

**Observations on the simple dysentery, and its combinations : containing a review of the most celebrated authors who have written on this subject, and also an investigation into the source of contagion in that, and some other diseases / By William Harty.**

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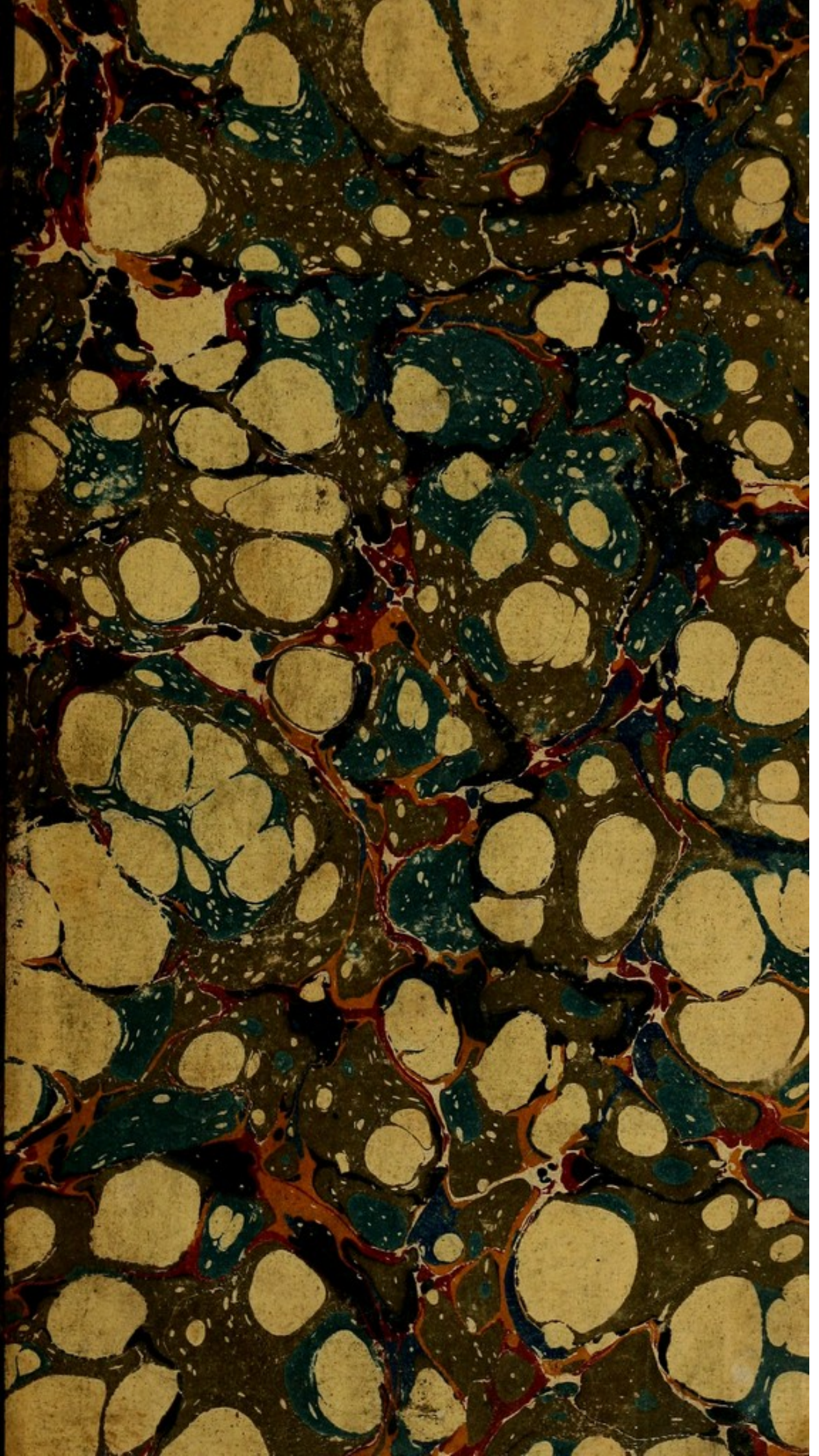
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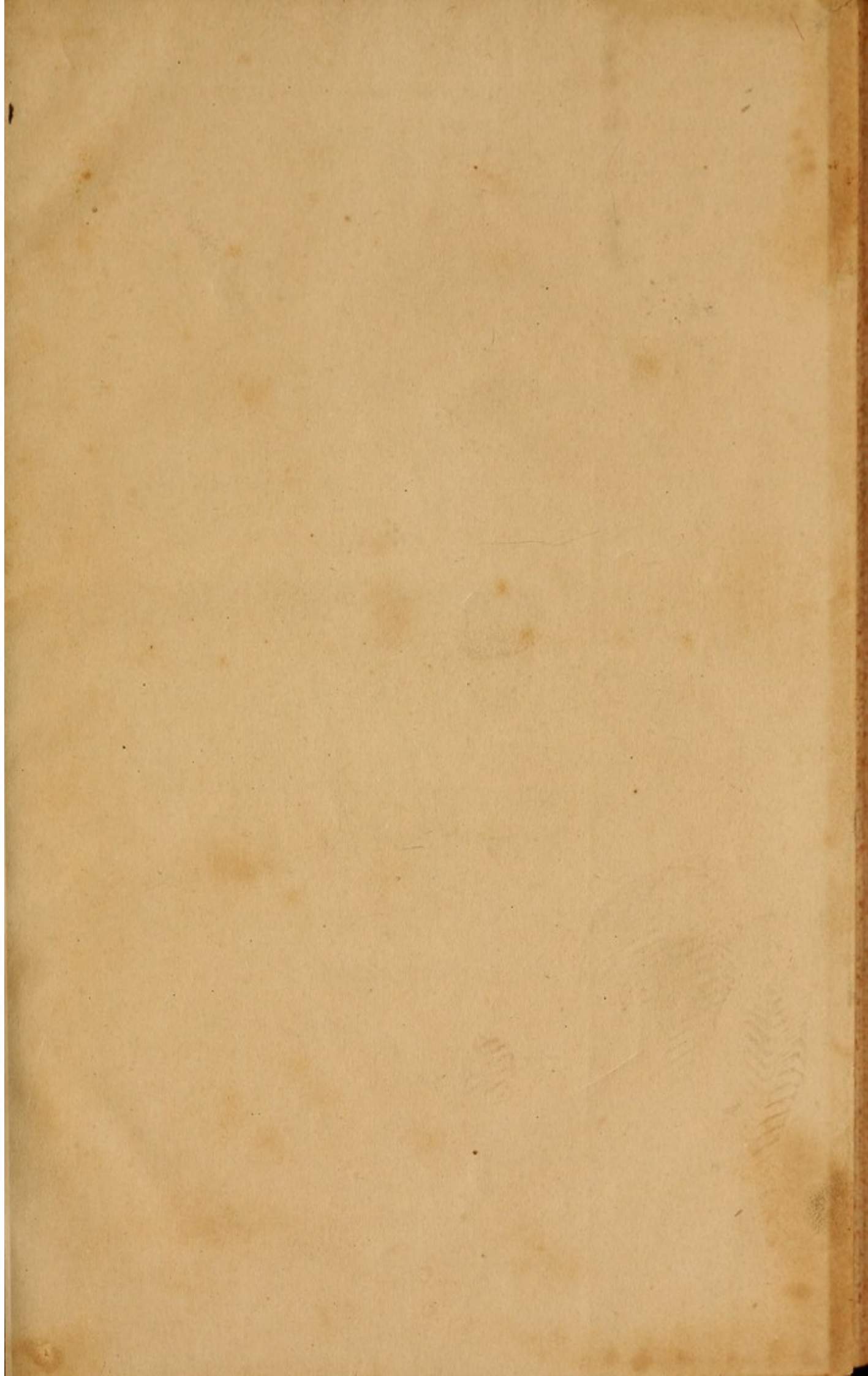




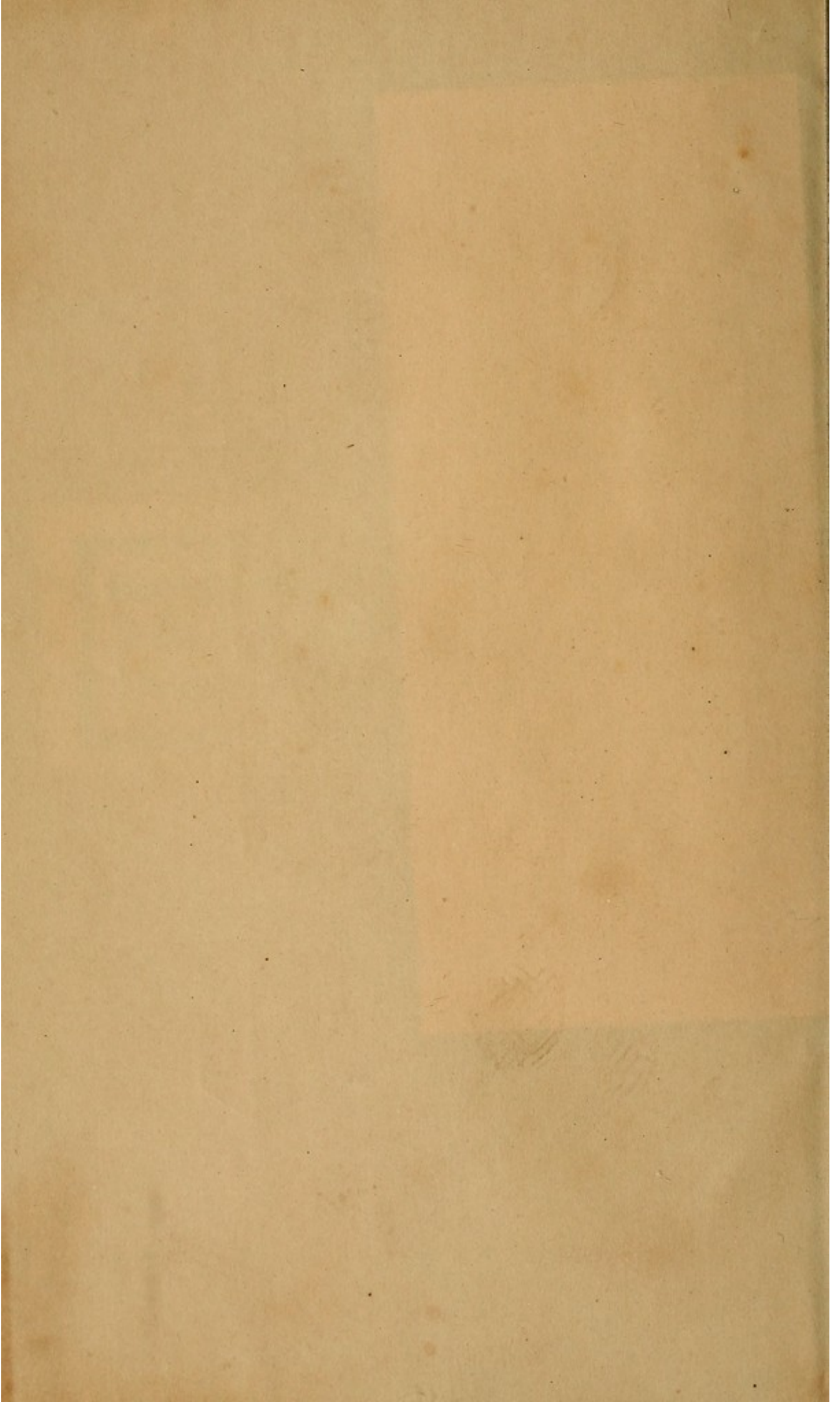


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OBSERVATIONS

ON

SIMPLE DYSENTERY,

AND

ITS COMBINATIONS,

CONTAINING A REVIEW OF THE MOST CELEBRATED  
AUTHORS WHO HAVE WRITTEN ON THIS SUBJECT.

AND ALSO

OF THE NATURE AND CAUSES

OF THE

SOURCE OF CONTAGION IN THAT

AND

SOME OTHER DISEASES.

BY

WILLIAM HARTY, M.D.

OF THE UNIVERSITY OF CAMBRIDGE.

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AUTHORS WHO HAVE WRITTEN ON THIS SUBJECT,

---

Ὁ βίος βραχύς, ἢ δὲ τέχνη μακρὴ, ὁ δὲ καιρὸς οὖτος, ἢ δὲ  
πειρὰ σφαλῆρη, ἢ δὲ κρίσις χαλεπή.

*Hippocr. Aphorism.*

Multum egerunt, qui ante nos fuerunt, sed non perege-  
runt; multum adhuc restat operis, multumque restabit,  
nec ulli nato post mille sæcula præcludeter occasio  
aliquid adhuc adjiciendi

*L. An. Seneca, Epist. lxxiv.*

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PRELIMINARY OBSERVATIONS.

---

THE great and ultimate object, proposed in the publication of this Essay, is, to call the minds of practitioners to a *Species of Contagion*, which, though occasionally noticed, has not as yet engaged that share of their attention it so well merits. This species, the Author, for want of a better name, has denominated Compound Contagion, to distinguish it from that, which propagates one single disease; the most striking and essential property of this contagion is, the power of communicating to one and the same individual, (under circumstances, that in each case shall be specified,) two distinct, and otherwise independent diseases, which in him shall appear conjoined, as one single disease,



but which, when separate, are found uniformly to differ in two respects: the one consisting in a deranged state of the *whole* system; the other in a *local* affection; the former being always contagious; the latter never.

The more immediate object of this essay is, to demonstrate the action of this contagion in the production of a most dangerous species of disease, consisting in the combination of two diseases, one of which is directly, the other indirectly, debilitating.— This it is designed more particularly to illustrate in the case of Dysentery.

Into the general enquiry itself, the Author was imperceptibly led, in consequence of a great multitude of facts having conspired to convince him of its existence in that disease, which constitutes the chief subject of this treatise: to which, some years since, he had been induced, from particular considerations, to dedicate no small portion of his  
time,



time, and to which, and its analogous diseases, he has not, since that period, remitted in his attentions.

The state of public opinion relative to the Dysentery cannot be unknown to any person, who has perused a single author on the subject: we find little harmony of sentiment among them on points of the first moment: some insist that Pyrexia is, and must be, always present: others too indiscriminately deny it; some will have the operation of *cold*, as the sole exciting cause: others in an unqualified a manner, confine its production to the agency of *contagion*; some depend on purgatives, some on sudorifics: some rely on mercurials, and others again on the bark.—Such, I believe, will be found the state of public opinion respecting this disease; so much indeed do authors differ from each other, that we may remark of many of them, as Pringle does of Sydenham, and of Willis, that in the descriptions given by them of Dysentery, they scarcely agree in any one



article, except the name of the disease, they are describing.

The Author, after a careful perusal of the different writers who have treated of this subject, after an attentive comparison of, and mature reflection on their opinions, thinks that they have furnished him with good grounds for concluding, that there is truly *one species* only of the disease, different however from that which Cullen supposed it to be; but that there are several very important combinations of this species, which have frequently been mistaken for it, and to all of which the name of Dysentery has been indiscriminately applied. The Author believes he can establish the following positions:

1st. That, *the genuine and Simple Dysentery is unattended by idiopathic fever, and is never of itself contagious.*

2d. That, *every other form of the disease when epidemic, is a combination of the Sim-*

*ple*



*ple Dysentery either with intermittent, remittent, or Typhus Fever; and*

*3d. That the combination with Typhus Fever alone, is contagious.*

There is yet another part of this essay, to which it may not be improper here to advert. The Author, conscious of the objections which might naturally be started against the *supposed* novelty of his opinions respecting Dysentery, wished to ascertain, if possible, whether that disease stood singular in the source, whence it derived its contagious property. For this purpose he extended his enquiries into that class of diseases, denominated contagious: and after some research, he believes he has obtained grounds more than sufficient to authorise a conjecture, that several diseases, well-known by *name*, bear a striking analogy to Dysentery in that respect, and that there were perhaps many more, which might occasionally acquire a contagious character, in consequence of the same combination. The grounds for these



conjectures (he wishes he could use a stronger phrase) the Author will subjoin, principally indeed, for the purpose of attracting the attention of those to the subject, who have had more extensive opportunities for observation, and greater intimacy with the diseases alluded to; and as one of his motives in publishing this essay, was a desire to receive, as well as impart information, he would address himself to such persons in the words of the Poet:

---

“ Si quid novisti rectius istis,  
Candidus imperti; si non, his utere mecum.”\*

In intruding on the Public, the best excuse which a medical author thinks he can offer them, is, that he brings forth the fruits of long observation and personal experience: to this excuse, in respect to any of the diseases alluded to, the Author is sorry he cannot take refuge: with the Dysentery an

\* The foregoing observations appeared in a Prospectus of this work, delivered previous to publication.



