the printer was not over cautious or scrupulous, especially where there was a prospect of gain. He is a man of good enough character, not one of the impudent, *free-born, audacious rascals*, that print what they please, and set all concerned at defiance. He read to us Mr Balfour's letter, and his own answer. He talked with modesty, —was sorry he had not known your mind,—and threw him-

self upon your good nature and generosity,—would do whatsoever you desired,—only he did not seem to think that he ought to be a *loser*, because he had meant no harm.

We told him, as he did not choose to make any particular offer to you, we could say nothing till you had taken your own resolution after having perused the book itself. We are to meet him again upon hearing from you.

We think you must choose one of two courses, viz. either, if you find the book unworthy of you, even though published by a student, to oppose the sale by all lawful means, which the lawyers will manage; otherwise to allow the sale to go on when the publisher has paid all your expenses, and made such a present, in money, as you shall name, for the benefit of the University or College Library, or for the Edinburgh Infirmary, or any charitable institution. We think you cannot handsomely pocket any money by the occasion, and that it will be very difficult after paying the expenses, to get such a sum from Lowndes as will be worth pocketing.

In this, or in any other service, you will freely command us, and particularly, dear Sir, your devoted,

WILLIAM HUNTER."

Extract of a letter from Professor Tode of Copenhagen, (physician to the King's Household), dated Copenhagen, 11th May 1773, requesting that Dr Cullen would favour him with such alterations on, or additions to, the pirated edition of his Lectures on Materia Medica as he might wish to have introduced in a German translation of that work which Professor Tode proposed to undertake.

" Consider, dear and honoured Sir, how much you thereby would oblige him, who, of all your pupils, is most interested in vindicating the superior worth of the Edinburgh Medical School against that of Vienna and Leyden; as I have been a true apostle in this city, preaching the fallibility of Boerhaave and other great names, introducing the British doctrine, and ridiculing the German methods, &c., in all which I have met with the encouragement of the King's Chief Physician, Mr Von Berger, and the assistance of some few gentlemen, who likewise

have been in Great Britain, and have drunk of the pure fountain of medical knowledge. I am, with the most perfect sense of esteem and obligation, honoured Sir, your most humble, most obedient servant,

T. D. Tode."

Reply to Dr Tode by Dr Cullen.

"Dear Sir, Edinburgh, 19*th June* 1773.

I had the honour of your letter of the 11th past, and am extremely happy to hear of your prosperity. It is, however, what I expected from your good sense and agreeable talents. I will not attempt to make any return to the many polite expressions you have honoured me with, but shall speak only to the proper business of your letter.

The publication of the Lectures on the Materia Medica has given me a great deal of uneasiness. These lectures were very imperfect as I delivered them, and they are more faulty still as now published. I attempted to stop the sale, but came too late, as a great many copies were sold before I could take measures to prevent it. I stopt the further sale for a long time, but certain considerations, which I cannot now trouble you with, prevailed upon me to allow of the sale of the remaining copies of the impression. It was, however, with a resolution to allow of no other impression till I should find time to review the whole, and to give it, both with respect to matter and style, a more correct form. When that time will come I cannot say; and, considering my time of life, and present occupations, it may perhaps never come. In the mean time, I cannot, in any shape, acknowledge the work as mine. Though, in permitting the renewal of the sale, I took care that the editors should reprint the first sheet entirely, and, with respect to the rest, add some sheets of Emendanda, these corrections only respected the more gross errors, and the work is still extremely imperfect, and very faulty. I am afraid, therefore, that the spreading of the work will do me no great credit, and I think it should hurt my reputation if it were supposed to proceed from myself. I cannot, therefore, directly countenance

any translation of it; but I shall say, that, if it is to be translated into the German language, I could wish it to be done by you rather than by any body else. I must, indeed, say further, that, as many of the faults of the English publication are faults of the style and composition, most of these might be corrected in the translation, and, at the same time, by taking into the body of the work the Emendanda which are now printed separately, the work might appear much better than it does in the English. If, therefore, there is any probability that the work will be translated, I not only consent, but wish, that you may do it; and so far as there is any merit in the work, I shall think myself much honoured by your giving it such a mark of your approbation; and I not only allow, but beg, of you to make such corrections, both of the style and matter, as you shall judge proper. I have only one favour or condition to beg of you, which is, that you will publish the whole, or so much of the English prefaces, as will give my excuse for the work's not being at first more perfect, and shew that, however necessary it was for me to do somewhat as a Professor, I have not been guilty of obtruding an imperfect and incorrect work upon the public. I shall be glad to know what course you take in this business, as I may possibly offer you some further advices on the subject. I beg leave to offer my respectful compliments to the gentlemen who formerly did me the great honour of being my pupils. Messrs Claussen, Lafont, Fabricius, and Shcondkeyder, have, and always shall have, my best wishes. I hope it will be agreeable to you to know that our University thrives exceedingly. The medical students last winter were to the number of 350. I am now in the Practical Chair, and Dr Drummond, whom you knew as assistant in the Infirmary, is lately chosen Professor of the Institutions. Amidst many fatiguing occupations, I enjoy as good health as I can expect. Two years ago I published a new and much enlarged edition of the Synopsis Nosologiæ, which I hear is since reprinted in Holland. I hope soon to give the public something of more value, perhaps a text-book for my practical course. I shall always be proud to have any thing of mine well received in Denmark. I beg you may believe me to be with great esteem and respect," &c.

NOTE R, page 161.

Announcement, by Dr CULLEN, of the short Course of Lectures on the Practice of Physic which he delivered in the summer of 1768.

" I have within these few days received several letters subscribed by a number of gentlemen, to whom I should perhaps have returned a written answer, but, as most of them are here present, and several reasons make it proper for me to give my answer very publicly, I hope the gentlemen subscribing these letters will excuse my giving an answer by word of mouth, and other gentlemen will excuse me encroaching a little on their time.

The purport of the letters I speak of is, in general, desiring me to give some Lectures on the Practice of Physic during the course of this summer. Several times before, I have had applications to the same purpose, and, for many reasons declined the labour; but, this spring, having had applications from gentlemen that had left this place, and were not to return but upon this condition; and having at the same time applications from gentlemen, whose affairs determined them to leave this place before next winter, I own I was sensibly touched with the regard and respect which their applications expressed, and was desirous to gratify them, and that I might do so with more propriety, I have obtained Dr Gregory's consent. I agree, therefore, to give to certain gentlemen some Lectures on the Practice of Physic in the course of this summer, beginning the 1st of June and ending the 1st of September, or soon after. But it is very necessary that I give these gentlemen notice of what sort of lectures they are to expect. They are not to be, nor to be called, a Course of Lectures; they cannot be so full as I think a first course should be, and cannot be so full as Dr Gregory's course is. I think a college or course of lectures should proceed upon the supposition that the pupils have neither heard nor read upon the subject treated of, and therefore every thing fundamental should be laid before them, whether derived from books or from the Professor's

own experience. On the contrary, the lectures I propose to give proceed upon the supposition that my hearers have all heard a course before, and that therefore I may omit many things delivered in every course; and, particularly, I suppose that all my pupils have heard Dr Gregory, and therefore I shall omit many things that he has delivered better than I could do. In short, though I am to touch, in order, the principal heads of the Practice of Physic, I am to deliver little on each but what I think almost peculiar to myself;—some things indeed derived from books, but either such as I think are not common, or such things as are not commonly understood in the same manner as I understand them, or that have not afforded to other persons the same conclusions as I draw from them.

Such, gentlemen, is to be the application of my erudition; and to that I shall add what my own observation has offered with regard to the history, and what my experience has offered with regard to the cure, of diseases; and along with this, guided by my own facts, and those of others, I shall frequently attempt what may be called a theory, but what I call the general principles of diseases, which, for the most part, I hope to show to be as much a matter of fact as an inference of reasoning. By these last attempts, I hope on many occasions to distribute the symptoms of diseases, so as that they may fall into proper distinct species, and often to unite in one plan of cure what may seem a parcel of unconnected empirical remedies. On all these points, gentlemen, I presume to think for myself, and in the lectures I shall give, very little else shall be admitted. I hope, therefore, it will be readily perceived that they are neither intended to supply nor to supersede Dr Gregory's course. I know pretty well what that course is; as complete, I believe, as any that was ever given, and, in the collection of facts, either in the history or in the cure, more complete than any; and I know that every part is improved by reflections which his own judgment and experience furnish him with, and I believe it is not possible for a student to lay a better foundation for the study of the Practice of Physic than by attendance on his lectures. When a student has learned one system, if he cannot think for

himself, he should keep to the system of his master as religiously as he can, and allow no other views to disturb him; but if a student can think for himself, and every one believes that he can, he will no sooner have learned a particular system than he will be disposed to differ from it; he will immediately find that others also do so, and that the conflict of opinions is unavoidable. He will, therefore, wish to view matters in different lights, and, though he prefers no one Professor, yet, by comparing them, he becomes stronger in reasoning, and firmer in his own conclusions. I hope, therefore, it may be understood that the lectures I am to give proceed neither from partiality on your part, nor from envy on mine.

This is enough in answer to the principal part of the letters I have received. In some of them, gentlemen are pleased to put questions about alternate teaching, to which I can give no answer. I cannot give you the opinion of either the University or the Town Council of Edinburgh, as neither of them have been consulted on the subject that I know of. In the mean time, it would be preposterous and improper for me to give any opinion of my own.

One other particular, and I have done. So often as I have spoken on this subject, I have declared my resolution of admitting nobody to these private lectures but such as had been pupils of Dr Gregory, but I have, however, the honour of a letter from several gentlemen who have never been such. The gentlemen subscribing that letter, I have as great a personal regard for as any others, and I should consider their attendance as doing me a great deal of honour, and I think they can hardly doubt of its being my choice; but if, for many reasons not to be explained here, I cannot comply with their desire, I hope they will excuse me. I shall be ready to oblige them in any other manner in my power."

NOTE S, p. 455.

Dr Foart Simmons to Dr Cullen.

" Dear Sir, *October* 1777.

I return you many thanks for your very kind letter.

* * *

I cannot help giving you an extract from a letter I received the other day from M. Fouquet, a physician of some eminence at Montpellier. I think it will amuse you; it is the character of M. Barthez, who is at the head of the university there, and I think it is well drawn. I know Barthez personally.

