A dissertation on the institutes of medicine, particularly relating to the pathology of fever / By William Stoker.

A DISSERTATION

ON THE

INSTITUTES OF MEDICINE,

PARTICULARLY RELATING

TO THE

PATHOLOGY OF FEVER.

By WILLIAM STOKER, M. D.,

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"Medicina non ingenii humani partus, sed temporis filia."

BAGLIVI.

DUBLIN:

RICHARD MILLIKEN, 104, GRAFTON-STREET;
JOHN CUMMING, 16, LOWER ORMOND-QUAY; AND HODGES AND M'ARTHUR, 21, COLLEGE-GREEN;
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This Dissertation was intended, until nearly printed, to have been prefixed to a succinct account of the Epidemic Fevers of Dublin for more than twenty-five years, prepared by the Author to form the second part of his "Pathological Observations;" and in presenting this statement of his pathological principles, without the advantages of the illustrations afforded by the history of epidemics, he is aware how much it further needs the kind indulgence of the public. Being desirous, however, to embrace the present interesting period, he now purposes to include the whole of the current year; wishing, in the interval, to avail himself of the highly-valued judgment (on this introductory part) of some friends, as well as of the censorship of the periodical journals, from which he has already received not only encouragement, but very important aid—aid which must be peculiarly estimable to him when engaged in pioneering a way, though not entirely new, yet so long neglected and choked up, that he could not venture to encounter the difficulties it presents, did he not hope for the assistance of those whose success in exploring the paths of science, has been generally acknowledged.

AN

INTRODUCTORY DISSERTATION

ON THE

INSTITUTES OF MEDICINE,

By WILLIAM STOKER, M. D.

Intelligo quam scopuloso difficilique in loco verser: Quamobrem Nihil dico de meo ingenio, neque est quod possim dicere, neque si esset dicerem.

Non ita est, sed uniuscujusque temporis, valetudinis, facultatis ad agendum, ducta ratio est.

M. T. Cic. in Q. Cœcilium.

AN

INTRODUCTORY DISSERTATION

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How comprehensive the Institutes of Medicine are, and dependent upon each other, can be conceived by those only who are acquainted with the wide space they occupy in the Sciences, most intimately connected with the temporal existence and happiness of mankind; and the definitions of them which I am about to propose, though necessarily brief and partial, on the present occasion, are yet intended to indicate the great extent of each of them respectively, their incalculable value to both the Student and practitioner of Medicine, and their high importance above all the other branches of Medical Science, which in strictness may be

deemed rudimental, in as much as the necessity of them, and the many advantages of their cultivation, are to be proved wholly, by showing how far they contribute to the knowledge and improvements of the Institutes of Physiology, Pathology, and Therapeutics, which should therefore be their great aim and end.

Definitions.

Physiology is the study of the phenomena of life, and differs from all other philosophical inquiries, by involving the final as well as the physical causes of these phenomena; hence a new principle of arrangement is introduced, which is scarcely ever applicable to the investigation of inert and inorganic matter. Pathology is that part of Medicine which explains the nature of diseases, their causes, and symptoms, to which the principle of arrangement alluded to in the preceding definition, is also in a great measure applicable; and Therapeutics imply such a knowledge of the means of preventing or curing diseases, as can only

be effectually promoted by a sedulous cultivation and comparison of the two other branches of the Institutes.

Such definitions have been admitted as axioms into most of the respectable Medical works which have yet appeared; yet, while thus acknowledged as the most steady and certain lights in the investigation of truth; in working the problem, or in their practical application, wide differences of opinion have arisen;—first, by reason of the difficulties of the subject, which involves (as intimated in the definition of Physiology) the consideration of final causes;—secondly, from want of due care in distinguishing between the laws of living and of inert or inorganic matter;—and lastly, on account of those skilled in the mathematical, chemical, and other sciences, vainly attempting to prosecute a knowledge of the Institutes of Medicine solely by those methods which each found successful in his own particular province; a circumstance which may also account sufficiently for the successive revolutions in Medicine, and for these innumerable new systems, which,

however remarkable for admirable symmetry, as specimens of the talents or industry of their founders, were still not better than extraneous and useless incumbrances to those engaged in such inquiries, if not obstacles to their more correct views.

In the limited scope and design of this Dissertation, it is not proposed to follow these various disputants to the extent their fanciful theories might lead on the one hand, or their mistakes of final for physical causes on the other. But from the experience of many years devoted to extensive and active inquiry, especially with a view to the theory of diseases, and cautiously avoiding the sources of error just alluded to, I intend to narrate the imperfections of these systems of medical instruction which were in vogue in the schools; to recount the facts by which such imperfections were exposed; and to recommend these facts as landmarks by which a return to the right way might be effected, and as the only certain guides by which Physiologists can advance in their road to truth.

SECT. I.

PHYSIOLOGY.

Seeing that the Physiology of animal life does not admit of an application of these means, which lead to such constant and certain results when employed on lifeless matter, or at least, that generalization which had been so successfully accomplished in the sciences of natural philosophy and chemistry, could be but very partially effected among the diversified and ever-changing phenomena which constitute the science of Physiology ; I naturally turned to contemplate the character of that design, so strongly impressed on all the phenomena of organic beings, which imperceptibly led me to associate the views suggested by their relative subserviency to certain purposes, with the more strictly philosophical relation of *cause* and *effect*, by which they also may be connected. However, while thus studying the functions of life, (I mean the purposes to which the actions of life are subservient,) my inquiry led me to examine

how far experiment might assist to illustrate these latent processes or schematisms of nature; wherefore, in the year 1798, when composing my thesis on *Chronic Hepatitis*, and having the complex structure of the liver, spleen, and their appendages fully before me, I readily accorded in the opinion then generally entertained by my fellow-students, that a system so extensive, and so situated in the animal frame, could hardly be confined in its functions and uses to the mere secretion of bile; as was the prevailing hypothesis of the time, though not supported by analogy with the functions of the other parts in the animal fabric, where, as well as in all the other works of nature, means are exactly proportioned to the objects to be obtained.

The opinion of the justly celebrated Hewson respecting the preparation of the red globules of the blood by the Biliary System (as it was called) for a long time appeared to me well founded, and the fair result of his judicious experiments; and though still I am persuaded that this is a portion of its office, yet further consideration

induced me to believe that other very important uses in sanguification belong to these organs, in consequence of certain changes in the hydrocarbonous principle with which the blood comes charged to the *vena portæ** thus fitted for the changes it has to undergo in the lungs. By effecting this change in the hydrocarbonous principle too, I supposed that the liver might be deemed the chief secerning organ of fat, and perhaps of fibrin, with the assistance of the lungs, for the supply of which, for the different parts of the body, there is no other appropriated apparatus such as may be found for each of the other secretions, though apparently far less important to the maintenance of life. In this point of view the hepatic system may also be deemed a chief source of animal heat, in as much as by favouring the mutual attraction and chemical cohesion, between elementary parts

* The experiments and observations of Drs. Prevost and Dumas have strengthened these opinions.—Vide *Examen du Sang et de son Action dans les divers Phenomenes de la Vie, par* I. L. Prevost *et* I. A. Dumas, *Eleve en Pharmacie, Member de la Societe de Physique et d'Histoire Naturelle.*

of the adipofibrinous matter, carried thither by the veins, a commencement of that intimate combination takes place, by which subsequently in the lesser, as well as in the greater circulation, caloric, in consequence of diminished capacity for it in the circulating mass, is evolved, according to the exigencies of life. A theory of animal heat, consistent with those of Drs. Crawford, Skey, and De la Rive,* which will be found further substantiated by evidence derived from observations in the course of diseases. The cases which afford such evidence shall be considered more fully in the next section, on Pathology; but those to which I would at present refer, were these of certain dysenteric forms of the epidemic that prevailed last Summer in this country, in which very large quantities of dark-coloured blood were discharged from the mesenteric and hæmorrhoidal veins; and where the temperature of the patients, ascertained by applying the hand to any part

* See Crawford in Phil. Trans. for 1781; Dr. Skey's *Thesis de Materia Combustibile Sanguinis*; also Prof. De la Rive's *de Calore Animali*.

of the surface, or to the air expired from the lungs, was so much diminished as to feel very cold to any of the bye-standers who made the experiment. In other cases (but one in particular) of chronic hepatitis, the surface of the body of the patient was cold and clammy, and for some weeks previous to death the colour of every visible part was dark, livid, and cold; the most strikingly remarkable circumstance in that case (and for which I allude to it here) was, that the blood taken some days before dissolution from the temporal artery, on account of a sudden apoplectic attack, as it flowed *saltatim*, had the colour not only more like to venous than to arterial blood, but was actually as black as ink.—See APPENDIX, *Case, at No.* 1.

In the former dysenteric cases, there was an evident deficiency of combustible matter, (as Dr. Skey, I think, aptly calls it;) and in the latter, or hepatic disease, there was as obvious an imperfection in the apparatus provided for kindling that combustible matter into action; and subsequently under the

controlling influence of vital power for evolving animal heat.

To effect such important purposes might be deemed an adequate employment for so extensive a viscus as the liver, whilst the secretion of bile might be considered rather as a secondary and residual process not absolutely necessary to warm blooded animals, as some are found totally destitute of any organ for its reception, retention, or transmission ; and I should observe farther on the more probable use of the liver, that at the birth of all animals the remarkable size of that viscus seems to indicate it as a provision for the rapid increase of bulk which then takes place chiefly by the sudden deposition of adipose substance in the cellular membranes. The increased temperature of the body in infancy beyond that at maturer periods of life, is likewise a fact well ascertained. The same proportions of this organ are also found in certain tribes of animals, whose bodies at certain seasons acquire bulk from the increase of fat, with a rapidity that is almost prodigious. With such facts too, may be classed the

tendency to obesity left by those agents which are known to increase the functions of the liver; and on the other hand, the effects of intense heat applied to the surface of the body so as to dissolve or diminish the external solid parts followed as rapidly by increase in the size of the liver, exemplified by the practice employed by poulterers to increase the livers of fowl for culinary purposes. The same connexion in certain diseases of sheep between sudden external emaciation and the increase of this viscus, and, *vice versa*, the great increase, and, at the same time, deterioration of their flesh apparently connected with the use of such grasses in certain soils as are known to disorder the functions of the liver. Facts like the foregoing, which I hope to illustrate farther by cases of disease in the human subject when I come to treat on Pathology, seem to me to favour my opinions respecting the uses of the liver, and appear to be further corroborated by the following experiments.*

* Comparative anatomy, which has afforded so many important illustrations of both Physiology and Pathology, may be

Experiment First.

Having provided myself as nearly as I could with such magnifying glasses of Leeuwenhock, as Dr. Hewson stated that he had employed in detecting those formed globules in the blood on its passage to the heart from the liver and spleen, which he

further referred to, to show that the uses of the bile, or the injurious consequences of its deficiency in the animal economy, have been greatly overrated. In the horse a familiar instance of an animal without either gall-bladder, appending ducts, or decussating stratum of muscles, provided in the œsophagus of such animals as can vomit, no inconvenience is apparently produced thereby, whilst the termination of the biliary duct below the stomach, and near the commencement of the intestinal canal, in most animals, provided for the reception and transmission of bile, seems to evince that the natural uses of that fluid are chiefly or solely derived from its stimulating, and perhaps antezymic properties, by the first assisting to propel downwards the residue left after the separation of chyle, and by the other correcting that putrescency of feculent matter, which otherwise would go on to a pernicious extent in the lower parts of the *prima via*. When I come to treat on Pathology, I shall have occasion to detail, why I think the study of that branch has been retarded, by attributing properties to the bile, which I am therefore anxious to show do not belong to it.

When treating on Pathology too, I shall detail cases remarkably illustrative of the connexion between the sudden decrease of bulk externally, and as sudden increase of the liver.—See *Cases, at No.* 1, Appendix.

could not discover by the same means in the blood of the *vena portæ;* I, with the assistance of Dr. Colles, (then resident Surgeon at Stevens's Hospital, and Professor of Anatomy and Surgery in the Royal College of Surgeons in Ireland,) suspended a dog by the neck, till that animal was nearly dead; blood was then taken directly and as speedily as possible,—first from the *vena portæ*, and then from the *vena cava*, immediately on its ascent from the superior surface of the liver. The issue of the blood from each orifice was directed into a separate recipient. We then proceeded to examine what differences could be discovered between the two denominations of blood, by the aid of those microscopic glasses which we had provided; but whether from our inexperience with these instruments, or from their imperfections, they were not available to us on that occasion. By the naked eye, however, we clearly perceived that the blood taken from the *vena cava* was many shades lighter in colour than that of the *vena portæ;* the distinction continued for several hours afterwards, which

mainly accorded with my anticipation of the changes wrought in the circulating mass at its re-entrance to the sanguiferous system, by the slow and circuitous course through the liver and its appendages, by which I supposed an essential preparation is made for that regenerative process the circulating mass undergoes in the minor circulation, in order to refit it for the renovating and reanimating purposes for which it is intended in the arterial or great circulation.

Experiment Second.

Being desirous, however, to avail myself of this opportunity as far as it would admit, with respect to the other object I had in contemplation, namely, whether those globular parts of the blood in the *vena cava*, which Dr. Hewson discovered, were any thing more of an adipose nature than they were previously to their undergoing the changes effected in them by the liver and spleen, I immersed a portion taken from the blood contained in each of the separate

vessels, in separate basins of water, at the temperature of 160°, with the hope of detaching the oily, and therefore lighter parts, and of allowing them to take their place on the surface; the result, which I exhibited to Surgeon Garnet, (then an intelligent and practical Chemist, and whose acquaintance I had the pleasure of enjoying,) was, that after some time an unctuous and dark-coloured film floated on the surface of the water in the basin which contained the *vena cava* blood, which continued for several hours, assuming, however, a more friable feel to the finger, and ultimately appeared more soluble in the circumambient water. No such appearances as these presented themselves on the surface of the water which contained the blood taken from the *vena portæ*.