*intimacy* is only to be acquired by personal service in our fleets and armies, or by a long residence in those climates, whence the disease is seldom absent: in the former he never served, and the latter he never visited. But as there yet remains another source of information, and of *experience*, which however, inadequate it may be deemed, as a substitute for personal observation, is undoubtedly one of its best auxiliaries, the author relies on the indulgence of his readers for a patient hearing, and does not despair of pardon and forgiveness, should he succeed in making good his claim to an *acquaintance* with the *experience* of others. To a neglect or contempt of this mode of acquiring knowledge, we may easily trace many of those shameful differences, too often observable among medical writers, several of whom do not hesitate to deduce the most unqualified conclusions from the small store of their own observations, and to suspect the veracity or accuracy of such, as chance to disagree with them; the first and favourite maxim of the father of observation



observation in medicine, might have taught such persons to place less reliance on their own experience, and more confidence in that of others. If conduct such as this be requisite on most occasions, it is more particularly so in respect to disease, which from its very genius is constantly varying the phenomena it presents to the eye of an attentive observer. Such diseases as prevail Epidemically, are limited to no climate, and consequently appear under every possible variety of circumstances, possess in a high degree that peculiarity of character just mentioned; of these diseases we can never acquire an accurate or extensive knowledge by personal observation *alone*: we must be assisted by the experience of others, or we shall imbibe very partial and narrow ideas of the truth, followed by the most prejudiced notions in favour of our own tenets, and a consequent contempt for the opinions of others. The propriety of these remarks might be justified by a reference to diseases, not few in number,



ber, and will hereafter appear fully illustrated in the History of Dysentery.

This is a disease, which has both claimed and engaged the attention of practitioners in all ages, and still continues to demand their most serious consideration: much as it has employed the pen of the author, yet is little argument required to prove, that its true nature is not generally understood, and that much still remains to be known, before we ought to rest satisfied with that information we possess respecting it. The many points of essential difference so strikingly perceptible in the best histories of the disease, the disputes which still exist relative to its supposed contagious character, and the great diversity of treatment recommended by the most respectable writers, are more than sufficient to justify the assertion that has been made. On the presumption of its truth, the Author of this essay has not hesitated in adding one name more to the long catalogue of writers, whose pens have been employed on  
this



this interesting subject, and has now ventured to submit to the public the propositions already advanced, as the fruits of much labour and unwearied research.

It forms no part of his intention to offer any regular or detailed history of the disease, but to limit his observations to the establishment of the distinctions proposed, and indeed principally to direct his attention to one combination of importance far superior to the rest, while at the same time he is particular in pointing out the simple and genuine disease, in distinguishing it from the different combinations of which it is susceptible, and in assigning to each its distinctive and peculiar characters.

The general tenour of this investigation is in most respects novel, and as important as it is interesting, and though that view of the subject which has been adopted, had not escaped the observation of a judicious writer,



writer,\* yet has it never hitherto been followed up with that zeal its great importance would seem to demand. The Author feels that he has left part of it imperfect, but it was impossible in so extensive a field to divide his attention equally among all its objects: another day may, he hopes, see it all more fair to look on. Still, however, at the present moment the application of its doctrines must be admitted to possess much interest in point of theory, and the utmost utility in directing our practice: they teach us when to lay aside our fears, when to guard ourselves with the shield of precaution, and on all occasions to embrace, without hesitation, the most vigorous and expedient measures. The admission of their truth was effected on his part in a very gradual, almost imperceptible manner; for he commenced these researches with a mind strongly impressed by the authority of a celebrated nosologist, who had

\* *Vid.* Willan's Reports on the Diseases of London, p. vii.

declared



declared of the Dysentery, “ Non nisi uni-  
“ cam speciem novi,” and in the same  
breath had pronounced that species to be a  
“ Pyrexia,” and that Pyrexia contagious,  
nor did he get rid of this impression  
till he found it impossible to reconcile that  
declaration with the subsequent definition,  
the accounts of various authors being incon-  
sistent with the first article of that definition,  
and their assertions contradictory of the se-  
cond. As this has been his own case, the  
Author cannot expect to flash sudden con-  
viction on the mind of his reader, he only  
requests of him, as far as may be, to enter  
on the investigation unbiassed by the dog-  
mas of any school, “ et nullius addictus  
“ jurare in verba magistri.”

There is a deficiency in this work, for  
which the Author must request the indul-  
gence of its readers: he means the *occasional*  
want of reference to the page in making quo-  
tations: some he has lost, and others he  
neglected taking, as in collecting the facts  
originally,



originally, he had no intention of laying them before the public; from the number of the works referred to, and the difficulty of procuring some of them, the Author is sorry it is not now in his power fully to remedy this defect: he has, however, taken care to do so where any passage was of so much importance that some would be inclined to doubt its accuracy without an exact reference; from the nature and manner of the quotations, and the general notoriety of the works themselves, this omission will not be found of so much consequence as might at first be imagined. To save repetition, he has subjoined an index of the authors consulted, and the editions referred to in this Treatise.

The advice of his friends has induced the Author, contrary to his original intention, to affix his name to this publication, not that in suppressing it he had any particular reasons for wishing concealment, or that he would thus think of screening himself from  
any



any censure the work itself might merit : his object in so doing originated partly in the desire of hearing the candid sentiments of those around him, but principally in the certainty that his name could give it no circulation in Great Britain, and in the possibility of its disserving the work elsewhere, “ for a prophet may be honoured, save in his own country.”



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ERRATA.

PAGE	LINE	
19	20	After <i>doluère</i> insert <i>que</i>
63	4	For <i>protraction</i> read <i>prostration</i>
87	26	For <i>incidentibus</i> read <i>incedentibus</i>
113	5	For <i>pestilentia</i> read <i>pestilentialis</i>
122	9	For the , substitute a ;
125	13	After <i>tom.</i> insert <i>III.</i>
126	17	For <i>Hildams</i> read <i>Hildanus</i>
136	4	After conjunction insert <i>with</i>
154	26	Dele the second " after appearance
184	24	For <i>tenemus</i> read <i>tenesmus</i>
184	25	For <i>laxitives</i> read <i>laxatives</i>
264	1	For <i>affimative</i> read <i>affirmative</i>



# OBSERVATIONS

ON

## SIMPLE DYSENTERY, &c.

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### CHAPTER I.

#### SIMPLE DYSENTERY.

**SYDENHAM**, who for three years only, had seen Dysentery epidemic, yet perceived in the appearances it presented to his observant eye, a variety more than sufficient to induce him to conjecture, that there might possibly be as many different species of this disease as of the small-pox. The truth of this conjecture it shall be the business of the subsequent pages to establish, without meaning, however, to assert, that such forms of the disease as shall be pointed out, are to be considered as so many species, in the technical sense in which this term is usually employed, but that its varieties, more properly speaking, are as essentially, and often more widely different, than the two very

B

dissimilar



dissimilar species of small-pox. In opposition to this conjecture of Sydenham's, we find Pringle, Clarke, and Mosely, agree in declaring the disease to be every where essentially the same, while in their histories, and other opinions respecting it, they differ very materially indeed from each other. The latter observes, that the multiplied distinctions made by authors, are only applicable to the various appearances of the same disease, as influenced by climate, season, constitution; to different stages and degrees of it, and to such cases where some other disorder or epidemic was united with it. This observation is undoubtedly justified as to fact; but are all the distinctions proposed therefore unnecessary? I trust it will not so appear, when we come hereafter to consider what those cases are, "where some other disorder or epidemic is united with the disease," how different in their history and danger, and how dissimilar in their treatment: and if such shall be the fact, how important it is that some distinction should be made between the simple genuine disease and its combinations, and how necessary it must be to assign to each its distinctive name, need not be told; indeed such



such regulation must appear indispensable, when we reflect that there is but one species of this disease, possessed of peculiar and characteristic marks, to which the name of Dysentery in right belongs, to which alone it should be given, and not, as it has been, indiscriminately to such cases where "some other disorder or epidemic was united with it." This is the case at present, and this will be felt by the reader to be the great source of confusion in the perusal of works on that subject: and that this observation may not lose its weight, it is worthy of remark, that these authors themselves, Pringle, Clarke, and Moseley, as well as many others, appear alike ignorant, that the disease they described in common under the name of Dysentery, was not a simple, but compound disease.

That we may proceed with some regularity to establish the distinctions already proposed in the preliminary observations, it will be requisite previously to point out that unica species, to which alone the name of Dysentery ought to be assigned, and which I have declared to be attended neither with idiopathic fever nor contagion: this species, without impropriety, may be denominated the



Simple Dysentery, to distinguish it from the different combinations of which it is susceptible. For this purpose it will not be amiss to take a short view of that definition of the disease most commonly given, and very generally received. How important the proper definition of terms, more particularly of such as we are in the daily habit of using, is to our reasoning rightly, need not be insisted on: it is well known how much our conduct may be influenced, as well as our sentiments perverted, by the employment of words to which we have neither distinct nor determinate ideas annexed. This reflection may be found to apply with some force to the disease in question, as its name, though in constant use since the days of Hippocrates, has scarcely had its precise value as yet affixed to it. In contrasting the definitions given by systematic writers, with the notions entertained of the disease by original authors, we may observe on the one hand too much restriction, and on the other too much latitude attached to their ideas of Dysentery; the former sometimes excluding the genuine disease from its appropriate name, while the latter permit other diseases to usurp a title to which they can claim no right, though



though they may urge some affinity. Cullen's definition may be brought to illustrate the first remark, while various authorities might be adduced to justify the second. His definition runs thus:—"Pyrexia contagiosa: dejectiones frequentes, mucosæ, vel sanguinolentæ, retentis plerumque fæcibus alvinis: tormina, tenesmus." It commences by stating the disease to be attended with Pyrexia, and that Pyrexia to be contagious. Now, to these words, as part of a definition of the simple and genuine Dysentery, I decidedly object; for frequently, as shall shortly be proved, from original observations of the disease, it is unattended by any Pyrexia, and in many cases where it is attended by Pyrexia the most obvious, it is not then necessarily contagious; hence these words should be excluded from any place in the definition\*; the remaining part  
 very

\* Cullen, in his definition of Catarrh, the analogy between which and Dysentery we shall hereafter have occasion to notice, is more guarded, and merely says of it, "Pyrexia sæpe contagiosa." Had he confined himself to similar language in speaking of Dysentery, his definition, though not correct, still would have been much nearer the truth. We do not deny a certain state of disease to be Catarrh, because we do not find Pyrexia



very accurately characterises the disease, which is known by gripings of a peculiar sensation, by the frequency and peculiarity of the stools; for these, when the disease is fully formed, never evacuate the natural contents of the intestines, but consist in a very scanty discharge of mucus, serum, and blood, and are accompanied by a most distressing tenesmus\*. These marks sufficiently distinguish the disease from all others, and very satisfactorily from Diarrhœa and Cholera, with both of which we shall find it not unfrequently confounded, and on some occasions the very same name given to it and Cholera, while their histories at the instant plainly mark them as very distinct diseases. A remarkable instance of this nature is afforded by Willis, in his account of the Dysenteria incruenta; for this, from his own descrip-

present, or because we cannot trace its origin to contagion. There are no good grounds why we should reason in a different manner respecting the Dysentery.

\* Hoffman thus elegantly describes a Dysenteric Paroxysm: "Cunctas verò dejectiones, quæ sunt numerosissimæ, et antecedunt, et comitantur, & sequuntur, crudelissima tormenta, non in uno loco fixa, sed per totum intestinorum canalem vago itinere progredientia, et sub quâvis sede sentitur molestissimus omnium quasi viscerum descensus."

tion,



tion, as we shall hereafter see, appears to be a most accurate statement of the history and treatment of Cholera. Similar instances are furnished by other not less judicious writers, of whom notice shall be taken in their proper place. It will be sufficient here to remark, that the want of copious, liquid stools, the severity of the tormina, and of the tenesmus, together with the duration and other general circumstances of the disease, will serve fully to distinguish it from any Diarrhœa; while the copious stools, and continued vomiting, too strongly mark Cholera, to admit of any mistake in the attentive observer; any error respecting this disease must soon meet with correction, from the rapidity with which it hastens to a fatal termination, unless checked by a mode of treatment, the opposite of that which is calculated to insure success in the Dysentery\*.

The

\* Hoffman, tom. iii. cap. 8. De Cholera & Diarrhœa biliosâ, thus accurately distinguishes the two diseases:—

“ Illa, enim, i. e. Cholera, ad acutissimos referri meretur  
 “ morbos, & intra paucos dies, ad summum septimo finiri  
 “ solet: cùm Dysenteria, nisi fuerit summè maligna, diu-  
 “ tius durare deprehendatur. Neque subinde Dysente-  
 “ rico fluori semper conjunguntur vomitiones, nisi forsân  
 “ in principio, aut vigente mali impetu, si accesserit ven-



The Dysentery, like many other diseases, may prevail sporadically or epidemically: and in either case, more especially the latter, it may assume every variety of form of which it is susceptible. Sporadic cases may occur at all seasons of the year indiscriminately, because they arise from causes acting partially, or on particular individuals, which may be applied at any time, and in general are such as never give rise to the epidemic disease: this latter is more usually limited in the period of its prevalence, because it very generally originates in a certain constitution of the air, dependent on, or influenced by particular seasons: the former, or sporadic, generally answer to that description given of simple Dysentery, being usually devoid of fever, or of any suspicion of contagion. The epidemics, on the contrary, will very generally be found to consist of this simple disease, united with fever of one type or other, in some cases contagious, in many others, not: at the same time it will appear, that in most

“tricoli inflammatio, illæ nonnunquam occurrunt: At  
 “Cholera numquam sine vomitu est, sed nec tam molesto,  
 “uti Dysenteria, tenesmo, stipatur, nec tam frequentes  
 “cruoris dejectiones, exhibet, nec denique contagium,  
 “uti Dysenteria, spargit.”

epidemics,



epidemics, many cases of the disease occur, without being preceded, or accompanied by feverish symptoms; and indeed there is scarcely an epidemic, of what form soever, in which instances of the Simple Dysentery are not to be met with. Every one, who has seen sporadic cases, will readily admit the truth of these assertions in that instance: the subsequent passages will justify them in respect to epidemics, at least so far as concerns the Pyrexia; on contagion I shall say nothing at present, reserving the full consideration of that subject for its proper time and place.

Sydenham, whose accuracy few will call in question, states, respecting his epidemics, that in the majority of cases, the gripes attack first, and stools succeed, without being preceded by any fever. Pringle, however, doubts this, and suspects, that though the patients may not have complained, that they had yet more or less of a febrile affection to precede the disease; though he afterwards himself confesses, that at times the men were suddenly seized with the flux, upon fatigue and exposition to cold, during the Dysenteric season. As we advance in this treatise, we shall be enabled to assign satisfactory



tory reasons for this difference in sentiment, inasmuch as Pringle mostly met the disease in a different form from Sydenham, except in these cases of sudden seizure, to which the former alludes, and respecting which they mutually coincide.