" M. Barthez ne cesse de repéter aux étudians qu'il n'a pas encore existé de vraie médecine avant lui; qu'il est le seul à qui l' intelligence suprême ait fait part de l'intelligence sur la véritable clef de la médecine; Qu' Hippocrate n'est qu'un génie médiocre en comparaison de lui; que le célèbre Cullen a quelque esprit, mais qu'il n'entend rien à la théorie de l'art. Il se prosterne aux pieds des étudians étrangers pour qu'ils le louent et le vantent comme le premier homme de l'Europe, dans leur pays: en dernier lieu, il caressa un étudiant qui composoit une bonne thèse pour l'engager à le citer avec éloge. Vous remarquerez s'il vous plait que ce petit professeur de Montpellier, n'a jamais fait une expérience, qu'il ne voit pas quatre malades de l'année, et qu'il n'a été occupé toute sa vie qu'à compiler et à faire faire des tours de force à son imagination. Vous serez peut-être étonnés, vous autres Anglais, d'après ce que j'écris du caractère de cet original, que les disputans de la dernière chaire (et moi tout le premier) l'ayent loué, comme ils l'ont fait; mais il faut vous dire qu'il avait fait menacer de refuser sa voix à celui qui ne le loueroit pas avec excès." All this is very just. When I saw M. Barthez, he shewed me some manuscripts (a Commentary on the Nova Doctrina he has published) which he said were to change all our ideas in physic—he spoke with great indifference both of Sydenham and Boerhaave, and said he never gave Haller credit for any genius till he read his poems. I have the honour to be, with great respect, Dear Sir, your most obedient and obliged humble servant,

S. F. Simmons."

Extract of a Letter from Mr Thomas Yalden to Dr Henry Cullen.

Marseilles, 25*th January* 1777.

"I take it that Montpellier is just now what Edinburgh was when Dr Cullen began to teach there. You know Dr Barthez's book de Principio Vitali and his Doctrina Nova de Functionibus Humanis. Since the publication of these, they have entered on the consideration of the Nervous System, but they adhere very strictly to morbific matter, the notion of specifics, and, above all, to the jargon of the ancient physicians, and this is delivered in their disputes in syllogisms; for they are as fond of the old school logic as of the old language of medicine. This I learned from attending some of their disputations. * * * Gouan's Hortus Regius Montpelliensis is a very ample catalogue, and as it is many years since he published it, and he has attended much to the science since, I hoped to have seen the collection of plants much increased; but I found his friends had not been strong enough to support him. The care of the garden has been taken from him and given to Dr Barthez, who is not only no botanist, but constantly speaks with sovereign contempt of it and all its followers. * * Gouan works very hard, and has a very large collection of plants and insects. He shewed me the Entomologia Montpelliaca in manuscript, to which he has adjoined the Physiologia Entomologica; he has likewise descriptions of the Fishes of the Mediterranean; he some years ago published a folio volume, entitled Illustrationes et Observationes Botanicæ, of which he has a continuation: there are many plates in it, and it is a useful work.

Another man of eminence as a botanist is Dr Cusson: he has of late years confined himself entirely to the class of umbelliferous plants, in which he has great knowledge. Your father knows him by his nosological inquiries. Sauvages' last class was the work of Dr Cusson, and he has bye him a complete nosology in manuscript." * * *

NOTE T, p. 458.

The two following Letters were addressed by SENAC to Dr CULLEN, as Secretary to the College of Physicians, on the occasion of his being elected an Honorary Member of that Body, in 1760.

" MONSIEUR,

Je n'ai jamais eu l'ambition d'entrer dans les Sociétés Littéraires, ni pour ainsi dire dans l'Académie des Sciences, dont je suis membre depuis près de quarante ans ; je me suis contenté de la place que j'occupe dans ce corps célèbre, mais je n'ai pas la même indifférence pour un College illustre qui cultive la médecine avec tant d'eclat ; rien n'est plus flatteur pour moi que d'être associé à des hommes qui ont attiré les regards de toute l'Europe. Faites-leur, je vous prie, mes très humbles remercîments ; ce que je puis vous assurer c'est que s'il ne falloit que des sentiments d'estime et de vénération pour mériter leur choix, personne n'en seroit plus digne que moi. Pour vous, Monsieur, dont j'ai cherché l'amitié, vous sçavez la justice que j'ai toujours rendue à vos talents ; soyés persuadé qu'on ne sauroit être plus sensible aux démarches que vous avez faites sans que je vous les ai demandées. Il y avoit longtemps que je souhaitois de rétablir notre commerce que la guerre avoit interrompu. Heureusement il nous est permis des deux cotés de nous entretenir sur ce qui peut être utile au genre humain. C'est là ce qui nous occupe l'un et l'autre, et une telle occupation ne peut jamais être suspecte. Des hommes qui ne cherchent que les secrets de la nature sont bien eloignés de vouloir entrer dans le secret des Etats. Je suis avec la plus haute estime et avec l'attachement le plus sincère, Monsieur, votre très humble et très obéissant serviteur, SENAC.

P. S.—Mon nom de Baptême que vous me demandés est Jean et non pas Pierre comme on a mis en divers écrits."

Illustrissimo viro Gulielmo Cullen, J. Senac Ludov. XV. Archiater.

Epistolam tuam accepi qua me jam apud vos non ut hospitem haberi, sed ut civitate quodammodo donatum renuntiasti. Quanti sit faciendum ejusmodi beneficium, testatur omnium gentium de vobis judicium; videbitur profecto posteris commendatione dignum eorum nomen qui inter socios vestros adscripti fuerint. Fatebor ingenue si gratum usquam ex meis laboribus percepi fructum, maxime istum de quo gloriari non erubescam. Ne credas tamen, vir ornatissime, quod tuæ de me opinioni omnino assentiri velim; cum opera quæ mihi forte imprudenti exciderunt tantis effers encomiis, et mihi et tuæ in me indulges benevolentiæ. In artem medicam symbolum suum quisque nostrum conferre debet; ego meum qualemcumque ut potui contuli; an incassum laborarim aliorum sit judicium, mihi satis rependi gratiam crediderim, si de medicinâ me nonnihil bene meritum esse existimetis. Tu autem, vir illustrissime, de meo in te studio ne dubites velim, id societatis, ingenii, et doctrinæ jure tibi licet reposcere, et ut debitum ubicumque profiteri non desinam. Vale.

NOTE U, p. 462.

CORRESPONDENCE WITH PUPILS.

Dr Cullen to Dr Mathew Dobson.

" Dear Doctor,

I think myself much obliged to you for the communication of your experiments (on the Urine in Diabetes), and you do me a great deal of honour in asking my opinion of them. A little indisposition has given me some leisure to consider them, and I give you my judgment with all the candour and freedom which friendship ought to beget.

I must say, in the first place, that I think you have made a

discovery. Many have taken notice of the sweet taste of the urine in diabetes, and though there are cases, as I know certainly, in which it does not occur, it was very wrong in any body to deny it altogether, and though, in spite of such assertions, there was little doubt of the fact, yet you have done something in putting it beyond all doubt by your experiments. This, however, is little, and you have done much more by shewing that a saccharine matter is present in considerable quantity, that such a matter is present in the serum, and therefore it must arise from a defect in the assimilating power of sanguification, if I may so speak. I think you judge properly in asserting that sugar is in some cases produced in the animal economy; but the origin of the sugar that appears, it is not necessary to explain, and it is enough for you to shew, that the saccharine matter introduced may subsist in that state longer than has been thought, and may give occasion to phenomena, the cause of which it was otherwise very difficult to assign. All this must throw light on the diabetes, and may probably suggest methods of cure more certain than those hitherto pursued, and the public must consider themselves as obliged to you for putting them in the proper train of investigation.

So far I can, with a very safe conscience, commend your work; but must now, with the same freedom, tell you what I think the faults of it." * * *

The various alterations suggested by Dr Cullen seem all of them to have been adopted by Dr Dobson in his Essay, as subsequently published.

Dr Dobson to Dr Cullen.

"Dear Sir, Liverpool, *August* 23. 1780.

As I have always considered you in the light of my medical father, I cannot determine upon any important step in life, and not make you acquainted with such determination. After more than twenty years' hard labour in my profession, and a very narrow escape, during the last winter, from a lingering and dangerous fever, I find it prudent to retire from the fatigue of bu-

siness, while I have yet some remains of constitution. It is with extreme regret that I quit Liverpool, where I have held such very valuable connections; but it must be so, or my days must be shortened. I am easy in my circumstances, do not covet great riches, and have only one child; my plan, therefore, is to pass what remains of life, in books, in conversation, and in *some employment* as a physician, for I am, from strong inclination, devoted to my profession. Bath is the place I have fixed upon, as best corresponding with all my views. About six months ago, I purchased a house there, and there I propose to finish the days of my pilgrimage through this mortal world. Wherever I am, it would afford me a very sensible pleasure to have it in my power to render any service to any branch of your family.

The step I am now taking, will I hope be of advantage to Dr Camplin, who appears to have very considerable merit in the line of the profession. September or the beginning of October will finish my residence in Liverpool. I remain with the sincerest gratitude and esteem, Dr Cullen's obliged and obedient servant,

MATH. DOBSON."

Dr G. FORDYCE to Dr CULLEN.

"MY DEAR SIR, ESSEX STREET, *October* 22. 1774.

No time, no situation, no occupation, can ever efface from my mind the obligations by which I am bound to you; the esteem, the love, the admiration, with which I hear you every where mentioned, gives me numberless occasions of happiness. I have long learnt to expect misfortune, blended with every enjoyment; but I have not as yet learnt to bear the loss of a brother without extreme pain. Give me leave to mingle my grief with yours, if that could but alleviate the distress of a father who lives in the heart of a thousand sons, and in none more than in that of

G. FORDYCE."

Dr Cullen took occasion to express his esteem for his pupil Dr Thomas Smith, in the following Latin address which, con-

formably with the usage of the University, he delivered, on that individual appearing before the Senatus Academicus to defend his thesis.

" Propter necessitudinem quam cum illo jamdiu habui, tum ingenium et sedulitas, tum profectus ejus mihi dudum comperta sunt, et nunc quidem omnibus palam facta sunt per dissertationem quam edidit et jam in manibus teneo. Hic de Actione Musculari, de re scilicet in Œconomia Animalium summi momenti, agit; et licet ob angustiam temporis qua Medicinæ toti incumbens studiosus semper premitur, omnia ab eo de hac re excogitata non hic protulerit, Thesin tamen primariam et fundamentalem stabilivit, et de ea tum acute et erudite disseruit, tum prudenter rem totam experimentis comprobavit. Si nunc more solito processurus sim, contra hanc dissertationem objectiones quasdam afferre debeo; sed neque locum his invenio, nec ut fingam mihi necessarium videtur. Certe mihi minime necessarium est ulterius probare vires candidati quem ab aliquo tempore consulendum potius quam consiliis juvandum putavi. Nec aliis necessarium esse puto ut amplius de doctrina et peritia candidati constet; pauci enim, ut opinor, nunc adsunt quibus candidatus noster et existimatio ejus ignota sunt. Comitate enim non minus quam doctrina præstans, diu inter commilitones suos pro studiorum duce, et quodammodo pro præceptore, habitus est.

Contra dissertationem ejus igitur nullas objectiones movebo. In ea quidem omnia mihi maxime probata sunt, et nunc tantum addam nihil fere felicius mihi contigisse quam quod ejusmodi discipulum nactus, etiam pro promotore ad gradum doctoralem ei adessem."

Mr (afterwards Sir) WALTER FARQUHAR to Dr CULLEN.