In republishing these experiments and results, my first and most imperative duty is, to explain more fully than I have hitherto done, why, during the quarter of a century which has elapsed since they were made, I have not repeated them, as suggested from quarters meriting the highest

attention, to be necessary to justify those practical corollaries which I have attempted to deduce from them. Explanation I am the more willing to enter on, from hoping thereby to give additional proof of the validity of the opinions which led to the institution of these experiments.

My reluctance to enter on the first part of these experiments, not arising from any sickly sensibility, was only overcome by the feeling of necessity in order to arrive at conclusions which I anticipated would be highly useful in their application ; and this reluctance, together with other professional avocations in which I have been constantly and actively engaged, tended considerably to retard a repetition of them ; but other circumstances connected more immediately with the nature and result of the experiments themselves, were, *bonâ fidê*, the main causes of delay. For instance, they could not be repeated like those made on inert or inorganic matter, which might be arranged so nearly under the same circumstances as to promise nearly a similar result. The object

of them was not to ascertain merely the state of a chemical process in a certain stage, but how far chemical combinations subtended to a vital process to which they were entirely subservient, and in which state of subserviency alone the former could proceed without producing great inconvenience in the animal economy. Such considerations, whilst they necessarily limit the bounds of experimental inquiry in Physiology, also render it probable that the chemical products of these experiments were chiefly owing to some debility or derangement in the vital part of the process which, in a state of health or in ordinary circumstances, would have developed them according to the exigencies of the system in general for which they were intended.

Decisive evidence appeared to me to be thus afforded, that *organic affinities* are conducive, in the vital functions of the hepatic system, to the elaboration of oily, gelatinous, and, perhaps, other substances intended for the supply necessary to the constant waste of the solids, however the

dying state of the animal, or any other peculiar circumstance, might have modified the control which vital power exercises over such organic affinities in ordinary circumstances, or in the healthy condition of the body ; and the result was more especially instructive to me, from its intimating to me modes of analytical investigation, which, as stated by a truly philosophic writer,* "have not been pro- "secuted by physiologists with the atten- "tion their importance deserves." My opportunities, however, for observing organic affinities, when least controlled by vital power, must obviously have been much more frequent when viewing the progress of diseases, or the approach of death ; and in the next section, on Pathology, I intend to avail myself of my very extensive opportunities of collecting and recording facts which have appeared to me to bear strongly on pathological questions, but still more favourable for deducing consequences, that throw considerable addition of light upon

* Dr. Roget.

the animal economy, but still more remarkably upon the theory of diseases.

In these pathological investigations, I have endeavoured to apply my discovery of the share the chemical or organic affinities have in the functions of the liver and spleen to the better understanding of some of the other functions also, especially to that at the entrance of the other avenue of supply to the sanguiferous system, as far at least as respects the materials carried thither, and the elaboration of healthy blood which succeeds, by comparing each in their healthy and ordinary conditions with the changes produced on them in the course of disease and after death. Such of these facts as are more immediately pathological may not be further anticipated here, but some more directly connected with the experiment under consideration, and therefore, with the physiological question at issue, will be admitted as a fit conclusion to this part of the subject.

Previously to the narration of those facts which seem to favour the opinion that oily

as well as fibrous matter* is sometimes found in the sanguiferous system, the result (as it appears to me) of that subtendency of chemical affinities in the vital functions, which, in the hepatic system especially, contributes to their ultimate elaboration; I am desirous not to be misunderstood as thus attempting to explain the *final* and chief causes of these phenomena; and neither would I have it supposed, that I feel at all competent to enter into the minute investigations which the physical causes themselves might demand, my object extending no farther than to illustrate the distinct and yet intimate connexion, of the various causes engaged in the vital functions, as may, I think, be most advantageously exhibited during the failure to a certain extent of the chief controlling power; for in the investigation of these physical causes I am aware that much talent and industry have been already wasted from

* My experiments (See my PATHOLOGICAL OBSERVATIONS, Part 1.) of the buffy coat of the blood, which will be more fully considered in the next section, have, I trust, in some degree, established that fibrin, in various degrees of formation, is detected in blood drawn during disease.

want of due regard to such distinctions; in so much that the extraordinary ingenuity and zealous perseverance of successive rivals have been found only just equal to discover the errors of their predecessors, but unfortunately not to effect or point out a more certain way. Such, I believe, is the case with respect to the question connected with those facts I am about to relate, e. g. It has been stated, on the authority of some of the most celebrated animal chemists, after the most cautious examination, analytical and synthetical, of the component parts of the blood, that they consisted of certain chemical and mechanical combinations, which others, as high authority, stated did not belong to the nature of the vital fluid; no mistatement of the results, however, was attributable to the authors of the experiments, and it was afterwards found not only that scarcely perceptible differences in the subject, but also in the mode of the chemical process or in the apparatus employed, would cause much greater differences in the result; and, notwithstanding the utmost precaution has been used in the

subsequent adjustment of experimental inquiry, still various opinions prevail with respect to the true chemical characters of the various ingredients that enter the component partsof the blood. This may be seen by consulting the comprehensive work of Dr. Bostock, one of the most distinguished writers on Physiology, and of the largest contributors to the improvements of animal chemistry since the period when those sciences began to make the most remarkable progress; of whose well-earned fame it is not for me to speak more than the pleasure I have felt in seeing it extend, but of whose friendship, continued without the slightest interruption since our first youthful acquaintance, I am happy and proud to acknowledge on any occasion. I must refer to the whole of the fifth and sixth chapters of the first volume of his Elementary System of Physiology, for the various information to be obtained from them connected with my subject; but from the immediate application of the concluding paragraph of these chapters to pathological investigations of the changes produced

in the nature of the blood by disease, compared with its healthy condition, I must beg leave to quote it here in full.

" The general conclusions that we may " form respecting the nature of the blood " are, that it is a compound fluid consisting " of several ingredients of various physical " and chemical properties, dissolved, or " at least suspended, in a large quantity " of water. Of these the fibrin and the " colouring matter are disposed to unite, " to separate partially from the water, and " to form the crassamentum, or clot, to " which the iron is also attached. The " albumen, the incoagulable matter, and " the salts, remain in a state of solution in " the water, and compose the serum ; by " heat the albumen is rendered solid, and " may in this way be detached from the se- " rosity, which consists of a portion of water " holding in solution the incoagulable mat- " ter and the salts. By slow evapora- " tion part of the salts may be procured in " in the crystalline form, but the whole of " the saline matter can only be obtained by " calcining the residuum after evaporation

" when the animal matter is consumed, and " the neutral and earthy salts left behind, " although probably in a different state of " combination from what they originally " possessed."*

I have purposely prefaced the arguments I have to adduce in favour of oily matter, or fat, being sometimes found in the blood, with the foregoing extract,—because the general conclusions which it contains respecting the nature of the blood, may be deemed as full as the subject was capable of; and therefore in whatever degree the condition of the blood found in the living system, differs from that so described, in the same degree, it may be supposed the result of chemical affinities insufficiently controlled by vital power. The fact, however, of its being actually found in the sanguiferous system, or of the external character of the blood being otherwise changed, becomes the more important in a pathological point of view; because if such circumstances be extraneous to the natural

* See Elementary System of Physiology," by John Bostock, M. D., &c. &c. Vol. I. p. 492.

condition of the blood, they must be admitted not only as evidence or symptoms of disease, but also sufficient to excite further disorder in the animal economy, such as has been shown by experiments, especially those of Dr. Barry, to follow the introduction of any fluid extraneous in its nature or properties to that natural to the sanguiferous system.

I hoped to have been able to substitute facts recorded by others, for those which I might otherwise have supplied from my own experience, which favour the opinion, that fat or oil perfectly eliminated, is sometimes found in the blood, or at least commingled with it in the sanguiferous system. I have preferred doing so on occasions not very dissimilar, namely, when discussing questions more purely pathological; because on a subject so deeply interesting to me, I would not affect to be an exception to that general rule "*quod volumus, facile credimus*," to which I believe all are more or less subject; but as several of my medical friends, who had observed such facts, have from various circumstances delayed to fur-

nish me with their written authorities, I shall indulge in taking another quotation from the Elementary System of Physiology, because, as will appear, it at once contains the objection which those facts were intended to meet, and farther substantiates the evidence which they would afford of something like oil being detected in the blood under disease.

" In its chemical constitution, (says Dr. " Bostock,) fat appears to agree very nearly " with the expressed vegetable oils; like " those it varies in its consistence, or rather " in its freezing point, so as in the ordinary " temperature of the atmosphere, to be " found sometimes in a solid state, as is " the case with suet and tallow; and at " other times perfectly fluid, as we find it " more particularly diffused through the " cellular texture of the cetacea. We are " not acquainted with any apparatus that is " appropriated to the secretion of oil, nor " are there any facts which can enable us " to decide positively upon the mode of its " formation. As a substance of an oily " nature has been said to enter into the

" composition of the chyle, and as the for-
" mation and deposition of fat appear to
" bear a relation to the quantity of chyle
" that is produced, it has been conjectured,
" that the oleaginous secretions originate
" in the process of chylification; but it
" may be objected to this idea, that the
" fat cannot be detected in the blood. Indi-
" vidual cases are indeed recorded, where
" the blood has exhibited an appearance as
" if something like cream was floating in
" it, but we are not well informed of the
" nature of this creamy matter; it is only
" a rare occurrence, and should probably be
" considered as depending upon some mor-
" bid, or at least some unusual state of the
" system."*

If this passage be carefully analyzed, it would be found to coincide with those opinions which I have expressed as to the production of fat; for if it has been found " depending upon some morbid or unusual " state of the system" in the blood-vessels, it was probably the product of the secretion commenced in the hepatic system; because

* Elements of Physiology, Vol. II. p. 352.

it is stated, that fat cannot be detected under ordinary circumstances in the blood, which it must very generally have been, if the creamy fluid stated by Sir Everard Home* to be constantly met with near the lower orifice of the stomach, were to pass in its unchanged state by the lacteals, or if it was perfectly secreted in the colon, as he supposes, and carried directly from them by the absorbents to the sanguiferous system; either of which occurrences is rendered further improbable, by the well known fact, which seems to indicate that oily substances must be reduced to their elementary condition before they become miscible with the chyle; namely, that they are more tardily acted upon in digestion than any others; in so much, that when the organs connected with that mysterious function are deranged, the oily parts are often rejected, but very partially acted upon, and separate from the other parts of the ingesta, which had been previously assimilated and carried into the system by the lacteals. But on the other hand, the secretion of fat

* See Philosophical Transactions.

appears to me to be the result of a process that commences with the organic affinities in the liver, which in health are controlled by vital power, and afterwards conducted so as not to go to excess on its course, to fulfil those ends for which it was primarily designed.

The facts mentioned to me by others of my medical friends, were certainly much more decisive of oil being found mixed with the blood in circulation, than appears by the following letter from my esteemed friend, the Professor of Anatomy and Surgery in the Royal College of Surgeons in Ireland; still I am happy to have such a document from so accurate an observer.

York-Street, *August 2nd*, 1826.

Dear Stoker,

"The following case occurred "to me some time back, and as you think that you "can make some use of it in your present inves- "tigation, I send a statement of it to you."

"Mr.———— fell down stairs with considerable force: "on being raised, he found an inability to use the right lower

" limb. He was carried to bed, stuped and rubbed with some " liniment. Next day I saw him. On examination, I found " the thigh swelled on the outside, and the integuments de- " tached from the great trochanter, to within an inch of the " knee, without any external injury; a fluid occupied this " space. As there was no local inflammation, I drew off this " fluid with a trocar, closed the wound, and applied a compress " and bandage. The fluid consisted of a blood-serum, with a " considerable quantity of oily particles floating on its surface; " it nearly measured two quarts. I attributed this appearance " to the oil I had used on the trocar; but I afterwards found " this not to be the case, for I had occasion to let out the fluid " repeatedly, which I did with a lancet, yet the oil was always " observed in as great a proportion as at first. The fluid gra- " dually disappeared, and the cavity became perfectly oblite- " rated by keeping the detached surface constantly in contact " by compress and bandage.

" This case occupied about two months before perfect re- " covery took place."

Yours truly,

S. WILMOT.

To Dr. Stoker, York-street.

For reasons which I shall have occasion to detail more fully in a subsequent part of this Dissertation, my attention during the practical observations which my opportunities enabled me to make, was directed much more to the Physiology of the fluids, than to that of the solids, and constant

reference to the two great sources from whence the blood acquires the means of its fluidity, and of restoring the losses sustained in circulation, promised and subsequently afforded me the most efficient aid in detecting errors in the theory of Medicine, as well as in forming more correct views concerning it. And having entered as fully into the consideration of one of these great avenues of supply, as the occasion will allow, I shall now proceed, but with still greater brevity, to that of the other; namely, the union of the chyle and lymph in the thoracic duct.

The chyle and lymph, on this union in the thoracic duct, flow into the subclavian vein, and are then mingled for the first time with the blood at the right side of the heart; blood which had been in some degree prepared for that purpose by the process which we apprehend it must previously undergo in the liver, thus fitting the homogeneous contents of the right ventricle of the heart, for that more perfect animalization effected by the agency of the respiratory system; a function which

I have little doubt consists of organic affinities, controlled by vital power, as well as that of the hepatic system. The general condition and properties, however, of the chyle, just before it is submitted to the action of the lungs, are all that I can advert to at present, and I shall therefore confine myself to a brief quotation from the "Elements of Physiology," by Mr. A. Richerand, with which I prefaced my practical observations, published in the first part of my "Pathological Observations."*

"The chyle," says Richerand, "which is "necessarily affected by the various kinds "of food which we use, has different ap- "pearances in the same persons, varying "according to the quality of the different "substances on which we feed; indigo "gives it a blue colour; it is reddened by "madder and beet-root, and it is changed "green by the colouring matter of several "vegetables. In a great number of expe- "riments performed on living animals, it "has always appeared to me, such as it is

* See Pathological Observations, Part I. on Dropsy, &c. &c. p. 7.—Dublin Edit. 1823.