Zimmermann (p. 144.) remarks thus:—  
 “These species of Dysentery (meaning  
 “the malignant, benignant, &c.) partly on  
 “account of their frequent complication,  
 “and partly by reason of their changeable  
 “and deceitful nature, are very often not  
 “nicely enough distinguished in practice:  
 “when the Dysentery rages very much, and  
 “kills a great many, there is still always a  
 “number of slight and easily curable cases;  
 “and in malignant dysenteric epidemics,  
 “there are likewise a great many fluxes in  
 “the same place, that are not malignant.”  
 —He does not here expressly say, that in such cases there was no Pyrexia; yet may it readily be inferred from other passages, and the general tenor of his sentiments.

These authors, though they met the disease most generally accompanied by fever of one description or other, yet sufficiently testify that it is neither necessarily preceded, or accompanied by it; and thereby warrant



us in rejecting Pyrexia from the definition : this decision will be farther justified by the direct testimony of other authors. Aken-side and Stoll furnish us with abundant evidence to that effect : these in general met the disease in its simpler forms, and judged of it accordingly. The sentiments of the former are very decided, indeed too much so ; for his conclusions on the general subject, are drawn from those epidemics alone, which he had an opportunity of observing himself : to the Simple Dysentery his conclusions are fairly applicable, but not to any other form of the disease. He asserts that the Dysentery ought seldom to be classed among acute diseases : that so far from abating on the approach of winter, as stated by Sydenham, he found it during the years 1750, 1, and 2, continue through the whole of that season, neither less frequent, nor less troublesome, than it had been in the autumn. From thence he argues, “ *Dysenteriam totius anni propaginem esse, neque ulli certæ tempestati ascribendam.*” He further asserts, that it is scarcely ever accompanied with any fever ; that he did not observe one in ten, in whom a true fever could be said either to precede  
or



or accompany the disease. Finally, he concludes, “*Dysenteriam Rheumatismi, sive morbi articularis instar, nonnunquam febrem aliquam in se continere, aut secum sociatam ferre: longè tamen sæpius diuturnum esse morbum, atque ab omni propriâ febre vacuum, & hoc etiam multò magis quam ipse Rheumatismus.*”

It is not meant by these passages to insinuate, that feverish symptoms may not precede or accompany the Simple Dysentery, but merely, that they are such as never indicate the presence of idiopathic fever, being always proportioned to the severity of the pains, or their long continuance; for the pulse, except when symptoms of inflammation run high, is smaller, but not much quicker than usual, unless at such times as the pains of the abdomen are perhaps more acute, or of long duration: there are besides, neither heats nor chills; it may occasionally be with the Simple Dysentery, as Stoll has said of the *Dysenteria Rheumaticobiliosa*, “*Febre quidem evidenti carebat, non omni tamen motu febriculo.*”

When we have it asserted that Dysentery is a “*Pyrexia contagiosa,*” I presume it is meant



meant that the disease is attended by \* fever, not merely symptomatic, but peculiar to that affection, and that its contagion is also of as distinct and independent a character. Now, that it is not necessarily attended by idiopathic fever, the very nature of that fever, when present, and its total absence on other occasions, plainly shew; and that it is not of itself contagious will hereafter appear; as also, that when, in consequence of a certain combination it becomes so, that contagion is not peculiar to this disease.—I trust then I may be allowed to conclude with an assertion, which as we advance, will be receiving additional proofs, namely, that the genuine Dysentery is unattended by any, but symptomatic fever, proportioned to the violence and severity of the disease itself; while at the same time, as shall hereafter appear, it is capable of combining under appropriate circumstances, with intermit- tent, remittent, and typhus fevers.

\* When we hear authors, at one time, calling this fever inflammatory, and at another time, intermit- tent, or putrid, surely we must suspect that the disease can scarcely be possessed of an idiopathic fever, pecu- liar to itself, inasmuch as such fevers seldom admit of this variety.

If



If then it be admitted, that the symptoms already enumerated can, independent of "Pyrexia contagiosa," constitute a distinct, and well-defined disease; and for admitting this, we have sufficient grounds in the experience of every year, and in the testimonies of creditable writers, shall we refuse it the name of Dysentery, to which it appears entitled, merely because it does not chance to agree with a scholastic definition? Surely we should not: yet that some have been, and still are governed by it, in judging of the disease, an incident which occurred, while I was a student at an academy of deservedly high repute, will too plainly shew: A patient came into the clinical wards of the Infirmary, with every mark of Simple Dysentery. I examined her: she had no symptom of fever, and it was not possible to trace the disease to contagion. The Clinical Professor, a gentleman of considerable merit, and for whose attentions I ought to feel grateful, visited the wards soon after, examined this patient very carefully, and in his questions was most particular relative to the "Pyrexia contagiosa;" finding, however, that the former did not exist, and that there was no ground for believing in the  
agency



agency of contagion, though the other marks of Dysentery were all present, it would appear by the result, that he felt himself in some dilemma, how to decide between the disease and the definition; though he had no hesitation whatsoever as to the practice he should adopt on the occasion. At a subsequent lecture, he pronounced the disease to be a Diarrhœa, while he said he would treat it as a Dysentery. In consequence of this decision, purgatives, &c. were administered, and after a time the patient got well.—Strange, that our reason should be so warped by a definition, as to induce us to call a complaint by one name, and treat it under the name of another.

In the regular order, which had been proposed, we ought now proceed to the consideration of those other forms of Dysentery, arising from a combination of the simple disease, with fever of different types; but as we may find it advantageous here to enter on a digression, that may lead to a more intimate knowledge of the disease in question, and prove perhaps both instructive and entertaining, I shall not longer apologise for having first taken into consideration the



## SECT. I.

ANALOGY BETWEEN DYSENTERY AND  
RHEUMATISM.

THE idea of an intimate connection and strong analogy between these two diseases, was first, I believe, suggested by Alexander Trallianus, (who was in the habit of calling the former disease a Rheumatism of the Intestines), and has been more lately taken up by Akenside, Richter, Stoll, and others, who have given many instances of their frequent alternation. Independent of the authority of these writers to countenance us in admitting this doctrine, we find occasionally a few scattered facts in the writings of others, which must carry particular weight with them, inasmuch as those who recorded them, were utterly devoid of any preconceived opinions on the subject. —It shall be our business, in the first place, to prove, that such a relationship does actually exist between these diseases; and in the next place, endeavour in some degree to account for it.

*Akenside,*



*Akenside*, the most strenuous advocate for this opinion, states several facts in its support. He says he has frequently observed persons seized with pains like those of chronic Rheumatism after they had been freed from a Dysentery, and without having been preceded by rigor, or accompanied by any signs of fever: of such facts he declares himself in possession of more instances than he would wish to adduce; sometimes he has met men labouring under Dysentery and Rheumatism, whom he delivered from both by the same means. Sometimes he knew the gripings of Dysentery, conjoined with grievous pains of the whole body, and particularly of such parts as are usually affected by Rheumatism. And not only, says he, has Rheumatism supervened on Dysentery, but the reverse has also occurred.—He often noticed, in acute Rheumatism, when it was necessary to purge, that the stools were strongly marked with the dysenteric character, being attended by griping pains, and consisting almost entirely of mucus, so acrid at times, as frequently to induce tenesmus; and this result was uniformly the same, let the purgative be varied as you would.—He proceeds to recite three

C

cases



cases of this singular conversion of disease; the first is that of a woman, twenty-five years of age, who had laboured under a most severe general Rheumatism, which being subdued by bleeding, blisters, and guaiacum, she was in one day seized by genuine Dysentery: this yielded to remedies, and the Rheumatism again returned to the joints, though not with the same violence as before.

—The second case was attended with similar circumstances, except in the final result, for being an old woman, her strength was unequal to a second attack of Rheumatism.—

The third case was that of a man, who had three returns of Rheumatism, and as many of Dysentery, before he was restored to perfect health. After reciting these cases at full length, and after making many general observations on the subject, he concludes in these words, “Denique hanc morborum similitudinem toties observavi & perspexi, ut Dysenteriam jam pro Rheumatismo intestinorum habeam, & similem utriusque morbi causam & materiem esse contendimus.”

Stoll, who has written very diffusely on this disease, and has attempted supporting several species of it, a few only of which approach



approach somewhat to those it is my wish to establish, thus declares his opinion of the cause of the first, or simple form of the Dysentery; “*Nunquam accidisse hunc morbum visi, nisi si corpori sudore mananti, incauté admissum frigus fuerat;*” and with respect to the nature of the disease, he as plainly pronounces his opinion to be, “*Hancce Dysenteriam intestinorum Rheumatismum fuisse, ab eâdem genetrice materiâ natam, & a rheumate artuum tantum formâ, & sede diversam.*” In page 273. par. 3. he assigns his reasons for entertaining this opinion: “*1°. Quod Rheumatismos artuum videre contigit, repentè sublato, Dysenteriâ obortâ. 2°. Nonnunquam eundem hominem, et Rheumatismus vexabat, et Dysenteria. 3°. Dysenteria quoque subito cessabat, quò primum carpi, aut genua intumuère, dolumère ad eum modum, quo a febre Rheumaticâ solent, in artus incurrente.*” Their prevalence in the same seasons, and their requiring a similar mode of treatment, he alledges as further proofs of the truth of this opinion. Richter, in his *Medical and Chirurgical Observations*, bears ample testimony to the close connection existing between these diseases. He thus commences his



account of the Dysentery : “ What I long  
“ considered as highly probable, from the  
“ reasoning and experiments of Akenside,  
“ Stoll, and Vogler, I have for some years  
“ believed to be quite certain.” He then  
declares himself satisfied that it is a rheu-  
matic, or catarrhus affection of the intes-  
tines, particularly of the great guts; and  
in proof of this, he urges the general symp-  
toms, which set in, or accompany the dis-  
ease; and in pages 94 and 5, relates several  
instances of their mutual conversion. Zim-  
mermann too, we shall find furnishing us  
with cases not a few, of the conversion of  
Dysentery into Rheumatism, or rheumatic  
pains arising after, and in consequence of  
the use of opiates and astringents in the  
former disease. For particular cases, see  
pages 64, 98, et seq. 101 and 2. also page  
125.—p. 8. in enumerating the different ter-  
minations of the disease, when for the most  
part uninterrupted by medicines, he ob-  
serves, “ that with others, from whom the  
“ evil seemed to go away of itself, there still  
“ remained a great pain in the loins, and  
“ a Rheumatism in the joints.” In parti-  
cular, consult his chapter on the effects of  
astringent and constipating medicines, of  
aromatics,



aromatics, brandy, and wine. And here it may not be amiss to observe, that the effects of the constipation which followed the use of these remedies, were very different; for while Rheumatism and obstinate rigidity of the joints, almost always ensued on the employment of astringents, Inflammation, gangrene, and death, most usually followed the exhibition of aromatics and stimulants. In further confirmation of the point in question, I shall mention a single fact, recorded by an author of veracity, equal to the last, and certainly detailed by him without any reference to theory: Tissot, among a great variety of bad effects, consequent on the use of opiates and astringents in this disease, enumerates, “ Horrible rheumatic pains. I “ have been just consulted,” says he, “ on “ account of a terrible Rheumatism, which “ ensued immediately after taking a mixture “ of Venice treacle and plaintain, on the “ second day of a Dysentery.”

In Baker's treatise on Dysentery, p. 32. we find the following passage: “ Neque “ unico exemplo comprobatum fuit, Dysen- “ teriam hanc tam imperité tractatam, (i. e. “ astringentibus & opio) in Epilepsiam, “ *Rheumatismum*, Pleuritidem abiisse.” And



in the diseases of London, p. 293, Willan, in speaking of the Dysentery Epidemic, in the year 1800, observes: "When the violent grinding pain ceased in the bowels, pains were felt in the upper part of the thighs, sometimes in the shoulder and arms; sometimes the stomach alone was affected with pain periodically." But whether these pains, or any of them, were of the rheumatic kind, he does not say; nor is their any thing connected with the passage to determine.

After a statement of these facts, and more might be adduced, were it deemed necessary, we may, I believe, admit upon grounds that will not be questioned, the existence of a very close connection between the two diseases; indeed, on comparing them together, we may observe a striking coincidence in several respects. We know them to be the offspring of similar seasons; to arise often from the same causes; to agree in the general character of their accompanying fever, and to yield to the same mode of treatment: besides, that both frequently terminate in a chronic state, equally difficult of cure.—It may be asked, however, if there really exist such an intimate connection between these diseases,



diseases, as the facts above stated would warrant us in supposing; why do we not meet more frequent instances of their mutual alternation? It may be answered, that such instances usually arose from the improper treatment of Dysentery, by opiates and astringents, as till lately these constituted, if not the chief, at least the most common remedies in that disease: this circumstance made such cases more numerous formerly than they can possibly be at present, when the nature of the disease is better understood, and its treatment of course more judicious. This may serve to explain why Dysentery is not now so often succeeded by Rheumatism: at any period it is not to be expected that Rheumatism should so readily change into Dysentery, as the reverse, because the rheumatic action, being easy of removal, though not of destruction, can quickly shift itself from one part, or joint, to another, without necessarily fixing itself on the intestines, the whole canal of which may be commanded by medicines, not so, all the joints of the body.

In attempting an investigation of the causes, or an explanation of the affinity subsisting between Rheumatism and Dysentery,



tery, it will be necessary more particularly to enquire into the nature and seat of those diseases. In entering on this question, I cannot pretend ignorance on what delicate and slippery ground I am treading; on ground in which the greatest men have shewn themselves weak, yet on which all wish to exhibit themselves: and though each in turn fails in the attempt, still does he stand excused by the greatness of the object at which he aimed—nam “*magnis excidit ausis.*” So far as I venture after this object, I shall endeavour to rest on facts for my support.

The seat of any disease may be considered in two different points of view; 1st, as relating to those parts of the body principally affected by it, and next, as to the vessels, whose particular action constitutes the disease in such parts: in both respects the seat of rheumatic inflammation has been a subject of some dispute. This disease, in the first point of view, has been considered by several persons, and more especially by Dr. C. Smyth, as seated chiefly in the muscular fibre: the peculiar characters of the rheumatic inflammation he deduces from the structure and functions of that fibre. The arguments



arguments urged in favour of this opinion rest on three circumstances; 1st, on the parts affected; 2dly, on the pains excited by motion; and, 3dly, on the ground that these pains when shifting their place, follow the course of the muscular fibres. But these arguments, I am inclined to believe, will lose much of their weight, when we consider, in the first place, that those parts of the body most subject to violent attacks of rheumatic inflammation, are such as have no muscular fibres at all, as the knees and other joints; and also that there are other parts, largely possessed of muscular fibre, which this species of inflammation never molests, such as the heart. But if, on the other hand, we call to recollection, that there is no part subject to rheumatic pains, in which a serous membrane or membranous expansion cannot be demonstrated to exist, we may perhaps be disposed to think, that the seat of this inflammation is rather fixed in membranes, ligaments, sheaths of tendons, &c. for we find Rheumatism most frequently occupying the large, often the smaller joints of the body: in these parts it is not membrane, but muscular fibre that is deficient: so likewise on the cranium, nape of the neck,  
back,



back, thorax, in the diaphragm, among the \* abdominal muscles, and on the inferior and superior extremities, membrane, and membranous expansions, are to be met with in abundance: these we know to be common places for Rheumatism to fix itself. The membranes of the brain, too, are said to be sometimes affected with Rheumatism by Metastasis; if this opinion be true, it affords a satisfactory explanation why certain muscles, as the heart, are never subject to this disease. Burdin, in his Medical Studies, (vol. ii. p. 176) adopts the opinion, and observes, that “ the Rheumatism seems to have  
 “ its seat, at least in a special manner, in  
 “ the white fibrous tissues; such as the  
 “ articular capsules, the ligaments, the apo-  
 “ neuroses, the tendons, the tendinous va-  
 “ ginæ, the periosteum, &c. There is great  
 “ abundance of these parts around the arti-

\* That these muscles may be affected by rheumatic inflammation, was ludicrously instanced in the case of a poor dancing master, in consequence of which he was disqualified from following his profession, not being able to bend the body, and of course to make his bow: the case is related by Cullen: the affection seemed to have been seated in the recti muscles; these we know to be well supplied with membranous sheaths, and to have several tendinous bands or fasciæ.