" SIR, LONDON, 21*st June* 1777.

A thousand times have I reproached myself for not writing to you, in obedience to your very obliging desire, when I was last in Edinburgh. I have been my own enemy, but the constant hurry attending the cursed detail of the business I am engaged in, often makes me neglect my best friends when at a

distance. I am now happy to be obliged to break the ice, and, in future, I hope I shall be so much my own friend as to be more attentive to my old master. I know a promise of amendment will obtain forgiveness from you.

Give me leave, therefore, to introduce to you Mr Seward, the gentleman who will deliver this. He has been an invalid for many years, and now wishes to consult you, and try the effects of the keen northern air. He has been all over Europe hunting after health, and has consulted most medical men of any eminence from Baron Haller down to *Michel Schuppach*, or *the Physician of the Mountain*. He has been under the care of most of the wise ones here, but I refer you to himself for his catalogue of opinions. The cure is still left for you, and I know no one under whose direction he is so likely to obtain what he has been so long in quest of.

I know it will give you pleasure to hear that I have succeeded in business beyond my most sanguine expectations, but my line will soon be too laborious for me. Some years hence, I believe I shall be tempted to resume my studies under your auspices, and try to obtain the honour of being your brother.

I must, however, walk warily, and not quit certainty for hope, as I have a numerous family. I have often wondered that London did not, in former days, attract you. You are certainly calculated for the meridian of this great world, but I am afraid it is now too late to begin a new career; still you may command £4000 a-year when you please to fix yourself in Berkley Square. It will make me very happy to hear from you; and if I can at any time be of the least service, I beg you may honour me with your commands. With the truest respect and esteem, I am, dear Sir, your very much obliged and most obedient humble servant, WALTER FARQUHAR."

Dr WILLIAM STARK to Dr CULLEN.

"LONDON, 2*d January* 1765.

* * * "Since the conference I had with Mr Fitzmaurice, I find my thoughts bent entirely towards the practice of

medicine, the study of which I will pursue with all the keenness I am capable of, till I have made myself acquainted with diseases. I shall be enabled to execute what I intend to much greater advantage, if, when you favour me with a letter, you will take the trouble barely to mention the names of those practical authors you think the most proper for me to read and consult. I have not yet entered at any hospital; and, though St Bartholomew's is above two miles from where I live, and St George's not half a mile, I will rather attend the former, both from what I have heard of Dr Pitcairn, and that I may have the advantage of being introduced to him by a letter from you. This I hope you will send soon, as there is now nothing else retards my entering. I must also beg one to Dr Hunter, whose next course, which will begin about the 20th, I shall certainly attend."

Dr (afterwards Sir) JAMES CARMICHAEL SMYTH to Dr CULLEN.

"MY DEAR SIR, LONDON, 29*th October* 1784.

As I have never yet published any thing entirely my own, I have taken the liberty of sending you a volume*, to which I have contributed, by some trifling productions. You will there find that I have quoted your name, but I hope in a manner that shews the respect I shall always have for it. If I have in any thing differed from you in opinion, it was yourself who taught me to subscribe implicitly to no authority, not even your own."

Sir J. CARMICHAEL SMYTH to Dr CULLEN.

"DEAR SIR, LONDON, 30*th January* 1786.

Delicacy, with respect to Dr Saunders, prevented me, whilst he was alive, from applying to Mr Fitzmaurice to have our late friend Stark's papers put into my hands; and, since his death, though I have frequently intended writing Mr Fitzmaurice on this subject, yet, like many other intentions, it has passed over from day to day. Your letter, however, determined me

* Medical Communications, vol. i. 8vo. Lond. 1784.

not to delay it any longer, and I have accordingly written to him by this night's post. I need not assure you that, if the manuscripts are ever in my possession, you shall have every information from them you can desire; in the mean time, I obtained of Dr Garthshore (one of Dr Saunders' executors) permission to look into them, and I shall give you the best account I at present can of what relates to the experiments on sugar.

On the 12th of June 1769, Dr Stark began his experiments on food, by living on bread and water only. His daily allowance of bread was from 20 to 38 ounces troyweight; of water from 2 to 4 pints. He continued this regimen from the 12th of June to the 26th of July, at which time the weight of his body was reduced from 12 st. 3 lb. to 11 st. 9 lb.. but without having suffered any other very material alteration either in spirits or health.

On the 26th of July he began to use sugar with the bread and water; for the first eight days to the quantity of 4 ounces only a-day with 34 ounces of bread; the six following days to the quantity of 8 ounces with 30 ounces of bread. He remarked that the sugar increased the flow of saliva; and, contrary to what might be imagined, that a less quantity of water was now required to satisfy his thirst than when he eat bread alone, 2 pints being now sufficient, whereas, with bread alone, $3\frac{1}{2}$ pints were necessary. During the whole of the fortnight, his body was loose, and he had frequently a purging upon him; but his appetite and general health were good.

On the 10th of August he began to take the sugar with reluctance, and on the 11th and 12th with so great abhorrence (as he expresses it) that on the 13th he was obliged to desist. On the 14th he began to experience other disagreeable effects of his regimen: his gums became spungy and inflamed, with ulcerations on the inside of his mouth and cheeks; the inside of one of the nostrils was likewise red and inflamed: the purging increased, and was now attended with sickness and pain in his bowels, and purple streaks were observed on his right shoulder. These alarming symptoms obliged him to quit his sugar diet entirely, and adopt a diet of animal food with wine, from the use

of which, by the 18th, the preceding appearances were in a great measure removed, and, on the 24th, he was so far recovered as to make trial of another plan of regimen or diet.

I am sorry to substitute so lame and imperfect an account in the place of his own journal, which I hope soon to have in my power to communicate to you. In the mean time, I am happy on this, or any other occasion, to shew my readiness to comply with your wishes, and to testify the respect and esteem with which I always remain, dear Sir, your obliged humble servant,

J. Carmichael Smyth.

P.S. My affection and best wishes to Mrs Cullen and family."

Sir J. Carmichael Smyth to Dr Cullen.

"My Dear Sir, London, 25*th January* 1790.

Although for some years past we have had but little personal intercourse, yet I feel my attachment and regard for you nothing diminished. Nor can I ever call to mind your friendship and goodness to me at the early period of my life, but tears of gratitude drop from my eyes. You will not then be surprised, if, amongst the number of your friends, I take the liberty of expressing my sincere concern and regret at hearing that the state of your health rendered it necessary for you to quit a station which you have filled for so many years, with infinite honour to yourself, and advantage to the public. But though your health may not admit of your public exertions, I hope it is not so far impaired as to prevent you from enjoying the society of your family and friends, and that *otium cum dignitate* to which you are so justly entitled. That this, my dear Sir, may long be your case, is my most ardent wish; and to hear that it is so, will, I can assure you, greatly add to the happiness of your very affectionate friend, and obliged servant,

Jas. Carmichael Smyth."

Dr Morgan to Dr Cullen.

"Very Dear Sir, London, *November* 10. 1764.

Can you forgive me, if, upon my being just returned from my tour through France and Italy, I write you but a very short letter till I have been here a week or two longer, and got myself a little composed. At present what with a crowd of acquaintance every day, with the kindest intentions, breaking in upon that time I proposed to devote to writing to my friends, and the chaos of ideas which disturb my regular thinking at present, I find I cannot execute the task as I ought. Every thing I tell you now must be rather broken hints, than a connected relation.

I have not been able to see M. Senac whilst last in Paris. I was at Fontainbleau once with that view, but he was then for a night or two with the King at Choisy, which I knew not of at the time; and I was too much hurried to repeat the visit, as I wanted to reach London in time enough to sail in the Fall for Philadelphia; I think I cannot now sail till towards spring.

The most agreeable incidents happened to Mr Powel and myself, in our tour, which lasted about eight months. It was crowded with a great variety of the most interesting circumstances, full of pleasing scenes for the most part, and of a nature different from, and more agreeable, than what I have been commonly used to.

The order of our travels through Italy was Genoa, Leghorn, Pisa, Florence, Rome, Naples and its environs. After our return to Rome, it was on the Adriatic side of Italy, through Loretto to Bologna, Ferrara, Padua, Venice; we took Padua in the way again on our return, and passed through Vicenza, Verona, Mantua, the States of Parma and Placentia, to Milan and Turin. We crossed the Alps to Geneva, returned to Paris through Lyons, and from thence came to London about a week ago.

We were in the suite of the Duke of York at Leghorn, Florence, and Rome, where we were particularly presented to him,

had access to all the grand entertainments made for his Royal Highness, which were indeed superbly sumptuous and magnificent. We had a private audience of the Pope, four English gentlemen of us being presented at that time. He was affable and courteous. At Turin we had the honour of being presented to his Sardinian Majesty and the Royal Family, and obtained express leave from the King to see the fortifications of Turin, and those which defend the pass into his dominions by the Alps. When at Geneva we paid a visit to Voltaire, to whom we had a letter, and were entertained by him with a most singular politeness, for us I mean, perhaps usual enough in regard to Voltaire. * * *

There is a pretty good physical, I mean medical, University at Bologna, and Morgagni has a very crowded class at his anatomical lectures at Padua. There are some other schools of medicine in Italy; but, upon the whole, to me they seem to be behind hand,—medicine not being in high repute, or cultivated with that spirit it ought to be.

As to the grandeur of the ancients, from what we can see of their remains, it is most extraordinary. Arts with them seem to have been in a perfection which I could not have imagined. Their palaces, temples, aqueducts, baths, theatres, amphitheatres, monuments, statues, sculptures, were most amazing. The soul is struck at the review, and the ideas expand; but I have not leisure to dwell now on these topics.

I must return to the world where I now am, and to the one where I am just agoing—this as different from the former, I mean the rest of Europe I have seen, as that from Italy, and really to me it does not appear more so.

At Paris I took my seat in the Royal Academy of Surgery, of which I have the honour to be admitted as a corresponding member—a distinction from a resident fellow. I am now preparing for America, to see whether, after fourteen years' devotion to medicine, I can get my living without turning apothecary or practising surgery.

My scheme of instituting lectures you will hereafter know more of. It is not prudent to broach designs prematurely, and

mine are not yet fully ripe for execution. My best compliments to all your family, not forgetting them particularly to my Mamma Cullen, and to your eldest son. Believe me to be, with the greatest esteem, Dear Sir, your ever affectionate friend, and much obliged humble servant,

JOHN MORGAN."

Dr THOMAS PERCIVAL to Dr CULLEN.

* * *

"MANCHESTER, *November* 11, 1770.

"It gives me great pleasure to hear that you are engaged in publishing a Syllabus of your lectures on the Nervous System; I shall be impatient to see it.

Drs Dobson, Bostock, Haygarth, and myself have agreed to meet, for our mutual improvement, every three months at Warrington, which lies in the centre between Liverpool, Chester, and Manchester. Glad shall I be to receive any medical intelligence from you, that I may communicate it to our Society. Our next interview will be on the 2d of December."