" described by authors, white with a slight
" degree of viscidity, and very like milk
" containing a small quantity of flour. It
" is easy to collect a certain quantity of
" chyle, by tying the thoracic duct of a
" large dog or sheep, or even a horse, as
" was done at the veterinary school at
" Alfert. This fluid when exposed to the
" air,—on cooling, separates into two parts;
" the one, forming a kind of gelatinous
" coagulum very thin, and not unlike the
" buffy coat of inflammatory blood; the
" other in greater quantity, and liquid,
" rising above the coagulum on its being
" detached from the sides of the cup to
" which it adheres. The coagulated mass
" is semi-transparent, of a light pink colour,
" does not resemble the curd of milk; so
" that all that has been said by a few mo-
" dern physiologists, on the exact resem-
" blance which they have pretended to
" discover between milk and chyle, is totally
" void of foundation. The lymph, which
" constantly unites with the chyle before
" the latter enters the sanguiferous system,
" on being received into a vessel by Mas-

" cagni coagulated in the space of seven " or ten minutes, turned sour, and sepa- " rated into two parts, the one more abun- " dant, serous, in the midst of which floated " a fibrous coagulum, which by contracting, " formed into a small cake on the surface of " the fluid. Hence he concludes, contrary " to the opinion of Hewson, that lymph " consists, for the greatest part, of serum, " and that fibrin constitutes its least " part."*

When treating on the functions of the liver, I freely confessed, that the facts by which I had promised to illustrate Physiology, were chiefly derived from pathological investigation ; and this confession applies still more aptly to this source of the blood described by Richerand ; I must not however anticipate the exemplifications of disease which will be given under the proper head of Pathology, farther than to state, that these illustrations arise chiefly from the distinctness of colour, and of the external characters which designate various kinds of buffy coat on the blood, as such distinctions

* See Elements of Physiology, by A. Richerand, p. 158.

were found to indicate, with considerable certainty, the functional derangement of either the hepatic or pulmonary system; thus, in cases of simple pneumonia, or where the lungs only were engaged, the coat exhibited on the blood drawn, is generally of a clear white, but when the tunic is tinged, it is with bright red, and does not exceed a few lines in thickness. The cupping of the surface, as it is called, is also most remarkable on such blood; a circumstance which arises probably from the great thinness and contractility of the film, drawing towards the centre the external margin of the less contractile crassamentum.

In diabetic complaints, which there is so much reason to believe originate in imperfect digestion, or insufficient preparation of chyle, (and probably too, from derangement in the chemical affinities engaged in these functions,) for when blood is drawn, it is often found covered over with a whitish milky fluid, the crassamentum itself being of a brighter colour than that of venous blood. In simple forms of hepatic disease, on the other hand, the buffy covering on the blood

drawn, is generally of a more dusky white through its whole substance, than in pneumonia, and the external surface of it yellow; the buffy coat also occupies a larger proportion of the solid part of the blood than in pulmonary complaints. When, however, as not unfrequently happens, both the pulmonic and hepatic systems become equally engaged in the disease, such distinctions cease; but I have often seen them restored in the same proportion as the diseased actions, affecting either of those systems, were found to vary in their relative urgency.

I would be silent in this Dissertation, on the physiology of the solids, because it embraces so wide a field for speculation, not only as respects their organization and situation, but also their muscular, nervous, sensorial, and organic power, convinced it might be better on such a subject *silere quam pauca dicere;* the ultimate design, however, of this Dissertation being illustrations of the various branches of the Institutes, by facts collected in my investigation of the theory of diseases,* the remarks

* See Hamburgh Medical Journal.

made in his review by Dr. Gerson of Hamburgh, which are in every way entitled to my regard, call on me to explain, as fully as I am able, the meaning I attach to the terms *Solids* and *Fluids*.

His definition far transcends the bounds to which I am desirous to confine those two distinct states of animal life, for otherwise I apprehend very much that my objects would be mistaken; the first of which is to avoid those mechanical, chemical, and metaphysical disquisitions, on the nature and properties of solids and fluids, which appear to me hitherto so fruitless in promoting correct views of the theory of medicine. By the solids I would have understood the membranes and fibres of every part of the human body, as well as every substance, whether organized or otherwise, so stable as to contribute to the permanent form of these parts; and by the animal fluids, I mean all those in the living system either elastic or inelastic, either in motion or at rest, so long as they consist of "particles cohering so little "among themselves that they yield to

" the smallest force, and are easily moved " among one another."*

In proposing these limited definitions, I trust that I shall not be deemed by Dr. Gerson wanting in due admiration for the learning and talents which have enabled others to extend their definition to those minute characteristics which the various modes of analysis employed from time to time appear to justify, but respecting which there are still unfortunately various opinions, and the superiority of any is still to be decided by facts.

By adopting the simple definitions proposed, I wish to escape the confusion of final with physical causes, hoping thus to avoid what appear to me the great sources of those failures and errors that have occurred in pathological discussions.

The Author of nature and religion has every where established certain mysterious rites, and placed them as objects for the humble and reverend admiration of his rational creatures, so that they may approach sufficiently near to examine their unifor-

* See Encyclopædia Britannica, Vol. x. p. 701.

mity with the benevolent design that pervades all the works of creation besides. An examination for which the *mens sana in corpore sano* is all the necessary apparatus; the philosopher's microscopic provision for a more comprehensive view, and the chemist's cunningest contrivance for imitation, having alike failed.

But however the boundaries of experimental inquiry into the causes of the phenomena of life may be narrowed, I am still of opinion that these phenomena may be considerably illustrated by the analogies which subsist not only between the animal functions, but also between them and those of the vegetable kingdom. And having adverted in this section to the illustrations thus afforded of final causes in the animal kingdom, I shall conclude it with an instance in which the comparative physiology of vegetables appears to me to throw some light on the no less abstruse subject of the propagation of diseases and the varieties produced from the original semina of them.

In my Treatise "*On the Varioloid Disease*,

"*or on Small-Pox,** *under the form which it* "*presents in persons previously vaccinated,*" I stated my opinion, when remarking on my previous experiments with small-pock and cow-pock matter, that similarly engrafted diseases do generally, by repeated transfer, produce varieties differing more and more both in qualities and semblances as they recede from the parent stock. An hypothesis, supported by other analogies, which might be readily supplied from the histories of hereditary diseases, compared with the changes of form and colour which succeed either accidental or artificial unions of the varieties in the animal and vegetable kingdoms.

The facts which I wished particularly to illustrate were, that inoculation with the matter taken from the pustules of the modified small-pox, has been found to produce a vesicular disease in unprotected persons, but with characteristics as unlike those exhibited in the case from which it had been taken, as the symptoms of it were from

* See Observations on the Varioloid Disease, &c. &c., Dublin, 1821.

the original small-pox. The nature of the matter also appears to be changed as to its antidotal virtues, for that from such vesicular diseases does not appear to afford protection from the contagion of the natural small-pox, or even from that of vaccine infection itself, except in a certain modified degree, in proportion to their proximity to the orignal stock.

The numerous varieties, which I believe to have proceeded from small-pox, such as the cow-pock, chicken-pock, swine-pock, horn-pock, &c., have also analogies with the propagation of vegetables from their seeds. Thus, with the *solanum tuberosum*, we find the apple-potatoe, the most perfect of its kind, produces seeds, which, when sown, give every variety of this prolific root; but each variety, when transplanted, produces its like and preserves its semblances and properties through every succeeding transfer.*

* See my Letter to Dr. Thomson in the Edinburgh Medical and Surgical Journal, A. D. 1824.

SECT. II.

PATHOLOGY.

The facts or cases* intended for the illustration of those pathological or etiological investigations, that are to be the subjects of this section, being mostly such as I have already published with similar views, my objects, without some further explanation, might be still mistaken, as my publications have already been; and thus misapprehended, might incur the same censure of being designed for the restoration of the Humoral Pathology. I shall, therefore, endeavour, on making this second venture, to avoid the great risk incurred by the first, by explaining its objects more fully.

I shall, therefore, commence with the following paragraph from the Preface to the first part of my Pathological Observations for the sake of explanation :—" Im- " pressed, however, with an opinion that

* As the details of cases can be more conveniently referred to in an Appendix, such as I intend, they will not occupy any part of the sections of this Dissertation.

" many diseases, especially those included " in the preceding title-page, depend on " changes which take place primarily in " the fluids as others do in the solids, I " have been led to prepare the following " work, the object of which is, to exhibit " some of those facts, collected during an " extensive experience for twenty-five years, " on which that opinion is founded; and " I indulge a hope, that by directing a " more minute and general attention to the " state of the blood and of the fluids de- " rived from it both in health and in dis- " ease, than has been of late bestowed " by medical observers, I shall promote the " science of medicine, and further the im- " provement of the arts of healing." In explaining this quotation the first attention is to be directed to that sentence which expresses my opinion, that " some diseases " take place primarily in the fluids as " others do in the solids;" as this seems chiefly (as far at least as regards the Solidists) to be endowed with the extraordinary properties of positive and negative attraction, for it has brought down the

fulminating censure of some of them, and incurred the equally blighting neglect of others; I own too, I am not the less desirous to direct attention to that sentence, though I am sure it has contributed chiefly to the favourable reception the work met with by some of the Humoralists, as that favour, in so unexpected a degree, arose chiefly, as I persuaded, from the whole passage being mistaken by them to express a much greater leaning to their exclusive principles, than I intended to have conveyed. The security of this new venture, in which I am so deeply interested, requires my most vigilant exertions to conduct it from this conflict of contending parties to which I have inadvertently exposed it, and which it appears in some measure to have hastened. In order, therefore, to remove from a point on which there appears to have been the greatest hostility, and to escape a question so abstruse and difficult, that it must long rest as a matter of opinion, I may premise that it is not my design to prove that the commencement of diseased actions, or as aptly expressed by Dr. Gerson,

their *punctum saliens*, first takes place in the fluids; my leading object being always, as it is now, to show that morbid changes are effected in both solids and fluids, and that they are reciprocally affected by such changes; and it is my opinion that the train of symptoms so excited, claim the Physician's first attention. An opinion which I think well illustrated by the following passage from Gaubius's Institutiones Pathologiæ: "Dis- "tinctio igitur causarum in continentes et "non continentes nulla est. Morbus ultra "causam suam i. e. sine causâ non perdu- "rat. Vulnus cultro licet quo inflictum "est ablato una cum sua causa persistit."*

In availing myself too of Boerhaave's Pathological Definitions, as I did on former occasions, I deem it advisable, for the reasons just now intimated, to explain that I prefer them only because they appear to me to be founded, like their prototypes of the school of Cos, on the sure basis of experience; a foundation which I think has been more clearly established, in proportion as

* See Institutiones Pathologiæ Med. Auct. H. D. Gaubio, p. 29.—Edit. Leid. Batav., Anno Domini 1781.

the superstructure has been rigorously tried, by submitting it to the test of practical utility. The theories, however, of the same distinguished author, more strictly etiological, should be carefully separated from his Definitions. The former having been deduced more generally *a posteriori*, and therefore not so well founded as the latter, have contributed chiefly to identify him with the promoters of exclusive Humoral Pathology; and hence the many benefits he conferred on his profession, through his predilections for the study of the chemistry of the fluids, have been fatally opposed by the mistakes he was led into by too freely indulging the same predilections, and thus overlooking the condition of the solids in disease.

In adopting the following Definitions therefore, I wish them to be considered entirely independent of their author's theories, " Quæ scientia Παθολογια appellatur " eaque suas partes dividitur. Ut itaque " actiones, sic morbi distingui possunt; ut " conditiones ad actiones, ita et harum de- " fectus; hinc 1. Morbi partis solidæ sim-

" plicis organicæve; 2. Humorum morbi,
" horum naturam, copiam accidentia spec-
" tantes; 3. Morbi ex his binis compositi.

" Causa morbi vocatur, quæ morbum
" præsentem facit. Semper fere res phy-
" sica jam presens est. Aut re verâ producit
" novum statum in solidis, et in fluidis qui
" morbus ferme ipse. Aut tollit rem requi-
" sitam omnino ad exercitum functiones."*

If the signs of the times might be deemed a fair indication of medical opinions, I think these definitions of Pathology which were so much in vogue about the middle of the last century, may be predicted to be as much esteemed ere the same period of the present; a counter-revolution in medical science, to which the observations made both at home and on the continent, on the effects of changes in the fluids, in producing disorder of the animal economy, largely contribute. The progress of the restoration in France (particularly since 1823, when my experiments on the buffy coat were first published) of a pathology that

* See Institutiones Medicæ, &c. &c., ab Hermanno Boërhaave, p. 304.

would alike respect the animal solids and fluids, may in some measure be judged of from the following remark of M. Duges on the creeds of the Solidists and Humoralists, as extracted for the Medico-Chirurgical Review;* from the *Revieu Medicale, Mars.* 1824; by the reviewer of my Pathological Observations, that he might give that extract his unqualified sanction: "Il ne serait pas moins " de raisonnable d'adopter des idées toutes " contraires, et de nier les alterations du " sang, ou de les croire indifferentes aux " phenomenes de la santè, et de la vie. Un " juste milieu entre ces deux extremes est, " sans doute la veritable route a suivre."

The following extracts from an article by Dr. G. H. Gerson, (in the number of the Magazine of German Literature, for July 1824,) are intended to show that the progress of this counter-revolution has been much more rapid in Germany than even in France. And however gratified I may be by their praise, it can be no less satisfactory that I am enabled to produce such evidence on the present occasion.

* See Med.-Chir. Review, new Series,—Edit. Edin. 1773, No. 1., June 1824 . 155.