“ culations;



“ culations ; they are continued between  
“ the muscles, and serve to account for the  
“ severe pains which are experienced in  
“ moving.” This last observation leads me  
to notice, that the second argument proves  
nothing, for the pains excited on motion  
might equally arise from any affection of  
the membranes, as of the muscles themselves.  
Neither will the third argument, taken from  
the direction of the pains, prove much ; be-  
cause in the transition of pains or disease  
from one part to another, there is no ground  
for supposing any muscular, or other appa-  
ratus, similar to that in which the disease  
was seated, necessary for the purpose, as in  
these cases the sensation of transition arises  
merely from different and distant parts as-  
suming, in rapid succession, that inflam-  
matory action they could not all labour  
under together : the pains, therefore, appear  
to shift, or take the shortest direction to  
another part, which will be the direction of  
the muscles. In the sense above-mentioned,  
it is that pains seem to shoot from one part  
that is inflamed, when that inflammation is  
about to subside and occupy another ; as in  
inflammation passing from one eye to the  
other, from one tonsil or testicle to the  
other ;



other; beside, do not gouty pains often seem to follow the direction of the muscles, and who will therefore argue that Gout is an affection of muscle?

These considerations may, perhaps, induce us to believe, that membranous parts are at least more liable to rheumatic inflammation than muscular; and in this opinion we shall be more confirmed, when we come, by-and-by, to notice those vessels, which this disease seems chiefly to affect. I shall conclude this part, by observing, that dissections seem to favor that seat of the disease already pointed out, by the detection of a viscid and gelatinous matter in the joints, and sheaths of tendons, in cases of some duration.

Before we speak of the vessels, the immediate seat of rheumatic inflammation, it may not be amiss to say something of those causes, which give rise to a predisposition towards it and some other diseases; and in considering these causes, we shall set aside any predisposition arising from previous disease, as its primary origin is merely in question. The same exciting cause, acting under different circumstances, we find capable of inducing diseases, apparently very opposite in  
their



their nature: thus the operation of cold, may give rise to phlegmonous or rheumatic inflammation in one, to Dysentery, or Dropsy, in another; and in all such cases, the source of predisposition may, I believe, be referred to the degree of vigour and strength of action existing in the vital powers, or to the particular state and condition of the body at the time the exciting powers are applied. Thus in the adult, plethoric and robust, more especially among men do we find phlegmonous inflammation most frequent; while the rheumatic may be observed among persons neither so plethoric, nor robust as the former, who have either past, or not yet arrived at the adult state; and more particularly among women, though not equally exposed to its exciting causes: or it may also occur in persons of the former description, provided their strength has been previously reduced by labour or sweating. Hence do we find Rheumatism so frequent among soldiers, sailors, artists, workmen, and others, who, when heated and sweating, do not hesitate to relieve their uneasy sensations, by a free and imprudent exposure to cold. Dropsy is well ascertained sometimes to arise from the action of cold: it almost always occurs  
in



in weak and debilitated habits. I have several times seen it induced in persons labouring under the influence of mercury, after imprudent exposure to this general agent of disease. In some of stronger frame, it produced Rheumatism in place of Dropsy, or the latter soon followed after the former, and in many Hydropics, I have not unfrequently found rheumatic pains to precede that disease, which on the appearance of the Dropsy have entirely ceased.

Thus do we observe diseases of various characters, produced by the operation of the same cause, on different states of the body; and in each, we may notice a very different set of vessels to be concerned, or a different action of the same vessels to have taken place: in the full and vigorous frame, it excites the whole arterial system into violent action, speedily destructive of life, if the due remedies be either applied too slowly, or in too small quantity. This constitutes the phlegmonous inflammation, whereas the rheumatic, generally occurring in persons of a lower standard of strength, is not so much seated in the arteries, as in their exhalant extremities, which, when inflamed, constitute Rheumatism, and in consequence of  
that



that state, are pouring out a greater quantity of their contents, than the absorbents are equal to take up at the same time, thereby inducing that pain and swelling so common in the disease. On account of this peculiarity in the seat of Rheumatism, and the character of the temperaments most subject to the disease, it happens that neither venæsection, nor other evacuations, that by the skin alone excepted, are equally advantageous in this as in phlegmonous inflammation: indeed the principal object we should aim at, in treating Rheumatism, ought to consist in promoting the absorption or evacuation of the effused fluids, while at the same time we do not neglect to reduce that inflammatory action of the exhalants, which primarily induced that effect. This peculiarity in the seat of Rheumatism, may suggest some reason why we never find a purulent secretion consequent on the disease, and why the red flush of inflammation is so rarely to be observed on the parts affected by it. In Dropsy, consequent on exposure to cold, the exhalations from the extreme vessels, on account of the weak condition of the whole frame, is increased without any inflammatory action in them, and their fluid contents are poured out



out in greater proportion than can be readily reabsorbed. If this distinction between Rheumatism and Dropsy be well founded, it affords some explanation why the one may so often follow the other, and why we sometimes meet a disease compounded of both.

Dissection informs us, that Dysentery is almost exclusively seated in the greater intestines, principally affecting their inner, or villous coat: but for any further information of a satisfactory nature, we cannot rely on dissections; for, what can they exhibit to us, but the state of diseased parts after the diseased action has been able, from its length of existence, to induce death; thereby shutting us out from any distinct view, or conception, of that primary condition of the intestines, which constituted the disease: of this we are left to form our conjectures from the nature of the symptoms, and that insight into its effects which dissection affords. As it hath been contended, that "Pyrexia contagiosa," has no influence in inducing this disease, we must look to some local action in the intestines themselves, for an explanation of its essential symptoms. Inflammation, of one kind or other, seems to be more or less essentially concerned in the production



tion of the disease. Dissection shews it to have been present, often in so high a degree, as to induce death by mortification, and the symptoms of severe tormina and pain, more particularly on pressure, indicate the same circumstance. Of what kind is this inflammation? can it be of the phlegmonous character, as dissection proves it often the immediate cause of death? But this kind of inflammation usually excites an high and ardent fever, and is besides attended with constipation: “Quapropter,” says Akenside, “inflammatio contraria videtur Dysenteriae indoli, cujus magna pars in dejectionibus nimio plus frequentibus posita est.” Whether this conclusion be accurate, we shall see by subjoining to his logical Enthymem, the only minor præmiss, which could warrant him in making it. It should have run thus:—*Sed Dysenteria, non comitatur cum febre inflammatoriâ, nec constipatione intestinorum.* Now this præmiss, in both respects, would be false, for Dysentery is often attended by symptoms of fever and inflammation, and it has also a species of constipation, peculiar to itself, namely a retention of the natural contents of the intestines, while there are at the same time, frequent

D scanty



scanty stools of mucus, blood, and serum.— as then, the simple phlegmonous inflammation will not account for these peculiarities in Dysentery, so must it be overlooked, as exerting any essential influence in the production of the disease.

Most persons, conscious from the symptoms, treatment, and very frequent termination of the disease in a chronic state, that phlegmonous inflammation, or any modification of it, could not constitute a part of Dysentery, were then pleased to denominate that species of inflammation, which takes place on the villous coat of the intestines, Erysipelatous, without, however, meaning to deduce any explanation of the symptoms from that circumstance; but, I believe, rather thereby wishing to express something essentially differing from phlegmonous inflammation. Little need be said against this, as the same objection applies to it as there did to the former, namely, that it offers no rational explanation of the characteristic symptoms of the disease.

In offering that opinion which, from a variety of considerations, I have been induced to adopt, I wish to have it previously understood, that I look upon spasmodic contraction,



striction, which Cullen ingeniously, though not \* originally supports, as the proximate cause of this disease, to be no more than one of the several effects of that cause, whatever it be; for supposing spasmodic constriction existing to any extent in the intestinal canal, how will that alone enable us to explain the phenomena of Dysentery? Why should we have stools at all, or whence should arise that inflammation which so peculiarly affects the inner coat of the intestines? besides, on several occasions where we know spasm to exist there, do we perceive symptoms, any way resembling the Dysenteric, to follow? Hence we must look for some other cause, equal to the production of this, as well as the other phenomena of the disease. That cause I would refer to a rheumatic inflammation of the inner coat of the intestine, in the sense in which I have employed that term, or, in other words, to an inflammatory action in the innumerable exhalent vessels, which open on the inner coat of the intestines. From this state

\* Haller, in Hist. Morb. Uratislau. A. D. 1669. p. 84. makes this remark on the condition of the intestines in Dysentery: "In Dysenteria totum canalem, qui sub nomine primarum viarum communiter venit, spasmodicè concutit."



of the vessels, the quantity of their secreted fluid is considerably increased, and its quality, perhaps, somewhat affected: the intestines by this stimulus are excited to spasmodic constriction, and from both these circumstances united, all the other symptoms may be deduced: hence we have frequent stools with co-existing constipation: hence we have tormina with tenesmus. The circumstances which induced me to embrace this opinion, were, in the first place, the insufficiency of all other proposed explanations, and the readiness with which this accounts for the various phenomena of the disease; besides that, in the next place, the symptoms which it presents, the treatment it admits, and, in particular, its close affinity, and frequent alternation with Rheumatism, all countenance us in receiving the doctrine.— It obviously explains why the dysenteric pains, which, in the language of Hoffman, “*Per totum intestinorum canalem vago itinere progrediuntur,*” so like the Rheumatic are constantly shifting their ground, and also how it happens, that the dysenteric action, when disturbed in the intestinal canal, by opiates and astringents, immediately appears in the joints, under the form of Rheumatism;



Rheumatism ; in the same manner, that the rheumatic action, when disturbed in one joint, flies to another. But whether this speculative reasoning be admitted, or not, still will it remain established on grounds too strong to be easily controverted, that such affinity does really exist, between Rheumatism and Dysentery, as to induce us to agree with Stoll in entitling them *παθηματα αδελφεια*.



## CHAPTER II.

### COMBINATIONS OF SIMPLE DYSENTERY:

#### AND FIRST OF THE

#### INTERMITTENT & REMITTENT DYSENTERY.

HAVING already pointed out the simple disease, and shewn how different it is from that which Cullen has defined it to be, we now return to the great object of our pursuit, from a long, and, probably, vain digression, during which we had attempted, though perhaps to little purpose, to procure such additional light, as might guide us in our search of truth, and enable us to dispel some of that darkness which yet envelops the nature of this disease. We shall forthwith proceed to the consideration of such important combinations, as the Simple Dysentery appears to be susceptible of. That diseases are liable to combinations, which exert a material influence mutually on each other,



other, and effect considerable changes in the characters and treatment of each, few who are conversant with the subject will doubt, and that Dysentery is one of these, no one, I believe, will hesitate in admitting. There are two species of combination to which diseases may be said to be subject: the one is such a combination of the character and features of each, whereby they seem to incorporate together, to possess but one nature, and exercise one dominion: the other is, an accidental union of two or more diseases in the same individual, which are naturally, and continue to be, mutually independent of each other; the latter are in general local diseases, as Itch, Syphilis, Herpes, &c. or one or more of them may be local, while a third is a general affection of the system. But then the local complaints arise from specific contagion, as in the combination of Typhus, with Itch, &c. Such combinations as these, exert no influence over each other, whereby their characters or treatment may be affected: not so the former combination, which will always be found to consist of a general febrile affection, and one or more local diseases, not originating in contagion. This species might be illustrated in almost



every epidemic of Dysentery, of Catarrh, of Angina. But that we may be better enabled to comprehend this part of the subject, it will not be amiss to take a short view of those epidemic constitutions, (as they are termed by Sydenham), which are found so materially to influence the nature and treatment of such diseases as prevail under them.

Sydenham, from long and careful observation, concluded, that epidemic diseases changed their nature twice a year, to wit, about the vernal and autumnal equinox: hence he has divided these diseases into vernal and autumnal epidemics, though on many occasions it may be a difficult matter to fix precise boundaries between them, as they imperceptibly meet and run into each other. The experience of later days, has but tended to confirm the propriety of the advice given by Sydenham, carefully to attend to that disease, which rages most violently about the vernal and autumnal equinox, as that imparts, if not its name, at least its character to the constitution of the whole year. But besides any particular epidemic which prevails, there may, and do occur other diseases, not dependent on the reigning constitution, though existing under it: these

Sydenham



Sydenham has called Intercurrents, and in numerous instances has shewn the importance of distinguishing them. Thus, for example, Pleurisies, Anginas, Dysenteries, may arise from their peculiar causes, and in their appropriate seasons, have no connection with the reigning epidemic, and are then to be treated, each according to its peculiar nature, as essential diseases, without any reference to that epidemic under which they occur: but these self-same diseases, which thus appear as Intercurrents, may exist, only as manifest symptoms of the reigning fever, and are then to be treated, not as essential diseases, but by the method that fever requires, adapted to each particular case. This being a point of some importance, it may not be amiss to subjoin the means of Diagnosis between them as essential and symptomatic diseases: this distinction is to be taken from the symptoms that accompany them in their commencement: if these symptoms are those of the reigning fever, then the disease is symptomatic, and to be treated accordingly: besides this, the appearance of the disease out of its more appropriate season, will also assist in deciding our Diagnosis: the necessity and importance of attending to this latter point



point, Sydenham clearly demonstrates in several instances. It is after this manner, and in this sense, that Dysenteries may prevail as essential or symptomatic diseases, and as Sydenham met them when Epidemic in the latter character, he was thence induced, according to his usual practice, to denominate the disease a "febris introversa:" but more of this by-and-by.

Dysentery, I conceive, to be one of those diseases, that admits, in a striking manner, the first species of combination just pointed out, and although, when observed in individual cases, there are few combinations of the second kind, to which it may not be liable, yet when viewed in the light of an Epidemic, they become much more limited, and may, I believe, be very properly reduced to three, namely, to the intermittent, and remittent Dysentery, and to the combination with Typhus, or malignant fever: to some one of these, or to the Simple Dysentery, we may, I imagine, refer the history of any Epidemic on record;—I shall proceed to point out a few of such as may be classed under the head of intermittent and remittent Dysentery, and from the great analogy of the two combinations, I need offer no apology



logy for considering both together. On this head, however, I shall be here more concise than on the former, or subsequent one, inasmuch as we shall have occasion to take further notice of it in considering its treatment, and also, as it is by no means of such importance as that we shall next proceed to speak of.