Dr PERCIVAL to Dr CULLEN.

"DEAR SIR, MANCHESTER, *October* 17. 1785.

I recollect with cordial pleasure and gratitude the advantages which I derived from your instructions, from your society, and from your friendship, during my residence at Edinburgh. You will therefore, I trust, excuse the solicitude I feel to introduce my son*, who is the bearer of this letter, to your notice, and to obtain for him some share of those invaluable privileges which I myself enjoyed. He has been successively a member of the Warrington Academy, and of St John's College in Cambridge; and I have every reason to be satisfied with his assiduity in study, and with the attainments which he has made in knowledge. It is my wish that he should not confine himself solely to medical pursuits at Edinburgh, but that he should avail him-

* Dr Percival's second son, James, who died at Edinburgh in 1793. See "Memoir of Dr Percival," p. clxxix; and Note F, p. cclv.

self of the opportunity he will there enjoy, of making further proficiency in other branches of science. At present he is a novice in physic, having attended only one course of anatomy and two of chemistry at Cambridge. You will, therefore, I doubt not, concur with me in opinion, that the lectures of Dr Monro and Dr Black will afford him sufficient employment, in the line of his professional designation, for the ensuing winter.

I rejoice to hear that you are happy in the enjoyment of health and spirits, and still engaged in those useful exertions which have so long and so honourably distinguished your character. With every sentiment of esteem, respect, and gratitude, I am, Dear Sir, your faithful, obliged, and humble servant,

THO. PERCIVAL."

Dr WITHERING to Dr CULLEN.

" DEAR SIR, BIRMINGHAM, 30*th October* 1779.

I took an opportunity last year to send you by Mr Stokes a small pamphlet, descriptive of a disease that then attracted much of our attention in this part of Britain *. It has since that time appeared with the same train of fatal symptoms in several other parts of England, and I have the satisfaction to find that the method of treatment, by strong vomits and diuretics, particularly vegetable fixed alkali, has been found equally successful, wherever the practice was adopted, as it was with us. Mr Stokes has favoured me with a very accurate account of the appearances after death, as he found them in a body which he ventured to dissect, but he did not escape with impunity, as he himself can tell you. The peculiar nature of the infection is, I think, fully established by the inoculation which he involuntarily suffered. He has permitted me to make use of this dissection, in order to complete the Historia Morbi, as it will appear in a new edition of the pamphlet to be published next spring.

Mr Stokes' purpose is to spare neither time nor expense to make his medical education as perfect as possible; and, in order to do

* An account of the Scarlet Fever and Sore Throat, or Scarlatina Anginosa, &c.

this, he cannot meet with a more able guide than my truly respectable friend, from whom I have received so many marks of favour, and so much instruction in former times. I say nothing of Mr Stokes' abilities or acquirements; they are sufficiently conspicuous to distinguish him from the crowd. That you, my Dear Sir, may long continue to benefit mankind by the instruction of *such* pupils, is one of the best wishes that can arise from the heart of your obliged and obedient friend*,

WM. WITHERING."

Dr WITHERING to Dr CULLEN.

" DEAR SIR, BIRMINGHAM, 19*th October* 1786.

The benefits I derived from your instructions, and the notice you was pleased to take of me at Edinburgh, are yet fresh in my memory, and gratitude for the advantages I derived from that situation, makes me happy to be the means of forwarding any part of the rising generation in the same advantageous train. The bearer of this is nephew to my excellent friend Dr Ash, whose merits in his professional line cannot be unknown to you. Mr Ash possesses abilities which will do honour to any tuition. He has taken his bachelor's degree at Oxford, and means to pass two years at Edinburgh. He has made great progress in mineralogy and chemistry, and I am sure that Dr Cullen cannot have lost his attachment to the latter, which, under his metho-

* " It was the unceasing endeavour of Dr Cullen," says Dr Withering's biographer, in the Memoir of his Life, &c. prefixed to an edition of his Miscellaneous Tracts (p. 17.), " to encourage every laudable effort. His labours in the principal medical chair, and his publications, have stamped his reputation for zeal and science; but the generosity of his soul, displayed in unceasing kind offices, can only be justly appreciated by those who had the happiness to dwell within the sphere of his benevolence. Respecting this eminent professor, Mr Withering thus expresses himself: 'His affable disposition as it were engages us in the pursuit of knowledge; his modesty is such that whatever he advances as new he cautions us thoroughly to examine, and only to embrace with 'a slow-consenting academic doubt.'"

dical genius, first took the lead in philosophy, and for the first time could, with his assistance, claim the title of a science.

The frequent opportunities I have of hearing of your health and persevering vigour, are so many occasions of pleasure to your obliged and faithful friend,

WM. WITHERING."

Dr HAYGARTH to Dr CULLEN.

" DEAR SIR, CHESTER, *June* 1772.

As I have always observed that men of superior abilities entertain a particular regard and esteem for each other, I think myself extremely fortunate in having an opportunity of introducing to you Mr Falconer, brother to the Doctor, your late pupil. You will soon find that Mr Falconer has acquired an uncommon degree of knowledge in every branch of both ancient and modern learning, and in natural history. He is very desirous of becoming personally acquainted with the men of genius in our sister kingdom, who have become so justly celebrated for their excellent publications in every part of polite literature; and, on this account, I think myself most fortunate in introducing him to one who is so intimately known, and so highly respected by them all.

Your old pupil thinks himself extremely happy in having an opportunity of returning his most grateful acknowledgments for the excellent and most useful instructions he has received from Dr Cullen as his professor, and for the innumerable kind offices he has experienced from him as a friend. Old physicians generally complain that practice contradicts the theories they had been taught to trust with implicit faith in their medical schools. The reverse of this seems likely to be my case. In the study of physic, I was very sceptical in admitting theories, and can now, with truth, observe that most of the general doctrines I then adopted, particularly those of Dr Cullen, have rather been confirmed than confuted by practice. The cause of this difference seems very obvious. I continue, with increasing respect and esteem, your most grateful pupil,

J. HAYGARTH.

Mr Falconer will give you a plan for a register of births, &c. in Chester. You will observe that no distinctions are attempted but such as a parish-clerk might understand. Any improvement you would be so kind as to communicate, I shall be glad to adopt."

Dr Haygarth to Dr Cullen.

"Most respected Professor, Chester, 11*th Sept.* 1779.

With sentiments of the warmest gratitude, I return my sincerest thanks for the favour of your friendly and philosophical remarks upon my paper concerning Variolous Contagion. A reluctance to engage unnecessarily any of your time, which is so honourably and so usefully employed, was my motive for delaying so long these most cordial acknowledgments, and a request for your farther criticisms on some alterations and additions to the Inquiry. I have taken your kind and judicious advice to study more attentively the nature of contagion, and with all the impartiality I could. Till I received the favour of your letter, I never had read any account of the Plague except Sydenham's, and one or two still more imperfect than his. I have since carefully perused most of the books and passages that you pointed out, and was not a little pleased to find that they so amply confirmed some part of the doctrine I had endeavoured to establish in regard to the Small Pox.

By the enclosed papers, you will observe that farther consideration of the subject has confirmed my former opinions, or, perhaps, you may say prejudices. However, I have here more distinctly stated the chemical arguments which first suggested the thought, and cannot yet discover their fallacy. I entreat, as a very particular favour, that you would consider and rigidly criticize the arguments deduced, both from the chemical theory and the facts.

Whether you approve or disapprove of the doctrine I advance, you may be assured that no uncandid nor improper use shall be made of your remarks; they shall be regarded as a *private* and sacred token of friendship. Since the date of the printed certi-

ficate, and the enclosed specimen of our register, many other facts have occurred that, in my opinion, clearly confirm the doctrine; but these facts I have not yet arranged. We have gained twenty, thirty, or more victories, but cannot yet boast that we have completely conquered the contagion.

I shall expect with much anxiety your remarks on the enclosed papers, and particularly I request, as a *private* favour, your explicit answer to the queries at the conclusion. Your opinion shall be regarded, as it ought, with the utmost deference; and, indeed, I cannot on this occasion express my sentiments more exactly than in the words of Pliny to Tacitus:— 'Hæc est adhuc sententia mea, quam mutabo si dissenseris tu: sed plane cur dissentias, explices rogo. Quamvis enim cedere auctoritati tuæ debeam, rectius tamen arbitror in tanta re, ratione quam auctoritate superari. Proinde si non errare videor, id ipsum quam voles brevi epistola, sed tamen scribe; confirmabis enim judicium meum; si vero erraro, longissimam para.'

I am, with the greatest respect and esteem, your much obliged and sincerely grateful J. Haygarth."

Dr Haygarth to Dr Cullen.

"Dear Sir, Chester, 10*th Jan.* 1787.

Several years ago, I was honoured with your remarks on my manuscript *Inquiry* how to prevent the Small Pox. I feel, and shall for ever acknowledge, the sincerest and most affectionate gratitude for such a favour. I am conscious how much I owe, on this occasion, to your friendship. Your criticism contributed, in a very important manner, to diminish the imperfections of my little book, but shall, as you desired, be ever held sacred and private. I hope that you duly received a copy of the *Inquiry*, of which I requested your acceptance, as soon as it was printed.

As I have no doubt that you long ago made up your mind on this subject, I hope that it will not much intrude upon your time, and the important concerns which constantly employ your attention, to favour me with an answer to one of the queries

which I had proposed, now altered into the following, or any other form you please. "Q. 2d, Do the *Rules* of *Prevention* (p. 118.) appear fully sufficient to prevent the Small Pox, as far as you can judge from facts which have fallen under your own observation, or which you have received on the evidence of credible testimony, either as stated in the *Inquiry* or by others?" Your former criticism, I solicited for my own private satisfaction. I now request the authority of your name, if you continue to think that this dreadful pestilence may be avoided by practical regulations, and if you judge that the propagation of such an opinion may be of service to mankind. We know one another too well to suppose that, on this or any other occasion, I have a wish that you should assist one word beyond full conviction. Though your entire approbation would give me great satisfaction, yet I should esteem, as much more important, the detection of doubtful facts, or false conclusions. To explain in what circumstances the *Rules* of *Prevention* are defective, would be of the greatest service to the cause, and give me the highest satisfaction.

I send a circular letter, on this occasion, to my medical friends who honoured the MS. *Inquiry* with their remarks. You will think this an unusual, and perhaps an unwarrantable, method of establishing medical opinions; but to me the cause appears important, and to justify such a measure. I need not intimate the high authority of your name, and the wonderful influence it would have, not only in this island, but in every other part of the world enlightened by literature. I hope and believe that a spirit of beneficence and philanthropy would promote societies to prevent the natural Small Pox, if the means were generally allowed to be practicable. If such institutions were successful, the best foundation would be laid for a general law.

A short answer, either public or private, as *soon* as may be convenient, would confer the highest obligation on your most respectful, affectionate, and grateful pupil,

JOHN HAYGARTH.

