Inaugural dissertation on the connexion between some diseases of the liver and of the intestines : submitted to the Medical Faculty of the University of Edinburgh, in conformity with the rules of graduation by authority of the Very Rev. Principal Lee, and with the sanction of the Senatus Academicus / by F. Beaugeard.

Professor Bennett
With the author's best regards
13

INAUGURAL DISSERTATION

ON THE

CONNEXION BETWEEN SOME DISEASES

OF THE

LIVER AND OF THE INTESTINES:

SUBMITTED TO

THE MEDICAL FACULTY

OF

The University of Edinburgh,

IN CONFORMITY WITH THE RULES OF GRADUATION
BY AUTHORITY OF THE

VERY REV. PRINCIPAL LEE,

AND WITH THE SANCTION OF

THE SENATUS ACADEMICUS.

BY

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ON

THE CONNEXION BETWEEN SOME DISEASES

OF

THE LIVER AND OF THE INTESTINES.

SECTION I.

1. The existence of a connexion of some sort or other between many diseases of the Liver and of the Intestines, has been acknowledged for a very long time. The authors who have written on the affections of those organs are numerous; and, of course, it was to be expected that a difference of opinion should exist among them with regard to such a connexion. But this difference only relates to its kind, or merely perhaps to its frequency of occurrence; its existence they all more or less clearly recognised; and at this conclusion they had arrived from their own experience of those diseases, and from the observation of facts which are too plain and forcible to be denied. As far as I am aware, however, none went deep into the matter, and endeavoured to bring out fully its nature; and especially amongst the earliest authors it was mentioned merely incidentally, and was far from being clearly apprehended. More modern writers have given some explanations, but only of the connexion between few diseases, and principally between dysentery and abscess of the liver, when such occurs: and it is not perhaps too bold to say that, among these few, several have not offered any thing like a satisfactory solution to the as-yet somewhat obscure problem; whilst others, though certainly accounting for some features in those maladies, have still done so in all but a generalizing way. Indeed, their theories may safely be reckoned as explaining very well some particular cases, whilst they cannot be applied to the greater number. We shall not at present refer to those from whom we venture to differ: as we proceed, we shall come to mention

their opinions, and to express our views of the truth or fallacy of such, as far as we have been able to peruse their works and understand their meaning.

2. Such a connexion shows itself in most, if not in all, of the affections of those organs, provided they are of sufficient duration; although it is far from being an invariable rule that it should become apparent in every individual, or even that disease in the one viscus should necessarily involve the other into some morbid process. But those exceptions—even if they were more numerous than they actually are—cannot invalidate the rule which, we think, must remain a general one. And if the opinions of so many eminent authorities, and the conclusions drawn from innumerable facts by practical observers, were not sufficient to uphold such a view; we would only point to the anatomical and physiological relations existing between both sets of organs, to prove that the one can and must react upon the other under certain circumstances. According to those who maintain the theory that disease originates most frequently in the liver, the striking feature among those anatomical relations is the circulation through that gland; and to it has been granted by them a far greater influence in the production of morbid action than we are ready to concede. We cannot here enter minutely into the description of that circulation; we shall only call the attention to the too well known fact, that any obstacle to the flow of blood through the liver is followed by more or less congestion of the mucous membrane of the bowels. This congestion has been reckoned by many the primary step to dysentery and several other intestinal diseases; some few have even gone farther, and thought that no other cause than such obstacle in the liver would be efficient to produce them. Now, although we do not deny that such congestion may lead to some alterations of function or structure, having their origin in the mucous membrane of the intestines; yet we do not see how it can be the cause of dysentery, or any other of the affections of the bowels terminating in disorganization of that membrane. We shall have occasion to refer again to this point afterwards. We, on the other hand, think that by far the most important is the physiological fact, that the secretion of the liver is derived from the blood returning from the intestines, which circulates besides so abundantly through the former viscus. For, in this circumstance, as we shall see, as soon as such blood becomes morbidly altered,

we have all the elements necessary for the production of disease in the liver, gradually, though often slowly, accumulating; until the constant, yet not always severe, recurrence of the causes at last brings about their necessary effects.

3. It may be deemed perhaps superfluous to state that, in speaking of the diseases of those viscera, we shall of course take into account the least alterations, not only of structure, but also of function. This, however, becomes necessary, as it would seem that some will recognise disease only in those cases where structure becomes implicated, and will scarcely reckon an alteration of function a deviation from health; or, what nearly comes to the same thing, doubt that any morbid process ever took place in an organ, because they have not palpable proof after death of a change in its texture. But I certainly think, and very likely few will hold a contrary opinion, that when the products of an organ are altered from those that are generated in health, it may be safely inferred that disease must be lurking somewhere. In the case of secreting glands especially, such alteration of products can only depend on two causes: either the gland itself is structurally changed, and therefore cannot perform its functions satisfactorily, though—and this must be particularly borne in mind—such change of its texture may not be apparent to our eyes; or else the fluid, from which its secretions are derived, is of a morbid nature. In this latter case, the former condition may not be present at all, so that the gland itself can scarcely with propriety be called *diseased;* yet, as it is the means of eliminating unhealthy products, and as we have no term to express its condition when performing such a function, we cannot either reckon it healthy, and must rank it among diseased organs. We shall have occasion to revert to this in speaking of the morbid appearances presented by the liver after death; and we shall at present only remark further, that functional derangement being by far of more frequent occurrence than organic lesion,—being also present, as a general rule, whenever there is structural change; whilst this state does not of necessity, and indeed seldom does, follow that,—acquires, from those circumstances, an importance which (we may be allowed to say) has been too often overlooked.

4. The consideration of symptoms in their different relations to the time of appearance of what is sometimes con-

sidered the primary disease, as well as to their individual periods of manifestation, is also a rather uncertain subject. Their existence is not always readily detected ; and to this difficulty, which not unfrequently reaches a very high degree, we attribute, without hesitation, the vagueness with which some authors have brought forward their assertions, and from thence the doubts that must often hang over these. Unfortunately, the obscure nature of some symptoms is a thing that cannot be helped, as, even in the present state of medical science, much remains to be done before settled rules can be laid down with regard to their detection at the proper time, especially in diseases of the liver. So much is this the case, that it is asserted—and with too much truth—by men of the greatest experience, that in many affections of this last-named organ, and even in the most fatal, symptoms are sometimes so totally wanting, as to give no clue to the detection of the mischief going on. If such be the case, we may *à fortiori* expect that complaints of a less serious description should, not unfrequently, pass unnoticed ; and from the consideration of such facts, we need not wonder at the somewhat hasty statements made by some, not any more than at the wrong inferences drawn by others. When it is remembered that the rules of precedence in the supervention of disease in those organs, must be founded on such uncertain data as the time of appearance of the symptoms ; and this again depend so much on the powers of observation of the writer, combined with the amount of his experience : we shall then easily account for the discrepancies so often met with between even the highest authorities, and plead such difficulties as an excuse for the conclusions of whatever nature they thought themselves entitled to draw.

5. But whatever connexions may have been found between diseases of the liver and those of the intestines, and whatever influence disease in the one may have in the production of such in the other, there is a *third kind*, which seems to have been hinted only by few, and completely overlooked by the majority. We must not forget that both sets of organs have certain functions to perform in the economy, and are therefore dependent on the general system. What these functions are—besides those of digestion, which have been pretty well made out—we shall endeavour to establish as fully as we can ; and show, how in the performance of these, they may take on a diseased process ; as well as in what relation

they then more particularly stand with regard to the general economy. This dependence, which we consider principally exemplified in the consentaneity of affections in both organs—though it does not thence necessarily follow that existence of disease in the one must be accompanied by the same occurrence in the other—will form the *first* class of our connexions: namely, that in which disease, when present in both, is considered by us as the effect of one cause, acting upon them at once, but not originating in any of them; and, therefore, in this class, there is no other connexion between them—at least in the early stages, and certainly during the establishment of the morbid process—than their co-existence, due to the similarity of function of the organs. The *next* class is that in which disease of the intestines is the cause of that which makes its subsequent appearance in the liver. We shall treat of this class next, because such connexion seems to have been better settled by modern researches, and the weight of authority, in establishing such sequence, greater. Here we have the immediate dependence of one set of organs on the other; and this is also exemplified in the *third* class, which will comprise those diseases which, originating in the liver, subsequently influence the intestines.

SECTION II.

6. Among the affections which may be included under our first class, we shall consider *dysentery, as connected with disease in the liver.* This view—making both dependent on one cause influencing the general economy, and not the consequence of one another—seems to have been, as far as I am aware, merely hinted by Copland; who even then makes the one consecutive to the other, inasmuch as he thinks that the liver becomes the seat of an unhealthy action, when the dysenteric affection is suddenly stopped—a theory which certainly does not agree with what we know of the co-existence of disease in both organs at one and the same time. We can perceive, however, that he refers both to a primary general cause, namely, the existence of morbid matters in the circulation.[1] Most authors, on the contrary, make the one

[1] "This form of the complication (liver disease) is evidently caused by the sudden cessation of the dysenteric affection; which, being very

disease the direct agent in the production of the other,[1] or else again nothing but the symptom of the existence of the latter;[2] whilst Abercrombie says that their co-existence "*is not sufficient to establish any connexion betwixt the two diseases, and it seems to be now ascertained that the connexion is incidental.*"[3] The most prevalent opinion also is that, that series of symptoms which has been called dysentery—whether it be co-existent or not with alteration, functional or organic, of the biliary organ—is to be referred primarily to quite a local affection, which may and does in the end react on the system. In our view of the subject we reverse the proposition, and we think that some primary involvement of the system is the cause of the local disease; which is therefore the mere manifestation of a more general lesion, which may or may not, according to its intensity, or individual predispositions, affect also the liver.

7. As the alvine discharges in dysentery seem to have been made by most writers, up to this time, the most characteristic features of the disease, it is not immaterial that we should, before proceeding any further, inquire into what these discharges are. Cullen[4] and, following in his steps, a host of others, who saw the complaint only in Europe, set them down as being *scanty, mucous, and bloody*, and thereby easily distinguishable from those obtaining in diarrhœa. But the experience of the Indian practitioners, who have had far

intimately dependent upon the excretion of morbid matters from the circulation and the economy in general, cannot be abruptly suppressed, without inducing continued or remittent fever, or inflammation, congestion, or enlargement of excreting organs."—Copland's Medical Dictionary, vol. i. p. 705.

[1] R. W. Bampfield, on Tropical Dysentery, p. 53. Annesley, Diseases of India, vol. i. p. 658; vol. ii. pp. 199, 200. Sketches of the most prevalent Diseases of India, pp. 270, 449. W. Geddes, in Calcutta Med. and Physiol. Transactions, vol. vi. p. 290. Dr Brown, in Cyclopædia of Pract. Medicine, vol. i. p. 658.

[2] James Johnson, Influence of Tropical Climates on the European Constitution, p. 351. On Derangements of the Liver, p. 39. Annesley, Diseases of India, vol. ii. p. 199. W. Geddes, Clinical Illustrations of the Diseases of India, p. 311.

[3] Path. and Pract. Researches on Diseases of the Stomach, &c. (3d edit.) p. 243.

[4] First Lines, vol. i. chap. on *Dysentery* (edit. contd. by Dr Gregory, 1829).

more numerous opportunities of studying dysentery in all its forms, has contradicted this absolute statement; and, indeed, we read in their works, that the dysenteric evacuations, at all events at the beginning of the attack, are most often similar in all appearance to those produced by diarrhœa. Even in those forms which have been called *sthenic* or *sporadic*, the stools, though they frequently assume such appearances as those described by Cullen, often do so only after having gone through a preliminary stage in which diarrhœic evacuations are present. In the other forms of the disease, named *asthenic* or *hepatic*, the discharges are always more or less abundant, and not always distinguishable from those of diarrhœa; and when they most approach the mucous and bloody characters, they are then fœtid, slimy, or sanious.[1] Impressed by those facts, Abercrombie came to the conclusion, "*that this highly dangerous disease may be going on with every variety in the appearance of the evacuations.*"[2] But this apparently endless variety may, I think, be divided into two great classes, namely, 1*st*, that in which the stools are *scanty, mucous, and bloody;* and, 2*d*, the other in which, whatever characters besides they may possess, they are *more abundant, and do not present pure mucus and blood.* These two sorts of evacuations may correspond with the two primary divisions of dysentery of Copland, the *sthenic* and the *asthenic.* The first, we would take to be indicative of a state of the mucous membrane partaking more of the sthenic, simple inflammatory nature,[3] whether they be present

[1] Sir John Pringle, Observ. on the Diseases of the Army p. 227. Sir G. Ballingall, Observ. on Fever, Dysentery, &c. pp. 46, 50. Twining, Diseases of Bengal, pp. 4, 5. Copland's Medical Dictionary, vol. i. pp. 695, *et seq.* Abercrombie, Path. and Pract. Researches on Diseases of the Stomach, &c. pp. 240, 241. Parkes, Remarks on the Dysentery and Hepatitis of India, p. 50. J. Brown, in Cycl. of Pract. Medicine, vol. i. p. 654.

[2] Op. cit. p. 240.

[3] NOTE.—The *scanty, mucous, and bloody stools* seem to us to have been made wrongly a pathognomonic sign of dysentery; for we doubt very much whether they are at all characteristic of dysentery, a disease undoubtedly so peculiar in its nature—not to say so *specific;* or merely of a more or less exalted inflammatory state of some portion of the mucous membrane of the intestines. It is a question with us whether those cases which terminate favourably within a short space of time, which are preceded by well marked rigors, in which the pulse is more decidedly affected, and those peculiar stools present; and which

from the beginning of the disease, or only subsequent to the appearance of more diarrhœic discharges;—the second may point out, according to their various characters, either an incipient inflammation, in which condition secretions, we know, are increased; or else the development of a specific kind of that morbid process, in which (to borrow the words of Abercrombie), "*the difference between diarrhœa and dysentery consists in the nature of the disease, and can be learnt only from a diligent attention to the concomitant symptoms, not*—we would add *always—from the character of the evacuations.*"[1] But what we want particularly to draw the attention to in this place, is the fact which obtains in by far the generality of cases, that dysentery begins with evacuations which are fæculent and copious, and presents nearly throughout its most severe forms—the adynamic—the same discharges, only more morbid in quality, if not in quantity: indicating thereby an increased secreting or excreting action of the mucous membrane, and, as we shall see afterwards, very probably of the biliary organ also. Let us now proceed with our subject.

8. After going over the writings of some of the best authorities on those diseases, I have come to the conclusion that dysentery is produced by atmospheric agencies of a peculiar nature, which, we cannot refrain from venturing to think, constitute a malaria, distinct from that producing different kinds of fever, but certainly allied to it in nature. The experience of those eminent men—especially when they

have been ranked by Copland amongst his milder forms of sthenic dysentery, or called by others—perhaps not wrongly—sporadic, are genuine cases of dysentery. Supposing we had a simple acute inflammation of the lower portion of the mucous membrane of the intestines, would any other symptoms be present? I think not. We would obtain, as in all other tissues, extravasation of blood—as in all secreting bodies, inspissation with diminution of the secreted fluid: thus producing the *scanty, mucous, and bloody stools*, together with a sense of heat or pain, and more or less affection of the circulation. Now, why could we not have in these parts, as well as in others, a simple inflammation produced by simple causes, say cold for instance, assisted by sundry circumstances or individual peculiarities? And as this, we imagine, is not at all unlikely, we doubt whether all those instances of disease which have presented the above symptoms, and been given to us as cases of dysentery, can justly be called so.

[1] Op. cit. p. 242.

have studied the disease where it is most rife, namely, in warm climates—has led them to such a conclusion; and although they have also admitted a variety of other causes as capable of giving rise to it, yet they have, in general, agreed that those peculiar atmospheric agencies are the most powerful in their action; and that, the more intense they are, the more severe forms does the complaint assume.[1] As for us, we think that those other causes, of which they all speak more or less, tend only to aid the influence of that particular poison, which we shall denominate, for want of a better term, *dysenteric malaria*.[2] A few may assist its development, cause the breaking out of the malady, or favour its progress; but we do not consider their action to extend any farther.