Though the Dysentery of which Clarke has given us so distinct an account, is not, as shall shortly be pointed out, of the same character with either of those we are now treating of, yet, as he commences his history with an observation, which respects the general subject, I shall here introduce it in confirmation of that susceptibility of combination we have noticed as strongly characteristic of this disease: "In unhealthy situations," he observes, "when epidemic fevers rage, the Dysentery is very dangerous, beginning with great rapidity, and rather seems to be a symptom of the fever, than an original disease." Morton also, from the distinct remissions, observable in his epidemics, every, or every other day, was led to embrace the same opinion: "Dysenteriam, nempe, symptomatis duntaxat rationem habuisse, febrem autem, eamque



“*eamque verè Συμεχῆν, primarium fuisse morbum.*”\*

I have already alluded to the idea which Sydenham entertained of the nature of Dysentery, and to the ground on which he entitled it, a “*Febris Introversa:*” indeed, in giving it this name, he seems obviously to refer to the same cause for which Morton and Clarke denominate the disease a symptom of the fever. That this opinion of Sydenham’s originated in the frequent combination

\* We may here notice an objection, if it deserves the name, which some might feel inclined to offer against the titles assigned to the combinations of Dysentery: As the fever, say they, in these cases forms so material a part of the disease, and is that on which its treatment and character chiefly depends, the compound should not be called intermittent, remittent, or typhus Dysentery, but a Dysenteric intermittent, remittent, or typhus, inasmuch as it merely consists of one or other of these fevers conjoined with some dysenteric symptoms.—The author subscribes to the remark, and is almost indifferent by which name the compound disease may be known, but is, on the present occasion, inclined to prefer that already given, because it is his wish, and is here his object to point out, the distinctive difference between the Simple Dysentery, and those combinations, which very generally have been considered and spoken of under the name of Dysentery alone: he would besides remark here, that what are denominated by many, “*some dysenteric symptoms,*” truly constitute the simple and genuine disease.

of



of Dysentery with intermittent and remittent fever, in his days more common throughout England, than they have been since, will appear probable from the account of an epidemic remittent, which he has given us subjoined to his History of Dysentery: this he calls the “*Febris Dysenterica*,” and this he supposed to be the “*Febris Introversa*,” productive of Dysentery, in consequence of the material influence he perceived it to exert over the symptoms of that disease.\* If such be not Sydenham’s meaning, and such the origin of his opinion, I confess I cannot

\* His description of the *Febris Dysenterica* is as follows:  
 “ At the same time the Dysentery raged, a fever arose,  
 “ which much resembled, and often accompanied this  
 “ disease. It not only attacked such as were afflicted  
 “ with the Dysentery, but those likewise who remained  
 “ wholly free from it; unless that sometimes, though very  
 “ rarely, the patient had slight gripings, sometimes with,  
 “ and at others, without a looseness; for it always had the  
 “ same apparent causes with the Dysentery, and was also  
 “ attended with the same symptoms, as the fevers of those  
 “ who had the Dysentery; so if we except the evacua-  
 “ tion by stool in the Dysentery, and the symptoms there-  
 “ on necessarily depending, this fever should seem to be  
 “ wholly of the same nature with that disease.” Sect. 4.  
 Chap. 4.—This is obviously the description of a fever,  
 which sometimes attacked alone, and sometimes in combi-  
 nation with Dysentery.

otherwise



otherwise understand, or make sense of it : for if we are to take his phrase of “ Febris “ Introversa ” in its literal, and most obvious sense, the opinion which he adopted, must appear unfounded in fact, there being no evidence to prove the reality of a Febris introversa, because in that case, the fever should disappear, the Dysentery being substituted in its place : now, on the contrary, the fever does still exist, and in combination with the Simple Dysentery, and without having any connection as to cause with that disease. This opinion of Sydenham’s, while embraced by some, has been controverted by others : “ This kind of flux,” says Clarke, (meaning that which rather seems to be a symptom of the fever, than an original disease,) “ has been justly considered by Sydenham, and others after him, as the same “ disease affecting the intestines.”—In further confirmation of this observation, Moseley thus expresses himself : “ I have invariably “ found the truth of Sydenham’s opinion, “ and have remarked, that as the flux con- “ forms by the number of stools, and by “ its rapidity to the degree, so it does “ to the state of the fever of the sea- “ son, when it prevails ; the stools being “ more



“ more frequent, and all the symptoms  
“ more aggravated, at those hours when  
“ the current fevers are in their exacerba-  
“ tion, and the reverse, when these fevers  
“ are in their remission,” p. 232. Prin-  
gle, however, in drawing an analogy be-  
tween intermitting, remitting Fevers, and  
Dysentery, though he would at first appear  
to incline to Sydenham’s opinion, yet adds,  
that “ upon a nearer view we shall find this  
“ notion more ingenious than solid, since  
“ the circumstance of its being *contagious*,  
“ shews that the Dysentery is essentially  
“ different from these fevers.” But this  
mode of reasoning was utterly inadmissible  
in the eyes of Moseley, whose view of the sub-  
ject, on grounds that shall shortly be men-  
tioned, differed very materially from that  
of Pringle, on whose observation, above  
stated, he has passed the following unmerited  
censure: “ By this, says Moseley, he must  
“ suppose, what I fancy no other person  
“ does, that Dysenteries are always infec-  
“ tious, or that Fevers never are; or that  
“ epidemic Dysenteries are infectious, when  
“ epidemic Fevers are not.” Now most  
certainly this was not Pringle’s meaning,  
nor will his reasoning on a little reflection  
appear



appear at all unfair: for as that Dysentery, which he had most frequent occasion to observe, was undeniably contagious, and of a very different character from that which Moseley was in the habit of seeing, how was it possible, Pringle necessarily inferred, that a disease of a contagious nature, could originate from the introversion of fevers which were not: surely not meaning thereby that fevers never are contagious, but that the intermitting, and remitting were not. We may, I believe, explain the difference of opinion between these authors on two grounds; in the first place, because each was influenced in his general conclusions respecting Dysentery, by that form of the disease, they each chanced most frequently to meet with: Moseley in combination with remittent fever, a form which is not contagious: Pringle in combination with typhus or malignant fever, which is; and in the next place I fear that part of Moseley's anger against Pringle arose from his admission of contagion in Dysentery, which the former absolutely denied in that disease, and even suspected the existence of in any; it must however be urged in extenuation of Moseley's sentiments, that that form of the disease

he



he had most occasion to observe, was undoubtedly not contagious\*.

I have dwelt longer in the investigation of this subject than might to some appear necessary, but as it may furnish us with a clearer insight into that combination of which I am treating, and at the same time serve to explain some of those circumstances which lead to that diversity of sentiment among the most eminent authors, I trust I shall stand excused for the length of the discussion, though some points in it have been taken for granted, that must yet be proved. I shall conclude this chapter by subjoining a few passages from other writers not less eminent than the former, in order more satisfactorily to demonstrate the reality and existence of the intermittent and remittent forms of Dysentery: Cleghorn, Hunter, Roederer, Willis, and Rollo furnish us with abundant proofs to that effect: I

\* Still I cannot avoid thinking that there is an unusual degree of harshness in the different comments which Moseley takes occasion to pass on Pringle, much greater than that which he employs against other authors who embrace the same doctrine of *contagion*, and certainly much greater than he could deserve, even for "an attempt to correct the irreproachable accuracy, and to assist the incomparable genius of Sydenham."



shall commence with the latter, as from him we have a concise, neat, and just account of these two forms of the disease, which he chiefly met with, in his West-India practice; and so constantly did he find them in combination, that, although he assigns very satisfactory reasons for considering them as independent diseases, which, from a particular conjunction of circumstances, and the joint action of their individual causes, took possession of the same patient, he yet fell into the error of thinking fever, an essential part of the Dysentery, and this fever, he says, assumes the form of an intermittent, or remittent: p. 27. When it assumes the intermittent form, the symptoms peculiar to Dysentery, appear in the paroxysm, and they disappear, or are much alleviated when it goes off; when the fever is remittent, the dysenteric symptoms increase, and abate with each exacerbation and remission. When the disease terminates early in death, the fever, he says, has not disappeared, but assists in producing the fatal scene; when the disease leaves a Diarrhœa, the departure of the fever is evidently marked, generally in fourteen or twenty days, and when the disease goes off without any consequence, the



the fever generally terminates before the fourteenth day, most commonly about the seventh or eighth: p. 6. These latter observations apply more particularly to the remittent or more continued forms, for when the fever is intermittent, it may be of short duration, as the disease, by an early and judicious application of medicine, can be often speedily removed. P. 15. in speaking of the occasional causes of the disease, he observes, that when the Dysentery prevails, intermitting and remitting fevers frequently appear, and it generally assumes one or other of these forms, more commonly of the latter, and they are in their symptoms similar, except in the difference of such as are peculiarly dysenteric. In this latter remark does he not appear to contradict a former position, that fever constitutes an essential part of the Dysentery? In the next chapter I shall have occasion to take notice of some ingenious and well-founded conjectures offered by this author, whose clear-sightedness in this early part of his life, has been only equalled in his subsequent researches into more abstruse, and complicated questions. Cleghorn noticed such a great similitude in many respects between tertian Fevers, (the epidemics



of Minorca) and Dysentery, as to have been frequently induced to make trial of the bark in the last named disease: “when the fever  
 “and gripes, says he, were regularly exas-  
 “perated every day, or every other day, at  
 “stated periods, the bark has often effec-  
 “tually put a stop to both, especially if the  
 “exacerbation began with chilliness, and  
 “terminated in sweats: at other times it  
 “removed the fever, the flux continuing  
 “without much alteration;” this last pas-  
 sage most satisfactorily proves the existence  
 of Dysentery in combination with inter-  
 mittent fever. Roederer, in a note, p. 21,  
 in speaking of the relation subsisting between  
 Dysentery and Intermittents, mentions a  
 striking instance of their combination:—  
 “Memorabilis est observatio huc trahenda,  
 “quod hoc anno multi febreintermittente,  
 “et Dysenteriâ simul laborarint, sive vera  
 “intermittente Dysentericâ. In vico autem  
 “vicino epidemica grassata est febris in-  
 “termittens sola, ut quinque et ultra in  
 “eâdem domo laborarent; in alio vico  
 “paullo remotiori, montibus vicinis cincto,  
 “eodem tempore sola Dysenteria sæviit,  
 “multosque jugulavit; in alio denique vico  
 “inter priores medio, uterque morbus  
 “rarissimus



“rarissimus fuit.” Between this disease (meaning Dysentery) and the remittent fever in Jamaica, Hunter observes that there subsists an intimate connection, the one frequently changing into the other, and both often complicated with various degrees of violence. In the worst cases of Willan’s Epidemic of 1800, a considerable degree of fever prevailed from eight to ten days: the pulse was 100 in the morning, and 120 in the evening; there was constant flushing of the face, and coldness of the extremities, and a periodical aggravation of pain for three or four hours every forenoon was to be observed. Reide remarks, that more or less fever attended the Dysentery: in some, he says, the paroxysms ran very high, and went off with a profuse perspiration, during which, the patient had fever stools, and hardly any griping. I might furnish a variety of examples to the same effect as the foregoing, but enough has, I trust, been offered to satisfy our minds of the reality and existence of those combinations, it was proposed to speak of in this chapter: in treating hereafter of the exhibition of bark in this disease, I shall have occasion to make mention of other similar instances, in which the efficacy of



that medicine was conspicuous. It would have been perhaps more correct to have introduced here some account of Willis's *Dysenteria cruenta*, which was the same epidemic with that observed by Sydenham, but as on this occasion we could not do so at full length, or with propriety draw any contrast here, between it and his *Dysenteria incruenta*, I have thought it better to reserve the entire consideration of either, till such time as we could treat of both satisfactorily.

I shall now conclude this chapter by offering a few remarks on the connection between Dysentery and intermittent and remittent Fevers, an explanation of which may not be so difficult as would at first sight appear, and also of another circumstance, which was such a problem to Pringle, how it happened that exposure to the same cause (as he thought) will indifferently produce intermittent, remittent fever, or Dysentery, or either of the former in combination with the latter. Exposure to cold, will, we know, under certain circumstances readily induce Dysentery, and exposure to marsh miasmata, both intermittent, and remittent fevers. A number of soldiers are exposed in inactive service, perhaps after fatigue and sweating,



sweating, or without the shelter of warm clothing, both to the cold of night in a warm season, and also to the miasmata of marshes at the same time; can it under such circumstances be at all surprising, that one man should be attacked with Dysentery, another with the fever, a third with both, while a fourth shall escape all? We cannot be surprised at the occurrence of Dysentery, much less that the fever should be a consequence of such exposure, and why are we, that the causes of both diseases should operate on the same individual, producing in him a Dysentery with distinct intermissions or remissions, when we see from the passages already quoted, that such a combination is so frequent in those countries, where these diseases often exist distinct and independent of each other. This subject is explained after a similar manner by Dr. Rollo, who in p. 15 and 16 remarks, that the Dysentery is produced in place of, or at the same time with these fevers, when the effluvium or marsh miasma is joined in its action by the exterior application of cold and moisture, to which, he observes, those affected with Dysentery, have been always previously exposed,



posed, independent of the causes of inter-  
mitting and remitting fevers. We now pro-  
ceed to consider the last, and most important  
combination of the Dysentery.



## CHAPTER III.

## COMBINATION OF SIMPLE DYSENTERY

WITH

## TYPHUS, OR MALIGNANT CONTAGIOUS FEVER.

TO this combination from its importance and singularity, I shall dedicate a larger share of attention, than has been bestowed on the two former, which, however deserving of notice, fall far short of that degree of interest, excited by this form of the disease: to the patient it must appear an object of dread from the great and imminent danger attending it, and to the attendants, an object of constant terror, from the well-founded belief in its contagious character. This is the combination, which is never free from symptoms of fever, and to which alone the property of contagion belongs: this is the true "Pyrexia contagiosa:" it is after this form that Cullen has framed his definition



definition of the disease; it is this form, which has always inspired such horror at the name of Dysentery. The strong features of such a combination are very decidedly marked in the histories of the most celebrated epidemics of this disease: and between this and all other forms the reader may trace the most essential differences in their access, progress and termination, in the danger attending them, and in the mode of treatment, which success warrants in each.

In treating of this combination, there are two very material points to be proved, the 1st, that such a combination of Dysentery and Typhus does really take place; and the 2nd, that this combination alone is contagious.—The first point shall be considered in this chapter, the second will form the subject of a subsequent one; in discussing the first point, however, it will be impossible to avoid occasionally anticipating some part of the second: still nothing more of that nature shall be introduced, than what unavoidably intrudes itself.

That we may proceed to ascertain in the most satisfactory manner the first point in question, the proofs necessary for this purpose shall be arranged under three heads, each  
of



of which will be kept as distinct as possible. Under the first, shall be considered such proofs as may be deduced from the train of symptoms, which mark the presence of Typhus; under the second, shall be enumerated those circumstances in the general history of each Epidemic, which may lead us to conclude the presence of the same disease highly probable; and under the third head, shall be comprised such proofs as may be drawn from the mode of treatment best suited to the different forms of the Dysentery.—This last species of proof however must be omitted here, as it will come under our consideration much more appropriately, in speaking of the general treatment of the disease: and as we not unfrequently find the same author furnishing us in the same epidemic with proofs which might class under either, or both of the two first heads, I have thought it preferable, occasionally to introduce matters under one head only, that more properly should be arranged under both, than to run the risque of confusion by separating, or giving in detached parts, the observations of the same author on the same epidemic.