Please to present my best respects to Principal Robertson,

Professor Dalzel, and to Dr Duncan, with my request that he would please to insert the enclosed in his Medical Commentaries, in his list of lately published books."

Dr Haygarth to Dr Cullen.

"Chester, *October* 16. 1780.

* * *

"By Dr Jones I had the pleasure of receiving the improved edition of your Synopsis, which I perused with great eagerness and inexpressible satisfaction. He also brought a message which greatly surprised me,—that I should transmit to you any remarks I could make on the "First Lines." I would not willingly disobey the commands of one to whom I owe so many, and such important, obligations. I wish rather to incur the censure of being presumptuous than ungrateful. But it should be remembered, that when we attempt to descry specks in the sun, we mean not dark but less luminous spots. Your candour, I trust, will excuse the humble hints submitted to your consideration with all possible deference. If they should contribute to render one tittle more perfect, a work that is likely to continue long instructing future physicians in our salutary science, I shall think my character as a critic well hazarded. I always highly admired your method of arranging diseases. But the last edition of your Synopsis greatly exceeded my expectation. It is a book that will be ever in my hands, for I keep in concise Latin the cases of almost all my patients, and afterwards arrange them into genera according to your system.

* * *

Dr Rogerson to Dr Cullen.

"Dear Sir, St Petersburg, *July* 27. 1785, O. S.

You have really done me much pleasure in making me acquainted with Mr Bell, who is an agreeable well informed young man. He has gratified my curiosity in a great many things I wished to know concerning the state of medicine in

your University, and the fates and fortunes of many of my old acquaintances. It gave me pleasure to learn that your health continues tolerably good. May you long continue to be useful to the University, to your country, and to mankind!

Dr Pallas is now occupied in publishing the Russian edition of the first fasciculus of the Flora Rossica. He will continue the work with the same accuracy; and as soon as the second part shall be published, a copy will be secured for you, whether I shall happen to be in the country or not.

After perusing the enclosed, I beg you will send it to Sir Alexander Dick. He will see by it, what the view of the Empress was in collecting a number of words of the first use and necessity in a great number of different languages. The fact is, that she amused herself last year, during a long state of convalescence after her great illness, in making comparisons and tracing the analogies between the languages that were known to her. This grew upon her hands, huge materials were thrown in, and a specimen of the work I believe will be published, which will furnish food for speculation for people who are fond of that species of learning. The misfortune is, that in that kind of labour, accuracy cannot be attained. I ever remain, with the truest esteem and regard, dear Sir, your faithful and obedient servant,

J. Rogerson.

P. S. I have a young friend in your University, who has for some years been attending the medical classes; I beg you will honour him with notice and your protection. When properly qualified, I wish he should proceed to take his degree."

Mr A. Balfour to Dr Cullen.

"Dear Sir, Bath, 12*th January* 1785.

I am favoured with yours of the 6th instant, and beg leave to return you my grateful acknowledgments for the very kind interest you have taken in promoting the reputation of my cousin's treatise. The opinion you was so obliging as give me

of it, when I had last the honour of seeing you, afforded me much pleasure, which is greatly increased by this fresh proof of your approval, and leaves me not a doubt of following your advice respecting the immediate reprinting of it. Under the sanction of your approbation, I entertain the most sanguine expectations of its favourable reception from the public: and though I once flattered myself it would produce some profit to the author, I now readily submit to your opinion, and shall be fully satisfied in the hope you give me of its adding to his reputation. I therefore (fully invested by him with the power for so doing,) shall esteem myself greatly obliged by your taking the trouble of getting it reprinted in Edinburgh, from the copy which I imagine Mr Riddell will be able to furnish, or, if he should not have any, I will immediately send you one from this. I shall take the earliest opportunity of informing my cousin of your kindness on this occasion, and will answer for his grateful sense of the obligation.

I return you my best thanks for your kind inquiries after my health, which I have to inform you continues pretty good, and I flatter myself will soon be re-established. I beg you will believe me, with the highest esteem, dear Sir, your very obedient servant,

A. Balfour."

Dr Bostock, on graduating at Edinburgh in 1769, wrote an inaugural dissertation on Gout, in which he supported the particular opinions respecting the pathology of this disease entertained by his preceptor Dr Cullen. It having fallen to Dr Cullen's lot to go through the ceremony of impugning Dr Bostock's thesis previously to the degree of M. D. being conferred upon him, he took an opportunity to express the particular regard he entertained for his pupil, in the following terms:

"Arthritidem tractare ausus est Dominus Bostock; opus sane arduum et difficile agressus est; diligenter tamen et ingeniose rem confecit. Quo natura morbi hactenus obscura melius elucescat, nova quædam præferre non dubitatus est, et hæc conamina apud omnes etiam laudata fore, facile sperat; sed in hac re, ni ego multum fallor, ille fallitur. Juvenis adhuc ille nescit tur-

bam medicorum, nec ingenio præditam nec doctrina excultam, nova omnia quæ intelligere non possunt prudenter satis aversari; nescit etiam multos esse, tum ingenio tum doctrina eminentes, qui tamen, sibi ipsis confidentes, ab aliis doceri nolint, et multo minus quædam dediscenda fateri velint. Hæc juvenem ipsum nova quæque avide inhiantem, nescisse, non miror; sed D. Bostock meo lateri jam adeo diu hærentem, ea nova quæ *ex meis dictatis* præcipue hausit minus libenter excepta fore, omnino vel nescisse vel prætervidisse, multum miror. Et dum meos conatus in hac re adjuvare et ulterius etiam pergere velit, timeo ne ille, et ego etiam, quorundam censuræ gravissimæ expositi simus; timeo ne me solas theorias crepare, ne illum solas theorias a me discere voluisse, dicant. Sed si fortassis ita dicant, candidatum egregium, ut opinor, minime perturbare debent. Si theoriam enim tractare velit,—etiamsi audacter magis quam ego vellem theorias protulerit,—etiamsi in quibusdam hallucinatus fuerit,—ei et impetui ejus juvenili omnino ignoscendum esse puto, cum bene novi illum theoriis suis non admodum fidere, nec alia praxeos fundamenta neglexisse. In hac ipsa Dissertatione illum non magis ad theoriam quam ad ea quæ natura ipsa ex ore Sydenhami dictaverit respexisse video. Quantam in studio practico operam posuit, norunt condiscipuli ejus omnes qui viderunt quot et quantos labores in Nosocomio exantlaverit, dum collegæ amicissimo et mihi assiduus comes et adjutor egregius, ipse praxin medicam penitus ediscere voluit, nec ex dictatis nostris sed noctu diuque ad lectos ægrotantium assidens, a natura ipsa quid faciat aut ferat noscere voluit. Hujusmodi candidato theorias quasdam ignoscendas esse nullus dubito. Sed de his satis."

"This amiable young physician," says Dr Withering's biographer, in alluding to Dr Bostock (p. 13.), "had scarcely settled as a practitioner in Liverpool, married advantageously, and become possessed of a son (who has amply enlarged the dawning reputation of his father), than he sank beneath an incurable disease, himself predicting the fatal termination, calmly resigning the sweetest blandishments of life, and in his last moments emulating the exit of a Socrates or a Seneca."

The mournful intelligence of his death was communicated to Dr Cullen by his friend Dr Dobson in the following note, dated Liverpool, 13th March 1774.

" I am just now returned from paying the last sad tribute to the remains of poor Bostock. I have accompanied him to his long home.

Since he fixed at Liverpool, he has had several attacks upon his bowels. During the fatal one, there was a great degree of fever, a general inflammatory disposition, but no fixed local affection. The aids of art were ineffectual; and early on the sixth day, he passed into ' that undiscovered country from whose bourn no traveller returns.'

He was perfectly sensible of his approaching dissolution, and though surrounded with every thing which could make life be wished for, he quitted it with astonishing resolution. About nine hours before his death, he said, ' Dobson, you see it is over with me, you must promise not to leave me till I am numbered with the dead; write to Cullen, and take care of my son.'

Two of these requests I have fulfilled, and I trust I shall not be inattentive to the last. From your afflicted, dutiful, and affectionate, MATT. DOBSON.

I was very happy to hear that your son was recovered from his very dangerous indisposition. Please to remember my respects to Mrs Cullen."

SIR CHARLES BLAGDEN to Dr CULLEN.

" GOWER STREET, BEDFORD SQUARE,

DEAR SIR, *July* 5. 1785.

As one of the most curious and important articles of philosophical news which it is in my power to send you, I enclose the copy of a paper by Mr Cavendish, lately read before the Royal Society, and now printing in the Transactions, where he shews that the phlogisticated part of our atmosphere is convertible into nitrous acid by means of dephlogisticated air. Whether it be according to the theory Mr Cavendish has here adopted, that phlogisticated air is nitrous acid saturated with

phlogiston, or according to M. Lavoisier's theory, that it is a simple substance, which, joined with dephlogisticated air, produces nitrous acid, cannot be determined, till the question now so warmly agitated relative to the existence of phlogiston shall have been decided; but Mr Cavendish's discovery is of equal importance, whichever of the two *systems*, Stahl's or Lavoisier's, obtains the preference. As phlogisticated air constitutes near four-fifths of our whole atmosphere, the source of the nitrous acid now remains no longer a difficulty.

Dr George Fordyce has probably transmitted to Edinburgh some topics of the experiments by which he means to prove that heat, whether sensible or latent, diminishes the gravity of bodies. These also will appear in the next part of the Philosophical Transactions.

I have received from M. Berthollet, one of the most promising chemists in France, an account of a series of experiments, by which he shews, I think very satisfactorily, that the volatile alkali consists of phlogisticated air united to inflammable air. This analysis, though indistinctly perceived long ago, is now, upon being fully established, to be considered as a real discovery, and of the most important kind. M. Berthollet makes the proportions of the two airs to be, six parts of phlogisticated, and one of inflammable.

This letter will be delivered to you by one of those gentlemen whom the King of Spain, in the most liberal manner, is sending for instruction to all the more enlightened parts of Europe. He means to spend two or three years in Edinburgh, to learn the profession of physic: his name, which is Luzuriaga, may probably not be unknown to you, on account of a paper he published lately in the Journal de Physique, on the air obtained from calces of lead, &c. These circumstances, I am sure, will sufficiently recommend him to your protection. I am, with the greatest regard, dear Sir, your faithful humble servant,

C. Blagden."

Dr W. Falconer to Dr Cullen.

"Dear Sir, Bath, *June* 8. 1777.