" About forty years ago Cullen first " advanced in Edinburgh the doctrine of " Sthenia and Asthenia; which had ex- " tended itself over Europe for half a cen- " tury under different names, and manifold " shapes. Its intellectual course passed two " points of Culmination; the former in " Brown, the latter in Broussais. And now " comes the second extinguishing blow from " a quarter near that from which the theory " of Solids, as a first flash of lightning, " blasted the old fabric of the Pathology " of Humors. A new, although not an " unheard of appearance.

" It is, however, astonishing, when we " reflect, that in England itself, where " Harvey made known his Exercitationes, " and before the eyes of that enlightened " people among whom the penetrating John " Hunter resided, the most obstinate op- " position was raised against the Patho- " logy of fluids, that has existed since the " time of Paracelsus and Van Helmont. " And yet, though Hunter was their praise, " they taught and acted like Brown.—What " a contradiction! Even after the neces-

" sity of practice had again torn from their
" minds the charming method, and opened
" an opposite road for their wants,—even
" then the same doctrine of fundamental
" principles, only under a different form,
" came out again, as it happens at present
" with the existing system of Pathology
" and Therapy in the place of the former.
" But now the new theory of Solids has
" finished its course with that new appear-
" ance of Brownism, which had been
" gathered into Broussais' doctrine, and
" concentrated its rays to a focus. It has
" vibrated to the left and to the right from
" truth, like a tremulous string, from Brown
" to Broussais, and the voice now issuing
" from Ireland, with which we unite, is the
" first messenger of the death of her whose
" birth had been loudly proclaimed from the
" same neighbourhood.—*Pages* 1, 2, 3.

" It is to be considered that on our side
" not a single German writer on Practical
" Medicine, of any consequence, except,
" perhaps, Marcus, gave himself up en-
" tirely to the system of the Pathology of
" Solids; while beyond the sea this theory

" in both extremes, led first to boundless
" indulgence, and then to boundless weak-
" ness. In the mean time, in our country,
" sound sense and profound physiology
" constantly preserved a due medium when
" considering the process of life, and only
" very limited understandings indeed have
" been able to find satisfaction in the theo-
" ries of the English and modern French.
" There even appeared in the bloom of
" German Brownism, about twenty-four
" years ago, the work by Dominling, with
" the almost bashful title of '*Are there ori-*
" *ginal Diseases of the Fluids?*'—'Is it really
" so absurd,'—thus he says in his Pre-
" face,—' to suppose original Diseases in
" the Fluids?' Yet it appears strange,
" that notwithstanding the certain path
" that physiology has taken since the time
" of K. F. Wolf; notwithstanding all the
" great impressions made on it by Kiel-
" meyer and his pupils; notwithstanding
" farther the assistance of natural philo-
" sophy, in which the spirit of the theory
" of Solids cannot dwell; lastly, in spite of
" the works of our great practical Physi-

" cians, chiefly Reil's; yet the majority runs
" after the brilliant uniform of the foreign-
" er, and seems fully satisfied at present
" with the theories of Inflammatorists, and
" even here and there with Broussais. Aye,
" we would even look with astonishment
" and alarm at this blood-thirsty young
" monster, the offspring of French experi-
" mental physiology, and of short sighted-
" ness of the practical observer, with a suffi-
" cient number of noisy Korybants for
" protectors; if we were not to judge from
" all appearances, and now from our voice
" from England, that this last meteor of
" the Solidists, whose elegy Doctor Stoker
" has already sung in Ireland, will soon be
" extinguished.

" It would be incredible how far almost
" all our fellow artists across the sea suffered
" themselves to be enticed into that quag-
" mire, the theory of Solids, by the appear-
" ance of solidity, as by an *ignis-fatuus*, if
" their writings did not prove it. One may
" even see the disjointedness of false ex-
" planation, and the wildness of their madly
" forced applications at the bed of sickness.

" The adherents of the pathology of humours " were regarded with real pity, disdain, and " contempt; and the gestures of those per- " sons are ridiculous, when nature presents " them with an inexplicable problem as a " barrier on their theoretical highway, and " forces from them a reluctant acknow- " ledgment, *(un aveu arraché par la* " *verité,)* as Broussais often expresses him- " self against Ontologists.—*Pages 35, 36,* " *37.*

" * * * * * Stoker now opposes himself " to this physio-pathology of his country- " men, and with the danger of having all " that applied to him, which was said against " humoral pathology in modern times. " Every well-intentioned person will praise " him for his courageous love of truth, " (for it requires courage to speak the " truth to those who we are certain will " ridicule us, more even perhaps than to " those who would stone us;) yet it is " important to examine how he has per- " formed his task. I must again remind " my readers of a former assertion, that " the greater number of systems of medi-

" cine derive their origin from a practical,
" rather than from a theoretical want, and
" this is the case with this entirely new
" theory of the author, novel as it is prin-
" cipally to England. The property of this
" mode of invention, as it happened before,
" was, that whatever was new differed from
" that which had already subsisted only by
" some completing, aiding additions, as Fr.
" Hoffman's, and we shall see that this was
" the case with Stoker's trials. The man-
" ner in which he conducts his new doc-
" trine proves this at once, for he prepares
" and conducts the reader by practical cases
" as they occurred to himself. This me-
" thod is certainly practised by his country-
" men, and is formed on the fundamental
" principles of Locke, being peculiar to
" observers, who wish to deviate but little
" from direct evidence, and retire quickly
" to it again."*—*Pages* 40, 41.

* Magazen der Ausläudeschen Literatur der Gesammter Heclkunde, &c. &c. &c., Von Dr. G. H. Gerson, and Dr. Nichol, Henri Julius, Juli, August, 1824, Art. 2. Pathological Observations, &c. &c. &c., by William Stoker, M. D., 1824.—Translated by I. G. Abellshauser, Professor of Languages.

I would willingly extend those extracts but it would be incompatible with the limits of a Dissertation like this, being desirous to present such documents for the further explication of those definitions which I have adopted in support of my pathological principles, from an apprehension that the new array of facts, which I am about to produce, would not receive due consideration, in certain quarters, on a question I deem vitally interesting to physicians and their patients, if the merits of that evidence were not more fairly appreciated; an apprehension, I think, justified by the reception some of those facts already published have met with in the quarters just alluded to.

In order to review the evidence I have already offered, I shall relate my observations on the origin of the exclusive pathology of the Solidists, as it has been for some time inculcated in our schools of physic, on the imperfections necessarily attached to such a system, and on the failure of its partisans in forming correct *rationalia* as to the nature or treatment of diseases, but more especially of fever. In

making this retrospect I shall deviate little from the mode I pursued, with a similar intention, in an article published March last in the 325th Number of the "Medical and Physical Journal."

It would be necessary to go back to very early periods in the history of medicine to trace the errors of exclusive systems from their origin, and to examine the causes of the various revolutions this science has undergone in successive ages; causes which, I think, would be found not so intimately connected with want of zeal or talents in any of the methodical, mechanical, chemical, or anatomical sects of physicians, as with the dispositions of each to advocate their theories by their own creeds exclusively, and to detect, with microscopic eye, the fallacies or fictions of those which differ from them. So extensive an inquiry, however, would be incompatible with the limits to which I should here confine myself; and as I can refer to the works of the learned Cabanis, which are very full on that subject, I may commence from the period when Cullen,

Brown, and Darwin, by the aid of their extraordinary abilities, and the influence on medical opinions which their high characters had, transferred to Great Britain the doctrines of Solidism which had been previously promulgated on the Continent by the eloquence of Stahl, Hoffman, and Baglivi.

Influenced by the dogmas of these distinguished British physicians, medical writers, and lecturers, thenceforward framed all their theories, (but fortunately not entirely their practice,) taking little or no account of the state of the fluids in disease, notwithstanding that Sydenham's observations, as well as Boërhaave's more theoretic writings, had till that time taught, that the condition of the blood, and of its derivative fluids in disease, should form necessary parts of Pathology.

The new theorists were too acute observers not to perceive that the destruction of previous systems, at least so rapidly, was owing to their not bearing the test of the analytical method, which had been employed so effectually for the detection of

error in the other sciences; they gave theirs, therefore, as much as possible the semblance of being founded according to inductive philosophy. In their explanation of the phenomena of diseases, no particular notice of the condition of either solids and fluids was taken, and it presented little more than a brief exposition of the principal symptoms ingeniously interwoven with either an imaginary hypothesis of a proximate cause or essence of disease, dependent either on a transient condition of the solids, which could not be submitted to subsequent investigation; or connected with abstruse ideas of the laws of the animal economy, which the metaphysicians of that day pretended to have discovered, and by them vainly attempted to disclose the occult and ultimate relations between the operations of mind and body; and assuming them as data, employed arguments which would lead to Materialism in the moral, as certainly as to Solidism in the physical world. But however well contrived these fictions might be for deception, they soon sunk into well-merited contempt, under impartial and

vigorous scrutiny. But though that system of pathology, now known by the name of Solidism, was first introduced into England, and is still, to a certain degree, supported there on such speculations, it was in the founders of the great anatomical school in Windmill-Street that it was sustained by its ablest and warmest advocates, who, conscious of their extensive powers, derived from their anatomical skill in post mortem examinations, laboured with unwearied industry to form a system solely from the facts collected in examining the alterations of structure produced by diseases; a task recommended to their ardent minds, not only by attachment to their favourite pursuit, but also on account of its being opposed in principle to those untenable parts of the theories of the Humoral Pathologists, which they had been successfully employed in exposing.

The immense array of facts, which the distinguished brothers, William and John Hunter, made for this purpose, during their widely extended inquiries in dissection, recorded in their literary works, as well as

in their museums, has (independent of the great depositories since added by their pupils and followers) been justly the theme of admiration, as well for the persevering assiduity which it evinces, as for the importance which has been attached to it, or the influence which it has ever since had on those making similar collections, or on the improvements in medicine, but more particularly in surgery. Indeed, without meaning any conceit unsuitable to so solemn a subject, it may be said of the deathless name they have laboured so effectually to establish, that however "the evil they did "might live after them, the good was not "interred with their bones;" for there is scarcely any civilized country which does not at present possess either copies of their works or models of their museums. Fortunately, it is only necessary to refer, thus generally, to these works, and I shall confine my observations to that faithful epitome of them, entitled the "Morbid Anatomy," the text-book of dissectors since its publication; a work with which the well-earned fame of the late lamented Dr. M. Baillie is

intimately connected, and which was no less indebted for its celebrity to its author's highly respected name. In it the views of his distinguished relatives, in collecting preparations during the dissection of morbid parts, to which he was, even during their life-time, an active and large contributor, are zealously and ably extended by all that his own extensive opportunities, as lecturer in the great school of anatomy, presented in favour of similar principles. It was composed, indeed, with the avowed object of explaining "more minutely than had "hitherto been done, the changes of struc-"ture *arising from morbid actions* in some "of the most important parts of the human "body;" and, I might say, never would have led to the errors which others have committed under the affectation of having its sanction, if the subsequent attempts to found a purely solid pathology had been guided by the caution with which he delineated those bounderies he had reached, or if, like him, they had candidly defined their objects for extending them.

As an illustration of this opinion, and an apology for the freedom with which I shall venture to discuss some subsequent parts of this work, I am induced to take the following extracts from its Preface :—" The " human mind is prone to form opinions " upon every subject which is presented " to it, but from natural indolence is " frequently averse to inquire into the " circumstances which can alone form a " sufficient ground for them ; when, how- " ever, the mind shall be obliged to observe " facts, which cannot be reconciled with " such opinions, it will be evident that such " opinions are ill-founded, and they will be " laid aside." And afterwards he says, " Anatomy may be said to have arrived at a " high pitch of perfection, but our know- " ledge of structure, produced by disease, " which may be called Morbid Anatomy, " is still very imperfect."*

Now if the entire of the work be examined by the tests suggested in these extracts from its Preface, I feel well assured

* Morbid Anatomy, &c. &c., by M. Baillie, M. D. F. R. S., &c., London, 1812.

that the most accomplished anatomists would be most ready to acknowledge its imperfections, even in the diseases treated of, as a system of pathology; for in many cases similar to these, (especially such as are strictly medical,) the appearances stated to have been found on dissection, and on which the previous symptoms were supposed to depend, have been observed when no such corresponding symptom previously occurred. The converse of this is also true, for dissection has presented no corresponding alterations of structure to account for the symptoms that preceded death.

Such coincidents between diseased actions and altered structure as are stated to be sometimes detected by those engaged in morbid anatomy, cannot be doubted, and they, therefore, deserve close attention in pathological investigation. But if, as just intimated, such coincidences be not constant, neither can the illustrations they afford of the theory of diseases extend beyond the symptoms always found to precede the disorganization or effusion discovered after death. From very extensive

opportunity for observation, I am of opinion that these symptoms of disorganization or effusion are very generally limited (and especially in febrile diseases) to the very last stages. And from similar observation I am disposed still farther to limit such symptoms as essential characteristics of any period; symptoms, which, from their occurring in the last stages of diseases, have therefor ebeen noted by the Solidists, erroneously I think, as always indicative of disorganization or extravasation, but which I have frequently found to arise from the morbid condition of the blood while it still circulated in the sanguiferous system.

The symptoms I allude to are, interruptions or irregularity in the systole and diastole of the heart, with corresponding irregularity of the pulse; tendency to syncope, such as have been frequently set down as signs of disorganized heart; impeded respiration, accompanied by cough, hoarseness, *rales*, (according to the French phraseology,) indicative of disorganization or effusion of some part in the cavity of the thorax; and lastly, strabismus, loss of sight,

and paralysis, which have been as generally put down as symptoms of effusion on the brain, or on some other part of the nervous system; limitations which, I think, are of the utmost importance to the physician, as by extending them, he extends the scope of his professional duty, *(officium medici circa ægrum hominem est sanare;)* wherefore, with a view to an illustration of this part of the subject, I beg leave to refer the reader to the cases of spurious phthisis pulmonalis, apoplexia cephalica or meningeal apoplexy, paralysis, threatened *angina pectoris*, and also threatened dropsy of the chest and pericardium, which may be found at No. 2 of APPENDIX.