Before



Before proceeding further in this investigation, it may not be amiss to premise a few queries, extracted from Dr. Rollo's work on Acute Dysentery, and whose observations on the intermittent, and remittent forms I have already quoted with so much pleasure. Dr. Rollo, though led from his own experience unequivocally to deny contagion in that disease, (v. p. 23, &c.) yet, very unlike to others, was neither so vain of that experience, or so presumptuous in its infallibility, as to disbelieve on that negative evidence, the direct testimonies of the most creditable authors, that the disease was occasionally propagated by such agency: on admitting the fact, however, he proposes the following pertinent, and ingenious queries: "Is the Dysentery, as said to be reproduced and propagated by contagion, exactly the same, as when otherwise, and originally produced?" Again, as the affinity of the Dysentery to intermitting, and remitting fevers is indisputable, he asks, "Have these fevers, arising from the marsh effluvium, become in any situation contagious?" And again he asks another question, which in great measure embraces the preceding: "May  
" contagious



“ contagions arise from a living subject in  
“ a diseased state, and produce a similar  
“ state of disease, although this diseased state  
“ has been originally produced by other  
“ causes than contagion?”—These queries  
the author hopes, the reader will feel little  
hesitation in answering satisfactorily, after  
finishing the perusal of this essay.—For  
further information I can refer with much  
pleasure to this interesting treatise by Dr.  
Rollo, and in particular to p. 32, for some  
very relevant facts detailed by Dr. Lind in  
his paper on Fever and Infection, with the  
observations, and queries, which they sug-  
gested to the discerning mind of the former:  
the author of these pages received peculiar  
satisfaction from their perusal, as the same  
suggestions and the same train of reasoning  
had led him on to the present investigation,  
long before he had either heard of, or met  
with Dr. Rollo's treatise on this subject.



## SECT. I.

## PROOFS FROM SYMPTOMS.

As it has been shewn, that a few well-marked and obvious phenomena constitute the Dysentery: and that fever attended with intermissions, and remissions in conjunction with these, constitute the intermittent, and remittent forms of that disease, it cannot be unfair to conclude, that, if we find the symptoms of Typhus in combination with those of Dysentery, this must constitute that form of disease we are in quest of.—That such symptoms do exist in combination, little doubt will, I trust, remain on the minds of my reader after perusing a few of the subsequent passages. I shall commence with Clarke, who thus describes the Bengal Dysentery. “ It set  
“ in for the most part with lassitude, slight  
“ rigors, disorder at stomach, and bilious  
“ vomiting, at first exactly resembling the  
“ fever, but the paroxysm did not run so  
“ high, and the patients were not so apt to  
“ rave.



“ rave. In a day or two, sometimes later,  
“ the Dysenteric symptoms made their ap-  
“ pearance, and were attended with the  
“ *greatest protraction of strength, and*  
“ *spirits.* If there had been any remissions  
“ in the fever, they now disappeared; skin  
“ continued hot: pulse became quick and  
“ small: tongue very foul, and hiccup  
“ frequent. If the disease was not speedily  
“ removed, the symptoms were daily aggra-  
“ vated: tongue became very black, and  
“ the teeth were covered with a black tena-  
“ cious slime. The great frequency of the  
“ stools induced excessive weakness, and  
“ the countenance was inexpressibly ghastly.  
“ On mortification taking place, the usual  
“ symptoms occurred, and in all the  
“ patients at this period subsultus tendi-  
“ num, tremors and delirium were added:  
“ some had pustules on various parts filled  
“ with ichorous matter, which degenerated  
“ into black putrid sores; the disease was  
“ often fatal in a few days.”—This de-  
scription needs no comment; every cir-  
cumstance in it is so unlike the usual course  
of Dysentery, and so pointedly characteris-  
tic of Typhus, both in the access, progress,  
and termination of the disease, that Clark  
himself



himself remarks, the Dysentery seemed rather a symptom of the fever, than an original disease: the only circumstance I shall here particularise, is the *great prostration* of strength and spirits, which Clarke has noted as a primary symptom of the disease, in contradistinction to that *excessive weakness* induced by the frequency of the stools; this primary debility is quite unusual in the more common forms of Dysentery, but will be found the peculiar and constant attendant on that combination we are now treating of.

Degner, in describing the Dysentery, which prevailed at Nimeguen, has furnished us with a detail of symptoms, not less strongly characteristic of Typhus than those enumerated by the last author: he states the attack of the disease to be as follows: “Accedebant statim sub initium  
 “mali, fastidium ciborum, nausea, conatus  
 “vomendi, imo vomitus actualis, unà cum  
 “*magna virium prostratione*: in aliis statim  
 “a principio morbi lypothymiaë erant  
 “frequentes.” v. p. 16 & 17.—The pulse, which at first was nearly natural, soon intermitted, or was not to be felt: the tongue was dry: and the thirst unquenchable: there  
 was



was constant watchfulness. The disease varied much in its degree of severity in different individuals : the more sudden the attack, and the higher the above symptoms set in, the worse.\* The termination of the disease is thus strikingly marked by him :—“ Magna virium  
 “ prostratio, animi deliquia, cardialgia, con-  
 “ stans deglutiendi difficultas, visus obscuri-  
 “ tas, inquietudo, mentis alienatio, deliria le-  
 “ via, floccorum collectio, extrema valdè  
 “ frigida, oculi insigniter cavi, plerumque  
 “ lethalia indicia erant.”—These symptoms, though they may attend the close of other diseases, yet are found more uniformly present at the dying scenes in Typhus.

Bontius, in his History of the Diseases, &c. of the East Indies, records some particulars of a Dysentery, which prevailed A. D. 1628, in the town of Batavia, then so closely besieged, that the waters of the river, and the air, were tainted by the putrified carcasses of men and beasts, dead of famine

\* Degner has mentioned a circumstance relative to this Epidemic, which would appear somewhat singular, if we could not conceive that the general affection must be more or less influenced by the local disease; “ Nec  
 “ hoc omittendum est, inquit, me in tanto numero afflic-  
 “ torum, ne unicum de capitis dolore conquerentem au-  
 “ divisisse.” p. 27.



or their wounds. He cites four examples out of six hundred, which are all clearly referable to the combination now in discussion:—Of the first case, he reports, that the subject of it fell into a continual Fever, and afterwards into a Dysentery, then Epidemical: he died, notwithstanding every possible assistance; in the last stage of his illness there appeared black-and livid spots over his body, with a cold and fetid sweat.—The second was a fatal case, of a green and malignant kind of Dysentery, many livid spots, like the former, appearing on his legs and arms—*v. Obs. 1.*—The third was also a fatal case, of a *true* Dysentery, which continued five or six days, with a high fever; livid spots were so numerous over his whole body, that you could scarcely place the point of a pin where they were not to be found.—In the fourth example, the patient was seized with an ardent fever; Dysentery supervened: after some days there broke forth under the arm-pits, about the loins, the groins, and the neck, pustules and vesicles, full of green purulent matter, which eroded the flesh, and at first gave hopes of a crisis: but the Dysentery not abating, and the fever increasing, with a phrenzy, this  
case



case terminated like all the former—*v.* Obs. ii. & iii. in p. 101. & seq.—No evidence can speak more decisively than this: we shall have occasion hereafter to hear the sentiments of this author relative to Contagion.

There might, at a first view, appear to be some difficulty in determining the precise nature of the Epidemic, of which Morton makes mention in his Appendix, and in which he found the bark and opium so efficacious: he has given no exact detail of symptoms, and only a very general account of the Epidemic itself. However, from all circumstances considered, and from his own observations on the subject, I have little doubt in referring it to this combination:—the fever accompanying the disease would appear, from his account, to have been highly contagious, and attended often with very distinct remissions and exacerbations; he describes it also as very fatal, three or four hundred dying every week during its acme: the fatal days were usually the 14th, 17th, or 20th days:—“*Fatis cedebant, non autem absque siti inexplebili, aphthis albis vel nigris, pulsu celeri, deliriis, cæterisque febris Epidemiæ indiciis.*”—Morton and Degner make mention of a circumstance



common to both their Epidemics, namely, that during the height of their prevalence scarce any other disease was to be seen. Degner says, “*Paucissimos novi, qui alio quam ipso hoc morbo contagioso, lecto adfixi decubuerant;*” and Morton observes, that during its acme, “*vix quemquam alium morbum obstinuisse.*”\*

There is no one author, whom I have hi-

\* This is a fact relative to epidemic contagious diseases, which Willan greatly doubts, for he remarks, p. 105 of the Diseases of London, that “*Experience affords sufficient reason to controvert the truth of a position made by several medical authors, that epidemical contagious diseases succeed each other in a certain order or series; some of them predominating for a season to the exclusion of the rest. On the contrary, it is in general found, that the Small-pox, Measles, Scarlet Fever, and Hooping Cough, become epidemical about the same time, and continue their progress, though not always in equal degrees or proportion.*” This statement of Willan’s is undoubtedly true, in respect to the diseases specified by him, for these are genuine Exanthemata, and are no way dependent on any constitution of the atmosphere for their prevailing epidemically: not so, the disease treated of by Degner and Morton, for it requires a particular condition of that element for its general prevalence; and when that condition decidedly exists, it is not likely that other epidemical diseases should co-exist, which may be dependent on a different state of the same element.

therto



therto perused, that more distinctly declares the real nature of the contagious Dysentery than Etmuller, whose observations, though introduced here, unavoidably anticipate the subject of the subsequent chapter.—After investigating the general productive cause of Dysentery, which he attributes to some acrid substance irritating the intestines, he observes, Tom. ii. p. 149, “ Porro ratione hu-  
 “ jus causæ materialis, (i. e. acris cujus-  
 “ dam in intestinis) distinguenda est ante  
 “ omnia Dysenteria, dum alia est benigna,  
 “ vel maligna. Benigna ut plurimum *sine*  
 “ febre est, item *sine contagio*, et tantum  
 “ sporadicè, seu sparsim quosdam saltem  
 “ homines in hoc, vel isto loco affligit:  
 “ Maligna verò Dysenteria, ut plurimum  
 “ conjuncta est cum febre malignâ, inter-  
 “ dum pestilentiali, simulque Epidemicè  
 “ grassatur, ac sæpe integram regionem de-  
 “ vastat, et potenti contagio se multipli-  
 “ cat.” P. 142, he further adds, “ In ma-  
 “ lignâ sæpius est conjuncta febris plus mi-  
 “ nus intensa, eaque interdum petechialis.  
 “ Lingua subalbida, mucilagine densè ob-  
 “ sita, et si vehementius æstuet corpus, ni-  
 “ gra etiam et sicca observatur, unâ cum  
 “ *summa virium prostratione.* Grassatur



“communitèr in incolas instar Pestis,  
 “estque contagiosa, seque tunc temporis  
 “sub formâ *febris malignæ* multiplicat.”—  
 Now, that he and other authors, in using  
 the term malignant fever, thereby in ge-  
 neral understood a contagious fever, or our  
 Typhus, will appear by the following pas-  
 sage, extracted from the same author:—  
 p. 376, in speaking of malignant fevers he  
 says, “Porro febres malignæ sunt vel con-  
 “tagiosæ, vel quæ adstantibus aliis nullam  
 “noxam inferunt; frequentissimè autem  
 “simul sunt contagiosæ, aliosque inquinare  
 “solent; rarum est, febrem propriè dictam  
 “malignam, si gradus sit excellentior, non  
 “esse contagiosam.”

In an essay on Epidemic Diseases by J.  
 Rogers, he furnishes us with very satisfactory  
 evidence of the real nature of that species  
 of Dysentery now in discussion: he noticed  
 and has very clearly marked the constant  
 connection which existed between the ende-  
 mial Epidemic fever, or malignant Typhus  
 of Cork, and the malignant Dysentery: p. 4,  
 he states the years in which this destructive  
 fever prevailed, during which space of time,  
 he observes, that Dysenteries of a very  
 malignant sort were common among us:—



p. 5, he states that the winters of 1728, 9, and 30, (during which time this fever was again renewed) were notoriously infamous for bloody fluxes of the worst kind. Small-pox also prevailed at the same time very fatal and malignant.—P. 32, he remarks that these Epidemics of fever, Dysentery, and Small-pox, did not admit of the treatment recommended by Sydenham, but on the contrary, yielded to nothing excepting the highest alexipharmac course of medicine, and the most generous rule of diet. P. 57, he observes that evacuations by bleeding, vomiting, or purging even in a moderate degree, have so depressed the patient under this fever, that the most generous, warm, and active cordials have not been able to raise him: those who recovered of the Small-pox at the same time, owed their preservation to the same treatment, viz. to blistering, warm cordials, and sack whey:—p. 63, he adds, I must also take notice, that the Dysenteries, which kept pace with this Epidemic fever, seemed to partake of the same common cause, and yielded most happily to much the same manner of treatment, namely, the warm, generous, and cordial management. It was very common,



he says, to observe persons seized with this complaint, from the very *first*, to labour under great *depression of spirits*, langour, anxiety, &c. ; and all such grew sensibly worse upon the common method of bleeding, purging &c.—But then, he properly adds, we must distinguish between Dysenteries of the Epidemical kind, and such as are produced by particular, and accidental causes, as excess, stoppage of perspiration, &c. These two last authors leave little room for speaking to evidence: their testimony is so clear, distinct, and decisive, it speaks for itself: I shall accordingly omit all unnecessary observations, and proceed to conclude this section with some account of the two species of Dysentery, which Willis has treated of, under the names of the Cruenta & Incruenta, and upon which some comments have been made, not warranted by the text. As Zimmermann and some others have chosen to denominate one of these species of Willis, a malignant Dysentery, I have been induced to defer the consideration of both to this section: we may judge from their histories whether either be entitled to a place under this combination. Willis in his Pharm.  
Ration.



Ration. part 1. relates that two very different species of Dysentery appeared in the years 1670 and 71. “ In unâ sedes aquosæ, & quasi limpida cum subitâ virium prostratione; in alterâ cruentæ, attamen tolerabiles existunt. Anno 1670, circiter æquinoctium autumnale, quamplurimi Dysenteriâ incruentâ verùm atroci admodum laborabant. Affectus, & subito, et frequentèr absque manifestâ occasione invadens, laborantes cum vomitu immani, et sedibus crebris et aquosis, cito in maximam debilitatem, inque spirituum horrenda deliquia, & virium omnium prostrationes redigebat. Novi, inquit, plures pridè satis sanos, et valde robustos, intra duodecim horas, morbi hujus tyrannide adeo miserrimè dejectos, ut cum pulsu debili, et exili, sudore frigido atque respiratione anhelâ et elatâ, jamjam moribundi viderentur. In curatione, nulla evacuatio juvabat, quinimo phlebotomia, vomitus, catharsis nunquam non nocebant, verùm remedia ferè tantùm cardiaca, eaque calidissima.” The nature of many of the symptoms here detailed, and the great severity of others, would at first sight appear to justify Willis in calling this

disease



disease a Dysentery, and Zimmermann in entitling it malignant: if so, it is but fair to enquire, how it could happen, that Willis and Sydenham, two eminent co-temporary physicians and accurate observers, resident in the same city, should so widely disagree in their histories of a disease, to which they give the same name, and which they describe as occurring in the same year. I have already mentioned the surprise of Pringle at this extraordinary difference, and the observation it drew from him; Morgagni appears at first scarcely less astonished than Pringle, but is not so slow in devising an explanation of the occurrence: he supposes that two physicians practising in so large a city as London, might meet each with a different form of the same Epidemic, so that either in describing what he saw, should make no mention of that which the other had observed: and accordingly one would speak of watery fluxes only, while the other might notice mucous fluxes alone. The possibility of such a case, he supports on the supposition, that the bodies of those inhabiting one quarter of the town, might, according to constitution, air, mode of life, &c. abound with a more  
fluid,



fluid, and those of another quarter, with a more lentescent, and mucous serum. *vid.* p. 70 and 73 of the 2d vol. of Alexander's translation.—But neither the former supposition, nor the latter hypothesis, are at all admissible: and therefore we must have recourse to some other mode of explaining so singular a circumstance.—Let us see whether the *Dysenteria incruenta* of Willis be at all entitled to the name of Dysentery, and whether that of Cholera would not more exactly suit it: should such prove to be the case, it will easily be seen, how it has happened that Willis and Sydenham should appear so inconsistent. What of the characteristic symptoms of Dysentery are present? scarcely one; the disease came on suddenly, and often without any manifest cause: this is not like Dysentery; excessive weakness, and horrid faintings were quickly induced: How? by means of *dreadful vomiting* and frequent, *watery stools*: this surely is not Dysentery, besides that the rapidity of the disease and its treatment will put the point beyond all question; and there are no tormina, there is no tenesmus; indeed the only point of resemblance is the frequency of stools, and yet these differ  
from



from the Dysenteric both in quantity, and quality. Now, there is every circumstance and symptom present that can warrant us in asserting the disease to be Cholera: the season of the year, the mode of attack, the conjunction of vomitings and stools, the debility and other effects immediately consequent, the rapid fatality of the disease, and the mode of treatment which could alone check its progress: all unite together in pronouncing the disease to be Cholera, which, as Willis has declared of the Dysenteria incruenta, we know to be devoid of contagion, for he observes immediately subsequent to the account above detailed,