I have already repeated many of Dr Priestley's experiments on air, and find them extremely faithfully related. I have myself constructed a machine to measure the goodness of the air, according to Priestley's plan, and find it answer exceedingly well, and reducible to a standard. It measures the diminution to $\frac{1}{72}$d part, very easily, and may be made on a more extensive scale. If you choose it, and will direct me how to transmit it, I will send one to you, and another for Dr Black, if he likes it. I have accidentally discovered some odd phenomena in the branch of aërial philosophy, one of which is, that the nitrous acid in vapour and mild volatile alkali may be united together, without separating the fixed air, which, however, happens immediately on the neutral salt being put into water. The nitrous acid, also, in vapour, unites with the volatile alkali into a neutral, of a mild smell and taste, but more volatile than the alkali itself by far, insomuch as to exhale copiously in form of a white cloud. This Priestley observed in some measure, but he only remarked it, while the alkali remained in the nitrous air; but I find, if the alkali remains any time in the nitrous air it will unite into a compound with it, the most volatile in the open air of any saline compound I have seen.

W. Falconer."

Mr Benjamin Bell to Dr Cullen.

"Sir, London, 30*th March* 1772.

I confess that it ought to have been my duty, as much as it was my inclination, to have wrote you long before now, and that merely by way of return for the many civilities I had formerly received from you; for, being the only acknowledgment that I had in my power to make, it might readily be expected that I should not so long have omitted it. My only reason for doing so was, that I seldom met with any thing worthy of your observation, at least so far as to excuse the freedom of

troubling you with it. Nor should I at present have ventured upon it, but that the time of my return drawing near, I found, if I did not set about it, that it presently would be too late. Amongst other obligations which I lie under, that of your letter of introduction to Mr Hunter I consider as none of the most trivial, for by it I have had the pleasure of a most agreeable, and at the same time the most useful, acquaintance I ever met with; for there is scarce an article either in physic or surgery that Mr Hunter has not something new upon, and there is none more ready of communication than he is. He has frequently desired his best respects to Dr Cullen, and which now, for the first time, I deliver. I am likewise just now attending Dr Hunter's anatomical course, which indeed is extremely ingenious and satisfactory; but the Doctor is by no means so free or so ready of access as his brother.

There is not much new in the medical way here at present. Dr Hulme has lately published a treatise upon Puerperal Fever, which by this time you will probably have got down to Edinburgh. The Theory and Practice of Physic by Dr Macbride was published two days ago, but I have not seen it. I was present at the last meeting of the Royal Society, when a very ingenious paper upon the nature and properties of air by Dr Priestley was read, and which I hear will soon be published. He adopts Dr Macbride's theory of fixed air and putrefaction, and mentions the effects of such air in putrid diseases. One very bad case of putrid fever was particularly related, which seemed to be cured by it, and after every other remedy had been unsuccessfully made use of. The air was injected in considerable quantities by the anus; and from its very first application the patient seemed to be relieved. A number of other very curious experiments were likewise mentioned. It is generally allowed, and indeed now sufficiently proved, that any given quantity of air can soon be spoiled, or rendered unfit for the purposes of life, by one or more animals frequently breathing it in a confined place. Now, in course of time we should be apt to suspect that the whole atmospheric air would be so tainted, by the breathing of such a number of animals as are every where met with, as at last to

become equally poisonous with that met with in confined places. Dr Priestley, however, imagines that Nature has particularly guarded against this, and for which purpose he thinks it probable that she has several resources; and one of them he has been so lucky as to discover. Mephitic air being confined with growing plants, is not only thereby entirely changed, and again rendered pure, but the growth of such plants is likewise very considerably quickened. From this and other experiments to the same purpose, he concludes that the bad effects of the breathing of animals upon the atmosphere is counteracted (in a great measure at least) by the plants which are everywhere found upon the earth, and that such air is even, in some degree, necessary for the proper production of these plants.

About six months ago, there was published in Sweden an account of the discovery of a *new acid*, which had lately been made there by a famous chemist, *Scheele* of Stockholm. As I was very desirous to know the particulars, I got a Swedish acquaintance who took the trouble of making me an extract of the principal parts of the publication, and afterwards translated them. This you may, perhaps, have already seen; but lest you should not, I shall now beg leave to enclose a copy of it. The discovery may, perhaps, appear to you as trifling, but its being entirely new, to me at least, I hope will excuse my mentioning it; and as by this time you will probably think I have said enough, I shall, therefore, beg leave to conclude, and am, Sir, with great respect, your much obliged humble servant,

BENJ. BELL."

Dr RUSH to Dr CULLEN.

PHILADELPHIA, *December* 22. 1784.

" MY DEAR AND VENERABLE FRIEND,

I want words to convey to you the pleasure I derived from your very friendly letter by Mr Dobson. It has revived in me all that enthusiasm for science with which you inspired me in the years 1766 and 1768. I shall be unhappy till I receive your latest edition of your Nosology and Materia Medica; and

I shall not cease to pray that you may not only live to finish your work upon "the art of preserving health," but that you may stamp a value upon it that shall ensure (not its sale only), but its immortality, by living till you are an hundred years old, and much longer, if it shall please God to continue to you your powers of usefulness and happiness.

From seeing your name prefixed to the dedication of Dr Wilson's treatise upon the influence of climates upon vegetable and animal bodies, I was induced to read it. The Doctor has mistaken the influence of woody or uncultivated countries upon health in the conclusion of his work. The new settlements in every part of the United States, but more especially in Pennsylvania, are always healthy. Funerals are such uncommon things in our new countries, that they are resorted to as a kind of spectacle. A girl in an intermitting fever, 200 miles from Philadelphia, drew a crowd of people about her from many miles distance, on purpose to see whether it was possible for a person to be hot and cold at the same time. Her tremors threw them into consternation. It is only in proportion as these new countries are cleared of their woods that they become sickly, in consequence probably of a more free passage being opened for the propagation of exhalations from the rivers. On the River Susquehannah, 100 miles from Philadelphia, fevers, twenty years ago, were unknown one mile from the shore. They now extend above ten miles from the river, owing to the country being more cleared. It has been remarked further, that, in proportion as a country is improved by draining and the different species of agriculture, it becomes again healthy; so that we have been able to establish the two following propositions, viz. "that clearing a country makes it sickly," and that "cultivation makes it healthy."

The city of Philadelphia stands between two rivers, viz. the Delaware and Skuilkill. Before the war a wood grew between the city and the latter river. While the British army lay in Philadelphia, they cut down the whole of that wood for fuel; in consequence of which bilious, remitting, and intermitting fevers, have increased in our city in the ratio of five to one, com-

pared with years before the war. This has been ascribed to the loss of those woods which formerly intercepted the exhalations of the Skuilkill.

Some observations that occurred to me during my attendance upon the American military hospitals, have led me to dismiss opium as a remedy in the cure of the tetanus, and to substitute in the room of it large doses of bark-wine, with occasional doses of oil of amber, with great success.

I have always found tetanus from wounds connected with an *absence* of inflammation, and have therefore excited it by scarification, or by pouring irritating substances into the wound. The cases that have issued favourably under the above treatment have been carefully recorded, and in due time will be laid before the public.

Your First Lines accompany population and government in every part of this western world. An edition of your last volume is now in the press in this city, and will be published in a few days. I hope his Britannic Majesty will not hear this, otherwise your salary as his physician in Scotland will be in danger; for he ought, in justice to his former principles and conduct, never to forgive the man that has taught his once ungrateful subjects the art of restoring health and prolonging life—perhaps for the purpose of employing both hereafter in lessening his own power.

With respectful compliments to Mrs Cullen, I have the honour to be yours most affectionately,

Benj. Rush."

Dr N. Dimsdale to Dr Cullen.

" Dear Sir, London, *Red Lion Square, April* 25. 1781.

My father sets out on a journey to Russia in about a month, to inoculate two children of the Grand Duke, and he is desirous to publish a treatise on Inoculation, which is now in great forwardness, before he goes.

If my memory does not deceive me, I think it is customary in some parts of the Highlands to inoculate, by tying dried scabs

on the wrist of the person who is to receive the infection; he is of opinion, if this is the case, it must be slower in producing the disease than when matter is inserted in the usual way by puncture; and he would be very happy to have this point cleared up, and to know with certainty the time that intervenes, by that mode of inoculation, between the application of the dried scabs to the wrist and the access of the eruptive fever.

I have taken the liberty of making this application to you, from a conviction that no person is so able to give me the best information on the subject. I know, however, that your time is always so constantly and usefully engaged, that it is far from my wish to give you the trouble of making inquiries; but if you can from memory assist me, I shall be greatly indebted to you for the honour of a line. The part of the publication in which this matter will be considered is the last, so that any information you may be so obliging to give will be received in good time.

When writing to you, Sir, I cannot but wish to convey to you the sense I retain of the many favours I have received from your friendship and politeness; but I can truly assure you, I am unable to express the gratitude I feel for the repeated civilities I have received from yourself and family. I beg leave to present my most respectful compliments to Mrs and Miss Cullens, to Mr Robert Cullen, and the rest of your family, and to assure you that I shall ever remain your much obliged, obedient servant, N. B. Dimsdale.

My father, and brother Dr Jo. (who is well), desire to join in respectful compliments to yourself and family."

Sir Gilbert Blane to Dr Cullen.

"My dear Friend and Master, London, 19*th Oct.* 1783.

It has not been want of gratitude or regard that has prevented me from testifying my duty to you during my absence in different parts of the world. No information or entertainment I could have communicated would have been sufficient to pay the debt I owe you in these articles, and neither your time nor mine would admit of the intercourse of ceremonial civility. I

know how precious your time is, and therefore absolve you from making any answer to this, which is merely to assure you of the remembrance and esteem of one, who owes the little credit and good fortune he has had in the world to the seeds that you planted, and the principles you instilled.

I am happy to learn that you are still in the enjoyment of good health, as well as honour and prosperity; and that it may long continue, is the very sincere wish of, dear Sir, your much obliged, and most affectionate friend and servant,

GIL. BLANE.

I beg to be kindly remembered to Mrs Cullen and all your family."

NOTE V, p. 464.

Dr FOTHERGILL to Dr CULLEN.

"NEAR MIDDLEWICH, CHESHIRE,

"MY HONOURED FRIEND, 21*st September* 1771.

This, I expect, will be delivered by my brother Alexander Fothergill, who has undertaken this journey to Edinburgh at my request. My friends in London, not only of our own persuasion but others, are highly dissatisfied with the award against me, and insist upon my endeavouring to vindicate myself from the stain of defamation, and to such an amount as to require so large a restitution as £500.

I have yielded to their importunities; and, conscious of my innocence, both in act and intention, I am in hopes of giving ample proofs of it.

I ask nothing from the Faculty. My proofs will be of another kind; my opponent did not, he could not, perform any of the usual exercises expected from the Candidates for Degrees. Should proofs of this kind be laid before you, your own honour is engaged to disavow the impostor, not for my sake, I ask it not, but for your own. To be mistaken or deceived is not a crime. It happens to individuals, it happens to bodies, and is excusable. But to persevere in a mistake, and countenance im-

posture, makes those who do so partners in the guilt. A man who is capable of sitting for months together attending lectures in a language of which he knew not a sentence, and make notes of it at the same time, is surely a first-rate genius. The Professors could not suspect that such a case could possibly happen; but if it is proved to have happened, will it turn most to the credit of those who have been imposed upon to conceal or to disclaim the imposition?