I am the more desirous to dwell on those limitations, from being fully persuaded that from want of due attention to them it is owing that so little has been contributed to the *ratio*, or *methodus medendi*, by modern pathology, as cultivated by the strict sect of Solidists, whose superiority over others seems to be confined chiefly to the degrees of certainty with which they can pro-

nounce a fatal prognosis.* On the same grounds I protest against their modern invention of the stethoscope, at least so far as it has been employed by them merely for increasing the supposed number of organic diseases, because in every instance in which it is fallacious all hopes that would otherwise tend to exertions for relief, are laid aside. I am aware, indeed, of a few cases where the employment of this instrument was beneficial in rightly deciding a question that had been warmly disputed on account of the presence of many of the symptoms which were supposed to indicate tubercles, or disorganization of the lungs; and one case of this kind that occurred to me was so remarkable through its whole course, and the decision on it being made by Mr. Laennac himself, I have given its whole history, as well as the appearances on dissection, at No. 2 in the APPENDIX.

As the opinions of the author of the "Morbid Anatomy" respecting those preter-

* This superiority, happily for their patients, I have seen questioned; and in several cases death, that was predicted by these dogmatists, waited to be ushered in by some other way, and at some other time.

natural appearances which the blood drawn in certain diseases exhibits, must necessarily have influenced his notions of Pathology,* as well as those of all others who adopted them, they should not be passed unnoticed here; and I may confess additional gratification in referring to those experiments which I instituted to prove the fallacy of such opinions, since they have been considered decisive;† being fully convinced, that if I could thus succeed in abolishing this false principle of the anatomical sect of Pathologists, which Bacon would have ranked amongst the "*idola tribus*," greater freedom to the investigation of truth would be the consequence, and facts would be more fairly appreciated. Those alterations from the natural condition of the blood, as indicated by the preternatural appearances it presents when drawn, could not fail to be esteemed as sufficient causes for several of the interruptions of health, and adequate explanation of many of the symptoms of disease; as much so, surely, as those alterations in

* See Morbid Anatomy, p. 21—23.

† See Medico-Chirurgical Review, p. 155, London, June, 1824.

structure found after death, on which so much attention has of late been beneficially bestowed. Perhaps there is no circumstance more remarkable in the history of Science, than that John Hunter, who had attributed and almost proved the paramount importance of the blood to life and health, should have rejected the opinion, that morbid alterations in it must be succeeded by disease or death, so as to be now quoted as that successful champion of Solidism, who had totally exploded the Humoral Pathology.

The experiments and results just referred to, may be found detailed in the first part of my "Pathological Observations," *pages* 36, 37, 38; also 43, 44, 45; and though I feel encouraged by the evidence they afford in favour of my pathological principles, yet I am still more interested in the new light they thus throw on a circumstance of acknowledged importance in the practice of medicine, i. e. the variations in the nature and appearance of the coagulum, according to the state of the body when the blood is drawn, of which the most remarkable is, (what is called) the size, or buffy coat. That my objects, how-

ever, in these experimental inquiries, and my application of their results to pathological investigation, may be more clearly understood, I beg leave to quote the following passage from the "Elementary System of Physiology;"—first, that it may be compared with the description of the natural state of the blood given in another extract from the same work, in the preceding section; and secondly, as it is the latest and ablest publication on that subject, I wish to evince, how generally those opinions mentioned by the author are entertained,—opinions which I am desirous to rebut, as I hope to do by showing that they are unfounded.

"The size, or buffy coat of the blood, "a term employed to denote that state of "the crassamentum when the upper part "of it contains no red particles, but exhi- "bits a layer of a buff-coloured substance "lying on the top of the red clot. This "buffy coat is generally formed when the "system is labouring under inflammatory "fever, and when, according to the modern "doctrines of Pathology, there is supposed

" to be an increased action of the arteries.
" The immediate cause in the crassamen-
" tum is obvious; the globules, or other
" matter which give it the red colour, begin
" to subside before the coagulation is com-
" pleted, so that the upper part of the clot
" is left without them. The remote cause
" of the buffy coat is not yet ascertained,
" although many experiments have been
" made to discover it. Hewson thought
" that the fibrin became specifically lighter,
" and of course the red particles compara-
" tively heavier, whence they would be dis-
" posed to sink to the lower part of the
" clot; he also thought that the blood
" coagulated more slowly.* Hunter was
" inclined to account for the appearance,
" by the firmer coagulation of the fibrin, as
" it were, squeezing out the red particles:
" but this would scarcely explain why the
" upper part of the clot alone is left without
" them. Hey's opinion is perhaps better
" founded; that by the increased action of
" the vessels, the different constituents of
" the blood are more intimately mixed to-

* Experimental Inquiries, p. 39—59, *et alibi.*

" gether;*—while Dr. Davy opposes the
" opinion of Hewson as to the fact of the
" slower coagulation of inflamed blood.†
" From some experiments that were per-
" formed on the composition of the buffy
" coat by Mr. Dowler, it appears that it con-
" tains a very large proportion of serum,‡
" and this, by diminishing its viscidity,
" will more readily allow of the subsidence
" of the red particles. It is, however, not
" improbable, that Hunter's opinion is in
" part correct, for we find that the clot of in-
" flamed blood obviously possesses a firmer
" texture than in its ordinary state, so that
" sometimes, in consequence of the con-
" traction of the clot, after it has began to
" form, the surface has a depression in the
" centre, forming what is called the cupped
" state of the coagulum. And here we
" have another analogy between the blood
" and the muscles; for there are several
" circumstances which lead us to conclude,
" that the force of muscular contraction

* Observations on the Blood, p. 10—19, *et alibi.*

† Phil. Trans. for 1822, p. 271.

‡ Med.-Chir. Trans., vol. XII., p. 91.

" through the system, generally is increased " in inflammatory fever."*

In lieu of commentary on this extract, I willingly avail myself of a critique on the experiments referred to, extracted from the 107th page of the second volume of the Quarterly Journal of the Medical Sciences, published January 1825, as it appears to me to afford a clear exposition of the opinions those experiments were intended to refute, and of the experiments themselves, as well as additional evidence quite decisive of the question at issue.

" The next part of the work (' *Patho-* " *logical Observations*,' Part I., &c.) con- " sists in observations on the connexion sup- " posed to exist between the appearance of " the buffy coat on blood, and the time " required for its coagulation. It is well " known that pathologists in general have " looked upon this as a merely physical " operation, and have explained it by the " slow coagulation of the blood in the cases " in which it presents itself, and by the time " thus allowed for the precipitation of the

* Elementary System of Physiology, Vol. I., pp. 446, 447.

" red globules. Dr. Stoker rejects this ex-
" planation, as being incapable of being
" reconciled with the results of actual ob-
" servation. He has given a tabular view
" of the state of the blood drawn from
" twenty-seven patients; the time required
" for its coagulation, with notices of the
" present, or absence of the buffy coat, &c.
" We do not think it necessary to transcribe
" this, but shall contrast one or two of the
" cases which appear to fully support the
" doctrine Dr. Stoker has advanced. In
" the second case, ℥x of blood were forty
" minutes before they began to coagulate,
" and an hour before coagulation was com-
" plete; there was not any buffy coat; the
" complaint was pain in the chest, cough
" and pyrexia; the pulse 96, and hard. In
" the eighteenth case, ℥viii of blood taken
" from a patient suffering from hoarseness
" and stitches, the consequence of influ-
" enza, began to coagulate in seven minutes,
" were not completely coagulated for four
" hours, but did not present any buffy coat.
" On the other hand, in the seventeenth
" case, ℥xii of blood taken for the relief

" of combined pneumonia and hepatitis " began to coagulate in three minutes, were " completely coagulated in ten minutes; " were cupped, and presented a cream-co- " loured buffy coat. In the twenty-fourth " case too, ℥x of blood taken for the cure " of pneumonia, began to coagulate in four " minutes; were coagulated in another mi- " nute; were cupped, and presented a " light-coloured buffy coat. The cases we " have quoted, which are among the most " marked, will serve to show that there is " not any direct relation between the ap- " pearance of the buffy coat, and the time " required for the coagulation of the blood. " We believe that Dr. Stoker is not the " first who has questioned the existence of " such a relation, but undoubtedly he has " the merit of having experimentally dis- " proved it. We ourselves had long ago " been led to similar results, though not so " unequivocal, by the consideration of the " appearances of the coagulum of blood " having the buffy coat. It will be seen " by a moment's reflection, that if the latter " were the product of slow coagulation, it

" ought always to take place to a greater " or less degree, and that the red globules " should be found dispersed through a " vertical section of the coagulum, in une- " qual quantities, being most abundant at " the lowest, and gradually decreasing to- " wards the surface. If such a section, how- " ever, be examined, no such appearance " presents itself; and on the contrary, the " buffy coat does not contain any sensible " quantity of red globules; whilst the tran- " sition from it to the red coagulum is not " gradual, but sudden and defined; the " distance from the surface at which it " takes place, varying according to the " thickness of the layer of fibrin.

" The occurrence of the buffy coat is " attributed by Dr. Stoker either to a want " of due preparation of the fluids at the " two chief sources of supply, and of the " subsequent changes these fluids should " undergo in their passage through the " pulmonary, sanguiferous, and hepatic " systems; or to the injurious effects of " diseased functions in the organs of san- " guification."

Since the article, from which this extract is taken, was published in the "Quarterly Journal of Medical Sciences," I have learned that my friend the Surgeon General had previously made similar observations on the dissection of crassamentum of blood with a sizy surface; but in conversing afterwards with himself on the subject, he informed me, that although he found that to be the case in some kinds of buffy coat, yet that others appeared to him to be connected with slow coagulation; and that in such cases he did not deem it indicative of increased action or inflammation in the animal from which the blood was drawn. This, founded, like his other opinions, on observation, accords entirely with my views as to the remote causes of sizy blood. I conceive that inflammatory action may sometimes precede, or even induce the functional derangement which impedes healthy sanguification, as well as that the state of the circulating mass at either avenues of supply, may also affect or disturb sanguification, so as thus to excite that preternatural condition of the blood,

indicated by such sizy surfaces, which hence becomes the source of morbid actions; but although the tenuity of the blood, assisted by the inflammatory action, may cause the slow coagulation which attends the appearance of the sizy or buffy surface in such cases, yet it does not follow that slow coagulation is to be deemed the cause of these preternatural appearances, in as much as we find in typhous fever and in many other diseases of debility, though the blood drawn coagulates very slowly and imperfectly, yet no such sizy or buffy surface is then to be observed.

I have further to remark, from much practical observation, that the buffy coat on the blood, though often, is not always indicative of inflammatory or increased action in the system from whence it was drawn; for I have found that preternatural appearance very remarkable in dropsical affections, though the pulse then, and for a long time previously, was even below the natural standard of frequency; and from similar observation I am convinced, that the state of the blood, on which these

preternatural appearances depend, however produced, is much more generally the cause than it is the effect of that increased or inflammatory action. A view of the remote cause of the buffy coat on the blood, obviously of great moment in studying the theory or improving the practice of Medicine.

As the various appearances of the blood drawn in disease can no longer, I think, be fairly attributed to any process subsequent to extravasation, the following explanation of them appears to me to be natural:* that one kind of these sizy surfaces, or buffy coats, is produced from disordered chyle (the consequence of unwholesome aliment or impeded digestion) not being duly prepared for the changes it should undergo in the minor circulation, the functions of which it disturbs, and then passes into the sanguiferous system, to be the source of morbid actions. Another kind arises from the morbid condition of the venous blood, in

* The application, to aid diagnosis in diseases, which I have made of these distinctions, between buffy coat that arises from imperfect sanguification in the lungs, and that which is indicative of imperfect sanguification in the liver, will be illustrated by cases noticed in my succeeding Essay on Epidemic.—*See* General Appendix.

its return from the greater circulation by the vena portæ, unfitting it for the preparatory process, which in health takes place in its passage through the liver, that it may be duly qualified for the functions of perfect sanguification, by which it was to be again refitted in the pulmonary system for the vital purposes of the greater circulation. These explanations appear further supported by the experiments on the blood, instituted since mine in London and Paris. I am therefore led to hope that they will hereafter be found of practical utility both in the *ratio symptomatum*, and *methodus medendi* of diseases.

The explication given by Dr. Scudamore of such sizy appearances of the blood is, I think, in many instances just; for it is very probable that the state of the solids may be such, that they cannot receive the entire quantity of fibrin conveyed to them by the extreme arteries, and hence it may accumulate in the sanguiferous system, and become extraneous to the venous system, by which it is returned to the heart.

The various appearances which the blood

presents in idiopathic and typhoid fevers, probably arise from changes effected on the vital fluids by morbific matter supplied to the lacteal or lymphatic system by the absorbents, which, entering by the thoracic duct, I suppose has the same sedative influence which poisons have on the pulmonary and sanguiferous systems. This view of the subject may also assist in explaining the length of time, or latent period, between the application of contagion, or infection, and the commencement of febrile paroxysms, or morbid actions.

A knowledge of the chemical properties of morbid blood, as it appears when drawn in both idiophatic and typhoid fevers, might have promoted the further investigation of this subject; for this, however, I had neither leisure nor sufficient knowledge of practical chemistry. It may be said that such considerations are more strictly physiological, and not needed in the consideration of the facts of altered condition of the blood and their necessary effects in disease, which are so important in a pathological point of view.

In addition, I must refer here to the Cases

and Dissections detailed at No. 3 in the Appendix, for illustrations of my pathology of several diseases that have been deemed to originate in organic derangement; which, I think, will be found, on closer investigation, to arise, in the first instance, from that condition of the blood indicated by sizy surfaces: the diseases to which I allude to at present are, croup, polypous concretions and hypertrophy of the heart, enlargement and adhesions of the thoracic and abdominal viscera, as well as effusions into all the natural cavities of the human body.