9. Some have thought that dysentery is a simple inflammation, produced by local irritating causes. That there is an inflammation of the mucous membrane of the bowels owing to such, irritating poisoning affords us an instance; but then we obtain what may readily be anticipated: a less degree of morbid action, the further we recede from the spot to which the cause was first applied,—in this case, the stomach. And it should no doubt be the same, were dysentery produced by irritation of any kind, acting locally; as, for instance, acridity of bile. For to those who reckon this as its cause,[3] we would ask whether the bile, by its mixture with intestinal mucus—which mixture becomes greater, of course, the more it recedes from the liver—is not likely to lose its irritating characters, and therefore to exert less influence on the large than on the small intestines? And we certainly have no proof that the mucous membrane of the large intestines is so much more sensible than that of the small; quite the contrary, for we know that it is constantly subjected to the stimulus of what may truly be called a foreign body—the fæces—and seems, under ordinary circumstances, scarcely to

[1] Annesley, Dis. of India, vol. ii. pp. 236 *et seq.* 245. Twining, Diseases of Bengal, p. 19. Copland, op. cit. vol. i. pp. 695, 698. J. Brown, op. cit., vol. i. p. 658. Lebert, Physiologie Pathologique, vol. i. pp. 218, 219, 520. Cruveilhier, Anatomie Pathologique, liv. 40, p. 4.

[2] This expression, as far as I can remember, is to be met with in Lebert's Physiologie Pathologique.

[3] Annesley, Dis. of India, vol. ii. p. 200.

resent it. Irritation by retained fæces, or by particles of food or bile having acquired such properties from decomposition,[1] we scarcely think capable of producing dysentery. We cannot see how irritation by fæces or by irritating—*not corrosive*—particles of whatever nature, can cause such disorganization, obtained sometimes in such an astonishingly short period, as is found in cases of dysentery. Such irritation, we know, if it acts at all in an injurious way on the mucous membrane of the intestines, stimulates it to a more abundant secretion of mucus, which, combined with an increased peristaltic action—also a result of the same irritation—effects the expulsion of the offending matters. Further, with regard to constipation, what we see of that complaint nearly every day, cannot enforce upon our minds the possibility of its producing dysentery; and extraordinary cases are on record,[2] which have simulated or given rise to nearly every sort of abdominal affection—diarrhœa among others—but I scarcely think ever to dysentery. As to the putrid corpuscles of Broussais, acting by contact after having been swallowed with the saliva, they need not arrest our attention.[3] Electricity has also been hinted by Annesley, as being, very probably, efficient in the production of dysentery.[4] Without denying that such may possibly be the case, we shall only say, that this agent has been held the cause of all sorts of phenomena, the original source of which was thought, but not clearly perceived, to be in the atmosphere.

10. Heat and cold singly applied, or in alternation, as well as diet and regimen of a peculiar kind, have been also reckoned as causes of dysentery.[5] But those that have principally given them as such, did not deny the possibility of malaria existing at the same time.[6] Indeed, the variations of temperature, often accompanied with sudden alternations of wet and dry weather[7]—the changes of locality from a

[1] Annesley, Dis. of India, vol. ii. pp. 239, 240, 246. Copland, op. cit., vol. i. p. 715. Broussais, Histoire des Phlegmasies, p. 204.

[2] Abercrombie, op. cit., p. 135. Andral, Clinique Médicale, vol. i. pp. 51, 52.

[3] Broussais, Histoire des Phlegmasies, p. 198.

[4] Annesley, Dis. of India, vol. ii. p. 250.

[5] Annesley, Dis. of India, vol. ii. p. 235. Twining, op. cit., p. 19. Cruveilhier, Anatomie Pathologique, liv. 40, p. 4.

[6] Dr Brown, op. cit., vol. i. p. 658.

[7] Annesley, Dis. of India, vol. ii. pp. 236, *et seq.* 244.

healthy to a notably unhealthy one[1]—or the intensity of the heat, dysentery having seldom been deemed the result of intense cold—all which circumstances have been observed to precede or accompany general outbursts of the disease; would tend to enforce the more than possibility of generation and existence of a peculiar poison, which had perhaps already exerted its influence on the constitution, before the immediate exciting cause was applied. What have been called *sporadic* cases, that is to say, cases thought to have been produced merely by the causes mentioned above, do not prove any thing against our position, as their existence is more than questionable. Let us select, for instance, exposure to cold. We are far from denying that, in some conditions of the body and under certain circumstances, this may give rise to inflammation of the mucous membrane of the bowels. But the great question to us is: does it and can it generate dysentery? We know that the changes which take place in this disease are peculiar, and always fall upon a particular organ. Is it then likely that the general agent "cold," whatever may be its mode of action, will cause mostly inflammation of the large intestines, and leave the small ones uninjured? We scarcely think this possible, especially when, as at periods and in localities where dysentery prevails greatly, we see those same peculiar changes wrought with scarcely, if any, variation in their characters. Experience further would point out that the action of cold, when it takes place upon the intestines, is far more apt to give rise to diarrhœa, or even to Enteritis. It is true that many a dysentery can be traced only to exposure to cold; but, besides that tracing any complaint to its primitive cause is an exceedingly difficult task, we have here an analogy in a numerous class of diseases—the exanthematous fevers—not every case of which can be traced to exposure to their peculiar causes. Yet, nobody is prepared to assert that cold alone can generate, for example, small-pox. In the same way as in these diseases, we reckon cold, heat, &c. in dysentery as exciting causes, but no more. These two, combined with moisture, especially the latter, have no doubt a great share in determining the breaking out of the complaint. We shall not enter into details to show how they generally predispose the constitution to the reception of any morbid influence; as

[1] Annesley, op. cit., vol. ii. pp. 236, 245,—Cruveilhier, op. cit. liv. 40, p. 4.

to the particular laws which they develope under particular circumstances, as in the production of this or other diseases of the bowels and the liver, we shall refer to them at length, and endeavour to establish the existence of such laws at a further stage of our subject.

11. Another proof that dysentery is not a local disease, is that the same alterations have been met with in serous or bilious diarrhœa as are found in it ;[1] and that the symptoms which constitute that malady are by no means attendant upon a constant particular state of the intestines.[2] If, in the first case, dysentery were a local disease, surely we would see its existence when the conditions supposed to produce it are present ? And, on the other hand, it may be thought astonishing that it should manifest itself even when the local affection, which constitutes its essence, is absent ; this would be having an effect without a cause, and how far such a thing can exist, we have not yet found the means of determining. It cannot be brought against us that, to constitute dysentery a disease affecting the system primarily, we should have an amount of constitutional disturbance far greater than is generally witnessed in this complaint. Such an argument, if it prove anything at all, will indeed tend to prove our position. For in all idiopathic inflammations involving a certain amount of tissue—mucous membrane not excepted[3]—we have the affection of the circulation and the constitutional disturbance which supervene upon the local lesion, well marked ; whilst in dysentery, those who have had most experience of the disease agree in saying that it is by no means so. Out of numerous authorities, we shall quote the words of Sir G. Ballingall, who says :—"*The dysentery of India often makes considerable progress, and has very seriously, perhaps irreparably, injured the intestinal canal, before any urgent symptoms of pyrexia become either distressing to the patient, or conspicuous to his medical attendant.*"[4] If this disease be then a simple local affection, following common general laws,

[1] Andral, Précis d'Anatomie Pathologique, vol. ii. p. 206.

[2] Andral, Clinique Médicale, vol. i. p. 428.

[3] As in cases of acute bronchitis, gastritis, cystitis, &c., see also Copland for idiopathic infl. of the muc. membr. of some parts of the intestines, vol. i. p. 278 ; vol. ii. p. 569 and even 575.

[4] Observations on Fever, Dysentery, &c., pp. 40, 46. Annesley, op. cit., vol. ii. p. 151. Twining, op. cit., pp. 4, 6. Parkes' Remarks on the Dysentery, &c. p. 55.

how is it that the system does not resent it to a greater degree? and may we not think with Parkes, that on such grounds "*it is hardly conceivable that inflammatory action, in cases of dysentery, so severe as to produce almost universal ulceration of from one to four feet of the large intestines, could exist without co-ordinate constitutional disturbance.*"[1] Many have been the surmises to account for such an apparent anomaly which has struck more than one observer, even those that uphold the local origin of the malady; and we think that the solution of the problem is to be looked for in the peculiar nature of the inflammation, rendered so itself by the peculiarity of its cause.

12. To uphold besides the opinion that dysentery is produced by a certain malarious poison, we have the authority of the most eminent men,[2] and a body of facts which is too strong not to carry with it all but entire conviction. The instances in which the presence of such malaria existed for a certainty—as far as man can be certain of anything which is not immediately perceived by his senses—we need not quote at length; they are to be met with especially in those writers who have studied the disease in warm climates. But in few affections is its existence so apparent, its action so unmistakable, and its consequences so palpable, as in the form of dysentery which assumes a malignant character, and has been called by Copland "*nervo-adynamic* and *putrid.*"[3] It cannot be said that the cause of that particular form of the complaint—whatever it is, but supposing here that it is not dysenteric malaria—acts on debilitated or otherwise unhealthly constitutions, and therefore that the peculiar consequences which follow are due to this. Facts cannot be brought forward to uphold such a supposition. In the instances recorded by tropical and other writers, the health of those that were attacked by that form was, to all appearance, as robust as that of others who, in different situations, suffered only

[1] Op. cit. p. 3.

[2] Sir John Pringle, Obs. on the Diseases of the Army, pp. 251 *et seq.* Annesley, op. cit., vol. ii. pp. 237, 245. Copland, op. cit., vol. i. pp. 695, 698. Dr Brown, op. cit. vol. i. p. 658. Baly, in the London Medical Gazette for 1847 (on the Pathology and Treatment of Dysentery), pp. 529 *et seq.*, 533. Christison, in Monthly Journal of Medical Science, for December 1853. Lebert, Physiologie Pathologique, vol. i. pp. 218, 219, 520.

[3] Op. cit., vol. i. Art. *Dysentery.*

from milder or less fatal descriptions of the disorder[1]; whilst here the disease developed itself in localities known to be unhealthy, the cause of such unhealthiness being clearly owing to all those circumstances under which malaria becomes generated. It is in the *nervo-adynamic* and *putrid* forms that we see the greatest analogy between dysentery and some kinds of fever; and though we do not intend to follow such analogy step by step, yet we must observe that the disease is principally met with in places where fevers are known to be rife; it exists with them, it supervenes upon them, it even supersedes them.[2] How is such coincidence of existence to be explained? No one, I think, is prepared to deny that fevers are due to atmospheric impurities; whether these be of a general nature, affecting each individual as his constitution is predisposed to this or that type; or of a more particular one, that is to say, each type of fever being due to its own peculiar miasma. And if such be the case, are we not entitled to think that the cause of dysentery must be allied to that of fevers? Besides, pursuing the analogy, we see that in fevers the mucous membrane of the intestines becomes often more or less involved. In this, indeed, there is a difference from what we meet with in dysentery; but we think that difference to be more in kind than in nature. The pathological principle that most of the diseases depending upon miasmatous emanations have a peculiar influence on that mucous membrane,[3] has been proved to be true by what we witness nearly every day around us. How this action on that particular part of the human frame comes to be established, we shall further on endeavour to make out by what we see takes place in the case of dysentery. We shall not enter here into the question, whether the bowel affection is a cause or a consequence of the constitutional disturbance by which it may be accompanied; in the case of fever, the latter has been sufficiently established by the British School against a vast body of continental physicians[4]; and we think that

[1] Annesley, op. cit., vol. ii. p. 236.

[2] Sir John Pringle, op. cit., pp. 253, 255. Annesley, op. cit., vol. ii. pp. 248, 290, 342. Cheyne, in Dublin Hospital Reports, vol. iii. pp. 16 *et seq.* Watson, Principles and Practice of Physic, vol. ii. pp. 498, 499.

[3] Cruveilhier, op. cit., liv. 11, p. 2.

[4] Abercrombie, op. cit., pp. 289 *et seq.* Andral, Clinique Médicale, vol. i.

the same arguments, combined perhaps with a few more which we shall attempt to adduce, can establish the same proposition with regard to dysentery. What we only want is to direct the attention to the fact that there is to be found in both kinds of complaint an affection of the intestines; and that, as in every other disease, so in dysentery, through all its forms, severe or mild, the same features can be traced —the difference between the phenomena which they severally give rise to, being one of degree, and not of kind.

13. Our ignorance of the nature and mode of action of the dysenteric malaria, is no proof against its existence, as some would unreasonably object.[1] Nobody knows anything about the nature, or mode of action either, of any atmospheric poison, "malaria," if the term must be used;[2] even though the laws, under which a particular one becomes developed or checked, may have been partially ascertained, as in the case of that of ague. So many diseases are too probably due to such an atmospheric influence, that it is impossible to deny the existence of such agents; and yet, not any more in any of those in which they are readily admitted as the main, nay, the sole cause of the malady, than in dysentery, does any one know what they are or in what way they act. Such an objection, therefore, is too specious to be of any weight; for, supposing we were all blind, it is not because we could not see the sun that we would deny its existence, at the same time that we were feeling its influence. No one has yet said anything about the dysenteric malaria; and certainly it is not I that will be able to solve the question. But on considering the phenomena to which it gives rise, and comparing them to those which are developed in typhoid fever, we may venture to assert that it is akin to that which generates the latter complaint. The next question is, whether it is different in kind or merely in degree? This no doubt is rather difficult to answer; but I think that it may be said to differ from that of fever, in that it is either more irritating in nature and therefore gives rise to a greater inflammatory tendency; —or that its not acting so powerfully on the nervous system,

[1] "What avails our saying that epidemic causes produce dysentery, when we neither know what these are, nor the way in which they act? We have not yet defined the origin of these causes, as we have those of ague, and the nature of the epidemic dysenteric agent has not even received a name." Parkes, op. cit., p. 132.

[2] Lebert, op. cit., vol. i. p. 520.

leaves sufficient strength in the constitution to endeavour to shake it off completely. Whichever of these two hypotheses be the true one—and we certainly feel more inclined to adopt the latter—the important fact is that such poison tends to be eliminated by the mucous membrane of the intestines,[1] and that the inflammation which supervenes in the bowels and in the liver (when the latter does take place) is established during these efforts. This we shall next proceed to show; and we further think that the establishment of such action in both these sets of organs, tends not a little to prove the existence and mode of action of the dysenteric malaria.

14. The excreting functions of both the liver and the intestines are perhaps not acknowledged to be so great or so important as we think they are. Their action is thought to terminate with the process of digestion, after which the general impression seems to be, that they remain perfectly at rest. That this view is too limited, is our firm opinion; and their relation to the whole economy, as depurating organs, is very likely far closer and of much greater range than is commonly received. It may not be amiss then to say a few words in attempt to show how great these excreting functions are, in morbid states of the system especially. Of course it cannot be expected that we shall run over the whole series of diseases in which they may act as excreting organs, and bring forward proofs of such being their then function; this would occupy us too long, and be besides altogether foreign to our subject.

15. Physiology teaches us very little of their excreting powers by direct proofs. They are so located in the human body, that their secretions or excretions, or both, cannot be obtained sufficiently free from admixture with foreign matters to be subjected to any analysis. From this, however, we might perhaps except the bile. Even their quantity, not merely their quality, cannot be justly estimated; and here we make no exception for the bile. We know, however, that

With regard to the intestines, their mucous membrane, in its normal state, constantly secretes a fluid in great abundance. No precise data are known as to the quantity of that secretion, which has been thought by Haller to reach the

[1] Pringle, op. cit., p. 255.

amount of 8 pounds in twenty-four hours ;[1] but by others such a high number has, reasonably, been deemed impossible.[2] That this fluid, nevertheless, must be thrown out pretty abundantly, we cannot doubt for a moment, when we reflect on the length and vascularity of the canal which yields it. Besides this secretion or excretion, there seems also to take place an exhalation which has been compared to that of the serous membranes. And further, the innumerable glands with which that enormous surface is studded, have also their peculiar products, which, in the aggregate, must rise to a certain amount.[3] It is scarcely credible that these phenomena should take place only when the canal is most active, that is to say, when it discharges its peculiar duties of chymification and absorption ; and that, during the intervals, such a large and vascular organ should be altogether of no use to the system. Far from this being the case, it is performing active and important functions, even when in a comparatively quiescent state, as has been irrefutably proved by a strong argument of Liebig, who shows that as "*in starving persons, in many sick persons who for weeks take no solid food, the evacuation of the fæces is not suppressed ; it can hardly be doubted that in certain parts of the intestinal canal, a secretion of matters which the organism, in its actual state, cannot change, goes on.*"[4] The presence of such matters is no doubt due to secondary digestion, which is nothing else than excretion under another name ; and points out to the intestines as having a share, and no insignificant one, in eliminating effete and injurious matters, as well as other organs. But these direct proofs, however highly conclusive, are still not so forcible as those that are furnished to us by some pathological states of these same organs ; and by the phenomena developed, under certain abnormal conditions of the body, through their agency. It is known that in many cases, when the excretions of the skin have been checked, those of the bowels become increased. Such a fact establishes between those organs an antagonism which had been long noticed, although never so

[1] Abercrombie, op. cit., p. 7.