I have wrote to Dr Monro on the same subject, and likewise to Dr Hope. You will please to confer together, and consult the credit of our Alma Mater. I have wrote to a gentleman of Cambridge to procure me, if he can, the usual form of their suspensions, and on what occasions they are principally issued.

I have enclosed a letter from Dr Watson in London, which I received yesterday, together with a very polite letter from the man in the world, except Leeds, from whom I should least of all have expected it, Sir William Browne, mentioning the motives that induced him to propose me to the College as a Fellow, which were much in my favour, so that his motion, I believe, was honourably intended, though it miscarried. I am very glad it did so; for if they would not have given me my proper rank and standing I would not have entered the College. I am much better pleased to remain where I am; and I will endeavour to act in such a manner as to afford no disgrace to the corps I am in, nor the corps itself to the profession.

At leisure moments, and they have been extremely scarce with me, I have looked over your Pharmacopœia, and taken more liberties in retrenching than perhaps I ought. I could wish to have had more time, and been at home, but these are impracticable. I have wrote three or four hours almost every day since I came down, for other people not myself. MSS. on one subject or other were put into my hands, by my friends when I left London, and some I have been obliged to rebuild from the foundations.

As I received the request to look over the Pharmacopœia from Dr Boswell, I could not refuse sending him the short notes I had made, which I have sent in a packet addressed to him, to my brother's care.

Be kind enough to make my acknowledgments to the gentlemen of the Medical Society for their very polite letter. I hope to acknowledge it myself when I return to London. I am, with great respect, Dr Cullen's assured friend,

JOHN FOTHERGILL."

Dr FOTHERGILL to Dr CULLEN.

" LONDON, 15*th October* 1771.

* * * *

My brother succeeded so well as to get an ample affidavit from J. Brown, which, with the evidence I have obtained from other quarters, will, I hope, enable me to justify myself, whether my opponent thinks fit to lead me into Westminster Hall or to bring the matter again before our own people, from whom I have every thing just and reasonable to expect. And it is not more to be wondered at that he prepossessed three weak, though honest, men in his favour so strongly as to render them deaf to all reason or force of evidence, than that he should have had art enough to obtain a degree from gentlemen so eminent for their knowledge and abilities.

It hurts me not a little that the affair has already been so widely published. But the circumstances unfortunately rendered it impossible to conceal it. The gross ignorance of Leeds, the injustice done me on his account, the partial decision in his favour, all contributed to interest a multitude of persons in the event. The University will be the greatest sufferer. Should a professor of any eminence appear at Leyden, the Americans would flock thither, and others would follow them. This may be thought to be the language of resentment: it is not. Experience will demonstrate the reality of this suggestion. As I have not time to write at present to Drs Hope or Monro, or any other professor, be pleased to acquaint them, as occasion may offer, with the account I have given of the two young men who are the bearers of this, and introduce them as they may appear worthy.

The College here, I find, have enacted some new by-laws, not

upon the whole more liberal than the former. A person who has studied at *any* University two years, may, if found properly qualified, be admitted a licentiate. After standing on this list seven years, he may offer himself as a candidate to become a Fellow of the College. He must then be examined in Greek, *ad aperturam libri*, in Hippocrates, Galen, and Aretæus, besides his other exercises; and, after all, be ballotted for. No person practising midwifery is to be admitted, be his qualifications what they may.

We shall take no steps at present, but endeavour to keep ourselves together, and watch opportunity.

Perhaps it may not be improper just to mention, that I procured inquiry to be made in the University of Cambridge, whether they ever degraded any graduate, on what account, and in what manner. I find they have degraded several, but chiefly on account of immorality, or flagrant misbehaviour, or infidelity: none on the account of fraudulently obtaining a degree. It may, however, be worth while to consider if a degradation may not be justly founded on the legal proof being adduced that Leeds's exercises were not performed by himself, which he had produced to you as his own; for he did not perform any one of those exercises which he exhibited. But I shall say no more on this subject than that the University is more concerned in it than they are aware of. Whether he is degraded or not, my defence is the same. Believe me to be, with great respect, Dr Cullen's, and all the Professors', assured friend,

J. FOTHERGILL."

The following is the letter from Dr Watson alluded to by Dr Fothergill, in his first communication to Dr Cullen.

DR WILLIAM WATSON to DR FOTHERGILL.

"DEAR SIR, LONDON, 16*th September* 1771.

I wrote to you a few days since to inform you of what I had learned of the transactions at the College of Physicians on

that day, since which, I have had more information on that subject; and as I have reason to believe that you will not be displeased to be acquainted with the events of that day, I send you this further account. I mentioned to you that, in the interval between the repealing the old statutes and enacting the new ones, Drs Webb junior, Donald Monro, Grieve, and Schomberg, were elected Fellows. This was not done without much grumbling from some of the Fellows present. Dr Reilham said, he had heard of a drag-net to bring in Licentiates; that he now saw there was a drag-net for Fellows. Dr Warren proposed Dr Webb, and Dr John Monro his old friend Schomberg. I am not certain who proposed the others. To the surprise of every one present, Sir William Browne rose and proposed you, and, as I am told, said handsome things of Dr Fothergill. *An propter invidiam, an propter amicitiam ambigitur.* In his speech or manner there seemed nothing ironical; it was rather, as Dr Heberden says, in serious sadness. As you are an arch-rebel, this was not expected, and every body stared. However, after a considerable pause, both Dr Heberden and Sir John Pringle rose, and seconded Sir William's motion. Upon the ballot there were thirteen negatives and nine affirmatives. Dr Reeve was obliged to go away before this unexpected vote and some others were put, otherwise, he told me, you would have had ten. Among others, I know Dr Thomlinson voted for you. Your defeat, however, was glorious to you, in having so many votes, even when the resentment occasioned by the lawsuit was not subsided, and your sins were fresh. Had the ballot determined in your favour, Dr Heberden told me that he intended then to have proposed me, and mentioned to his neighbour to have seconded him; but, upon seeing the sense of the Fellows towards rebels, he declined it.

The new statutes are now, for the time at least, completed, and are, upon the whole, more liberal than I could have expected, considering the number of Fellows there are who were for making no relaxation in their former powers. The necessity of obtaining degrees from Oxford and Cambridge is now at an end, an admissibility to licentiates of seven years' standing, though

under the new Greek examination, being granted. But from these are to be excepted men-midwives, who, by these new statutes, can never be candidates for the fellowship; and so are physicians practising as apothecaries, who, even if admitted fellows, are liable thereby to be expelled. I have seen none of the men-midwives since this determination, but I know it must be highly disagreeable to them. This lessens vastly the number of at present admissible licentiates, as all below Dr Garthshore are less than seven years in standing.

The statute precluding any one's being examined as a licentiate without having been two years at some University, previous to his taking his degrees, arose originally from Sir John Pringle. He did it to prevent the scandalous prostitution of some of the Scotch Universities, and, therefore, moved that the graduates should have been resident at least one year before they should offer themselves to be examined. Dr Thomas moved for two, and Sir William Brown for four, years' residence. The enemies to relaxation all joined in forwarding Sir John Pringle's motion, and afterwards Dr Thomas's, and it was carried for Dr Thomas's. He spoke a great while upon the subject, and mentioned that both the Court of King's Bench and the Licentiates called upon them for it. The former had, he said, desired that a medical education should be insisted on, and had declared that though the College had been careful in the admission of Fellows, they were shamefully negligent in that of Licentiates, and that the Licentiates themselves were ashamed of many of their brethren, and thought them brought there to disgrace them. This, therefore, though with many dissentient votes on the side of our friends, has been made, and is now, a statute.

I must not forget, that when the statute to prevent the admission of men-midwives to be Fellows was forming, Sir John Pringle moved, that the man-midwife to the person of the Queen should be excepted; but whether or no Dr Hunter's sins were too many to be pardoned at present, certain it is the motion was rejected by a great majority.

Dr Higgins has, in my opinion, been, not in a slight degree,

the occasion of the College so soon finishing these statutes. He was to have his answer on Michaelmas day; and now there is a statute directly against him. They will not examine him as a candidate for the fellowship, as having no degree from our Universities, and not being a licentiate of seven years' standing. He must be content to be examined as a licentiate or not at all. He hardly, I should think, would venture singly on a lawsuit with the College on this ground."

Long previous to the date of the transactions referred to in these letters, Dr Cullen's attention had been directed to the practice pursued by some of the Scotch Universities, of conferring medical degrees in absence, by the following letter, addressed to him while at Glasgow, by his friend Dr William Hunter.

Dr Hunter to Dr Cullen.

" Dear Sir, London, 1*st October* 1754.

I understand that my friend Mr Patoun applies to you at this time for his degree of Doctor in Physic, and that you require of him certificates of his medical studies. I therefore take this opportunity of acquainting you, that he has done me the honour of a diligent attendance upon my anatomical lectures; and I should say a good deal more about him, if I did not know that other certificates have been sent with this, and that he is particularly known to you and your brother professors.

Your great caution in conferring degrees gives me the highest pleasure, as I have a regard for the honour of my country in general, and in particular for the College of Glasgow. You no doubt know how contemptuously the College of Physicians here have treated all Scotch degrees indiscriminately. You must have heard what efforts have been made, and are now making, by our countrymen in this place to support the honour of our Universities; and you can hardly be ignorant that, notwithstanding this, the professors of one Scotch University at least, shamefully prostitute their degrees still to any one who

can pay them a small sum of money, and procure, perhaps purchase, a recommendation from some necessitous doctor. There is something so mean, and so unbecoming gentlemen in this, that I dare not say any thing further upon it, for fear of writing with unbecoming warmth. I will rather take the agreeable side, and thank you and the College of Glasgow for acting so opposite a part. I have had a long conversation with Dr Ross and Dr Clephane of St George's Hospital, with Dr Pitcairn of St Bartholomew's, with Dr Macgie of Guy's, and some other Scotch physicians of eminence in London, who are all greatly pleased to hear of your circumspection, and who desire me in their name to thank you and the College of Glasgow on this occasion.

But, after all, we are very inclinable to think that the prostitution of degrees, which we have so much reason to complain of, and which is indeed sometimes with justice, but often insidiously, thrown out against all Scotch graduates here, does not appear in that light to the authors of this evil (I mean, to say it frankly, the gentlemen of Aberdeen and St Andrew's), who may be influenced by good nature, and abused by some people here, who pass by the name of Brokers of Scotch Degrees. We therefore wish that you would be so kind as to co-operate with us, to bring about, if possible, some general resolution of the Universities in Scotland with regard to degrees. You may be assured that our mutual interests and reputation are equally concerned in this affair. For whatever will serve to remove that load of reproach which the graduates of Scotland are oppressed with in the metropolis, as it will open to them a fairer road to credit and honour in their profession, so it must reflect back proportionable credit and honour to the several sources of their education.

I am, Dear Doctor, always most sincerely yours,

WILLIAM HUNTER."