The foregoing observations, on the origin of the exclusive pathology of the Solidists and of its imperfections, respect medical diseases in general; but before I proceed further to remark on the failure of the partisans of that system, in which detail I intend to confine myself to their more recent pathology of fever in particular, I shall take the opportunity of transcribing a passage from the valuable "Digest of the Study of Medicine," lately published by Dr. J. M. Good, that I may add some explanation, which it seems to demand from me.

"The most triumphant fact in fever of the
" Boërhaavean hypothesis is, that the crust
" on the blood in inflammations,and cauma,
" or inflammatory fevers, is often found
" peculiarly dense. But as fevers (and cer-
" tainly the greater number) are found
" without any crust, and as a similar crust,
" though perhaps not quite so dense, exists
" under other and very different states of
" body, as in pregnancy and scurvy (por-
" phyra,) even this leading appeal has long
" lost its power of conviction, whilst the
" abruptness with which fevers make their
" assault from sudden occasional causes,
" and in constitutions of every diversity,
" forbids the supposition, that in such cases
" a lentor or sizy crasis of the blood, and
" especially a *glutinosum spontaneum*, can
" have time to be produced, however, as it
" may exist occasionally, and be, perhaps,
" the source of other disorders. The sub-
" ject, however, has of late been again taken
" up by Dr. Stoker of Dublin, with a view of
" reviving the Humoral Pathology in its
" more important doctrines, and of extend-

" ing the arguments which have hitherto " been urged in its favour."*

Thus called upon, I object, *toto cœlo*, to that part of the Boërhaavean doctrines which attributes fever in general to the sizy condition of the blood, which it may be seen is contrary to the tenor of my works on that subject, and so far from such sizy or buffy coats being the necessary attendants on fever, I questioned them even as proofs of inflammation, because, as already stated, they are found in dropsy and other diseases, where neither heat, pain, redness, nor quick pulse, (the ordinary signs of inflammation,) preceded. In typhoid fevers a condition of the blood, the very converse of the morbid one thus indicated, takes place, when the coagulum of the vital fluid is generally broken down, and its serum is of a dark, often of a greenish hue; and in symptomatic diseases, the buffy coat, I

* See Study of Medicine, by Dr. J. M. Good. When tracing the course of the epidemic, I shall attempt to show by cases and dissections, that a sizy crasis, arising from disordered sanguification, frequently takes place; and also, that tenuity, or want of consistence in the vital fluid, is a source of morbid actions, but especially in typhoid fevers.—See Cases in General Appendix.

think, generally indicates disturbed sanguification, which, as previously intimated, is sometimes merely the effect of inflammation. I would further observe, that I have never seen a sudden solution of disease in cases where the buffy coat has been deep or dense; such as Dr. Good very justly states, in some instances succeeds slight hemorrhages from the nose or other parts.

My remaining observations on the failure of the partisans of Solidism in the pathology of fevers, will consist chiefly of a narration of facts, justly styled the Philosophy of History, which have passed under my notice in the great Fever Institution in Cork-street, during twenty-three years that I have been Physician to it. By comparing the doctrines which would place idiopathic fevers amongst the phelgmasia of Cullen, manifestly the fruits of those metaphysical and anatomical theories, which I have already endeavoured to show were branches from the original stock of Solidism, I shall state the process by which I have examined their validity.

The new doctrines* of fever, which I deem branches of Solidism, in as much as they attribute fevers generally to inflammation, or altered structure of some vital part, and suggest blood-letting as their common remedy, are directly opposed in principle to that theoretic division of fevers into idiopathic and symptomatic, which I have ever deemed natural and useful in practice.

* The doctrines successively adopted by Drs. Clutterbuck, Armstrong, and Mills, in this country, and by Marcus and M. Broussais on the Continent, may be consulted in the works of each of these authors; and the very favourable reception these works met with, may be judged from the reviews of them that appeared soon after their publication.

That a strong counter-current in public opinion respecting them has commenced in England as well as on the Continent, as has been shown in the preceding pages, may be fairly inferred from the tenor of several articles that have lately appeared in some even of those periodical Journals where they were once noticed with approbation. In the first article of the first number of the "New Journal of Medicine and Surgery," published at Edinburgh in the beginning of the present year, this counter-current may be distinctly perceived. And in an able analysis of the doctrines under consideration, published at London in the last number of the Medico-Chirurgical Review, the return to what I conceive more correct pathological views, is equally remarkable. But though I hail these proofs of my principles being so generally adopted, yet I must confess that I cannot perceive with satisfaction, that my constant efforts in support of the tenets, which the authors of the articles adverted to now

It therefore became my duty, as Physician to the Fever Hospital, to examine the arguments closely on which these doctrines were promulgated and maintained; more especially as they were said to have been deduced from actual observation made in the course of diseases, or on the appearances of parts discovered after death.

The process of the examination which I accordingly made, through the opportunities offered to me in the largest and most fully occupied Fever Institution yet established, may be seen detailed in the Re-

maintain, have been so entirely unnoticed; more especially as these, my practical observations on the subject, were some time published.

In the able Essay by Dr. Gerson, in the Magazine of Foreign Literature, published at Hamburgh, for July and August of 1824, to which I have already adverted, a similar recurrence appears to have commenced both earlier and more remarkably in Germany than in Great Britain; and I have endeavoured to mark my respect for the author of this Essay, by acknowledging, as well as by availing myself of the information I derived from it; but however desirous I may be to conciliate his favourable opinion, I must beg to explain, that my views in the work, which has been honoured by so much of his notice, were by no means intended, as he seems to suppose, to overturn the pathology founded on altered structure; on the contrary, my object was to amend it by certain additions that might still be made by gleanings from the too generally exploded Humoral Pathology.

ports I published from that Institution, and in a separate Essay on Fever, printed at London in 1814.

In the three earliest of these Reports, evidence was adduced in favour of the division in question, showing, from several hundred cases taken without selection, that the same tendency still continued in idiopathic and typhoid fevers, to terminate favourably or fatally on certain critical days, as had been observed to characterize them by Hippocrates.* And in my Report for the year 1811, I added facts to prove that fevers subject to such periodical revolutions, were frequently the consequence of exposure to contagion or infection.

The Appendix† to my Essay on Fever, contains the result of a trial of the merits of these doctrines, which promised to contribute more to a satisfactory decision of the question, than any of the previous investigations, both on account of the extent of the comparison instituted between the op-

* See Collections, &c. &c., on Effects of Sol Lunar influence in Fevers, by Francis Balfour, M.D., London, 1816.

† See my Treatise on Fever, published at London in the year 1815, pp. 193—4.

posed modes of treatment, and the faithfulness of the records from which it was deduced. But previously to the brief extracts from this document, which I intend to adduce, for showing the result, I should observe, that after much reflection on the subject, I feel convinced, that in making this comparison, if on the one hand all those mixed cases of typhous and inflammatory fevers, in which blood-letting may have been too cautiously withheld, could be separated, and on the other, all of the same kind, or of those of actual inflammation, in which that remedy must have been beneficially employed, the results of the numerical calculations would have been much more decisive against the practice in purely typhoid fever than they now appear. For Dr. Mills, then one of my colleagues, who, though he had previously much experience in the treatment of fever, adopted Dr. Clutterbuck's views, with certain modifications, and practised blood-letting in a very large proportion of cases,—at times nearly with all his patients in the Cork-street Hospital. By the other Physicians, that

remedy was rarely employed, and the estimate of the value of repeated blood-letting in fever indiscriminately, compared with other modes of practice, (modified as just now suggested,*) still further confirmed my previous opinions. I feel it due, however, to Dr. Mills to add, that previous to his publication on the subject, I would not have ventured on general blood-letting in mixed cases of inflammatory and typhoid fevers to the extent which I have employed it since (sometimes with advantage) for the relief of co-existing or supervening symptoms of inflammation. It appears however, from the document just referred to, as the final result, that among the patients treated by blood-letting, the proportion of deaths to recoveries was as 1 to $11\frac{42}{65}$; and among those treated according to the more ordinary methods, as 1 to $12\frac{10}{110}$; a proportion differing from the former in no small degree, and justifying the conclusion, that the treatment of fever by small and re-

* See extract of a Letter from the Physicians of the Fever Hospital, Cork-street, which, with the original tables, are inserted in the APPENDIX.

peated general detractions of blood, is either of little efficacy, or injurious.

Dr. Mills's patients have remained in the Hospital on an average 16¾ days; the patients of the other Physicians, 17⅞ days; a difference very slight, and though apparently in favour of blood-letting, yet most probably arising not from this practice, but from Dr. Mills having under his care a much larger proportion of male patients than the other Physicians; "and these patients, "from obvious reasons, are less disposed "than the females to remain unnecessarily "in the Hospital after their recovery." *

The following division, therefore, as set forth in my Report from the Fever Hospital and House of Recovery, Cork-street, for the year 1819, appears to me, from being applicable to practical purposes, to be a very natural one of continued fevers :†—first, idiopathic fevers, comprehending those of spontaneous and contagious origin ;—se-

* See Dr. Browne's Essay on the controlling Power of Remedies in Fever.—Edinburgh, 1815.

† Amended definitions under the division of fevers proposed above, will be given in my Observations on the aggravated and fatal Forms of Epidemics, within the last three years and a half.

cond, inflammatory fevers;—and lastly, those in which two kinds coexist or mingle together; a division, however, though sanctioned by the first nosological authorities, not entirely unobjectionable, yet is adopted here for the reasons already assigned, and the most comprehensible part of that labyrinth in which a medical observer is involved by theorists of every age, to whom we may generally apply what Hippocrates said of the lecturers of his day:—"Unusquisque suæ "orationis testimonia, et conjecturas ad- "dit,—vincit hic, modo ille, modo iste, cui "potissimum lingua volubilis ad populum "contigerit." The medical prescriber can only pass securely from such perplexity, by constant observation of the phenomena of nature.

The cases admitted into the Cork-street Hospital belong in general to either of the first or third parts of this division; their relative proportions varying much at different seasons of the year, and in different epidemics. Cases belonging to the second part also occasionally get admission, (though not strictly those for whom the Institution

was intended,) it being sometimes advisable to remove persons labouring under inflammatory, and even other diseases, from situations exposed to all the circumstances favourable to the generation of fever and of contagion.

The characteristics of the first class of fevers, very generally, are sudden prostration of strength; failure of mental and bodily power; suspension or disorder of the secretions; irregular vascular and pulmonary action, producing both inequality in the distribution of blood, and of temperature in different parts of the system, and an uniform tendency to perform a certain succession of salutary or morbid changes, in definite periods. The distinguishing features and local origin of the second class, or inflammatory fevers, are well marked and generally known; but when their symptoms mingle in epidemics, with those of typhoid or idiophatic fevers, the importance is commensurate with the difficulty of ascertaining their relative degrees, so as safely to decide on the means to be employed.

The opinions too, that typhous and

synochus are not essentially inflammatory, but in their simple forms are diseases of debility through their whole course; the excitement, so observable in their early stages, being referable to constitutional reaction, accord with my experience.

Neither has morbid anatomy appeared to me to warrant the conclusions of those who hold the opinions of typhous fever being an essentially inflammatory disease. In some instances, I have observed the same partial turgescence of vessels which dissectors report, and likewise signs of inflammatory action in various parts of the bodies of those who died of fever; the former, however, by no means designated previous inflammation; and I could generally trace the commencement of inflammatory action, which led to the latter appearance, from local diseases, that preceded or supervened on fever, and sometimes at late stages; but in several cases, where I had witnessed the highest degree of febrile excitement before death, no such signs of turgescence, or of inflammation, were observable on dissection.

In support of such statements, I was happy to be able to add, in the same Report, the satisfactory testimony of two of our most distinguished Anatomists. And besides their unquestionable qualifications, and numerous opportunities for examining the bodies brought to their several theatres for dissection, their opinions on anatomy have peculiar advantages of being deduced from observations that were not influenced by preconceived theory, or by cases selected for particular views.

"In the midst," Mr. Kirby writes in answer to my queries on the subject, "of "all the discussions relative to topical con- "gestion, it was impossible however not to "remark the singular want of accordance "between the prevalent opinions and local "appearances. The brain, so constantly "supposed to be the seat of inflammation, "rarely exhibited the characters indicative "of such a state. In some instances this "organ was much paler than usual; in a "very few, amongst a great number of dis- "sections, was there any evidence of san- "guineous or serous effusion." And after

recounting his observations on all the other vital organs, he thus concludes :— "In "short, in a great majority of cases, so "little did any particular organ seem to "suffer, that I have wondered what could "have been the cause of death."

With the foregoing statement, the answers of Dr. Macartney, Anatomical Professor of Trinity College, Dublin, to the subsequent queries of Dr. Barker on the same subject, are coincident, and appear to me further to corroborate the opinion, that typhous and inflammatory fevers are distinct. I may, therefore, be allowed to add the following extract from Dr. Barker's Report for the Years 1817 and 1818, as published in the second volume of Transactions of the Irish College of Physicians, *page* 574 :—

"He (Dr. Macartney) informs me, "that having reviewed his notes on the "anatomical examination of persons who "died of typhous fever, he can state, as the "result of his experience, that morbid ap-"pearances in typhous fever are not those "of common visceral inflammation." And having stated more fully the actual observa-

tions on which this opinion was founded, he concludes thus :—

" Two facts deserve to be recollected : " first, that the duration of general fever " and visceral inflammation are not the " same ;—second, that internal inflamma- " tions are very common in hot-blooded " animals, but idiopathic fever is peculiar " to the human kind." It may be added, that processes of an inflammatory nature are fitted for repairing parts that have their functions interrupted, or their structure injured, but the effects of typhous fever have no such power.

Thus far the evidence, which I have adduced and referred to in the foregoing extracts, has chiefly regarded negative proof that signs of inflammation are not uniformly or generally found on minute dissection of those who die of typhous fever, as asserted by those who attribute it to inflammation; extracts which I would willingly extend, in order to describe more fully the process by which I have endeavoured to come to a right decision on a question, which I believe involves the health and lives of mankind more than any other of the science

of medicine; and by stating, as far as my prescribed limits admit, all the circumstances of the case, I wish to enable others to exercise their own judgment, when called on to return a verdict so fearfully important to their fellows.