[2] Bichat, Anatomie Générale appliquée à la Physiologie et à la Médecine, vol. iv. pp. 465, 466.

[3] Abercrombie, op. cit., p. 8.

[4] Animal Chemistry (3d edit.) p. 152.—Bichat Anatomie Générale &c., vol. iv. pp. 449, *et seq.*

fully dwelt upon as by Dr James Johnson ;[1] and which has been taken advantage of in the treatment of several diseases affecting especially the intestines. A similar antagonism exists between these and the kidneys, and also between them and the lungs.[2] Proof of this is had in several affections of the last named organs, when, from the increased secreting action of the intestines, and from the relief following this, it cannot be doubted that it then falls to their share to excrete, at least in part, those products which can no longer be eliminated by the diseased kidneys or lungs. They in such cases act, therefore, as depurating organs ; which becomes still more clearly evinced in several other complaints. In a great number of both simple and exanthematous fevers—which affections are due to peculiar influences conveyed through the atmosphere and affecting the blood principally—the intestines, especially towards the end of the disease, show an increased amount of gaseous and watery secretion.[3] In those cases, it is not always that lesions of the canal can be detected which will account for such increased action ;[4] so that the alteration of functions assumed by the intestines cannot be thought to be owing to mere local affections, and we are driven to the conclusion that it is the result of a general cause affecting the whole system: and further, that such a cause, morbid in its nature, is sometimes eliminated by the intestines during their altered action, is plainly shown in what has been called "Critical evacuations."[5] If we now reverse the medal, we perceive that the bowels perform their functions regularly so long as the other secreting and excreting viscera of the body are healthy ; and accurate observation has also proved that in the dark-skinned races, in whom the excreting powers of the skin attain such a high range, as long as these remain unaltered, they are remarkably free from all affections of the bowels.[6] On the other hand, again, when the functions of other organs, especially the skin and kidneys, are increased to an undue amount, the mucous membrane of the intestines becomes less active than in the normal state, and constipation is one of the results therefrom proceeding.

[1] Influence of Tropical Climates on the European Constitution.

[2] Copland, op. cit., vol. ii. p. 719. Liebig, Animal Chemistry, p. 143.

[3] Andral, Clinique Médicale, vol. i. p. 433.

[4] *Ibid*, vol. i. pp. 424, 426.

[5] *Ibid*, vol. i. pp. 118, 432.

[6] Annesley, op. cit., vol. ii. p. 249. Copland, op. cit., vol. i. p. 703.

16. Turning now to the liver, we see that its excreting agency has been more accurately ascertained and more generally acknowledged. In its separating hydro-carbon in large quantity from the blood, in the shape of bile, it proves itself an organ whose mission is to remove from the system what may be called "normal morbid products," as these are and must be constantly met with in the body. This action, purely of excretion when digestion is not considered, is decidedly antagonistic to that of the lungs; and this antagonism, which has been proved by Annesley, Liebig, and Copland among others, becomes most apparent in the fœtus, in which the liver is so large whilst the lungs are completely inactive; and during the changes from fœtal to external life, when the former organ diminishes in proportion as the latter take upon themselves more energetic functions. Bile is indeed a secretion, but it is as truly an excretion:[1] for with regard to every purely secreting gland, we know that when its products are so pent up as to become absorbed; or when, from some cause or other, the gland can no longer ful-

[1] NOTE.—The chemical school seem to hold bile as a pure secretion, on the mere grounds that it is *wholly* absorbed by the intestines. But there are here two obvious questions which present themselves: 1*st*, Is it *wholly* absorbed? 2*d*, Wholly or not, is it absorbed *as bile?* As to the first, chemists have gone so far as to say that the colour of the fæces is not owing to bile or any of its constituents. How, then, are we to account for the well known fact, that when it is deficient the fæces are colourless? and, better, that, in proportion to its deficiency, is the want of colour in these? And, 2*dly*, It seems, to me at least, incomprehensible, that an organ of the size, structure, and anatomical connections of the liver, should have been formed for the mere purpose of elaborating a fluid which, as soon as it has left it, returns unchanged to the blood from which it had been separated. The only proof that we need adduce to show that the bile is *not* reabsorbed *as bile*—even granting that it is wholly reabsorbed—is the simple pathological fact that, when absorbed *as bile* in the gall-bladder from obstruction of its duct, jaundice does follow; which is never the case when it is taken in after it has flowed into the intestines. As it leaves the liver, therefore, bile is as much an *excretion* as a *secretion:* a secretion, because it undoubtedly performs its part in the digestive process; an excretion, for the reasons stated above. Chemistry, to whatever conclusions it may come by means of its analyses, cannot overturn the inferences arrived at by the careful consideration of physiological conditions and pathological phenomena.—(See, on the one hand, Liebig, Animal Chem. p. 59, &c., and on the other, Carpenter, Princ. of Human Physiol. p. 609.)

fill its duties; although the particular process to the completion of which this secretion is necessary may be deranged, and particular parts or systems may suffer; yet those absorbed or non-secreted products do not act on the constitution in the same way that bile does, when absorbed as bile, or when it is not secreted. Further, when a pure secretion is not solicited for some time, by the suspension of the actions to which it is useful, it becomes greatly or entirely deficient: such is not the case with the bile, which, even though food be for a very long time withheld, is separated from the blood in scarcely diminished quantity.[1] Of whatever use, therefore, it may be in the process of digestion, all these facts tend to show that one action of the liver, and that not its least important, is essentially one of excretion. And on such physiological grounds alone, we might safely establish that it is, in a very high degree, a depurating gland;[2] presenting to us, in its mode of influencing the constitution, greater resemblance with the kidney, an absolutely excreting organ, than with any of the other purely secreting glands of the body. But Pathology, besides, with its numerous instances of diseased actions and altered functions, comes to point out to us, in an unmistakable manner, the part which the liver performs in removing from the economy what we shall now call "abnormal morbid matters." In those same affections in which we have found that the intestines become, as it were, excreting organs, the liver acts in a similar manner. In Phthisis pulmonalis, as well as in many other organic and functional diseases of the lungs, we have proofs after death of alteration of structure of the liver;[3] or only witness during life, at different periods of the malady, excessive evacuations of bile,[4] which generally bring a momentary relief to the patient, when they are not of sufficiently long continuance to diminish his strength. In fevers and the exanthemata, we see it, or better its secretion, changed from its normal characters,[5] indicating the efforts of the organ to eliminate from

[1] Liebig, op. cit. pp. 61, 74.

[2] Budd, on Diseases of the Liver, pp. 39, *et seq.* Bichat, Anat. Gén. vol. iv. pp. 449, *et seq.* Lebert, op. cit. vol. i. p. 287.

[3] Watson, Princ. and Pract. of Physic, vol. ii. p. 182.

[4] Hooper's Physician's Vade Mecum, p. 35.

[5] Bright's Reports of Medical Cases, vol. i. p. 180. Andral, Cl. Méd. vol. i. p. 405. Louis, Recherches Anat. Pathol., &c. sur la Maladie comme sous les noms de Gastro-Entérite, &c., vol. i. p. 444.

the system the impure matters from which it suffers; and also, in some cases when, during a high temperature, the abundant cutaneous exhalation is suddenly suppressed, we obtain increased secretion of bile; as we shall afterwards see takes place in sporadic cholera.

17. After thus endeavouring to establish the excreting powers of the mucous membrane of the intestines, and those of the liver—which functions, though not insignificant under physiological circumstances, acquire, we may say, a vital importance in several diseases—it remains for us to point out that, under pathological influences, such as those we hold to be productive of dysentery, the fulfilment of that function, necessarily somewhat altered in its nature and increased in its activity, can determine in those organs such a process as may easily run on to inflammation, and all the consequences found in them after death. And we shall so far show that those changes are not to be explained by a merely local cause acting locally—such a cause, we maintain, being inadequate to produce them—but can, and are in fact to be accounted for by a more general influence of the system, manifested more particularly in those parts.

18. That the inflammation which attacks the intestinal mucous membrane in dysentery is of a specific character, no body who considers its attendant phenomena can doubt. We have already spoken of the little comparative constitutional disturbance which accompanies the disease even in some of its worst stages and features; and now, considering the mode of occurrence and progress of the inflammation by themselves, we think that those who hold that it is merely a local process and the essence of the malady itself, would be at a loss to account on such grounds for the rapidity with which it runs on to its worst consequences. To explain such changes, we must therefore look for something else than a mere local action. Cruveilhier[1] gives it as his opinion, that this inflammation is of an erysipelatous character. Whether it be exactly so or not, we shall not presume to discuss; but so much, if it be the case, is it somewhat of a specific nature, as, not unfrequently, is erysipelas favoured by, if not due to, atmospheric agencies.[2] It is true, that it seems

[1] Anat. Path. liv. 40, p. 3.

[2] Watson, op. cit., vol. ii. pp. 829, 831.

an established fact in pathology, that congestion and inflammation of the digestive mucous membrane shows a greater tendency to terminate in ulceration than in any of the other tissues of the body. Why this should be the case, no one, I believe, has yet properly explained. We do not presume to attempt to solve the question; we are satisfied with the fact. But even this tendency to run on to that last stage of the inflammatory process, overrated as it has been by some, cannot account for the rapidity with which it occurs in dysentery. In extensive burns of the surface, we see ulceration rapidly produced in the mucous membrane; but here, without entering into any consideration why it should at all get inflamed,[1] we have such a shock produced on the nervous system, as to affect this greatly; and in all such cases, ulceration quickly results from the slightest degree of inflammation. Perhaps an analogous example more in point is to be met with in typhoid fever. In this affection—between which and dysentery we have already found so much resemblance—ulceration is arrived at, without its intervening stages being scarcely recognised; and indeed, as has been remarked by some author, we have in what takes place in the skin—bed sores—though in a minor degree and proceeding from different immediate causes, an illustration of what goes on in the mucous membrane of the intestines. In dysentery, truly, inflammation makes itself more felt and perceived; but in its chief features, there is a striking analogy with the same process as it is seen in the former affection. So true is it due to a specific cause, and is specific in its nature, that Andral, after his numerous observations, has come to the conclusion, that we cannot admit a ratio between the intensity of the hyperæmia of the mucous membrane, and the rapidity with which ulceration supervenes in it;[2] nor are the alterations in a constant proportion with the duration or the symptoms of the disease.[3] He seems, however, to be willing to account for such facts by individual peculiarities, and supposes he has proved this theory by his experiments.[4] No doubt the constitution of an individual must exert a great influence over such processes taking place in his body; but it is clearly impossible, that the

[1] Budd on Diseases of the Liver, pp. 70, 71. Andral, Précis d'Anat. Pathol. vol. p. 223.

[2] Andral, Précis d'Anat. Path. vol. ii. p. 10.

[3] *Ibid.*, vol. ii. p. 205.

[4] Andral, Cl. Méd. vol. i. p. 380.

same constitution should be met with in all those who are attacked by dysentery. Although we are ready to allow that such individual tendencies may modify in part the course of the inflammation, yet we maintain that the great reason for its being so peculiar in its features is to be found in the influence of that same cause which originally produced it, namely, "the dysenteric malaria;" and to that cause also, according as it is intense or not, is mainly to be referred the greater or less rapidity of the ulceration. Too plain an example is offered of this latter case, when in those forms of dysentery, which from their characters have been called "putrid," we have the ulceration proceeding at a wonderful pace, and even sloughing of portions of the mucous membrane occuring soon after the manifestation of the affection. From the consideration of such facts, we cannot refrain from thinking, that they all tend to prove that the elimination of the dysenteric poison, carried on by the mucous membrane of the intestines, can very well cause and account for the inflammatory changes which take place in it; the peculiar character of those changes, again, tending to demonstrate the specificity of the cause, as any simple and ordinary local action could not have produced them.

19. It may now be asked, if we consider the mucous membrane to be one of the organs that eliminate the poison, why is it not generally affected? why is it mostly the large intestines? and even in these, why some parts more than others? We confess that we can give no satisfactory answers to such questions, and feel altogether unable to account for such occurrences; although we are convinced that they take place undoubtedly in obedience to certain fixed laws, otherwise there would not be such regularity in their development. We shall only say that dysentery is not the only disease in which such partial affection of the mucous membrane cannot be fully understood; we have the same inexplicable facts in continued fever, in which ulcerations, when they exist, are principally met with in the lower third of the ileum. This has been said to depend upon the glands which are situated in greater abundance in that portion of the intestine;[1] and many authors hold a similar opinion with regard to what takes place in dysentery.[2] Whether

[1] Watson, op. cit. vol. ii. p. 740.

[2] *Ibid.* p. 500. Broussais, Histoire des Phlegmasies, p. 241.

the solitary glands, as it is thought they have been seen in this latter malady, are the first to take on a morbid action ;[1] —or whether that morbid action, though it be the same in the surrounding parts, goes on in them sooner to disorganization on account of their greater vascularity ;[2]—we cannot say. We are unacquainted with the nature of the secretion of those glands ; we are even totally ignorant of their functions : until such points be ascertained, it will of course be impossible to point out the laws of disease which are apt to influence them sooner than the surrounding tissues. But our being unable to explain why such differences should exist, cannot in the least invalidate our inference with regard to the functions of the mucous membrane in the disease under consideration : this is the great fact which we wish to lay down ; that is only a peculiarity which is included within a more general fact, and whose existence or non-existence can in nowise affect it. Other theories cannot explain more satisfactorily the occurrence of a higher degree of inflammation in those peculiar situations. The irritation-theory has attempted it by speaking of the remora of the irritating matters in those localities, which remora is by it attributed to conformation of the part or its depending situation during disease. But, besides that the theory, as we have shown, is altogether unacceptable,[3] the conformation of the bowel, which is the same everywhere, is no more an argument in its favour than the position of the patient during disease, either in the recumbent or sitting posture, can prove the dependence of such parts as the end of the ascending or the beginning of the descending colon, or its transverse arch.

20. With regard to the liver, we have already observed, and it is not perhaps needless to repeat it, that change of texture does not alone constitute an affection of that viscus ; but that alteration of its secretion is to be considered as an indication of disease as well, somehow or other connected with the organ. Now, it is the opinion of some that its affection in what has been called dysentery, is merely an incidental coincidence ; whilst others seem to think that, even

[1] Annesley, Dis. of India, vol. ii. p. 265. Parkes, op. cit. p. 6.

[2] Baly, in the London Medical Gazette for 1847 (on the Pathology and Treatment of Dysentery), p. 446. Andral, Cl. Méd. vol. i. p. 380.