“ Porro hic loci quamvis plurimi agrotabant, morbus haud per contagium propagari, sed tantum prædispositos afficere videbatur, nam in eâdem familiâ cum affectis conversati, haud magis quam eorum contubernia vitantes corripiebantur.”—Indeed Willis himself plainly mentions that there was nothing peculiar in the Epidemics of 1670 & 71, for he says, that he has often, and long since observed, that there are two different species of flux, which almost every year appear in London about autumn; in one the stools are watery, and



as it were, clear, with sudden prostration of strength: in the other they are bloody, but the strength remains tolerable; these are obviously Cholera and Dysentery, the former of which usually takes precedence in point of time, as it generally appears earlier in the autumnal season.\*

That no less intelligent writers have fallen into the same error with Willis, of confounding Cholera with Dysentery, though they point out at the very instant the great danger of treating them alike, will appear by a short extract from Zimmermann, whom we have seen countenancing Willis by referring his *Dysenteria incruenta* to the malignant species of the disease.—Dr. And. Wilson in a sensible treatise on autumnal Dysentery, has pointed out one passage, where he seems to reduce Cholera to the class of Dysentery, if I am not misled, says Dr. Wilson, by what Zimmermann states in p. 164; “The

\* Hoffman, whose accurate Diagnosis between Cholera and Dysentery I have already given, understood full well the nature of the *Dysenteria incruenta*, as appears by the very first passage in that chapter whence the Diagnosis has been taken: “*Cœlius Aurelianus, inquit, Choleram per fellifluam passionem interpretatur, et Willisius Dysenteriam incruentam adpellat.*”—



“ stools are sometimes inconceivably copious,  
 “ and this is so very dangerous a circum-  
 “ stance, that the patient will appear in a  
 “ dying condition in the space of twelve  
 “ hours, and often really dies in that time.”

Dr. Wilson properly remarks, that if this is a Cholera here described, it is a dangerous mistake to confound it with Dysentery; for if treated with cooling laxatives, and not with cordials and anodynes, the patient must inevitably die in a very short time. In p. 237, Zimmermann furnishes further proof of having confounded these diseases in name, though not in treatment: he says that V. S. is entirely to be rejected, emetics too, and purges must be omitted, when the excrements are quite watery, and so *inexpressibly copious*, that the patients seem within the space of twelve hours, as if they were dying, (and sometimes actually die) in which case all evacuating medicines should be avoided, and recourse be immediately had to strengthening and constipating medicines.—Willis mentions that spirit of wine burnt on sugar was a popular and as it were epidemical remedy in the *Dysenteria incruenta*, “ atque in tali Dy-  
 “ senteria, he adds, ferè semper proficuum,  
 “ etsi



“ etsi in alterâ cruentâ indifferenter usurpa-  
 “ tum, sæpe noxium deprehenderetur.”—  
 We shall now proceed to consider the nature and character of Willis's second species, the *Dysenteria cruenta*, and see how far it agrees with Sydenham's Epidemic of the same year; for it must be observed here that when Pringle takes notice of the great apparent disagreement between these authors, and speaks of it in such strong terms, as to say, that they only seemed to agree in the name of the disease they are describing, he could only have alluded to that species already treated of, and respecting the nature of which, if I have been correct in my suggestions, these authors in fact only differed in the name, they had given it; for we may plainly perceive by that account which Willis has left of the *Dysenteria cruenta*, that this form of the disease differed in no essential respect from that described by Sydenham.

Willis relates, that a very cold winter, and as hot a summer, succeeded the autumn of 1670; and in the autumn of 1671 he says, “ cum Dysenteria cruenta Londinenses  
 “ afflixit, febris intermittens tertianæ Hemi-  
 “ tritææ similis reliquam Angliam infesta-  
 “ bat:”



“bat:” to this epidemic fever he attributes, and with justice, many of the characteristic features of the *Dysenteria cruenta*, in the same manner that Sydenham does to the *febris Dysenterica*, in which respect, and several others, these fevers would appear to be identically the same. With respect to the *Dysenteria cruenta* itself, Willis remarks as follows: “*a prima hujus morbi invasione, plerumque cum ventris dolore, & torminibus, cruor copiosè et crebrò dejiciebatur; vigiliæ pertinaces cum febre et siti ingenti urgere solebant, usque tamen vires mediocritè constabant, ita ut affecti postquam circiter hebdomadam ægrotantes, fere vigesies quotidie dejecerint, lecto exurgere potuerint.*” In this epidemic, he observes, that there occurred cases of every degree of severity, from slight *Diarrhœa*, to the most severe tormina, and profuse hæmorrhagy: no such variety occurred in the former species. There can be no necessity for drawing any contrast here between these two diseases, very different indeed in their nature, though Willis conceived them to be species of the same; the short history given of each will fully answer that purpose; I shall therefore close  
 this



this view of Willis's Epidemics with his concluding observation on the Dysenteria cruenta, after an enumeration of the different circumstances which contributed to its production: "Propter hujusmodi apparatus, " inquit, Dysenteria Londinensis ordinaria " et non admodum maligna exoriri solet, " quæ licet præ sedibus cruentis, horrenda " statim & plerumque diuturna est, haud " tamen valdè contagiosa, aut sæpius lethalis existit: aliquando tamen, hic morbus " virulentus, & quasi pestilentialis, plures " interficit, & miasma suum per contagium " latè explicat." Though I shall offer no explanation of this last passage till we come to the subsequent chapter, I thought it better to introduce it here, than hereafter to give it detached from the subject to which it belongs.

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## SECT. II.

### PROOFS FROM THE GENERAL HISTORY OF THE DISEASE.

HAVING discussed at some length a matter which will probably appear to most a digression from the proper subject of the last



section, I shall now proceed to consider such proofs as may be suggested by various circumstances in the general history of the Dysentery, and which, perhaps, may tend to demonstrate in a manner more satisfactory than those already adduced, the existence of Tphus Fever in combination with that disease. We may advantageously commence with a few extracts from an author of considerable reputation and credit, whose observations may be relied on, as the result of an extensive experience, afforded him by great and official opportunities of seeing the disease in its worst forms. Pringle thus commences his account of the Dysentery: he says, that  
“ early in the season some cases of the dis-  
“ ease occur, but never so bad, nor nearly so  
“ frequent as towards autumn: at that time  
“ they become *epidemic* and *contagious*: they  
“ are always most numerous and worst after  
“ hot and close summers, especially in *fixed*  
“ *camps*, or when the men lie wet after a  
“ march in warm weather.”—The more early cases, we may here well suppose, were cases of the more simple Dysentery, being of a mild character, and not very frequent, while the disease in autumn, from the strong disposition of that season for its production, became



became epidemic, and *from another cause*, contagious; for it must appear obvious, from what he adds, that the cases were then most numerous, because of the men lying wet after a march in warm weather, and afterwards proved so bad, from such cases being confined in *fixed camps*, the great source of contagion and malignity in diseases of the military.

We have already noticed the doubts which Pringle entertained of the truth of Sydenham's assertion—that the disease, even in the majority of cases, began without any feverish sensations; every intelligent man should, no doubt, be slow to suspect the accuracy of Sydenham's observations, yet should he be equally ready to respect the motives, and give due weight to those grounds which might naturally lead the mind to entertain such suspicions. Pringle had fair grounds in his own experience for so doing: his motives cannot then be called in question: for too frequently he had occasion to observe the disease after its character had been materially affected by a residence in *fixed camps* and military hospitals, where indeed it seldom attacked without well-marked fever; the following passage will more than warrant



this assertion, where, after speaking of a low and dangerous kind of fever, which sometimes accompanies this disease, he says, “ the  
“ most fatal sort of fever, which so often  
“ attends the Dysentery of the army, though  
“ not *essential* to it, is the *hospital* or *jail*  
“ *distemper*, which at all times infects foul  
“ and crowded wards, but never so much as  
“ when they contain men labouring under a  
“ putrid disease.” Nothing can be more distinct, nothing more to the point, than this; the subsequent passage is scarcely less so: after taking notice of that epidemic of which Degner has treated, and to which I have already referred, he makes this remark well worthy of attention;—“ As to the violence  
“ of the symptoms mentioned by that au-  
“ thor, I own it exceeds any thing I have  
“ observed upon the first seizure, but when  
“ a number of men, *even with the most fa-*  
“ *vourable cases*, have been crowded into  
“ the hospitals of the army, the Dysentery  
“ has *then* appeared with all the virulence  
“ that it did at Nimeguen.”—These passages need no comment, and indeed it would almost appear unnecessary to adduce any thing further in proof of our point, but as a review of other authors cannot be uninteresting,



ing, I shall not think a good cause injured by the strength of its forces. Besides that already quoted, Pringle adds, that “ when  
“ mortification takes place, the distemper is  
“ most contagious, whether in producing a  
“ simple Dysentery, or one combined with  
“ *the common hospital fever;*” and although he objects to the distinction of Dysentery into benign and malignant, yet he allows that such Dysenteries as occur in the spring, are, like the fevers of that season, attended with more inflammation, and less putrefaction, and that most cases, if taken at first, and properly managed, will have a favourable issue. Pringle divides the treatment of Dysentery into three parts, adapted to three states of the disease:—the 1st, when recent; the 2d, when of long standing; and the 3d, when from the putrid fomes within the body, or the foul air of the hospital, a malignant fever is joined, and a mortification threatened. For the treatment of this last combination, he refers to that of the *malignant fever of the hospital*.

The author I shall next notice, furnishes us with evidence scarcely inferior to the last. In the Appendix to the 3d vol. of the *Acta Nova N. C. in Germania*, we have from



Grimm (an author referred to by Dr. Cullen) three distinct essays on three different diseases, which appeared in the same territory in subsequent years, and which (particularly the two first) from their characters and the order of time in which they followed one another, would seem to have been a good deal connected. The first essay contains the history of a malignant fever, *quæ in urbe & agro Isanacensi annis 1759, 60 & 61, Epidemicè grassata est*; the second gives the history *Dysenteriaë malignæ, quæ autumnò an. 1760 & 61, in iisdem locis grassata fuit*; and the 3d gives an account of the malignant Epidemic measles of the year 1762. The fever began in December of 1758, and raged till the end of the spring 59, when it remitted somewhat of its violence, though he remarks that for the remainder of that year, “*semper aliqua morborum occurrence in malignitatem pravitas remanserit.*”—In the winter, and spring months of 1760, a fever of a similar nature returned, which in the August following gave way to another plague, more destructive than that in the winter, to wit, the Dysentery, which continued to the end of November, when it again passed into the malignant



lignant fever [transiit hæc rursus in malignæ indolis febrem illam.] That this Dysentery was such a combination as I allude to, will appear more than probable from the subsequent passages ; p. 173, he thus begins his account of the Dysentery : “ Alter morbus, qui  
 “ tantâ atrociâ plurimos est adortus &  
 “ multos sustulit, Dysenteria, cum malignâ  
 “ suâ febre copulata, fuit.” The place and circumstances under which it arose and afterwards spread, will tend strongly to confirm this supposition ; “ Comparuit primum  
 “ inter *pauperes*, in ultimis suburbii occidentalis domiciliis, habitantes: lento  
 “ pede sensim adiit in anteriori, urbique  
 “ parti vicinæ degentes, donec tandem  
 “ ipsius oppidi cives, versus septentrionalem  
 “ plagam adflixit:”—it appears by a reference to another author that this disease had been accustomed to take the same route, as that now traced out. He further adds in speaking of the subject of this malady,  
 “ Memorabile præ reliquis puto, morbum  
 “ eo tempore imprimis familiarem vitæque  
 “ infensum fuisse spurcitiei deditis, pedibus  
 “ nudis incidentibus, in conclavibus angustis, obscuris, madidis, mox frigidulis,  
 “ mox vehementer calefactis viventibus,



“ tenui victu & duro utentibus, in ratione  
 “ spurcitiei, angustiaeque hypocaustorum,  
 “ vitaeque miseriae.” The commencement  
 and whole course of the disease, as he de-  
 scribes it, plainly point to Typhus fever in  
 combination with the Dysentery; among a  
 variety of other circumstances he remarks,  
 p. 177, “ comprehendit mox morbi initio  
 “ ægros debilitas tanta, ut nec pedibus initi,  
 “ nec caput tenere erectum potuerint: aliis  
 “ paullò tardius accessit insignis illa virium  
 “ imminutio:” P. 181, in speaking of the  
 manner in which death took place, he says,  
 “ Ratio vitam ponendi, omnibus ferè eadem,  
 “ ac in febribus malignis fuit.” The man-  
 ner in which the Dysentery is here described  
 to have propagated itself, is so strikingly  
 like to the Typhus fever, that no one can  
 miss the resemblance; this circumstance is  
 still more pointedly marked by the writer  
 next to be mentioned.

Roederer, in his treatise de Morbo Mucoso,  
 gives a distinct detail of several kinds of Dy-  
 sentery, and among others a very clear and  
 satisfactory one of that species, which he  
 denominates the febris mucosa acuta maligna  
 (v. p. 63): this, from the symptoms de-  
 scribed, and other circumstances recorded of  
 it,



it, is obviously that combination we are now treating of. The disease originated in the camps, and prevailed principally there, and in the hospitals of a town that had been long in a state of seige, and was now reduced to great distress from the multitudes crowded within its walls, and from a dearth of provisions: indeed it appears to have been attended with all and the worst symptoms of malignant fever; p. 30, in enumerating the causes which led to the disease, he says “*Locuples morborum semini proptuarium fuit*” “*nosocomium castrense, magnâ semper*” “*ægotantium frequentia stipatum, neque*” “*ventilatore munitum.*” P. 64, he adds, “*Exinde populabatur per urbem, pau-*” “*perum tabernas, & sordida plebeiorum*” “*cubilia pulsabat, tandemque in reliquum*” “*populum diffusa ruebat.*” The most fatal days in this disease were the 9th, 14th, and 21st.—I shall have occasion hereafter, again to refer to this epidemic, when we come to speak of hospital gangrene.

I shall now conclude this section with an interesting detail of facts relative to the general history of the Dysentery, after offering such observations as they might naturally suggest, and hazarding a few plausible,  
if



if not very probable, conjectures.—Heberden, jun. in his very useful work on the increase and decrease of different diseases, as founded on the Bills of \* Mortality in London, has furnished us with the following statement respecting Dysentery:—In the 17th century the number of deaths, under the title of bloody flux and griping of the guts, appear never to have been less than 1000, and in some years to have exceeded 4000, and for 25 years together, from 1667 to 1692, they every year amounted to above 2000: but from the beginning of the 18th century things were materially changed; after the year 1733, the article of griping of the guts was joined to that of the colic: taking then the three diseases of bloody flux, colic, and gripes, we may observe their decrease to be nearly as follows:

\* Though it must be allowed that the Bills of Mortality are not to be relied on in any nice calculations respecting the generality of diseases, yet must it be admitted that every credit may be given to their statements respecting diseases of so obvious a nature as Dysentery: this is not like the article of Consumption, Convulsions, &c. where the deaths are often set down to the credit of a symptom, while the original disease is overlooked; Dysentery has characters too plain to be so mistaken.