In tracing the steps of my inquiries further, I mean to illustrate my opinions on the present occasion, by extracts from my previous publications, which tend to show not only, that fever is not essentially derived from disorganization of the solids, but that its cause, as well as the alterations of structure, may be often traced to originate in the blood, or in the fluids from which it is derived. Illustrations however, for reasons to be assigned, will be more full in my account of the epidemics of Dublin under their aggravated form the last four years.

With such views, I stated in my Report for 1820 and 1821, the remarkable coincidence found between the symptoms and appearances after death of the fatal cases which occurred in 1818, when famine excited an extraordinary degree of fever in this country, and those poisonous effects related by Dr. Kerner, to have followed the

use of certain unwholesome animal substances at Wirtemburgh ;* and I shall repeat the extract I then gave from his work, premising that he also found the whole system of the ganglionic and cerebral nerves generally affected ; a circumstance so frequently observed by Dr. Robert Reid, in the dissections of those bodies he examined after death from the fever of 1818, that he supposed that system to be the chief seat of the disease. The similitude between the symptoms to those fatal cases will, I think, be readily recognised by the experienced, and I am, therefore, induced to give the following extract from Dr. Kerner's publication :—

" The symptoms of poisoning commonly
" commence twenty-four hours after the in-
" gestion of the aliment, rarely sooner, and
" sometimes later ; a severe burning pain
" is then felt in the epigastrium, and vomit-
" ing of the sanguineous matter occurs from
" time to time ; the eyes becomes fixed,
" the lids immoveable ; the pupils are dilated,

* Nouvelles Observations sur les Empoisennoments Mortels que arrivent si souvent dans le Wirtemburgh par l'usage de Boudius fumes, par le Dr. Kerner, Tubingen, 1820, 12 Morn.

" and remain insensible to the action of
" light ; the patient sees double ; the voice
" is affected, and there is often aphonia,
" more or less complete ; respiration is im-
" peded ; the beating of the heart cannot
" be felt ; there is frequent syncope ; the
" pulse is weaker than natural ; the veins
" of the neck are dilated and prominent ;
" deglutition is extremely difficult ; fluids
" fall into the stomach as into an empty
" vessel ; solid food remains in the œso-
" phagus ; all secretion seems suspended.
" There is an obstinate constipation, or the
" excreted matters are hard and dry, earthy,
" and not tinged with bile. The intellec-
" tual faculties remain perfect. In some
" instances the patient becomes irascible ;
" rarely is there insomnia ; the appetite
" often remains ; the thirst is great ; in-
" teguments insensible ; the patient scarcely
" perceives the impressions of heat and
" cold ; the palm of the hand is hard and
" coriaceous ; the skin in general is cold
" and dry ; it is impossible to restore trans-
" piration. Urine is copious, its excretion
" difficult. The motions are slow from the

" syncope which threatens the patient on
" the slightest effort; but there is no fatigue
" in the muscles of the back or loins.
" When death follows, it is at the end of
" from three to eight days; respiration be-
" comes impeded, the voice is lost, the
" pulse sinks, and life ceases sometimes
" after slight convulsive motions, the pa-
" tient having retained his faculties to the
" last. In case of recovery, convalescence
" is very long; a sort of exfoliation often
" occurs from the mucous membranes. The
" symptoms vary in different cases, and
" some are occasionally observed, of which
" we have not spoken, as, diarrhœa, de-
" lirium, vertigo, atrophy of the testes," &c.

Of late, more particularly within the last three years, the cases which have been reported to succeed punctures received during dissection in the various schools of anatomy in the United Kingdom, appear to me to throw much light on the pathology of fever, particularly of typhous fever, to the symptoms of which those of such unfortunate cases will be found, on comparing them, to bear the closest resemblance; in-

deed, I do not think it would be possible, amongst the multitude of cases of typhous fever with which Dublin has for a long time so deplorably abounded, to select more exquisite examples of that disease than those published in the collection made by Dr. Duncan from the effects of wounds received during dissection, or than that of his own case so ably drawn up by Dr. A. T. Thomson. The similarity of the mode of attack and of the symptoms, induced me also to suppose a similarity of the exciting cause, i. e. a specific virus received, though by different modes of ingress, into the sanguiferous system; and the identity of the nature of both appears to be farther established from the same treatment being indicated for both. Accordingly, on a former occasion, I adverted to these facts as affording evidence in favour of my pathological principles. In the cases which I have noted, to illustrate the course of the epidemic, this similarity may be seen; and also, that I had adduced the same corollaries in the Fever Hospital and House of Recovery, Cork-Street, as had been after-

wards successfully employed in the treatment of typhoid fevers, produced by infection received by puncture. I should next proceed to illustrate my pathology of fever, by tracing the rise and progress of those pestilential forms of epidemics, which have committed such havoc on all ranks of our population within the last three years and seven months, by showing the remarkable coincidence which during that period occurred between the intensity of the symptoms of both sporadic and epidemic diseases, and the marks of altered condition of the animal fluids:—"Luctaus cum morbis na-" tura quam pathologia exhibet facultates " affectionesque suas etiam explicatius pro-" ducit."* Such pathological investigation will be made in the succinct account of the epidemic, which I propose to introduce with this Dissertation on the Institutes of Medicine, or perhaps I shall rather say on the attempts I have made to extend the knowledge of them, and, I trust, improve the theory of Medicine.

* Institutiones Pathologiæ Medicinalis, Auctore, H. D. Gaubio.—Edit. Leid. Batav. 1781.

SECT. III.

THERAPEUTICS.

My observations on precepts for the prevention and cure of diseases (the last but not least important branch of the Institutes of Medicine) must not only be very brief, but strictly confined to those points of view which may be extended by the illustrations afforded from the Pathological Physiology insisted on in the preceding sections, the limits intended for this Dissertation having been already passed. In order, however, to prepare for such partial views of Therapeutics, I shall select from Boërhaaves' comprehensive Definitions (to which I beg leave to refer) such parts as I have applied these illustrations to, for examining the theory of diseases, but particularly for improving the *ratio* and *methodus medendi*.

1069.*—Hæc autem remedia sunt applicanda unicuique ægroto singuli ita, ut in eo fiat mutatio necessaria (1068;) quare

* Institutiones Medicæ, Hermanno Boerhaave.—Edin. 1773, p. 398.

Medicus scire debet primo quidnam in eo debeat mutare, atque deinde quibus auxiliis ad hæc uti; ideoque et nosse debet effectus, qui ab applicatis his in hoc ægro sequuntur; quæ ambo tantum potest discere ex iis, quæ in hoc ægrotante ipsi innotescunt ita certò per sensus, aut per accuratum ratiocinium, ut inde perspeciat actionem quæsitam, et auxilia.

The Pathological Physiology which I have adopted, indicates diseases in the Fluids as well as in the Solids, but that they more generally commence with changes in the former, than in the latter, and often terminate in disorganization or altered structure of parts; further, also, that altered structure may lead to morbid actions, and thence to changes in the animal fluids by functional derangement. In studying the *methodus medendi*, however, the first place should be ceded to the consideration of the cause of diseases while they are still confined to the fluids within their proper vessels, not only because they frequently originate there, but also that it is then that curative treatment can be most

effectually employed; for when altered structure or extravasation lies beyond the Surgeon's province, other means seldom succeed, farther than to palliate distressing symptoms.

I must refer to my publications on Fever and Dropsy, and also to my Observations on the Epidemic Diseases of Dublin, which are intended to be placed after this Introductory Dissertation for the suggestions afforded by my pathological principles as to prophylaxis, or the means of prevention of diseases; and with respect to the remedies or curative treatment I have been led to employ on the same ground, the following catalogue of those remedies which I have been enabled to recommend, in addition to the more ordinary mode of treatment, with indications annexed, and with references for illustration, must for the present suffice.

External pressure and friction, assisted by the use of Digitalis and Mercury internally, for the removal of tumours, specially those forming within the abdomen, illustrated by cases of spontaneous aneurism, the simplest form of altered structure from vascular dis-

tention, connected with alteration in the blood, and impeded function, in which these remedies were successfully employed. *See Transactions of the Association of the Fellows, &c., of the King and Queen's College of Physicians in Ireland*, Vol. I. p. 11. Remedies with which I commence my catalogue, on account of the indications being obvious, and the share which the internal remedies employed had in the cure being illustrated by their efficacy, unaided by mechanical means, in arresting the progress to organic derangement or hydropic effusion of other diseases while still in the fluids.

The *modus operandi* of digitalis and other vegetable bitters, especially those ranked amongst the poisons, I believe too may be best illustrated by the well-known effect of such poisons in attenuating the blood of those who have been killed by their too free exhibition.

Antimonial Medicines in general, but more particularly James's Powders in febrile diseases, especially when accompanied by local tendencies to serous effusion on the brain, or its appendages. The indications for these remedies were the condition of the fluids, and

the morbid actions connected with it, whether regarded in the relation of cause or effect, and on which these remedies appear to exert a corrective influence.—*See Cases of threatened Hydrocephalus relieved by these remedies, and fatal cases of that disease, with the appearances found after death, which were published by me in the Dublin Medical and Physical Essays*, A.D. 1806.

Blood-letting, local and general, as suggested in the Works of Drs. Blackhall, Wells, and Crampton, *in dropsical tendencies, connected with distention and increased action in the vascular system*; conditions produced as well by the morbid alterations, as want of the proportion of the ingredients of the blood itself; and also, by the accumulation in the sanguiferous system to which these alterations tend.—*See my Pathological Observations*, Part I., p. 156 to 164.

The remedies too which I have been led to employ in addition to the general treatment of febrile diseases, more particularly as they will be stated more fully in my account of the epidemic diseases of Dublin, and their indications illustrated by cases, may be very briefly and partially noticed here, viz.

Barm or Yeast in petechial eruptions, as formerly suggested by the Rev. Mr. Harwood.* *Blisters to the right hypochondrium, and leeches to the verge of the anus, assisted by mercury and digitalis internally*, to relieve green vomiting, which is often accompanied by eruptions of the worst kind of petechiæ, and jaundice of the interstices, *urina flammea*, or bloody urine, and sometimes with purple extremities, and tendency to dropsical effusion.—*See Pathological Observations*, Part I., from p. 164 to 175, inclusive.

Tartarized Antimony in solution in tendency to phrenitic fevers, and dropsical effusion on the brain.—*See my Reports from the Fever Hospital.*

Infusion of Green Tea, as suggested by the late Dr. E. Perceval, in comatose affections, especially those accompanying typhous fever.—*See Transactions of the King and Queen's College of Physicians, &c.*, Vol. II, p. 44.

Alum in large doses in solution, as suggested by Dr. Scudamore, in internal hæmorrhages, particularly dysenteric cases.

The plant of the *Verbascum Thapsus*, or

* See Essay on Fever, London, 1824.

White Mullein, boiled on milk, in obstinate dysenteries, a popular remedy which I was induced to make trial of in some of the worst forms of the epidemic dysentery that prevailed last Summer.

Friction with ammoniated and camphorated oil, and bandages over the abdomen, assisted by stimulating and antispasmodic enemata for the relief of tympanitic distentions of th e abdomen.

Vinegar in large doses to relieve hickup, assisted by frictions and blisters to the abdomen.

To these articles which I have selected in the treatment of fever, I would add, *the immediate weaning of infants suckled by mothers labouring under petechial fever*, the necessity of which has been indicated in the Fever Hospital and House of Recovery, particularly since the commencement of the pestilential epidemics which have prevailed since 1823; a necessity which I suggested to the Managing Committee of that Hospital, and recommended that wet nurses should be provided for such children; to which the members of that committee attended with their usual humanity. But finding it im-

practicable to provide nurses as speedily as the exigencies of the cases demanded; regulations were made to provide for the care of the weaned infants, by additional nurse-tenders, and also to have such delicate kinds of food prepared, particularly Arrow-Root, as the Physicians might wish to prescribe. Regulations by which I am persuaded that the lives of many infants have been already preserved.

Another improvement was suggested in the management of patients labouring under violent delirium, by the injury sometimes sustained by such patients in their resistance to restraint from the strait waistcoat, they having drawn it so tightly over the principal thoracic or abdominal viscera, as greatly to impede the function of these organs, especially when nurses were engaged in some other part of the wards, or were asleep at night: in order to obviate this, I have found that ankle-straps attached by leather fastenings to the foot-board of the bed answered the purpose well, and allowed of the waistcoat being left so loose, that no injury was sustained from it.

Many of these remedies may seem to

a cursory observer, of trifling moment, but having often witnessed their utility, I do not hesitate to recommend them, however insignificant they may appear, as inventions. Besides, with respect to the treatment of fever in general I may observe, that a remedy very simple in its nature when timely employed, may often render a more complicated one unnecessary. In this way I am inclined to account for the application of fomentation to the hypogastrium being so often effectual in superseding the use of the catheter, and for fomentation to the feet and legs, rendering arteriotomy at the temple as frequently unnecessary.*

I am not less inclined to assert my claim to improvement in the treatment of fevers, by omitting some remedies that I found injurious, than by adding others which I found beneficial. From want of space,

* I have learned from our intelligent surgeon, Mr. Trant, that he has had occasion to employ the catheter in very few instances of late. And I can assert, that I have not directed arteriotomy for the last four years, in the proportion of one case in ten of those admitted to the Cork-street Hospital in the earlier years of that institution.

however, I must defer the consideration of these, especially as regards the omission of blood-letting in *typhous* fever, till I enter on the general description and treatment, as I purpose, of the epidemic diseases of Dublin.

APPENDIX.

No. I.

CASE I.

CHRONIC HEPATITIS.