[3] See § 9.

functionally, it is by no means a common occurrence. That its implication is not merely incidental, but necessary, is what we think may be justly concluded both from what has already been said of its excreting powers in diseases especially depending on atmospheric agencies, and from what we shall state presently. And that its secretion is more or less modified in nearly every case of dysentery, is borne out by facts, and acknowledged by a large majority of writers on this subject, and by almost all those who have studied the disease in warm climates.[1] Few exceptions, if any, could be found to this last statement; at the same time, we are ready to grant that alteration of the bile may not, *of absolute necessity*, occur, though it does and must certainly obtain in almost the totality of cases, for reasons which we have already given. On the other hand, structural lesion is not at all to be considered an important condition to the development of dysentery; nay, we may add that of itself it could prove nothing whatever. And yet, though it is often dependent upon other contingencies than the mere increase of action and greater vascularity of the organ determined by its functional derangement, and cannot therefore be deemed a direct consequence of dysentery itself; there is reason to think that it is oftener present than has been admitted by a few. These latter have founded their opinion on the supposition that every affection of the liver should leave after it a trace of its nature or of its existence. But, first, the difficulty of ascertaining the presence of disease in that viscus, even in a post-mortem examination, has been dwelt upon by every one who has studied carefully and on a large scale its affections. "*The colour and texture of the liver are such as to make it difficult, with the imperfect means of research hitherto employed, to detect and define in the dead body the various effects of disease, unless where this has gone on to disorganization or complete change of structure.*"[2] If such be the case, can it be denied, on any sure grounds, that no lesion of this organ existed in those numerous cases of

[1] Sir James M'Grigor, in Medico-Chirurgical Transactions of London, vol. vi. p. 430. R. W. Bampfield, on Tropical Dysentery, pp. 14 *et seq.*, 46. Sir G. Ballingall, op. cit., pp. 54, 79. Cheyne, op. cit., vol. iii. p. 22. Johnson, on Derangements of the Liver, p. 41. Annesley, Dis. of India, vol. ii. pp. 205, 210. Parkes, op. cit., p. 116.

[2] Abercrombie, p. 314. Budd, p. 78. Parkes, pp. 2, 222.

dysentery in which mere ocular and manual examination was employed to judge of its condition? And, secondly, supposing that after death we could come to the certain conclusion that it is perfectly healthy, and that even the bile offers no altered appearances, are we entitled to say that no functional, or even structural, affection had existed during life? Surely not. It is by no means impossible that in such cases—and they are rare—the organ may have recovered perfectly. And in support of such an opinion, let us quote the words of one whose practical experience of its diseases must give them the greatest weight:—"*I am by no means disposed to infer, from the want of morbid appearances in the liver, that this viscus may not have been, in many cases, the seat of diseased action during the life of the patient.*"[1] We shall further on examine how change in its texture comes to be established; let us at present consider the alterations of the bile.

21. Some have stated that bile is completely wanting during the acme of the malady, even when the dysenteric evacuations were examined microscopically, as they were by Lebert.[2] If such entire disappearance, whether due to non-secretion or re-absorption, be true, it must be indeed very temporary; for icterus does not supervene upon it.[3] And even granting that Lebert's observations were liable to no fallacy whatsoever; in the cases particularly examined by him, is it not possible that the liver had then reached that stage of the inflammatory process when all glands cease from their function of secretion? Or else that the bile had then become so altered in its nature, as that its constituents no longer preserved their normal characters, or had disappeared altogether? in both which cases, the microscope could not of course detect their presence. These two theories, which are the only ones that could account for the absence of bile, would at any rate give us distinct proofs of co-existent functional affection of the liver; which is further evinced by the nature of the evacuations at the beginning and at the termination of the disease, and by the morbid appearances of the bile found in the gall-bladder after death.[4] It is laid down with great justness that

[1] Ballingall, p. 53.
[2] Vol. i p. 520. Parkes, p. 47.
[3] Parkes, p. 47.
[4] Ballingall, p. 57.

alteration of the products of the liver, as of any other secreting organ, is dependent not always so much upon any changes of its texture, as upon other lesions which often escape our knowledge.[1] In the case of dysentery, it cannot be said to have its origin in any organic affection of the gland, since this, to all appearance, is the exception ;[2] it can be the result only of the bile being derived either from the blood contaminated by the absorption of morbid matters from the diseased mucous membrane, as has been held by some ; or as we are rather of opinion, from the contaminated blood which circulates through the whole system, the consequence of the "*dysenteric malaria.*"[3] The first supposition, no doubt, has many facts in its favour, inasmuch as many more direct proofs can be brought forward to its support. But against it, we have to argue that absorption of the morbid matters does not take place to that extent which was believed by the older authors ; for in chronic diarrhœa and dysentery, where affection of the liver can sometimes be more reasonably traced to such a cause,[4] it comes to take place only long after the intestinal complaint has continued.[5] The modern theory of ulceration—and I believe the true one—does not admit of absorption as the cause of that process ;[6] and we know also that the blood-vessels of the intestines are, in a case of dysentery, in a state of distension which is highly unfavourable to absorption.[7] Further, those morbid matters, to the account of which so much has been laid, are not so deleterious in their nature as to produce such immediate effects on the liver. They are mere blood and mucus, with increased watery secretion in the first stages of dysentery ; possibly somewhat altered in their qualities, but

[1] Andral, Précis d' Anat. Pathol. vol. ii. p. 612.

[2] Ballingall, pp. 64, 76. Dr Brown, vol. i. p. 658.

[3] NOTE.—We do not reckon the possibility of its being dependent upon depraved nervous influence, for there is no proof whatever of affection of the cerebro-spinal system ; and no sufficiently direct communication exists between the liver and the large intestines, by means of the ganglionic system of nerves, to account for it. And we trust that no hidden and mysterious *sympathy* will be brought forward to explain it.

[4] See § 41.

[5] Annesley, Dis. of India, vol. i. p. 438.

[6] Paget, Lectures on Surgical Pathology, vol. i. pp. 418, *et seq.* Miller, Principles of Surgery, pp. 143, *et seq.*

[7] W. Conwell, Funct. and Struct. Changes of the Liver, p. 139.

not apparently possessing any highly irritating powers: yet altered bile is to be met with at the very beginning of the malady.[1] Again, disease of the liver, especially when it runs on to structural changes, does not always proceed in a ratio with that of the mucous membrane; and often when the latter has reached its highest grades, no more or even less alteration is present, either in the quality of the bile or in the texture of the liver, than in other cases when, with less disorganization of the intestines, suppuration has supervened in the former viscus.[2] Probabilities, therefore, run strongly against the hypothesis that absorption of such matters is either productive of alteration of the bile or of the structure of the liver, as we shall see more at length. On the other hand, analogy demonstrates that the second supposition is the more likely to be the true one. For if we revert again to diseases produced by atmospheric agencies, and principally to fevers, we often meet with instances of alteration of the bile, without any affection of the intestines whatever capable of generating morbid matters.

22. We have now seen that both physiological and pathological reasons entitle us to think that in any disease resulting from a poison attacking the blood primarily, the liver acts as an excreting organ of the morbid matters. We have also seen that from the nature of the bile, or its absence in every case of dysentery, it does so act undoubtedly in that disease. But it is not with mere alteration of secretion that we have sometimes to do; but also with organic lesion, and even of a serious nature, which we believe can be as well accounted for as simple deviation of the bile from its healthy characters. From the action which goes on in the liver, the extra work which it is called upon to perform, a certain amount of congestion of this viscus must necessarily exist in all cases of dysentery;[3] and this condition may either remain stationary, or pass into inflammation, chronic or acute, which can be proved on the following grounds. It is a known fact that secreting glands, when in a state of active hyperæmia, are very apt to run on to inflammation, if they are required to perform more work than usual, as is very likely the case with the liver in dysentery.[4] Further, such inflammatory

[1] Sir John Pringle, p. 258. J. Johnson, Derangements, &c. p. 41. Annesley, Dis. of India, vol. i. p. 403; vol. ii. p. 199.

[2] Annesley, vol. ii. cases *passim*. Parkes, pp. 39, *et seq.*; cases *passim*.

[3] Annesley, vol. i. pp. 343, 344, 403. [4] See § 16, 17.

disease as is met with in it, principally, if not exclusively, takes place in certain constitutions, such as the scrofulous ; —or in those who have been enfeebled by fatigue, privation, or disease ;—also in persons addicted to drinking, who have often suffered from bowel or liver complaints, or who have been attacked by fevers.[1] In all those conditions, the liver is pointed out as an organ whose vitality has already been impaired, either by direct agencies, as in the last group ; or indirectly through the constitution, as in the two former, especially the first. It is not difficult therefore to conceive how, under such circumstances, inflammation may become engrafted upon the congestion originally produced by the disease ; and it can be as easily understood why abscess does readily supervene to this last process. For, in idiopathic inflammation of all parenchymatous viscera,—and among them the liver,—suppuration is very prone to take place. Where is then the wonder that, with so many conditions to favour an already existing tendency, the inflammatory process should, if developed at all, run on to suppuration ? Many explanations have indeed been offered to account for this occurrence, but they are all insufficient and liable to too many objections. We shall not take up our time with examining those theories which have made liver-abscesses dependent upon absorption of pus, and stoppage of the pus-globules in the capillaries of this organ.[2] This might have been thought valid enough when the microscope had not determined the size of the pus-globules, and shown them to be no larger than the white blood ones ; and when there had yet been no case of leucocythemia, in which singular affection, though the white corpuscles of the blood are sometimes greatly increased in size and larger than any pus-globule ;[3] yet no instance of abscesses in the liver or elsewhere has been met with.[4] We shall only glance over the question of their being secondary abscesses dependent upon absorption of morbid matters, acting as these do generally.[5] But this evidently cannot hold good on the single ground that in so many cases in which they have been found

1 Twining, p. 98. Annesley, Dis. of India, vol. i. pp. 521, 523. Sketches, &c., p. 472. Copland, vol. ii. pp. 722, 736.

2 Budd, pp. 48, *et seq.* Lebert, vol. i. pp. 326, 331, *et seq.*

3 J. H. Bennett, on Leucocythemia, pp. 88, 116.

4 *Ibid*, pp. 92, *et seq.*

5 Budd, p. 64. Lebert, vol. i. p. 333.

in conjunction with dysentery, no similar ones were detected in any other organs of the body. If it were true that such abscess of the liver were really a secondary one, surely there was sufficient time in more than one instance to allow the formation of many more. And further, such an occurrence is the exception, not the rule, though in every case of dysentery we have those same morbid matters which, if they did cause such abscesses, would no doubt produce them oftener. The opinion of Lebert is one which, I think, carries great weight, but to which we object for many of the reasons already stated; he speaks of *phlebitis* as the source of this abscess.[1] No doubt some cases have been demonstrated to have resulted from such; and others again, mentioned by Budd, most probably had a like origin.[2] But however good may be Lebert's opinion with regard to some other secondary abscesses, and however willing we are to grant that a few cases have taken place in the way in which he describes, yet we cannot accept it as the general explanation of the production of liver-abscesses in dysentery, on the ground of the infrequency of their occurrence. The elements of the causes to which Lebert attributes them exist in every case of dysentery; and therefore we might expect reasonably, if his theory were true, that their effects should follow oftener than facts do prove. And again, as in the case of absorption of pus by the open veins or of morbid matters, we would ask, why are they found in the liver alone? If pus here circulates with the blood—under whatever form—and as we know that it does pass from one capillary circulation into the other,[3] why does it not follow the general rule of secondary abscesses, and cause them to be generated in all, or nearly all, the organs and tissues of the body?[4] Besides, the symptoms of the production of liver-abscess in dysentery do not at all accord with those known to accompany that of secondary ones, presenting none of that sudden urgency which characterizes these.[5] Twining speaks of some sort of irritating matters which, not being excreted by the intestines, acts on

[1] Vol. i. pp. 285, *et seq.* Andral, Cl. Méd., vol iv. p. 6.

[2] Op. cit., pp. 160, *et seq.*

[3] Andral, Essai d'Hématologie Pathologique, p. 116. Cruveilhier, op. cit., liv. 11, p. 8.

[4] Andral, Cl. Méd., vol. iv. p. 18. Précis d'Anat. Pathol., vol. ii. pp. 602, 603. Lebert, vol. i. pp. 287, 288.

[5] Annesley, Dis. of India, vol. i. pp. 527, *et seq.* Cruveilhier, liv. 11, p. 1.

the liver and produces abscess in it.[1] Copland holds a similar opinion, with some slight difference; and though his nearly coincides with what we take to be the true one, still he makes the one process the cause of the other.[2] We do not deny that it may have been *apparently* so in a few cases; or that the sudden arrest of the intestinal evacuations may have hastened the morbid action which was developing itself in the liver; yet we think that in most cases it had begun before the cessation of the intestinal symptoms, and that the establishment of suppuration would have been brought on by any other cause influencing the organ in a similarly detrimental way. We cannot subscribe to the idea that that inflammation is of a scrofulous nature;[3] still less that those collections of pus are mere deposits of that abnormal product, unattended by any inflammatory action whatsoever.[4] Both these theories are founded on the well-known facts, that not unfrequently the process preceding the formation of those abscesses is of an exceedingly short duration; and that so obscure and insignificant are the signs of such momentous changes, that they have often been discovered only after death, when nothing during life had led to suspect them.[5] But even in idiopathic inflammation of the liver, the symptoms which indicate it are not by any means on a par with the mischief which is going on;[6] and in dysentery, the natural dulness of the organ, the more urgent sufferings caused by the affection of the bowels, together with the constitutional disturbance present at the time, contribute not a little in masking or even completely obliterating any signs that could have led to the discovery of the liver disease. That it is an inflammation of a peculiar nature, nothing therefore tends to enforcethe view; though we find it greatly in persons of a scrofulous diathesis, it is no less true that it also supervenes in others who do not present the least traces of such a tendency. It is not at all necessary to call to our aid any such theory to explain the manner in which it

[1] Op. cit., p. 99. Parkes, pp. 114 *et seq.*

[2] Med. Dict., vol. i. p. 705.

[3] W. Geddes, in Calcutta Med. and Phys. Trans., vol. vi. pp. 333 *et seq.*

[4] Andral, Cl. Méd., vol. iv. p. 18. Lebert, vol. i. p. 140.

[5] Ballingall, pp. 53, 91. Annesley, Dis. of India, vol. i. p. 403.—W. Geddes, in Calcutta Trans., vol. vi. p. 289.

[6] Annesley, Dis. of India, vol. i. pp. 405, *et seq.* 471.

comes to take place in the liver; the consideration of the circumstances under which it is developed will sufficiently account for all those phenomena. Besides in Europe, abscesses are very seldom met with,[1] though dysentery there no doubt attacks many scrofulous patients. As to their being mere deposits of pus, unpreceded by any inflammation, it still remains to be proved that pus can be produced without a certain amount of that process, small though it be under some circumstances;[2] whilst, on the other hand, more than one instance has been recorded in which decided signs of inflammatory action accompanied their presence.

23. It may not be amiss to say a few words with regard to the presumed difference existing between tropical dysentery, and that occurring in cold climates; which, no doubt dependent merely upon the circumstances under which both develope themselves, is entirely one of degree and not of kind. As it is met with in Europe, dysentery is less severe than in warm countries; more seldom assumes those forms which have been denominated *adynamic* and *putrid;* and is rarely accompanied by structural diseases of the liver, though by always greater or less alteration of the bile. If we remember that malarious agencies, to which we have attributed its production, are beyond any comparison more rife in warm than in cold countries, we readily perceive *one* reason why there should be such a difference in the prevalence and intensity of the cause. But this is not all. The peculiar action of heat, above all when combined with moisture, in heightening the sensibility of the intestinal mucous membrane, and rendering it thereby more prone to resent any irritation;[3]—the fact that, in the disease under consideration, it is called upon to make up, by an augmented secretion of water, for the arrest of those cutaneous exhalations which, in the tropics, attain such a high range;[4] —and above all, the increased vascularity which is the result of both these conditions;[5]—will readily account again for the different features assumed by dysentery in different climates. And thus it is that, on the one hand, the greater

[1] Dr Baly, in op. cit., p. 488. Andral, Cl. Méd., vol. iv. p. 214.

[2] Lebert, vol. i. p. 65.

[3] Broussais, Hist. des Phleg. pp. 196, *et seq.*

[4] Annesley, Dis. of India, vol. ii. pp. 249, 396. Copland, vol. i. Art. *Dysentery.* Parkes, p. 54.