NOTE W, page 493.

Dr Barlow, in observing that "a reunion of surgery with physic, though called for by every dictate of experience and common sense, has yet never been attempted," remarks: "For this, many causes may be assigned. The profession has insensibly settled into subdivisions, which, sanctioned by time, and interwoven with public habits and prejudices, acquired a prescriptive authority, which it would have been deemed almost sacrilegious to dispute. It was supported, therefore, in its forced and unnatural constitution, by all who hold mere antiquity in reverence; and they who are thus influenced are ever a numerous body. More inquiring minds, deluded by the advantages resulting from the division of labour in the mechanical arts, were misled into a persuasion that the distinctions in physic must have sprung from some similar influence, and gratuitously admitted what they were at no pains to investigate. Had they traced these distinctions in their causes and in their effects, they would soon have discovered that neither evinced the slightest connexion with the principle which had dazzled their imaginations. In no inherent necessity for subdivision did these distinctions arise; no public or professional good has been attained by their maintenance; on the contrary, it can be clearly shewn that their existence has been prejudicial both to the public welfare and to the progress of science, and that it has been solely owing to the public will, exercised in defiance of corporate rights and legislative prohibitions, that injury far more signal has not resulted *."

The practical impossibility of maintaining a broad line of separation between the different departments of the healing art, was very fully recognized by the House of Lords so long ago as the year 1708. In an action raised by the College of Physicians against an apothecary, for furnishing a patient with advice and medicines, the Court of Queen's Bench had determined that "it is the sole province of the physician to judge of the disease and its

* Edinburgh Medical and Surgical Journal, No. xxviii. p. 337.

nature, and of the constitution of the patient, &c. to judge of the fittest and present remedy for the disease, and to direct and order its application; and that the proper business of an apothecary is to make up and compound, or prepare, the prescriptions of the doctor, pursuant to his directions. In this decision it has been remarked, the Judges were unanimous, for the law was clear; whether they thought it good or bad, they had no option. But the jury, we are told, would have given a verdict in favour of the apothecaries, but for the charge from the bench. Whether the House of Lords, who felt authorised to judge according to equity and justice, rather than the strict letter of the law, saw the matter in its true light, and considered it a hardship that the public, whilst they were prevented by the college monopoly from having a sufficient supply of medical advice from the usual and ordinary sources, should also have all other sources of medical aid shut against them; or whether they were convinced by the statements of the apothecaries, into whose hands nine-tenths of the practice of physic had long actually fallen; certain it is, that they reversed, and very properly reversed (since they did not think fit, which would have been still better, to abolish the college monopoly) the judgment of the court of Queen's Bench *."

NOTE X, page 494.

The detail of the successive prosecutions raised by the College of Physicians in London against graduates of universities, surgeons, apothecaries, and unlicensed empirics, for trespassing on their privileges, by practising physic in London, or within seven miles; and of their contests with their own licentiates respecting their rights to participate in the privileges of the Corporation, presents a melancholy record of ignoble compromises, of inglorious defeats, and of victories, if possible, still more inglorious. Of the earlier of these prosecutions it has been justly remarked, that "unauthorised practitioners were deemed worthy

* Exposition of the State of the Medical Profession in the British Dominions, 8vo. London, 1826, p. 112.

of punishment or otherwise, not as they killed or cured his Majesty's subjects, but as they were insolent or humble to the College." "In cases in which there ought to have been only prosecutions for murder, supposing the informations laid to have been true, fine, submission, and the intercession of persons of quality, were deemed sufficient satisfaction and atonement;" and "a royal recommendation was, with respect to medical competency, more than equivalent to a diploma from a university*." And in what, it may be asked, have all these prosecutions or persecutions ended? in this, that, in the present day, the quacks are unmolested by the College, and never were so few in number; the apothecaries have become, in a very considerable degree, the patrons of the fellows and licentiates of that body, and the surgeons their rivals in the practice of physic; and it is only from University graduates that the College can now pretend to demand homage and tribute.

NOTE Y, page 504.

In another introductory lecture to his Course on Chemistry, Dr Cullen observed to the same purpose:—"The knowledge of the structure of our own bodies cannot fail to excite our curiosity. The study of the human economy is certainly to us one of the most interesting. But, further, if any part of the knowledge of nature can engage a young man's attention, the study of medicine, which leads to the study of all nature, must certainly attach him very strongly. That physic leads to the study of all nature, a few obvious considerations will immediately discover to you. It is plain that the human economy cannot be well understood, unless we study also that of other animals. The history of animals, therefore, becomes necessarily a part of the study of physic. Again, the remedies of diseases are taken from a great variety of vegetable and mineral substances. Those fit to be employed must be accurately known and carefully distinguished, and this necessarily leads to the general history of all vegetables and im-

* Exposition, &c. pp. 41, 43, and 44.

nerals. The history of all nature, therefore, is necessary to a physician. This history is certainly entertaining, but perhaps not enough so for a student of medicine. Simple facts, or even a variety of objects, may not be sufficiently engaging to an active mind, which always requires the exercise of speculation. But for such also, physic furnishes sufficient employment. A physician must not be content with distinguishing the productions of nature; he must aim also at finding the causes of most of the phenomena in the natural world. He must, in short, carefully study the principles of natural philosophy, which afford employment, and will furnish entertainment also for the most speculative mind. To come a little nearer to our purpose, natural philosophy consists of two parts, the one of which explains the phenomena depending on the general properties of matter, and is called the mathematical or mechanical part of natural philosophy. The other part explains the phenomena depending on the particular properties of bodies, and is called the chemical branch of natural philosophy. The mechanical philosophy is sufficiently engaging, by the pleasure it affords and the advantages it promises. It seems, however, at present, to be less attended to by students of physic than it deserves. It would be easy for me to convince you that it is very necessary for a physician, but this is not so much my business here as to say that the chemical philosophy is still more so, and to say likewise, that it is this necessary part of physic that is also one of the most entertaining."

NOTE Z, page 505.

To effectuate the important object of obliging medical students to enter upon the different branches of their professional education in what may be called a *natural* order of succession,—without extending the course of medical study to a period of six or seven years,—would seem to require the adoption in this country of the practice pursued in many of the Continental Universities of having two sessions of five months' duration in the course of the year. The two intervening months would af-

ford ample time to the students for relaxation; and, if necessary for the accommodation of the professors, the order of the classes might probably be arranged in such a way that no professor would be obliged, unless he were so inclined, to lecture during more than one session in the year.

During the *first* session, the student might be instructed in the Elements of Chemistry and in Descriptive Anatomy; during the *second*, in the Physiology of the Human Economy and in the Natural History of the three kingdoms, Zoology, Botany, and Mineralogy; during the *third*, in General Pathology and in Practical Anatomy and Chemistry; during the *fourth*, in Therapeutics, or Materia Medica, and in Surgery, with a second course of Descriptive Anatomy; during the *fifth*, in the Practice of Physic and in Clinical Surgery; during the *sixth*, in Midwifery and Clinical Medicine; and, during the *seventh* and *last*, in Medical Jurisprudence, with a repetition of the Practice of Physic and Clinical Medicine. This arrangement would leave ample room for attendance, at the proper periods, on Courses of Lectures on Logic, Natural Philosophy, and Moral Philosophy, where these lectures had not been previously followed, as well as for a repetition of attendance on any of the medical classes, according as inclination or occasion for it might suggest.

NOTE A A, page 508.

The intelligent reader cannot fail to remark how wide a difference exists in the spirit in which Dr Chalmers urges his arguments in favour of improving the general education of the members of the learned professions, and that in which the restriction of the fellowship of the College of Physicians in London to graduates of the English Universities is vindicated by one of their own members. "It has been urged," says Dr Paris *, "that the education of a physician is thus rendered materially and unnecessarily expensive; and that the delay of twelve years, which are required for the full completion of the highest medical

* Medical Jurisprudence, vol. i. p. 5.

degree, proves another great and vexatious hardship. To all this we reply, that we should politically resist any measure that had the least tendency to divest medical education of its pecuniary sacrifices, and to open the temple to a crowd of needy and half-educated adventurers." Any comment on this observation seems altogether superfluous.

NOTE B B, page 513.

The Commissioners for Visiting the Universities and Colleges of Scotland seem to have felt that the practice of the examination of candidates for medical degrees being conducted by their Teachers, is objectionable in point of principle, but to have been deterred from proposing to transfer this duty to Examiners specially appointed for the purpose, by some supposed "serious difficulties arising from the peculiarities of the subject, and the great opposition of sentiment apt to arise in medical discussions."

The inexpediency of intrusting the power of granting licences to practise the healing art, to Colleges or public Bodies having a pecuniary interest in the number granted, seems to have been felt by the House of Commons in 1818, when they threw out a bill introduced by Mr Courtenay, for the better regulation of Surgery throughout the United Kingdom, one of the provisions of which was, that no one should practise surgery, without a testimonial from some of the regular colleges of the United Kingdom. On that occasion Mr, now Sir Robert, Peel, is reported to have "declared himself inimical to the principle of the bill itself, for, if it were sufficient for a party merely to appear before a certain Board, in order to procure a license or diploma, on payment of a sum of money, it was to be feared that the practice would ultimately degenerate into one of considerable abuse. It was natural to suppose that a competition would soon be entered into between the several Bodies who had a power to grant licences, for the purpose of procuring the greatest quantity of fees. The more testimonials they granted the greater would be their profit. It would evidently be to their advantage to grant as many as possible, and

therefore it appeared to him, that, unless they had some other test of ability beyond a mere diploma, a great abuse would be generated." "He very much feared that the fees required for the diploma would alone be looked to, and that the examination would become a mere formality."

In illustration of the inexpediency of conferring an exclusive power of licensing medical practitioners for a particular district on those already settled in that district, and having necessarily an interest in excluding rival candidates for public favour, it has been remarked by Dr Barlow, that "the College of Physicians of London deals very differently with candidates for admission, according as they announce their intention of practising in London or the provinces; candidates for provincial practice, or extra-licentiates, being permitted to qualify, by undergoing a very lenient trial indeed, and paying an inconsiderable fee, while licentiates, who aspire to practise in the capital, are tried by much severer tests, and subjected to higher fines. Whence this difference arises, or why the lives of his Majesty's liege subjects are to be deemed more precious in the city of London than throughout the rest of the kingdom, I leave to the College of Physicians to explain. To me the fact appears to admit of no second interpretation, and seems well calculated to throw light on the policy which a corporation spirit, pursuing its own ends, unchecked by the higher considerations of natural equity, and regard for the welfare of the community, is ever prone to."—*Edinburgh Medical and Surgical Journal*, xiv. 16.

NOTE C C, page 518.

THE expediency of separating, wherever this is possible, the emoluments derived from the practice of the medical profession from profits on the prescription of drugs, has been ably insisted on by the writer of an article in the Edinburgh Medical and Surgical Journal, entitled 'Observations on Medical Legislation' (vol. XIV. p. 190.)

FINIS.