Mr. S———, 54 years of age, of robust make, and saturnine temperament, called for my attendance at Harold's Cross, on the 16th of December, 1824, and I found him labouring under general Dropsy and Enlargement of the Liver, which, notwithstanding that ascites had taken place, might be felt indurated and scabrous beneath the lower margin of the right hypochondrium. These complaints I learned had commenced some months before on the spontaneous stoppage of hæmorrhoidal discharges, which had been periodical. The dropsical effusion, however, had increased most remarkably during the preceding week. He expressed a distressing sense of oppression in every part of his body, and great mental anxiety; the surface in general was cold and livid, and covered with a clammy perspiration; the face was occupied with a dark flush, the lips and tongue being of a deep claret colour. The breathing was laborious, though not materially affected by posture, as he was lying horizontally, and did not express relief at being raised erect; the air expired was cold; the pulse was full, quick, and irregular, intermitting at every third beat, accompanied with corresponding irregularity of the action of the heart—action which might be distinctly felt over a great portion of the left side of the thorax. There was a synchronous pulsation at the epigastrium; cough frequent and hard, with scanty expectoration, accompanied by bronchial wheezing; alvine discharges frequent and melænous, but scanty. The secretion from the kidneys also scanty and turbid. Thirst urgent; nausea frequent.

Under such circumstances, there was nothing to justify any material addition to the judicious treatment previously adopted by Mr. Kirby, whom I found in attendance; but on the succeeding day, when he and I were suddenly convened, on account of an apoplectic attack under which we found our patient labouring, the distention and increased action of the blood-vessels in the head indicated the opening of the temporal artery as the only means for even momentary relief. As the blood flowed the apoplectic stertor was mitigated, and the blood itself as it issued saltatim from the artery, was as black as ink.

A fatal termination occurred on the succeeding day, and the friends of the deceased would not suffer the body to be examined.

N. B.—This is the case referred to in the Section on Physiology, p. 26.

CASE II.

DYSENTERY.

On the 7th of December, 1825, at midnight, I was called on to meet Mr. Kirby, in the case of Mr. C————, æt. 27, who had been labouring for some days under Dysentery, and who had such a hæmorrhage from his bowels in the course of that night, that the attendants stated he had lost what they considered amounted to between five and six quarts of grumous blood. A deadly pallidness prevailed over every visible part, and the surface was cold and clammy—the air he expired was also cold; his speech was tremulous, and the few words he uttered were incoherent; his respirations very slow; pulse quick, weak, and small, too indistinct to be numbered; some hickup and vomiting; swallowing difficult.

By the application of warm flannels to the surface, nutritive stimulating injections, a blister over the right hypochondrium, and, as far as the stomach could bear it, wine, and a mixture of

compound infusion of mint, holding fifteen grains of alum to each ounce in solution, vital power was considerably restored, and the hæmorrhage stopped in the course of the night; so that on our visit on the succeeding day we were able to resume the preparations of mercury, which had been previously in use, and to employ nutritive means, especially gravy in the form of enemata, so as to preserve and improve vital power. In a few days afterwards, however, aguish paroxysms commenced, which assumed the form of an irregular tertian. To the ague succeeded hepatitis, and on hepatitis supervened dropsy of the right leg and thigh, that commenced suddenly with the symptoms of *phlegmasia dolens*. It is unnecessary to the objects for which this case is given, to detail the various means employed for these diseases, farther than that various preparations of Peruvian bark, given with the concurrence of Dr. Jackson, (in consultation,) were beneficial in arresting the ague; and that mercury evidently evinced its usual efficacy in removing the supervening liver-disease and dropsy, and in restoring the patient to that perfect health which he has since enjoyed.

The circumstance for which the case was referred to in the Section on Physiology, (at page 26,) was the remarkable coldness of the body, and particularly of the air expired in breathing, connected with the hæmorrhage from the bowels; exemplifications of which will be further referred to when treating on Epidemic Fevers.

No. II.

CASE III.

SPURIOUS PHTHISIS.

[Referred to at page 100.]

Mr. C————, 50 years of age, of athletic make, but gouty habit, and whose health, in the course of the preceding year, had been frequently interrupted by attacks of irregular

gout, first consulted me on the 16th of August, 1823, on account of extensive erysipelas of the right leg and foot, which commenced several days before with gouty inflammation of the great toe, accompanied with considerable fever. The fever and erysipelas, attended with loss of rest and appetite, were now his chief complaints: bowels costive; urine scanty and turbid. For some days, under the use of the mercurial pill, assisted by draughts of rhubarb and magnesia, so as to keep the bowels moderately free, with a cooling regimen, and plentiful dilution with mild drinks, these symptoms declined; the urine at the same time becoming copious, and depositing pink-coloured sediment copiously. On the 21st of August, however, a new train of symptoms set in with considerable accession of fever and icteric discoloration of the surface, particularly of the albuginea of the eyes, a sense of uneasiness in the right hypochondrium, and frequent irritating cough, which was not relieved by any of the usual demulcent expectorants, even with the addition of opiates. On the day following a copious expectoration of dense white *sputum* commenced, but without any relief either of the cough or fever; on this account, and strong febrile exacerbations being felt daily, the patient was removed into country air, and put on the use of asses' milk and vegetable diet. The fever, however, assuming more and more of the characteristics of hectic, and the expectoration becoming extremely fetid, his friends became apprehensive that the organization of the lungs was affected; and though I had seen equally urgent symptoms then and since attendant on certain forms of epidemics prevailing in this country, which terminated favourably in their own climate,* I did not oppose the decision of a consultation with Drs. Colles and Cheyne in the beginning of September, that this gentleman should pass the winter in the South of France. The journey thither, however, was undertaken with a precipitancy which I did not expect; as from the

* See my Pathological Observations, Part I. from p. 210 to p. 218, inclusive.

nature of the disease, which I deemed functional derangement of the liver caused by translated gout, I could have wished that the patient had remained a few days longer at rest, so that these complaints might assume a more decided form.

I did not see this patient again until the 13th of July, 1824, when I learned from him that in the interval he had travelled to London, where he consulted Dr. Warren, and thence to Paris, where he consulted Drs. Morgan and Todderick, in conjunction with M. Laenac, by whom he was examined with a stethoscope, and pronounced then free from organic disease of the thoracic viscera; in consequence of which he returned to England, where he placed himself under the judicious treatment of Dr. Blackall at Exeter, until within a few days previous to his calling on me after his arrival in Dublin.

I found him much emaciated, with considerable derangement of the biliary system; the liver itself (on examination) was evidently enlarged; the *sputum* expectorated of the same chalky white colour and friable consistence as when I first witnessed it; there was no fever, and the functions of digestion appeared to be chiefly impaired; the respiration and pulse were perfectly regular.

Under these circumstances, I recommended a residence in the country near Dublin; a mercurial plaster to the right hypochondrium; a light, but nutritious diet; preparations of dandelion and soda to be taken daily, and occasionally blue pill, assisted by saline aperients.

Under this plan of treatment, pursued with little variation, every symptom of ill-health gradually disappeared during the remaining part of the summer and succeeding autumn, and his flesh and strength were in a great measure restored, until the month of February, when on taking undue liberties in diet, he was attacked with *cholera morbus*, to which dysentery succeeded, which again reduced him very considerably; and he had scarcely recovered from their effects in the following month, when unfortunately he was induced to resume his residence and offi-

cial employments in Dublin, and soon after was attacked with an influenza, which then spread very generally, owing to the long continuance of dry easterly winds. His strength and flesh rapidly ran down under the fever accompanying this new attack; and having suffered for some days severely from the same kind of hectic fever and expectoration, which he had formerly laboured under, he died in the beginning of May, 1825.

DISSECTION.

The body examined the day after death, with the assistance of Surgeon Stokes of French-street, presented the following appearances, viz:—

Externally the emaciation was extreme, the trunk and extremities presenting little more than skin and bone; for not only the fat, but even the muscles had disappeared, and the only preternatural prominences which presented themselves, were of the left side of the sternum and over the right hypochondrium. An incision through the integuments of the abdomen discovered the omentum and mesentery abundantly supplied with fat, and the abdominal viscera, in general, appeared on first view in a sound condition; but on examining the liver more closely, it was found increased much beyond the natural size, and completely resembled the "*Foie endurcie et engorge par le matiers muqueuse*" of Portal, such as I had often witnessed. Its colour was not remarkably changed, except at its superior surface, which was occupied by a circumscribed tumour of a conic form, that arose out of the substance of the liver, and pressed up the diaphragm, and formed a corresponding protrusion into the right side of the cavity of the thorax. The still lighter colour externally of this tumour of the liver, and striated vascularity of its surface, led me *prima facie* to suppose it contained an abscess; but on cutting into it, was found of the same dense substance with the rest of the liver. The

stomach when opened contained much coffee-coloured matter, such as had been spit and vomited up by the deceased several days before.

Thorax.—On cutting through the skin, for there was no fat, and scarcely any muscle externally, the cartilages of the ribs were found much more firmly ossified than usually happens at the age of 52 or 53 years. There was, however, very little adhesion between the parietes of the thorax, and the contained viscera which first presented themselves wore a perfectly natural appearance, but were, like those of the abdomen, much more plentifully supplied with fat than could have been reasonably expected with such extreme emaciation externally. On examining even more closely, the heart, lungs, &c., appeared most singularly free from marks of disease, until a strong adhesion between the right lobe of the lungs and that part of the diaphragm protruded by the tumour of the liver was discovered; and that part of the lung connected with this adhesion was wasted to the extent of a about an inch and a half in circumference. This waste extended itself upwards in a funnel shape, in the direction of the œsophagus, but not continued by any communication that could be discovered either to that passage outwards, or to the bronchiæ. In this diseased portion of the lungs, there was a small quantity of glairy *sputum*, and this was the only preternatural effusion found in any part of the chest.

No symptoms indicative of any affection of the brain having ever occurred during the life-time of the deceased,—the head was not examined.

For cases with symptoms resembling those of hydrocephalus, I must now refer to a paper on *Apoplexia Cephalica*, in the second volume of the Transactions of the King and Queen's College of Physicians; and for those of Paralysis without effusion or disorganization, to my Report from the Fever-Hospital and House of Recovery, Cork-street, for the year 1823, pp. 83, 84, 85, 86, 87, and 88, as an exemplification of Angina Pectoris, without disorganization. I have only room

for the following case, taken from several others like it, which will be given to illustrate certain aggravated forms which epidemics assumed in connexion with morbid changes in the animal fluids, of late years.

CASE IV.

ANGINA PECTORIS.

Mr. H————, 55 years of age, who, previous to the attack for which he consulted me on the 4th of November, 1824, was a remarkably robust man, complained of constant oppression of breathing, and a painful sense of stricture over the whole chest, extending down the left arm, and particularly in that part of it where the deltoid muscle is inserted. He stated, however, that the sense of oppression and weakness which he felt by day, was not at all equal to that which generally seized him at midnight to such a degree as threatened immediate dissolution. This seizure was accompanied, as stated to me, with violent palpitation of his heart, so loud that it could be heard by himself and those about him. On examination, I found his hands cold and skin generally clammy; his countenance darkly flushed; his lips and tongue of a deep claret colour; the action of his heart was extremely irregular, and could be felt and heard over a great portion of the left side; there was also a corresponding pulsation at the epigastrium, symptoms which he said had increased considerably in urgency on being bled a few days before. The blood was stated to have been very sizy, being deeply and densely buffed soon after being drawn; pulse, intermitting at every second beat, was very full, not exceeding forty in the minute when I first examined it, but afterwards varied during my visit considerably in frequency. The abdomen felt rather fuller and tenser than natural, and there was considerable soreness felt on pressure over the right hypochondrium; the lower extremities leucophlegmatic, and in some

degree anasarcus, especially at the ancles and over the shin-bones; belly costive; alvine discharges scanty and melænous, accompanied with a great discharge of flatus, from which he experienced some relief; urine scanty and limpid. I prescribed blisters to the region of the heart and the right hypochondrium in succession, a cordial expectorant mixture, with compound spirit of ammonia, Hoffman's anodyne liquor and water, to be taken occasionally to relieve the sense of weakness and spasm; pills, composed of the mercurial pill and asafœtida, one to be taken every four hours, assisted by turpentine and aloetic enemata, till the bowels be freed.

By these means considerable amendment was effected in the course of the succeeding night; and in about ten days afterwards, mercury, joined with digitalis, having been in the mean time pushed so far as moderately to affect the mouth, recovery was so far advanced that the patient, of his own accord, resumed his usual active employments. Health, however, could not be said to be yet completely established, for the cheeks were occupied with a dark circumscribed flush and the pulse irregular; but feeling himself free from oppression or uneasiness, he declined submitting to the regulations which I proposed for him, particularly with respect to removal into country air and suitable diet. He, consequently, ceased to be under my guidance; but I learned that in some weeks afterwards he had an attack of erysipelas in the face, accompanied with considerable fever, which appeared to be critical, in as much as afterwards his general appearance, as well as his vital and natural functions, were much more completely restored than they had previously been, and he has since enjoyed good health.

I must reserve my commentary on this case until I present others of the same kind, as I have promised; and because, as already intimated, the Dissertation itself has exceeded the limits which I intended for it. I hasten to conclude this Appendix, with a reference only to the cases I mentioned at page 83, by which I propose to show that Croup, Hypertrophy, and

Adhesions of both thoracic and abdominal viscera, as well as several of the eruptive diseases, depend on that condition of the blood indicated by sizy surfaces. The cases and dissections intended for such demonstrations, must be postponed for the General Appendix, which I mean to attach to the account of Epidemic Fevers of Dublin; and as they will connect the symptoms and the alterations found in the blood during diseases, with the appearances of altered structure, (arrested, as it were, *in transitu*,) and found after death,* I trust they will be deemed satisfactory on a question which appears to me deeply interesting in respect to the pathology of diseases.

* *Post mortem* examinations, illustrative (as suggested above) of the Pathology of Purpura and Dysentery, may be seen in the first part of my "Pathological Observations," from p. 25 to 28 inclusive, and also from p. 101 to 108 inclusive.