J. Johnson, Infl. of Trop. Climates, &c. p. 19.

intensity of the cause renders the disease itself more severe; whilst, on the other, the peculiar condition of the tissues in which the malady manifests itself, will sufficiently explain, even were the intensity of the cause the same, the difference in the severity of the local affections it produces. It must not be believed, however, that it never shows itself in Europe as it does in warmer regions; let the circumstances that attend upon its production, and the causes that give rise to it, be but met with in the same or nearly the same degree, and it is seen acquiring similar characters, either in its epidemic tendency or its fatal severity, as in these latter climates.[1] The more serious implication of the liver, in the so-called tropical dysentery, can be accounted for on nearly the same grounds; and in the same way that heat has a peculiar action on the intestinal mucous coat, so it exerts a similar influence over the liver. In warm countries, it falls to the share of this gland—and we believe somewhat to that of the mucous membrane of the bowels also—to make up by greater activity for the diminished power of the lungs in excreting carbonic acid and water.[2] This unusual action, besides necessitating an increased amount of blood in the viscus, further depresses its vitality by its continuance; and therefore predisposes it to assume more readily any morbid process; and when such takes place, impairs its power of resistance to its progress. That this is the case, is readily proved by considering under what circumstances the different grades of its affection come to be established. In the natives of those countries—who are by no means exempt from dysentery—in whom it is but reasonable to presume that they have a liver adapted to the wants of their climate; though we meet with great functional derangement of the organ, greater indeed than in other nations, yet structural disease is of exceedingly rare occurrence.[3] If we go a step further, we see that alteration of its texture becomes rather more frequent in those who have newly arrived to tropical regions, and in whom it as yet performs its ex-

[1] Sir John Pringle, op. cit. pp. 19-23, 37. Cheyne, pp. 2, *et seq.* 22. Sir J. M'Gregor, in Med. Chir. Trans. of London, vol. vi. pp. 423, 426, *et seq.* Baly, p. 485.

[2] Annesley, Dis. of India, vol. i. p. 298. Copland, Art. *Liver.* Liebig, Animal Chemistry. Gregory, Organic Chemistry, p. 505. Carpenter, Human Physiology, p. 502.

[3] Twining, p. 3. Annesley, Dis. of India, vol. ii. pp. 253, 350. Parkes, pp. 118, 119.

creting functions, though in an increased degree, with great vigour ; and have not yet been brought under the influence of debilitating causes. But most often is organic lesion present in old residenters, whose whole frame is enfeebled by a thousand circumstances, and whose liver has had its vitality depressed by the causes mentioned above, or has even already been actually affected —or again, among those new comers who have laboured under many difficulties on their passage, and meet disease only with impaired or exhausted vital powers.[1] These facts will explain sufficiently the supposed difference between dysentery, as it manifests itself in warm or cold climates ; and it will be obvious, that such difference is only one of degree of intensity, both in the cause and effects of the malady ; and not one of kind, depending upon causes different in their nature.

24. I have thus endeavoured to show, in the first place, that inflammation of the mucous coat of the bowels, *in what has been called "dysentery,"* is not a purely local disease depending upon those causes to which it has been mostly attributed, as local irritation, heat, cold, &c., and affecting the economy secondarily ;—but, on the contrary, is due to a cause exerting its influence on the system generally at first, and through it on peculiar tissues and organs of the body ; which belongs to the class of the malarias, and of which we see most positively the effects, though ignorant of its nature and mode of action. I have also pointed out, after establishing the excreting powers of the intestinal mucous membrane and of the liver, that the peculiar malarious poison tends to be eliminated by these organs ; and that it is during those efforts that the peculiar affections which they present in that disease become developed ;—at the same time, calling the attention to the specific characters distinguishing the inflammation of the mucous coat of the intestines, and to the fact that alteration of the biliary secretion, not necessarily of the biliary organ, is present in almost every case of dysentery. And, thirdly, I have proved that diseases of those organs are not dependent the one upon the other ; explaining how structural lesion of the liver comes to take place ; and lastly, showing that there is no difference in the nature of dysentery, as it is met with in warm or cold climates. I come now to conclude that these affections of the intestines

[1] Annesley, Dis. of India, vol. ii. pp. 238, 285.

and of the liver, in the malady called dysentery, are not diseases *per se*, nor either dependent upon one another, but are *the conjoined effects of one cause, having no other connexion between them than their co-existence, and their ultimate similarity of origin in the primary affection of the whole system.*

25. We shall not enter into any lengthened consideration of the diseases of the liver and intestines in Fever ; still less enquire into these minutely in the different forms of fever and their types. Such a task would be unnecessary ; and besides plunging us into researches which would prove well-nigh interminable, would also be rather foreign to our subject. For our object is not to study what kinds of lesions develope themselves in those organs during fevers ; but when they occur—which nobody denies—what connexion can be said to exist between them.

26. The principle from which we start is, we believe, one that has been adopted by the British school of medicine, and particularly by that of Edinburgh ; and it is this :— That these lesions, enteric and hepatic, are the effects, not the cause of the malady.[1]

27. In spite of numerous and ingenious theories, the question remains still to be solved : what may be reckoned the proximate cause of fever ? that is to say, what tissue of the body, fluid or solid, being primarily affected, influences in its turn the whole organism ? But this is too vast and too intricate a topic to venture upon ; and in the consideration of our subject, it will be nearly immaterial whether it be assumed that the blood is the first tissue diseased, or that the system of organic nerves is the one that is first morbidly impressed ; for those who are of the latter opinion—and we must say that we do not feel convinced by their arguments —allow that the secretions of the body are disordered, owing, at least in the advanced stages of fever, to the alteration of the blood.[2] Dr Graves holds indeed, *that changes in the secretions are "simultaneous" with changes in the blood ; and that they are all the result of "a common unknown*

[1] See § 12. [2] Copland, vol. i. pp. 895, 914.

cause;"[1] from which words we may infer that the changes in the secretions are altogether independent of those in the blood. But whatever that "unknown cause" may be, it seems to us rather incomprehensible that such a one should exist, under whose agency all the secretions of the body—so different in their nature, and proceeding from organs so various in their structure and in their situation—should, without being influenced through (as it were) a common link binding them all, be thus disordered *simultaneously*. Without referring to the authority of so many who adopt an opposite view, how far more likely does it seem that the changes in the secretions should be due to the alteration of the blood from which they are all derived—the connecting link between them—becoming more abnormal in proportion as this fluid is more affected; increasing its diseased condition by accumulating in it the effete products which they should have excreted; or resuming their own natural characters as it becomes less tainted; and restoring it, by the elimination of the morbid cause, to its former purity?

28. It is during those efforts made by the excreting organs that we believe the structural lesions, which are sometimes met with in them, become established. It is not our province to show how these, under the circumstances attending their development, may occur in the different organs. Nor is it necessary to point out those local changes as they take place in the liver and in the intestines; for in the same way as they are produced in dysentery, so they are also in fevers. Though their affections may be present in a comparatively few cases, and in some forms of fever, such as those which have been called *bilious* and *enteric*, more than in others; and though their frequency of occurrence may seem, in no slight degree, to be influenced by locality and season:[2] yet they show themselves, in the generality of cases, only some time after the outburst of the disease; and present in their development, progress and consequences, features analogous to those observed in dysentery. As a general rule, however, it would appear that the bile is far oftener altered in all cases of fever than the intestinal secretion;

[1] Graves, Clinical Lectures on the Practice of Medicine, vol. i. p. 101.

[2] Graves, Clinical Lectures, &c. vol. i. pp. 102 *et seq.* Copland, vol. i. pp. 905, 913, *et seq.* Watson, vol. ii. p. 742.

very likely this is due to our readier means of testing the healthy characters of this former fluid than the products of the latter organs. On the other hand, I think that actual disease of the intestinal mucous coat is more frequent than structural change in the parenchyma of the liver; though in this again the same fallacy, which we have seen is apt to mislead in dysentery, may also exist here.[1]

[1] NOTE.—These attempts at generalization are the result of the impressions left on my mind by my various readings on the subject of fever. I do not know of any particular authority saying exactly the same thing — As an *excreting organ*, the mucous membrane of the digestive canal seems to admit of no very marked division into two distinct parts, viz. small and large intestines; at least in the way in which most authors have considered such a division to take place: at the ileo-cœcal valve. It is true that in some diseases, one or other of those portions appears, as a pretty general rule, to be more affected; but it is certainly not exclusively so in every instance. For if we take as illustrations of our point the lesions of that membrane in dysentery and typhoid fever, which lesions are usually reckoned as confined to the large in the first case, and in the second to the small intestines; it becomes at once evident, from so many recorded observations, that such a distinction is far from holding good in every case. Thus in dysentery—the true dysentery, not the simple idiopathic inflammation of the mucous coat of the large intestines—we have very often the lower part of the small also implicated, though in a minor degree (Ballingall, Annesley, Copland, Abercrombie; Parkes seems to be of the contrary opinion); and in fevers, the large intestines are, together with the small, very often diseased, and sometimes in an equal, if not a superior degree (Bright, Annesley, Jenner, Chomel, Andral). It would seem, indeed, that the portion of mucous membrane extending from the middle of the ileum to the middle of the ascending colon is that which, in diseases analogous in their nature to those mentioned above, performs in a most energetic manner the work of excretion;—that as the cause of the disease becomes more intense, the lesions which result from over-action of the membrane extend on both sides, but in by far the majority of cases, along the remainder of the mucous coat towards the end of the rectum;—and that in some where the blood is contaminated to the highest degree, the beginning of the canal, even up to the pharynx, becomes injured. It is very seldom, however, if ever the case, that the *whole* digestive mucous surface suffers; and the involvement of its various parts appears very much to differ with different epidemics, even of the same malady—a phenomenon certainly most inexplicable by all that we know of that tissue, and of the diseases which are liable to act upon it. As a general rule, however, it may safely be laid down, that the more intense the cause of the disease, *cæteris paribus*, the

29. From the consideration of such facts, it becomes obvious that the diseases of both organs cannot be considered with regard to each other as cause and effect; and that the mere colouring of the coats of the duodenum, jejunum, and ileum with bile, no more than the purging or vomiting of bilious matters, can prove the affection to be primarily seated in the liver,[1] and the lesion of the intestines to be merely resulting from the effect of acrid bile upon the latter. Bearing in mind what we have already said of the excreting functions of those organs, we infer that their diseases in fevers are the result of that one cause which acts primarily on the system generally; and that, as in the case of dysentery, *the connexion between them consists merely in their co-existence, and their dependence upon the same cause.*

30. We proceed now to notice a disease in which the liver and intestines are sometimes disordered to a very great extent, and only functionally; namely *sporadic* or *bilious cholera.* There does not exist here so much discrepancy as to what are to be reckoned causes and what effects; though again we have one class of writers holding that the derangement of the intestines is nothing but the result of one previously established in the liver; whilst the other class merely reverse this order. But I believe it is admitted by most that the original cause, predisposing at least, is external to the body, and to be referred again to atmospheric agencies, deemed by some specific in their nature, by others more general, that is to say, depending simply on great elevation and sudden vicissitudes of temperature. This latter opinion seems the true one, as we shall endeavour to prove; nor do we reckon affection of one of the two organs to originate that of the other, but both to be produced by one common cause, acting on them in the same way.

31. There is not the least doubt that in sporadic cholera both the liver and the intestines are involved nearly at the very first manifestation of the complaint. The evacuations by

greater will be the amount of mucous surface affected; the severity of the affection itself, certainly influenced by the degree of intensity of the cause, depending perhaps more on individual peculiarities and contingent circumstances.

[1] Copland, vol. i. p. 939, 949, 1013.

vomiting and purging, which sometimes go on to such an alarming extent, bear witness to this statement. The presence of bile is indisputable; whilst proof of an extraordinary amount of secretion and exhalation from the intestinal mucous membrane is to be had in the quantity of liquid ejected. By some it has been said that the first effect of the cause of the disease is to produce an irritation of the mucous coat, which is propagated to the liver by continuity or sympathy; by others, this order has been reversed, and it has been conceived that the irritation of the mucous coat is due to the altered properties of the bile. We shall have occasion to revert to the question of production of morbid action in the hepatic organ, from so-called "sympathy" with the duodenal mucous surface;[1] here we need only say that, however possible this is, and although when once increased activity is established, this so-called "sympathy" may tend to keep it up in the liver, yet it is difficult to understand how such intense and general affection of this gland may at once be produced by it.[2] So sudden indeed is its occurrence, so coetaneously does perverted function shew itself in both liver and intestines, that on this many have founded the theory that disturbance of the former precedes that of the latter. The mode of action of the cause of sporadic cholera is besides too clear and too simple, and too decidedly influences the liver directly, to make it necessary to resort to any such explanation of the establishment of disease in it. As to its propagation by continuity from the intestines, even in duodenitis, a higher morbid condition than mere irritation, and more likely to be propagated along the same tissue, inflammation of the biliary organ does not always supervene upon that of the bowel; so that we scarcely think that mere irritation can be so invariably and so instantaneously conveyed to the former, as has been imagined to be the case in sporadic cholera. On the other hand, the quantity and quality of the bile have been blamed for the production of the functional derangement of the intestines. It has been said that a great quantity of bile, acrid too in its character, had accumulated in the gall bladder and liver; and that on a sudden the whole burst upon the intestines, and produced irritation of their mucous coat.[3] But we are not told what had given rise to

[1] See § 44.

[2] Copland, vol. i. p. 322. Dr Brown, in op. cit., vol. i. p. 391.

[3] Cullen's First Lines, (*continued by J. Gregory*, 1829) vol. i. p. 135. Watson, vol. ii. p. 484.

such an enormous secretion of bile which, according to this theory, must have taken place before the outburst of the disease? Nor what had kept it accumulating there? Nor what had made it acrid? These were facts assumed, but not proved to exist, merely to account for both the undoubtedly great amount of the biliary fluid and the intestinal secretion; and we shall attempt to show how unnecessary such assumptions are to explain the phenomena of the disease.

32. Sporadic cholera occurs mostly, and in temperate climates always, during a warm condition of the atmosphere, when the functions of the skin are being performed with the greatest vigour. This warm condition of the atmosphere during its prevalence is invariable; the complaint, besides breaking out always under such circumstances as cannot be attributed to pure coincidence, becomes more severe and more general, the more this obtains; and it has further been remarked that the earlier the hot season sets in, the earlier also do we meet with instances of the malady.[1] This may be reckoned the predisposing cause, besides the great fact, so justly insisted on by Broussais, that during such a state of the atmosphere, especially when moisture is also present in the air, the intestinal mucous membrane is in a more irritable condition than under other circumstances.[2] We do not possess such certain proofs of the liver being in a similar state of irritability; but this we know, that during high ranges of temperature, it is likely that this gland will be more or less congested, from its being obliged to excrete, in conjunction with the skin and the intestines, that portion of the carbonic acid and aqueous vapour which the lungs no longer separate from the blood.[3] Yet it does not follow from this simple occurrence of congestion, that there should be such an enormous secretion of bile; nor that the bile thus secreted should possess acrid properties, as some have gratuitously assumed. It is admitted on all hands that the exciting cause of the disease acts when the body is in the conditions already noticed, namely, a highly active function of the cutaneous surface, an irritable state of the intestinal mucous membrane, and greater or less congestion of the liver.

[1] Cullen, vol. ii. p. 136. Watson, vol. ii. p. 483.
[2] Broussais, op. cit., vol. ii. p. 182. [3] See § 23.

That exciting cause is most generally a chill, or a sudden alternation of temperature, or anything tending to upset the nicely adjusted balance between the skin on the one hand, and the liver and intestines on the other; for even when it cannot be distinctly made out, there is irrefutable evidence that the secretions of the skin have been suddenly arrested.[1] This we believe to be the immediate exciting cause of the disease: *arrestment of the functions of the skin, whilst it was in the greatest activity.* Now, bearing in mind the peculiar antagonism which exists between the skin and the mucous membrane of the intestines and the liver,[2] it is easily understood how the mass of blood that was circulating in the cutaneous surface is directed to those viscera in which there already existed a predisposition to irritation, namely, the liver and intestines. This, no doubt, is the origin of that exaltation of functional activity which soon comes to take place; the vitality and vigour of the organs thus avoiding the inflammation that would necessarily follow, were not the congested state, into which they are suddenly thrown, thus relieved.[3] Incredibly abundant as these evacuations sometimes are, increased secretion and exhalation from such an extensive surface as the mucous membrane, together with the unusual quantity of bile, can yet account for them. We are far from considering that bile is present to the enormous amount that some have believed; at the same time, we need not wonder that it can also be considerably augmented in quantity, when we reflect that all the elements of an abundant secretion are met with in this disease: accumulation in the blood of those materials which are destined to be excreted by the liver, watery vapour and carbonic acid—a highly congested state of the organ; those materials being thus particularly carried in great quantity to it—non-absorption of the greater part of its products by the intestines, as during physiological conditions; the whole being thus ejected externally. As to their acridity, we have no more proof of this than that the intestinal mucus is acrid. The fluids evacuated have often this character; but I believe that it is due to the secretions of both organs—which, as a natural consequence, become altered from their

[1] Cullen, vol. ii. p. 137. Watson, vol. ii. p. 484. Dr Brown, in op. cit., vol. i. p. 382.

[2] J. Johnson, Infl. of Trop. Cl. &c. Annesley, Dis. of India, vol. ii. p. 249. Copland, vol. ii. p. 722. Bichat, Anatomie Pathologique, p. 77.

[3] Annesley, Dis. of India, vol. i. pp. 36, 522.

normal qualities under those morbid conditions ;—perhaps rather more to the one than to the other, but not exclusively to one of them alone.

33. A further proof that to the arrest of the cutaneous functions is due the origin of all the mischief, is that the re-establishment of those functions, as evidenced by a return of healthy perspiration—not the cold, clammy sweats indicative of exhaustion—is one of the most favourable symptoms of the cessation of the disease.

34. Sporadic cholera has sometimes been attributed to the influence of a malaria of some sort or other, principally on the ground that it takes place when malaria is most rife, and that some cases of fever are preceded or attended by it.[1] But in the first place, nothing is brought forward to defend this theory, except a few facts which may nearly be construed according to one's own fancy, and not numerous enough to found any theory upon.[2] Besides, if it be argued that it exists more generally and in greater severity during those states of the atmosphere which favour the development of malarias ; we answer that that greater prevalence and severity are more simply due to the greater intensity of the causes above mentioned : those very conditions of the atmosphere. Further, the general prevalence of the disease in countries which are not known to be malarious to any extent,[3] is opposed to that view. And lastly, the agency of a malaria is not needed to explain either its occurrence or the phenomena that attend it ; all which, we have shown, are to be accounted for in a simpler and less exceptionable manner.

35. Sometimes during the disease, the stools assume the rice-water characters ;, and that description of it has been thought by Johnson to be a variety of the *Asiatic cholera* ;[4] whilst Annesley thinks and proves it to be a mere modification of the *sporadic*.[5] The absence of bile, to which the peculiar appearance of the stools is owing, may be explained either by spasm of the ducts, or of the duodenum causing

[1] Copland, art. *Cholera*. Dr Brown, in op. cit. vol. i. p. 382.
[2] Dr Brown, p. 382.
[3] Great Britain in general, Edinburgh particularly.
[4] Infl. of Trop. Cl. &c. pp. 396, *et seq.*
[5] Dis. of India, vol. ii. p. 403.

constriction of these[1] ; or else by the congestion of the liver being carried to such a point as to put a stop to its functions. We shall not enter into the consideration of any of the other symptoms of the disease, inasmuch as they are beyond the scope of our subject, and they are readily explicable by general principles. Only, as there is no sufficient reason to think the bile particularly and exclusively acrid in this affection ; so the spasms of the muscular coats of the intestines cannot be attributed to the agency of this fluid alone ; more likely they are mostly due to the irritation seated primarily in the mucous membrane itself. And from these views we infer, that in *sporadic cholera* "the functional derangement of both liver and intestines is the effect of a cause originating in neither organ, but acting on both in a similar manner."

SECTION III.

36. After having run over some affections of the liver and intestines having no other relation between them than their co-existence, being inevitable effects of distinct causes; we are now going to glance at others which have a more intimate connexion, and which stand, with regard to each other, as cause and effect. Formerly it was thought that most intestinal complaints originated in the liver, whilst those of the latter viscus were generally idiopathic. The reaction now has taken place, and medical opinion inclines rather the other way. That *some* diseases of the intestines are primary, and, even if not so, may yet react on the liver and cause its implication, nobody, I think, can reasonably doubt ; and the greatest authorities, principally of late, favour this view of the subject. Several points, however, remain yet undecided, though the advance of anatomy, physiology, and pathology has done so much to clear many doubtful ones. And several disorders of these organs are attended by symptoms for us still so obscure, and by post-mortem appearances so vague, that much remains yet to be done before we can arrive at satisfactory general conclusions.

37. The second group of diseases we shall examine will

[1] Annesley, vol. ii. p. 404. Copland, vol. i. p. 321.

comprise those which, originating in the intestines, either idiopathically or otherwise, cause afterwards affection of the liver. We shall merely mention the production of abscess of the liver, from *phlebitis* occurring somewhere in the organs which return their blood to the *vena portæ*. In dysentery, a distinct school would have all the abscesses of that organ to be produced in that particular way; and though we have opposed that view, and ventured to point out the wrong foundation of such a theory, even when supported by such authorities as Budd and Lebert, yet we do not deny but that in a few cases it may have been so: maintaining, however, that these are the exceptions, not the rule, for reasons already given.[1] As to those which may truly be ranked under the head of the so-called secondary abscesses, it is not our object to notice them at length. Their pathology is pretty well known after the experiments and researches of modern observers, among others, Cruvelhier, Andral, Lebert, &c.; who, though they differ in some points, yet agree on the principal ones. They are seen in the liver principally, when the phlebitis takes place anywhere within the abdomen; just as they are formed first of all in the lungs, when the primary injury lies in any other part of the body. But their great feature is that they do not exist in any one of those organs exclusively, but more or less involve all those of the body.[2] Indeed it would seem that phlebitis occurring in any one of the abdominal viscera, by no means causes their production in the liver in preference to the lungs. For if we refer to the instances of secondary abscesses resulting from *uterine* phlebitis and angioleucitis, collected by Tonnellé—which can be depended on on account of the care bestowed on the post-mortem examinations—we find that in 134 cases, in which pus was discovered in the veins and lymphatics of the uterus after death following puerperal fever, he ascertained the existence of abscess in the lungs eight times, and in the liver only thrice.[3] If any circumstance seems to me astonishing, it is that they are not more frequently met with when there is extensive disease of the intestines, seeing that all the conditions from which they may result are, to all appearance, so often pre-

[1] See § 22.

[2] Andral, Cl. Méd. vol. iv. pp. 18, *et seq.* Précis d'Anat. Pathol. vol. ii. pp. 602, *et seq.*

[3] Archives Générales de Médecine, vol. xxii. pp. 484, *et seq.*

sent.[1] In some cases of phlebitis, both secondary abscesses (among other organs of the liver also), and affection of the bowels, are found existing together. These are very analogous to some of the worst forms of putrid fever, and the connexion between both complaints seems to us to be capable of explanation in different ways, or rather to be a combination of the two explanations we have already offered. The affection of the bowels is produced in the same way as we have seen it takes place in dysentery; and abscess of the liver follows the general rule of secondary abscesses which, according to us, partakes more or less of that which governs the production of dysenteric liver-abscess.[2]

[1] Budd, pp. 70, 71.

[2] NOTE.—*C'est à une Inflammation locale que nous attribuons tous les épanchemens purement métastatiques* (Lebert, vol. i. p. 337). Modern Pathology has established beyond all doubt the existence of an inflammatory process as preceding the production of the so-called secondary or metastatic abscesses. This is now a matter of fact. But it still remains a disputed point what gives rise to that inflammatory process. The latest doctrine seems to be the acknowledgment of a *diathèse purulente* (as Lebert calls it); or what comes nearly to the same thing, of an irritating property existing in the blood contaminated by the admixture of pus. Palpably assumed facts these are: and if it be allowed to *assume*, as the foundation for an argument, the existence of peculiar diatheses, as in this case; there is no phenomenon, however obscure up to this day, that will not be capable of a satisfactory solution. Without entering into details on the subject (which, of course, I cannot do here), nor citing any arguments against the doctrines that have been already propounded, or in support of the explanation which I think may be given of the formation of those so-called abscesses: I shall only say that I believe that the inflammation that gives rise to them is the result of a work of excretion, which, performed by organs whose vitality is impaired both by the general depression of the nervous powers and by the quality of the blood from which they derive their nourishment, is still greater than what they are accustomed to in healthy conditions. That inflammation—modified by the state of the system, of the blood, and of the tissues in which it takes place—goes through the same stages as an ordinary one. The exudation, poured out under those circumstances, deficient to the utmost in fibrine, becomes immediately transformed into pus; which cell-formation is further more or less abortive, as it can take place in perfection only under the influence of certain vital powers, and during peculiar conditions of the fluid in which it originates: conditions which, in this case, are wholly wanting or materially altered. These views are founded on the following grounds, which are merely heads of argument:

38. There are two other complaints that are found conjoined with disease in the liver: *chronic diarrhœa* and *chronic dysentery;* and about the nature of which we shall offer a few remarks, before proceeding to examine their relation to affection of this gland.

39. They are still generally reckoned as two distinct diseases. But if we consider the symptoms by which they are indicated; their effects on the digestive system in particular, or on the constitution in general; the fact that they pass into one another, and that they are reciprocally the results of the causes to which each is more commonly attributed; and the pathological appearances discoverable after death; it will be clear that they are one and the same malady.[1] They usually retain such names when they have been consequent upon preceding affections that presented either diarrhœic or dysenteric characters; but it is quite evident that no more should the one be called *chronic dysentery* because it has followed upon *dysentery*, than the other be denominated *chronic fever* when it has succeeded an attack of some *fever*. The intestinal lesions in both cases are nothing but symptoms during the disease which gave rise to them; and after this has disappeared, they constitute properly a disease *per se*. Whether in chronic diarrhœa or in chronic dysentery, those lesions are the same in nature, and are the results

1. That excreting organs eliminate all morbid matters existing in the blood; 2. That in health, *à fortiori* in the conditions here existing, inflammation results from their overaction; 3. That the secondary abscesses, or inflammations of a low grade, are principally found in excreting organs; that, as a general rule, those whose functions are most active in health are also most involved here; that in these, those parts where excretion goes on with greatest energy, as where the capillaries are most numerous, are most affected; 4. That the existence of purulent collections in what are generally reckoned secreting organs, as the serous membranes, prove nothing against this view; as I believe that all secreting may and do become excreting organs in many conditions of the blood—the difference between both having been besides but imperfectly settled by physiologists; 5. That when recovery takes place in those cases, it is preceded often by evacuations of matters (through some excreting organ or organs, such as liver, mucous membrane of intestines, skin, kidneys), altogether different from what is seen in health, and even in disease in general; and which are obviously critical evacuations.

[1] Annesley, Dis. of India, vol. ii. pp. 339, 341, 342.

of a chronic inflammation in all its grades, with the several consequences which it can produce secondarily, such as—thickening or thinning of the parietes, constriction or dilatation of the digestive canal; all sometimes met together in the same case. These changes, not even according to their situations, but according to some precedent or to some fanciful distinction, have been thought to constitute a difference between those forms of so-called diarrhœa and dysentery. For in both we find ulcerations, chronic inflammation and enlargement of the glands, of the mucous membrane, situated at the same time in the large and small intestines;[1] and unless a portion only of the latter, or of the lower extremity of the colon and rectum, be diseased, the dejections themselves cannot guide us in distinguishing between the parts involved.[2]

40. After those very few words on the nature and characters of the disorder, we must mention the theory which ascribes it to disease of the liver, especially to alteration of its secretion, whatever this may be, or to remora of the fæces or undigested food in some part of the canal.[3] Those theories are only corollaries to those others which attribute acute diarrhœa and acute dysentery to the same causes. But, to dismiss them in a few words, let us only say, with regard to the first, that affection of the liver may exist without the appearances enumerated above being present in the intestines;[4] and on the other hand, chronic diarrhœa and dysentery may run their course without any change taking place in the former.[5] As to the second, we oppose to it the same arguments which we brought forward against the opinion that the acute disease is due to the same cause.[6] And we need only add farther, that if the cells of the cœcum and colon have been found convenient for the lodgment, during a great length of time, of the irritating matters, there are no such cells in the small intestines; and yet exactly the

[1] Annesley, Dis. of India, vol. ii. pp. 341, 347 *et seq.* Abercrombie, op. cit. pp. 244, 245.

[2] Abercrombie, p. 246.

[3] Annesley, vol. ii. p. 342.

[4] Annesley, Dis. of India, vol. ii. *Cases* 207, 208. Abercrombie, *Cases* 142, 143, 149.

[5] Annesley, *ibid.*, *Cases* 204, 206. Abercrombie, *Cases* 105, 111, 114.

[6] See § 9.

same alterations are discoverable there as in the large. It is also argued, that when the secretion of the liver has been arrested for a long period, chronic diarrhœa may be produced; but it strikes me that there are no facts which can support this theory. *Cirrhosis* is mentioned as a case in which there is found enlargement of the glands of the small intestines; but mere enlargement of these glands does not seem sufficient to give rise to chronic diarrhœa,[1] for in cirrhosis itself, there is a greater tendency to constipation than otherwise.[2] We readily grant, and it is more than probable, that when the liver is involved from the beginning, or becomes so in the course of the disease, it may, directly or indirectly, increase or keep up the affection of the bowels; but we do not believe that whatever be the morbid action developed in it, it can cause it altogether.

41. It is comparatively of rare occurrence that those diseases exist primarily; they are mostly the consequences of more acute ones, such as dysentery, fevers, repeated attacks of diarrhœa, &c.[3] In fact, in the great majority of cases, they follow upon those conditions in which there is much reason to suspect that the liver has become also implicated. But, by whatever cause they may have been originally produced, and whether the liver had suffered or not at the beginning, we shall assume that, at some period or other of the disease, this latter viscus had resumed its healthy functions. The instances in which this last condition exists, are indeed comparatively few, as more usually the previous disorder of the hepatic organ, even though it may have diminished in intensity, does not disappear entirely. A consideration, nevertheless, of the symptoms in some cases, and the knowledge that, together with structural changes existing even to a large extent in the intestines, the gland has been found healthy; entitle us to hold, that *sometimes* its affection is consequent upon the latter. No doubt, that when it has been already seriously implicated, the state of debility in which it is left, even after apparently perfect recovery, makes it more liable to attacks of the same or other diseases, as obtains with any other organ in the body. We have mentioned very shortly of what nature is the lesion of the mucous

[1] Parkes, p. 4. Andral, Précis d'Anat. Pathol. vol. ii. pp. 29, 55.
[2] Budd, p. 148.
[3] Annesley, Dis. of India, vol. ii. p. 341.

coat. With such an alteration of this coat, digestion is necessarily very improperly performed ; and where extensive disorganization exists, it is scarcely performed at all. This is shown in many cases by the nature of the evacuations, which contain undigested particles of food sometimes in great abundance. With whatever quickness food so imperfectly changed may pass through the intestines—as must evidently occur in those instances in which several dejections take place daily—it must to some degree cause further irritation of the diseased parts of the membrane, and moreover, be partly absorbed by such portions of it as can still fulfil that duty. These absorbed matters, taken in when digestion has not sufficiently prepared them as in health, are carried on the one hand to the mesenteric glands, on the other to the liver. When the liver is perfectly sound, or even where it is but slightly impaired, it is probable that it will at first be scarcely influenced by such causes of irritation which, we believe, are neither large in amount, nor virulent in nature. But blood thus contaminated being constantly brought to it, *after some time* it will begin to resent those causes ; and its functions, which may have been altered more or less from the commencement, will become decidedly so. That the blood containing those irritating, or at least unhealthy matters, can have such an influence on the hepatic organ, is borne out by the analogous processes which, under similar circumstances and even in the same case, go on in the mesenteric glands.[1] These also undergo changes, that cannot but be referable to the absorbed matters they receive. And the analogy is even closer than at first sight may appear ; for whether by the absorbents or by the veins, absorption of the imperfectly digested food, together with constant removal of the effused products of the inflammation of the membrane, take place. These are then conducted to mesenteric glands and liver, and in both from blood containing such materials, by processes identical in nature if not exactly similar, a new substance must be formed: chyle on the one hand, bile on the other. If then, we have in the first case, of necessity, at first an unhealthy product formed from that blood, as no doubt chyle from such a source must be ; and gradually, though often after a *long*, and always after *some time*, a kind of irritation set up in those glands, running not unfrequently to inflammation : can we not reasonably infer that

[1] Annesley, Dis. of India, vol. ii. p. 350.

the same changes will take place—though perhaps less readily—in an other organ placed in the same conditions, and performing similar functions. It may be that in some instances, either from the very small amount of such absorbed matters, or from their being very nearly free from any irritating properties; or again, from some particular constitutional disposition of the liver, it resists all causes of morbid excitement, and gets, so to say, accustomed to them; as there is no doubt, that an organ may remain unimpaired under circumstances which are not generally met with in health. To that alteration of habit, to that sort of immunity from disease, we attribute the fact, that affection of the liver, at least functional, is not even more frequently met with in chronic lesions of the digestive mucous membrane. That, indeed, is found in the minority of cases;[1] for when, either from protraction of the malady or from an enfeebled state of the organ, it at last takes on a perverted action, it is probably due to its becoming less and less capable of resisting the constantly renewed irritation, and from a mere state of functional derangement—very likely existing in nearly every instance to a greater or less extent—falls into one of chronic or sub-acute inflammation, which at first minor in degree, may by length of time, or the supervention of contingent causes, assume a more acute character, and but very rarely indeed end in the formation of an abscess.

42. Such, we imagine, is the way in which liver disease becomes established in cases of primary chronic diarrhœa and dysentery; or where after having been affected functionally or otherwise, it had resumed its normal functions for some time. And in the same manner is explained how, in those other instances in which it had been from the beginning implicated, the continuance of the intestinal complaint after its cause had been removed produces increase of its affections.

43. The connexion between disease of the *liver* and that of the *duodenum*, to which we come now, is a double one; for it is probable that primary disorder of any of them may react on the other. In which of the two does it more often originate? is a question that lies still undetermined. The

[1] Annesley, vol. ii. p. 344.

opinions of those who have treated of this subject are much divided ; but yet it would seem that here again can be traced the modern tendency which would make disease to spring generally in the duodenum, and thence influence the liver. If we endeavour to study facts as they are presented by the symptoms which accompany their affections, we come to the conclusion that it is, more than once, difficult during life to state with certainty that that of one organ preceded the other ; and examination, when death has supervened, too often throws no light whatever on the question. There are evidences that disease was established in both ; we may even discover the link which connected the morbid processes together ; but still the question often remains : from which side did the link spring ? Of course such uncertainty does not always exist ; and its production in one case may be clearly understood, provided we obtain distinct appearances, as that in which *jaundice* supervenes on inflammation of the duodenum. In such a case as this, we know, on the one side, that jaundice does not of itself produce duodenitis ; whilst, on the other, it is certain that occlusion, partial or complete, of the mouth of the ductus communis choledochus can result from inflammation of the mucous coat on which it opens, that inflammation moreover involving generally to some slight extent the lining membrane of the ducts themselves ; and however precarious all other causes of jaundice may be, this is one of the best ascertained.[1]

44. It does not fall to our share to examine how *jaundice* is produced, but merely how its existence becomes established under certain circumstances. Some have asserted that it may also be due to occlusion of the common duct by inspissated mucus, and pretend that "many of the cases which have been attributed gratuitously to spasm, are probably of this nature."[2] We confess that we do not at all feel convinced of the truth of this theory ; for not only is there no proof that mucus, however inspissated, has occluded that duct ; but again that the inspissation of that mucus was such, as to prove a complete obstacle — for however short a time during which the jaundice may have lasted — to the dissolving properties of the bile pressing behind it. Besides this sort of mechanical production of jaundice—by the inflamed

[1] Andral, Cl. Méd., vol. iv. p. 20.

[2] Dr Burder, in Cycl. of Pract. Medicine, vol. iii. p. 5.

and swollen mucous membrane—there is another which Bichat ascribes to a certain sympathy existing between the liver and duodenum ; as he is of opinion that such a sympathy prevails between all glandular organs and the surfaces on which their ducts open ; and that irritation of the latter may, therefore, in some way or other, affect the former.[1] We shall not examine what he meant by the term *sympathy ;* but it is evident in this case, as in many others, that the nervous system becomes the link of connexion between both affections, as has been hinted by Andral ; and there can be little doubt that, as the latter thinks, it is far from unreasonable to attribute to such an agency some jaundices for which we can find no other satisfactory cause.[2] Anatomy teaches that as far as nervous connexions between the liver and duodenum are concerned, any irritation of the latter viscus may be transmitted directly to the former : for besides that numerous indirect communications exist between the nerves of the two organs, we have further the gastro-*duodenal plexus*, which is furnished directly by the *hepatic plexus*.[3] And principally by means of that distribution of their nerves, is to be explained the way in which any irritation, applied to the branches of the former plexus, would be conveyed to the latter, and reflected to the other organs supplied by it. Yet, however plausible such a theory may be, we must not be carried away by the idea that it forms at all an invariable rule ; nay, we had nearly said a *general* one. So many cases are on record where affections of the duodenum, of such a nature as must undoubtedly give rise to some degree of irritation of the nerves of this organ, are still unattended by jaundice ;[4] that nothing can yet be laid down as certain with regard to the production of this latter complaint under the preceding circumstances. After the subject has been more fully investigated, it is very possible that the exceptions, which are now so numerous, may either disappear on closer observation, or turn out to be the rule. The mode of action of the nervous system in checking or altering secretions is yet unknown ; although the existence of such an influence is so clearly es-

[1] Anatomie Génerale, vol. iv. pp. 614 *et seq.*. Stokes, in Cycl. of Pract. Med., vol. ii. p. 341.

[2] Cl. Méd., vol iv. p. 53 *et seq.*

[3] Quain's Anatomy (5th edit., edited by R. Quain and W. Sharpey,) p. 896.

[4] Stokes, in Cycl. of Pract. Med., vol. ii. p. 60.

tablished in many cases, that analogy fully warrants our believing that the secretion of the bile may also be altered in similar conditions. But that this same irritation, depending on nervous connexions, can go on to produce inflammation in the organ, may be deemed of rather difficult occurrence ; and although it is by no means impossible, especially if some contingent cause assists in the development of the morbid process, yet we believe that it must take place very rarely indeed.

45. *Hepatitis* is said by others to be often due to extension of inflammation from the duodenum to the liver by continuity of surface.[1] There may be some reasons to question whether this takes place so frequently as some authorities represent. When both organs are involved, we cannot always during life depend upon symptoms to point out which was affected first, as we possess so very few diagnostic signs to guide us in the separation of the one disease from the other ;[2] unless the derangement of either organ be so marked as to leave no doubt as to its seat and nature. Further, inflammation of the mucous membrane, above all when idiopathic as it is likely to be in the duodenum, is not so apt to extend as is imagined ; and of this continual proofs are furnished by the circumscribed patches of that process met with in the digestive canal. And even when it becomes in those tissues modified by its causes to such a point as to nearly take on specific characters, we still have that same circumscription of the morbid action. The length of surface which that inflammation, supposing it had a tendency to spread in such a manner, would have to run over before it could reach the liver, has been perhaps overlooked: the common duct is three inches, and the hepatic duct two inches long.[3] And it must surely involve not only all that extent, but again a certain portion of the mucous coat of the ducts within the liver, before it can cause affection of this gland to any remarkable degree. I cannot help thinking also, that the lining membrane of those ducts must be somewhat different, both in structure and function, from that of the duodenum, and therefore oppose in this way another obstacle to the spread of any perverted action originating in the latter ; for

[1] Stokes, in op. cit. vol. ii. p. 58. Andral, Cl. Méd. vol. iv. pp. 60, 61.

[2] Abercrombie, pp. 76, 97 *et seq.*

[3] Quain's Anatomy, pp. 1073, 1075.

though constantly exposed to the influence of the bile to such a point as to imbibe its colour, yet it does not suffer from the contact of that fluid when healthy. And last, though surely not least, such a propagation of the inflammatory process cannot be supported by what is commonly observed in examinations after death.[1] On these grounds, therefore, we feel inclined to conclude, that the inflammation which sometimes is present in both liver and duodenum, may, if originating in the latter, be set up in the former, *perhaps on rare occasions*, by irritation conveyed to it through nervous connexions, assisted by strong contingent causes; *sometimes* by continuous extension along the surface of the mucous membrane of the ducts; and *very likely most often* by the cause which has given rise to it in the duodenum having influenced the liver in a similar manner.

46. Those affections of the duodenum which assume, either from the beginning or after some time, an inflammatory form of a subacute or chronic kind, are probably the essential causes of that description of dyspepsia which has been named, from the supposed seat of the principal lesion, *duodenal dyspepsia*. We shall not stop to consider whether the duodenum is, in many such cases, affected without the stomach being so to a greater or less extent; and whether, therefore, such dyspepsia may not be as well, at least partly, attributed to lesion of this latter organ. But, granting that of the whole length of the alimentary canal, the duodenal portion be the only one diseased, we have attempted to show that it could produce disturbance of the biliary organ, either functionally or even structurally. There can be no doubt, however, that this order may be reversed;[2] and we shall consider here once for all, how primary disorder of the liver may, in its turn, act on the duodenum.

47. The liver, just like any other organ, is liable to diseases from which it suffers alone and primarily, or are the result of causes having previously acted on the system, or influencing it together with other parts of the frame. Yet in all those three cases, the duodenum may be at first perfectly free from disease, or not liable to be brought under

[1] Cullen, op. cit. vol. i. p. 505. Annesley, Dis. of India, vol. ii. p. 204. Stokes, vol. ii. p. 59. Andral, Cl. Méd. vol. iv. p. 229.

[2] Andral, Cl. Méd. vol. iv. pp. 68, 374.

the influence of those causes which acted on the hepatic organ. But on account of the irritation it is subjected to by the contact of altered bile, a morbid action in some instances becomes developed in it. Though it be not proved that in all affections of the liver, the bile is changed to any notable degree, nor, unless those affections involve a tolerable portion of the gland, is this necessary ;[1] yet in some cases of the former condition, and in nearly all of this latter, we can understand how the characters of the secretion of the diseased organ can be changed ; and acquire, among others, irritating and acrid properties, the existence of which actual experiment has demonstrated.[2] If bile, having such qualities, flows over the mucous surface of the duodenum, the natural result is, that *after some time*—shorter in proportion as the acridity of the biliary fluid is greater—a certain state of irritation is produced in those parts.[3] There is no tangible proof of this condition, or mode of action of the bile at the time ; but the feelings of the patient, the development of certain symptoms, together with our knowledge that such may be the case, can point out to us what is going on. That irritation, even if it continue for only a short length of time, necessarily disorders the digestive process at this spot, besides that the peculiar action of healthy bile on the ingesta is obviously stopped. And this is still described as *duodenal dyspepsia*, though originating primarily *not* in the duodenum. If the irritation lasts still longer, some degree of sub-acute inflammation may further supervene, which will be constantly kept up by the same cause that originally produced it. Nay, as chronic inflammation of mucous membranes in general, and particularly of that of the alimentary canal, are obstinate, the disease generated in the duodenum may give rise to as great derangement of the system as the original one — though the several effects of both cannot always be easily distinguished — and even survive the former, should this disappear altogether, or become less intense in degree. It may be objected, that if such irritation can be produced in the duodenum, it may also extend to the jejunum. We are not prepared to assert that it is not often so ; but we may say that, as in the duodenum it is due to a

[1] Andral, Cl. Méd. vol. iv. p. 130. Précis d'Anat. Pathol. vol. ii. pp. 611, 612.

[2] *Ibid.* Précis &c., vol. ii. p. 613.

[3] *Ibid.* Cl. Méd. vol. i. pp. 374, 406.

mechanical cause acting locally, the peculiar agency of that cause may be exhausted at that spot ; or, in other words, the amount and acridity of the bile may be sufficient only to exert its peculiar influence on that limited portion of mucous membrane with which it first comes into contact.

48. Thus it is that, on the one hand, the duodenum may come to affect the liver only functionally in most cases, and in some rare ones produce organic lesion, when the gland is strongly predisposed to it ; and on the other, the liver, in a like manner, may originate disease of the duodenum at first functional, but terminating, by continuous action of the exciting cause, in a certain degree of inflammation. But however closely related to each other they seem to be, when we take into consideration their nervous connexions, their nearness, the course which the bile must pursue, whether in a state of alteration or not ; yet it by no means follows, that as far as we have yet been able to ascertain, affection of the one should be *even generally* followed by that of the other ; for in numerous instances, a normal condition on the one side, is found co-existent with a morbid state on the other. Still there is reason to believe that when disease exists to some extent in one of those organs, it does cause functional disturbance in the other ; though such may not be apparent after death, and may, during life, be masked by the symptoms previously and more intensely produced by the primary affection. Herein, indeed, lies the difficulty of setting down for certain where disease is lying. For in some cases of sub-acute inflammation, or mere functional disorder, the local and general symptoms which it causes are so very much alike, that the diagnosis is frequently difficult, and sometimes impossible. Let us hope, however, that as affections of the duodenum have only of late seriously drawn the attention of the medical profession ; and as diseases of the liver and its functions are restricted within proper bounds, the accumulation of facts and observations will soon dispel the doubts and uncertainties which still hang over this important subject.

SECTION IV.

49. Our *third kind of connexion* is illustrated by those diseases which, seated primarily in the liver, cause subse-

quent disorder of the intestines. Such sequence was thought the most frequent by the older writers, who had an idea that bile, when altered, exerted a multifarious influence over the mucous membrane of the digestive canal; and that affections of the intestines were nothing but symptoms of hepatic diseases. But further inquiries have reduced such influence of the liver and its secretion within proper bounds; and have proved that bile, though greatly changed, and the liver, though extensively diseased, are not always productive of those extraordinary effects on the bowels which were formerly attributed to them.

50. Disease of the duodenum—whether inflammatory or not—when it follows upon that of the liver, belongs to this third group; and we understood that in its production, the influence of the altered bile, either from acridity not powerful enough, or insufficient quantity of the otherwise irritating fluid, is limited to that portion of the digestive mucous coat. Their diseases are then mild in degree, though their obstinate nature might reasonably lead one to presume that they are seated in organs, not simply altered in their functions, but also somewhat changed in their organization, as we often and most readily have proofs of in the duodenum. But in what has generally been called *bilious diarrhœa*, bile is supposed to be secreted in such abundance, and of such acrid quality, as to have an immediately irritating influence over an extensive portion of the mucous surface, principally and generally of the small intestines.[1] This irritation is naturally followed by unusual exhalation and secretion of mucus from that surface; and thus the evacuations which characterise the disease, are in great part produced. It is scarcely conceivable that these are composed altogether, or almost so, of bile, as some authorities will have it; an opinion supported neither by the great quantity, nor by the quality of the dejections, however so-called bilious they may be; nor again by the consideration, that if disorder of the liver be such as to give rise to an increase and alteration of its products, the intestines, directly or indirectly, must certainly be to some extent also affected, and thus furnish their share of the alvine evacuations. But we may be allowed to doubt whether the action of bile, however changed, on the

[1] Cullen, vol. ii. pp. 142, 143. Art. Diarrhœa in Cycl. of Pract. Med., vol i. p. 558. Broussais, op. cit. vol. ii. p. 117. Andral, Cl. Méd., vol. i. p. 406; vol. iv. p. 50.

mucous membrane, is the only way in which to account for augmented functional activity in this; for in all probability there are few, if any, causes which can produce an abundant and altered flow of the former, without at the same time having some effect on the intestinal mucous surface. Exposure to cold during warm seasons, is generally reckoned as one of the most frequent exciting causes of bilious diarrhœa; and we have seen how this same cause acts in the production of sporadic cholera.[1] This consideration has led us to believe, that many cases which are denominated bilious diarrhœa, come to take place in the same manner as, and are nothing but, minor instances of that former disease.[2] And the only reason why we have separated both —which, in by far the majority of instances, may safely be reckoned to differ only in intensity—is because it is far from unreasonable to think, though having few sure proofs of it, that the liver may be alone involved at first.[3] This can be due to several causes, among which may be mentioned an impaired vitality of the organ, rendering it more liable to be affected by, or sooner to resent, any cause which, applied either with more intensity or for a longer time, would also have acted directly on the intestines. Rare instances have been met with, when those purely functional disorders, generally of a momentary duration, have run on to inflammation; but I shall not stop to consider how this may occur, as it is more than probable that it is not unfrequently the result of bad treatment.

51. Only those cases of disease in which the secretion of the liver is more abundant, or more irritating, or both, than in health, and owing to those properties, morbidly influences the intestines; have, until now, drawn our attention. We shall next run over one important affection in which the opposite conditions react upon the intestines—necessarily in a different manner—and through them upon health in general, by disturbing digestion.

52. One of the requisites of this last-named process, when performed normally, and perhaps not one of the least important, is a healthy peristaltic action of the bowels.[4] Although

[1] See § 32.
[2] Cullen, vol. ii. p. 143.
[3] Andral, Precis &c. vol. ii. p. 207.
[4] Abercrombie, p. 69.

it is difficult to conceive exactly why it should be so, yet there is no doubt of the existence of the fact from what we see takes place when such a condition no longer obtains. This peristaltic action is promoted by two causes principally, the natural irritation produced by the food as it passes through the intestines, and that resulting from the contact of bile which, even in health, possesses some stimulating properties. The latter seems by far the more powerful agency, as proved by the precipitation of the vermicular movement, when it is in excess; or by the want of any such action, giving rise to constipation, when it is deficient. With this latter case we have to do.

53. From some cause or other the secretion of bile may be arrested; and when this continues for any long period, we think it is generally due—if there is no other lesion by which to account for it—to a certain amount of chronic inflammation existing in the liver. Bile may also be secreted of a less stimulating quality than in health; and although of this we have not many direct proofs, yet what has been seen in the gall-bladder in some cases after death, entitles us to assume that similar conditions of this fluid do sometimes exist during life.[1] Or thirdly, though of a proper stimulating quality, it may be greatly deficient in quantity, not however entirely absent. In those three cases, the most powerful stimulus to the action of the muscular coat of the intestines is partly or entirely withdrawn; and the consequence of such deficiency is constipation, which has, in general, a tendency to become more and more obstinate. By what chain of causes and effects constipation, when it has lasted for some time, comes to affect the whole digestive system, we do not intend to examine. Of itself, no doubt, it can thus influence digestion; still in the case under consideration, it must not be overlooked that there is also deficiency of bile, and that the dyspepsia which might have arisen from each singly, is here the result of both conjointly: the whole disorder originating, however, in the changes in the healthy qualities of that secretion. Here indeed there may be a source of fallacy, for to the liver may be attributed what is really not due to it. It is said that the pancreatic fluid has on the muscular coat of the intestines the same effect that bile possesses; but so obscure are the affec-

[1] Abercrombie, p. 359. Andral, vol. i. and iv. of Clinique Médicale.

tions of the pancreas, that it is of course difficult to make out whether a given case of inveterate constipation is owing to deficiency of the pancreatic or biliary fluids, unless this latter be so completely wanting as to produce the clay-coloured stools. However this may be, once constipation has become established, and especially when it proves obstinate, it tends to react upon the liver. It is to be presumed that in such a condition, the circulation of blood through the intestines is diminished, on account of its being less solicited to those parts than in health; and thus the quantity of that fluid returning through the liver being diminished, the secretion of bile is further proportionally deficient.

54. In all diseases of the liver in which the secretion of the organ is altered, it can scarcely be doubted that the process of digestion, or rather of chymification, is more or less disturbed; and this is readily understood when it is remembered that bile acts an important, though yet undetermined, part in that process. Many cases, however, where there is reason to believe that it is changed in quantity or quality,[1] are met with in which the digestive functions seem to be performed as usual; in which at least the patient suffers no actual inconvenience. These indeed must be exceptions which are seldom, if ever, found in the conditions that we have noticed. Whether organic disease of the duodenum gives rise to functional derangement of the liver; or whether functional or structural lesion of this organ causes at first functional, and afterwards structural affection of the former, or more general atony and torpor of the digestive canal; there always exists a certain amount of dyspepsia which is altogether inconsistent with health. The last case is, I believe, pretty readily distinguishable from the two former, between which diagnosis must still remain more obscure. Yet the importance of detecting to what organ *primarily* the general disorder is owing, must become evident when we endeavour to remedy it; for as the general features of the affections are the same, as the local signs may be very much alike, it is probable that we may often be led to treat symptoms which certainly will invariably recur, until the cause giving rise to them be removed.

[1] Abercrombie, p. 359.

SECTION V.

55. We have now examined the connexion between *some* diseases of the liver and of the intestines, and endeavoured to illustrate our opinion that this connexion is threefold, by considering a few under each head. The subject is necessarily far from being exhausted; and there are many other affections to which no allusion has been made; time, we confess, being wanting to run over the literature of those complaints, some of which have attracted the attention and been the subject of the writings of most eminent men in the medical world. Among those maladies in which both liver and intestines are evidently involved, are *Asiatic cholera* and *Yellow fever:* subjects far too extensive and difficult for us to presume entering upon, after the signal failures of so many who have endeavoured to handle them. These, however, we feel inclined to believe, would be ranked under our first group. Other diseases, affecting especially the liver, we have left unnoticed. They involve generally its structure, and also its functions, not unfrequently from the very beginning, sometimes only when they are far advanced, and more rarely death takes place without its being apparent that the secretion of the bile has been at all impaired. *Cancerous* or *tubercular*, matter; or *hydatids* may be more or less deposited in its texture;—*fatty degeneration*, or *hypertrophy of the cellular*, or of the *glandular*, structure of the organ may come to take place;—and, contrary to the views of the modern school, who hold that more often is its affection produced by one existing primarily in the bowels, than the reverse; it would seem but reasonable to think that so many different kinds of lesion must to some extent react upon the functions of the mucous membrane, especially of the small intestines. In such cases, it is true, the primary disorder sufficiently of itself impairs the constitution to such a point as to attract all our attention; but no less true is it that digestion may be, and is very likely as probably perverted by the direct action of altered bile on the alimentary canal as by the general impairment of the system. It is difficult indeed in such instances, as in any other of slight though inveterate dyspepsia, to separate the symptoms which are severally owing to the bad quality of the bile in its action upon the food, in its influence upon the intestines—their mucous or muscular coats,—and to the original involvement

of these latter tissues themselves. But it is evident that something must be owing to each of these three conditions, and that in some cases the two former are the first to become established; and that, had we sufficient means by which to guide us, we might sometimes trace all the evil, as far as regards the digestive system, to *the second.* These are truly mere speculations, however probable in their nature; and we shall now in a very few words endeavour to offer, with regard to TREATMENT, some general remarks founded on the distinctions we have drawn, and also on the pathological conditions present in those diseases; as the treatment of a disease, to be rational, must be founded on a knowledge of its pathology.

56. In the *first group,* it is clear that rational treatment must attack the source of evil, must remove the common cause which acts in a morbid manner on the liver and the intestines at once. Unfortunately it is not always possible to do so, very often on account of our ignorance of the nature of that cause; and should we be acquainted with it, of the means for opposing its progress or effecting its removal. In such a predicament, the next indication is evidently, if we know in what way the system endeavours to shake itself free from its influence, to help, by means often in our power, such a natural process. But we dare not do so in those cases in which the very organs which eliminate the morbid agent are, by their efforts, thrown into such a state of disease, as to call for all our attention; and, indeed, as to demand of us that we should shut up those natural channels through which the economy is getting rid of the poison affecting it. We have an evidence of this in *dysentery.* Modes of treatment almost innumerable have been put forward against it; some founded only on experience, and as variable as itself; others grounded in theories often false or incomplete, and, therefore, hurtful or insufficient. It is impossible to review all those different methods,[1] and we can only pass on to notice, not what we should do, but what we *can* do. As we have reckoned dysentery the result of a cause malarious in its nature, and tending to be thrown out by the intestines and liver principally; the first thought that strikes us is that we should try to assist such elimination by these organs, and obviously enough the

[1] See their enumeration in Copland's Dictionary, vol. i. pp. 727 *et seq.*

subject of *purgatives* occurs to the mind. But for whomsoever is acquainted with the pathology of dysentery, must not these words of Abercrombie sound full of meaning: *The use of purgatives in dysentery is a practice requiring the utmost discretion, being in some cases proper, in others unnecessary, "and in not a few injurious."*[1] To those who reckon it to be the consequence of impacted fæces, or some other such obstacle, purgatives are necessarily *the* remedy; but even though enforced by such an authority as that of Annesley, it remains to be questioned how far success was attendant upon a plan of treatment so obviously at variance with the general state of the intestines.[2] For, after all, the condition of these comes, in a practical point of view, to constitute the disease. As we cannot prevent that which we only reckon a symptom of the malady—inflammation of the mucous membrane—from developing itself, we must stop it in its onward progress, and prevent its fatal tendency. Though of a specific character, still it is an inflammation; and must be met, in its most marked forms, by antiphlogistic measures. Both general and local bleeding have been upheld by most writers; whether *general* blood-letting is always or ever imperatively demanded, is of course a question to be decided only by experience; but I feel inclined to doubt whether, as we possess means to draw blood (as it were) from the very parts affected, *local bleeding* is not by far the preferable method, repeated if necessary. I do not mean here, by *local bleeding*, leeches applied to the surface of the abdomen, as was formerly imagined; but leeches applied to the anus, to the hæmorrhoidal plexus of veins.[3] *Diaphoritics* are the next class of remedies that deserve to attract our attention; the almost invariably dry state of the skin is a sufficient reason for their exhibition. As an emunctory, the skin has, I feel convinced, been too much neglected. If we consider what an exhaling surface it presents, and how rich in capillaries, it seems to me striking that the kidneys should be the organs which, as if by common consent, are always acted upon. The skin affords all the advantages of the latter; and, as much as these, it is

[1] Op. cit. p. 275. Cheyne, in op. cit. pp. 41, 42. Baly, in op. cit. p. 535. The opinions of Stokes, quoted and approved of by Graves, in his *Clinical Lectures*, vol. i. p. 128, with regard to the practice of *purging* in fever, apply fully to the case of dysentery.

[2] See cases reported in Dis. of India, vol. ii. *passim*.

[3] Andral, Cl. Méd. vol. i. p. 77. J. Struthers, on Local Blood-letting, in the Monthly Journal of Medical Science, for April 1853.

one of the channels through which critical evacuations make their appearance. It is needless to recur to the antagonism between it and the mucous membrane of the bowels; but this is obviously one other reason why I should endeavour to act upon it. The third indication is the exhibition of emollient and sedative *enemata*. It is still a question whether these act merely upon the very part to which they are applied; as they are known to produce the greatest relief even when the seat of disease is far beyond their reach. If *purgatives* be absolutely necessary, I would venture only on the very mildest, and would certainly always combine them with *opium*, as this remedy has been found so beneficial by Dr Cheyne.[1] Men of the greatest experience have expressed their opinion of the impropriety of making use of strong astringents in the acute stage of dysentery; and such a practice is sufficiently opposed to all sound theory to deserve no further notice. The preceding are the principal indications which I would feel inclined to follow in the treatment of a case of dysentery,—modified, of course, as contingent circumstances develope themselves during the progress of the disease. Of the treatment of the bowel and liver complaints that complicate *fevers*, we need say nothing. As a general rule, so secondary are those symptoms in importance, that they fall more under the province of treatment of fevers in general: a treatment which, though it has of late so much assumed the expectant form, is still in that form far more rational than those others that were directed to nearly every organ of the body, in the thought that each was severally the cause of the disease. Since Sydenham, the treatment of *sporadic cholera* has undergone no material improvement, as it appears to require none. We would only again enforce here, as in the case of dysentery, the propriety of acting on the skin. To the arrest of its functions we have attributed the disease; to the restoration of those functions, at the same time that we mitigate the other symptoms, we would look for the removal of the mischief.

57. Passing over the subject of secondary abscess, which does not properly belong to us, we come to *chronic diarrhœa* and *chronic dysentery*. What can be said of the treatment of that disease? It has too often proved the despair of the physician; and when extensive or very chronic, we may well

[1] In Dublin Hosp. Rep. vol. iii. p. 44.

nigh be deemed powerless to restore the healthy state of the mucous membrane, or cure the ulcerations with which it is covered. And why? This is Andral's answer, and we may be allowed to quote it at full length: *Those ulcers shew such a great variety in their nature, that it would seem one same method of treatment could not apply to them all. The white, grey, or brown colour of their surface—the nature of the secretion which is thrown out by it—the inconsiderable or great thickening of the cellular tissue which forms their base—the appearance and conformation of their edges—the different degrees of consistence, thickness, and colour of the mucous coat which forms those same edges—the "décollement" of the membrane over a greater or less extent—its condition in the intervals between the ulcers—are so many circumstances which seem to require a variety of modifications in the treatment. . . . . It would often be necessary, over one portion of intestine, to apply astringent or tonic substances to the ulcers, and cover with emollient drugs the intervals which separate them; and vice versa. It is thus that the surgeon acts in the treatment of several ulcers situated at the surface of the body. He cures them by endeavouring to keep up the inflammation at a certain degree, above or below which it could not proceed to resolution.*[1] If to this we add disease which has already, perhaps, supervened in the liver, mesenteric glands, or some other of the abdominal viscera, and the general and often not inconsiderable impairment of the constitution; we shall have a true picture of the difficulties that must be encountered in the treatment of those affections—difficulties which can so seldom be completely overcome, that Watson says, *Chronic dysentery usually, in the end, is fatal.*[2] But however small is the chance of success, yet perseverance has in those very cases been productive of the greatest benefits; and the following are the indications that might be followed: Counteract the indolent tendency of the intestinal ulcerations; give tone to the mucous membrane in general, and support the system against the exhausting discharges and irritation of the disease. *Astringents* and *tonics* are the remedies

[1] Translated from his Clinique Médicale, vol. i. pp. 430, 431.

[2] Op. cit. vol. ii. p. 497.

which will meet these; but a wise discrimination in the selection of those appropriate to the case, as well as the greatest attention to diet and regimen, are required to ensure success in relieving, if not in curing.

58. Those diseases in which both liver and duodenum are affected by chronic inflammation, are nearly as difficult to overcome as the one we have just considered. The functional derangement of those organs may subside of themselves, or be arrested by some attention to diet; but when it has degenerated into a chronic inflammation that requires more active measures, I fear we are then possessed of few certain means by which to oppose the disease. In whatever organ it has originated, it is clear that we know of no remedy which, we can positively say, does act on either of them directly; and even in cases where affection of the duodenum is running into inflammation from constant irritation by altered bile, it is doubtful whether any remedy has yet been irrefutably proved—*mercury not excepted*—to restore the healthy characters of the hepatic secretion. All the means having any beneficial effect, seem to promote this indirectly by acting on the digestive system; and this, therefore, demands all our care and watchfulness. When inflammation of the duodenum is of a somewhat acute character, the greatest attention to diet is obviously the main indication of treatment: the blandest nourishment, especially such as is mostly digested and absorbed in the stomach, will, no doubt, be found advantageous; and may we not even, in some cases, procure complete rest to the affected organ, by substituting nutritive enemata for food taken by the mouth? The conformation and minute structure of the duodenum, as well as the symptoms which manifest themselves in certain cases of dyspepsia, show plainly that in it an important part of the process of digestion is performed. And if rest to an inflamed organ be the first indication to be followed out; if allowing that organ to perform its usual functions, nay, if causing directly its irritation be productive of disturbance in another organ, which, by its then altered secretion, may react on the former, and cause an increase of irritation in it—as in the case of the duodenum and liver—it becomes, I think, strongly advisable to employ such means (however uninviting at first sight), as will procure to the originally affected viscus the rest which it so needs, and avoid those circumstances that could not but tend to increase its diseased condition. I need not dwell

any longer upon the treatment of those affections, as such as we are best acquainted with is found detailed in all the works on the subject. There is no doubt, however, that the different modes of treatment which have been proposed, are all more or less uncertain, and that when even successful, they are nothing but palliative; merely preventing the occurrence of, or removing those circumstances that might have caused an exacerbation of the disease; and allowing the system to work its cure, by placing it in as favourable a condition as possible for effecting that purpose. This may be deemed a summary of our treatment of dyspepsia depending upon affections of the liver and intestines. In some instances, such as the constipation resulting from want of proper stimulating characters in the bile, or its partial or total absence, we may be more active; and most probably the *purgative* plan of treatment, when not pushed too far, may, by stimulating both liver and intestines to action, restore in them their lost power, at least for a time. But when this depends upon extensive organic diseases of the hepatic organ, how obviously precarious must all means be, and how too often fruitless all our efforts. Palliation becomes necessarily the only resource: a far better mode of treatment, and far more consistent with reason than those absurd measures—such as the exhibition of calomel—which formerly entailed on the patient useless discomfort and suffering; and rather tended, by depressing the powers of his system, to hasten onwards the progress of the local malady.

24 Rutland Street,
15*th March* 1855.