*The*



*The average number of deaths was annually about*

From the year 1700 to 1710.....	1070
1710 to 1720.....	770
1720 to 1730.....	700
1730 to 1740.....	359
1740 to 1750.....	150
1750 to 1760.....	110
1760 to 1770.....	80
1770 to 1780.....	70
1780 to 1790.....	40
And from ..... 1790 to 1800.....	20

The proportion of deaths in the beginning, middle, and end of the 18th century is as follows :

*Beginning, 1100; Middle, 135; End, 20.\**

Even in the years 1762 and 1780, when modern physicians have described the Dysentery as epidemic in London, the amount of the same three articles was, in the first year, only 209, and in the last 93.

\* Perhaps the improper treatment of the disease might formerly have added very considerably to its mortality : that circumstance alone, however, cannot, though granted, account for the vast disproportion of deaths by the complaint in the different periods of this century.

These



These are facts relative to the gradual decline of Dysentery, more especially of its mortality in London, not to be paralleled in the history of any other disease on record; the Plague furnishes no parallel case, because that more usually either rages violently, or disappears altogether: not so the Dysentery, which has declined, not so much in the numbers attacked by it, as in the numbers dead of it: it would almost appear to be rendered harmless, and to have lost its sting. The cause of so great an alteration in the health of the people of England, Heberden attributes to the improvements that have gradually taken place, not only in London, but all great towns, and in the manner of living throughout the whole kingdom, particularly with respect to cleanliness and ventilation.— The influence and operation of this assigned cause, I am by no means disposed to dispute: on the contrary, I am perfectly satisfied of its efficacy in the production of these happy effects; however, in admitting this, I must state that I do not conceive it to have been the *immediate*, but merely the *remote* agent in effecting this change in the mortality of Dysentery. The influence of this cause on Dysentery was exerted, as I am disposed to think,



think, through the medium of another disease, which, if I am correct, is the great source of *danger*, and of *contagion* in the former: its operation, I mean, was primarily visible on the Typhus, or malignant contagious fever, which this cause is all powerful in mitigating and preventing: by that means acting secondarily on the Dysentery, which, being parted from its associate, is no longer an object of terror to the patient or his attendants. The truth of this opinion rests on those facts relative to the Plague, which are to be found in the same author: from the survey he has taken of that disease, it would appear that it was seldom absent from London previous to the burning of the greater part of that city in 1666, and afterwards it never more visited that metropolis; it appears also by his statements, that Dysenteries, antecedent to that period were almost uniformly exceedingly malignant and destructive, and that in a few years subsequent to that event, they wonderfully decreased in frequency and severity.\* Now I conceive it

\* Previous to 1666, Dysentery was very fatal in London, and though it still prevailed from that time to 1692, a space of 25 years, it was yet much decreased in mortality;



it fully demonstrated by Heberden's Researches (to which I must here refer), that though the Plague of London, and of other places, was nothing more than the malignant contagious fever, exalted by auxiliary aids to such a pitch of destructive violence, as well to merit that name: the identity of the two diseases he satisfactorily supports by authority and facts. The singular and rapid decline in the mortality of Dysentery, at the period when the Plague ceased to visit London, or rather when fevers lost much of

tality; during this interval, while we give every credit that may be due to the diminished influence of the Plague, in accounting for this decrease, we have yet to counterbalance this cause by the operation of another of an opposite tendency, and which, though ultimately conducive to the best effects, must at the instant have acted towards a material aggravation of dysenteric epidemics: I mean the condition to which for a time the inhabitants of London must have been reduced by the conflagration of the greater part of their city.—Morton describes the Dysentery as epidemic from 1666 to 1672, and from his account, it would appear to have been at first exceedingly fatal and contagious, though less so towards the end of that period; and from Sydenham's description of the Dysentery in 1669, 1670, 1671, and 1672, it appears, except in a few occasional cases, to have been devoid of contagion: Willis's *Dysenteria Cruenta* of the year 1670 coincides also with this statement.

their



their malignity, cannot but strike the mind of the most inattentive, and lead him to conclude, that the immediate cause of this change in the history of Dysentery, was owing to the diminished influence and dominion of the malignant fever in London.\*

Having, as I trust, demonstrated in a manner not altogether unsatisfactory, the existence of Dysentery in its simple state, and in combination with intermittent, remittent, and typhus fever, I shall now proceed to the consideration of a question, hitherto involved in much needless obscurity, but which I am inclined to hope the steps already taken may tend, in great mea-

\* Is it not a general fact, that the malignity of Dysentery has ceased with the disappearance of the Plague? Is there any thing so common among medical writers, down to the 18th century, as accounts of malignant and contagious Dysentery? Do not their writings at the same time abound with histories of the Plague, or of very malignant fevers? Is not the reverse true with respect to more modern writers?—Indeed it must appear pretty obvious, that the malignity of the one disease has declined in proportion to the diminished influence of the other, and that either has now little chance of being exalted to the eminence of a Plague, except in those countries where the same causes still exist, which formerly gave origin to it in London and elsewhere.—Happy effects of cleanliness and precaution!

sure,



sure, to dissipate, and enable us to obtain a closer and more distinct view of those difficulties, which should alone impede our researches into the nature of the contagion productive of Dysentery.



## CHAPTER IV.

## CONTAGION OF DYSENTERY.

## PROOFS

THAT THE COMBINATION WITH TYPHUS IS  
ALONE CONTAGIOUS.

ANY person in the least conversant with the writers on Dysentery, must have perceived the most striking contradictions in their belief, and assertions on the subject of its contagion: he will find some denying it in toto, others as positively declaring it to form one of the strongest features of the disease, while a few, more moderate in their sentiments, though more undecided in their opinions, may be observed to waver between these extremes, and who, in admitting, or excluding contagion from some particular epidemic, do not venture to pronounce it absent, or present in all others, nor attempt to point out the particular cases, in which we might expect to see the influence of that agent exerted. If we

H

look



look to authority as the basis on which we are to rest our opinions (and to what besides this, can we resort in the decision of such a question?) in the circumstances just described shall we find our criterion situated; and what is worse, should we be disposed to weigh the opposite authorities against each other, the names of respectability appear so evenly balanced on each side the beam, that it will not, it cannot preponderate either way. Thus, if we suppose that the authors who have written on Dysentery, described under that name the same unvarying and identical disease, do we perceive that on the ground of authority we can arrive at no decision, inasmuch as the opposite assertions destroy each other. But if it be true, as I have stated, that there are forms of this disease very different in their nature and characters, and that these authors, as has been shewn, under the same name of Dysentery, described these its different forms, then may some hopes be entertained, that we shall be able to reconcile the most direct contradictions and opposite assertions, without injury to the character of either party; for as each described the disease he met, with the accuracy and fidelity of an historian, they  
must



must necessarily have differed from each other in their description of the disease, and in their opinions respecting it, because while one found the disease in its simple state, others saw it only in its state of combination. That such must have been the case, ought to have been supposed, from the very circumstance of men, well known for their veracity and talent for observation, *appearing* to contradict each other in so direct a manner, and to form the most opposite opinions on the *same subject*; and that such was the case, the facts already adduced have, I trust, been sufficient to satisfy any unprejudiced mind. If such then be the case, how grateful must it be to the mind of every man to think, that he may on this, as on other occasions, give some credit to the assertions of his fellow-men, without incurring much danger of running counter to the truth, or of falling into any material error.

The extremes of opinion entertained on the subject before us, are, on the one hand, that the disease is never contagious, and on the other, that it is always so, and that this property is owing to a specific virus. On this, as on all such occasions, we shall



find the truth of the adage, “*medio tutissimus*,” for between the extremes of opinion now mentioned, a proposition may be stated which shall embrace a part of each, which shall be agreeable to fact, and which shall meet with a general concurrence. The proposition, which it is my intention to uphold, has been already often alluded to, and it is this: That THE SIMPLE DYSENTERY IS OF ITSELF NEVER CONTAGIOUS, NOR THE INTERMITTENT AND REMITTENT FORMS OF THE DISEASE; That THE COMBINATION WITH TYPHUS IS ALONE POSSESSED OF THAT PROPERTY, and that THAT PROPERTY ORIGINATES, NOT IN ANY VIRUS SPECIFIC TO DYSENTERY, BUT IN THE CONTAGION OF FEVER.

The truth of these propositions I shall endeavour to establish in the following manner: We have already seen who the authors are that describe the disease in its different forms: of these and of a few others, not as yet referred to, I shall take a general survey, and after stating the sentiments of each on the subject of contagion, and contrasting them with each other, we shall, I believe, find reason to conclude, that such authors as describe the disease either in ITS SIMPLE FORM, OR IN COMBINATION WITH INTERMITTENT.



MITTENT AND REMITTENT FEVERS, uniformly pronounce it, NOT CONTAGIOUS; while those who met it in COMBINATION WITH TYPHUS FEVER, as regularly and decisively declare it TO BE so. The inference unavoidably consequent on such a conclusion, must be that the truth of the propositions, above stated, rests on the fairest and strongest grounds.

To proceed therefore I shall begin by considering the sentiments of that man, whose definition of this disease has had no small share of weight in influencing and deciding the opinions of others; and should I be able to shew cause, why he was naturally led to embrace such sentiments, without necessarily inferring that he was correct in so doing, I hope that his authority will no longer stand in the way of truth, nor exert improper influence over the minds of others. This is my only motive for taking any notice here of the opinion of Dr. Cullen relative to the Contagion of Dysentery, inasmuch as it has been, and is still my intention (with the exception of this deviation) to confine the present survey to original observers of the disease, to men who described it as they saw it, and not as they found it described;



among these Dr. Cullen does not, nor pretends not, to rank, for he spoke of the disease not from his own, but from the experience of others. On this account his authority is on a level with those, whose writings on the subject he has consulted, and whose opinions respecting it he has adopted: and as it was his uniform practice, after giving the definition of any disease, to enumerate those authors he had consulted, so he has himself furnished us with the means of judging of the weight that should be attached to his opinions relative to this disease. What these opinions are has been already in great measure pointed out: they are pretty plainly specified in the first sentence of his definition, which states the Dysentery to be a "Pyrexia contagiosa:" he even goes so far as to say, that he thinks it doubtful if the application of cold does ever produce the disease, unless the *specific contagion* has been previously received into the body. v. vol. III. p. 119. Now the reason why Dr. Cullen has adopted these opinions, will be pretty obvious, after enumerating the names of those authors to whom he has referred on the subject of Dysentery: I need but mention Pringle, Degner, Roederer, Zimmermann,



Zimmermann, Grimm, \* Helwitch, Bontius, Cleghorn, &c. &c. ; most of these, have already been quoted, and all of these excepting Cleghorn, who described the intermitting variety of Dysentery, will be found on an examination of their writings, to have seen the disease in its combination with Typhus, which they all with one voice (making the same exception) pronounce to be contagious. Had these been the only authors, whose opinions or observations relative to this disease could be relied on, Dr. Cullen would have been perfectly justified in the definition he has given of it, but such is far from being the case, and had Cleghorn stood single, it ought to have been shewn, why he has said nothing of the contagious property of the intermitting species of the disease. As it stands, we may perceive that Dr. Cullen has given us a definition of the combination with Typhus, in place of the definition of the disease itself.

In conducting this enquiry into the opinions of original writers on the Contagion of

\* This author's treatise on Dysentery may be found in Haller's *Disputationes ad Morborum Historias*, &c. As there was nothing very particular in it, I have made no quotation from the work.



Dysentery, I shall proceed as nearly as possible in the order already observed in treating of the different species of the disease, and shall accordingly commence with those authors who have described the disease in its simple state and in combination with intermitting and remitting fevers.

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### SECTION I.

THE SIMPLE DYSENTERY AND THE INTERMITTENT AND REMITTENT FORMS OF THE DISEASE, NOT CONTAGIOUS.

MOSELEY, who had a very extensive experience of this disease in its more simple and remittent forms, entertains sentiments relative to its supposed contagious property very different indeed from those of Dr. Cullen; and that he is not far from the opposite extreme may be judged by what follows:

“ As to *contagion* from *infection* in Dysentery, I must confess, says Moseley, I never saw an instance of it; nor do I believe there is any such thing, either in this disease, or in the Plague, or in any other  
 “ pestilential



“ pestilential fever. I cannot even venture  
“ to conjecture what that agent is, which  
“ determines the species, and spreads epide-  
“ mic diseases;” and after several indirect  
insinuations, he even seems to decide against  
the agency of contagion in any instance.—  
“ It has often happened, he observes, that  
“ hundreds of men in a camp have been  
“ seized with a Dysentery, almost at the  
“ same time, after one shower of rain, or  
“ from lying one night in the wet and  
“ cold. And yet it often happens, he adds,  
“ that the Dysentery begins with a few  
“ people, and spreads itself by degrees until  
“ a multitude are affected, and the disease  
“ becomes general.” p. 285. This last obser-  
vation is well worthy of remark: it plainly  
points to the agency of a cause which Moseley  
has affected to deny; for the Dysentery has  
very generally, and I believe very truly, been  
supposed to originate from the operation of  
cold and of contagion; the Dysentery  
arising from the first cause, seizes many of  
those who have been exposed to its influence,  
almost at the same time: while that arising  
from contagion begins, like other epidemic  
contagious diseases, with a few, and gra-  
dually extends its ravages. Moseley, in his  
last



last observations, has exactly described both, of each of which several instances might, if necessary, be adduced: Degner's Epidemic will be found a complete example of the Dysentery from contagion, and the case of Cromwell's army before Dunbar of the other, for the Dysentery suddenly raged throughout his tents, in consequence of exposure to a few days rain; while the fate of the English army, after the battle of Dettingen, furnishes an instance of both: the night after this battle a heavy shower of rain, to which the troops were exposed all that night, and on the next were encamped on wet grounds: previous to the battle there had been very hot and dry weather; in less than six weeks after, half the army was, or had been, afflicted with Dysentery. In this case the operation of cold gave origin to the disease in the first instance, and contagion, arising from a cause as yet to be investigated, no doubt contributed very considerably in propagating and keeping up the distemper. I have taken notice of all these circumstances for a double purpose: to shew, in the first place, that the Dysentery, as observed by Moseley, was not in his estimation contagious; and in the second place, that  
that



that unconsciously he furnishes us with evidence sufficient to overturn the general proposition he seems inclined to adopt, that the disease *never* is contagious.

“ The Dysentery, says Hunter, did not  
“ appear to be infectious in the hospitals  
“ in Jamaica, nor in the epidemic that pre-  
“ vailed in London in the years 1779 and  
“ 1780 ;” he does not, however, wish to be  
understood to mean that Dysentery is never  
infectious, but thinks there is some difficulty  
in determining a question of this kind,  
“ for, says he, unless the proofs of infection  
“ are clear and decided, they may be easily  
“ confounded with the effects of a cause,  
“ that is generally diffused, and operating  
“ upon all, more or less, such as the cause  
“ of Dysentery must be.”

Willan, who cannot be charged with any prejudice against the admission of contagion, speaks still more decidedly of the epidemics which came under his observation ;  
“ The disease, says he (meaning the epi-  
“ demic of 1800) almost always arose after  
“ some imprudent exposure to cold ;” and  
afterwards he adds, “ Neither in the pre-  
“ sent, nor any preceding period, has the  
“ Dysentery



“Dysentery in London appeared to me contagious.”

Akenside, Cleghorn, and Sydenham, in enumerating the different causes of the disease, take no notice whatever of contagion, though with respect to the latter, we can argue but little from this circumstance, for he scarcely makes mention of contagion existing in diseases most decidedly possessed of that property.\* Huxham, I believe, stands

\* Some cases in Sydenham's Epidemic would appear to have assumed a very bad form, as well from mismanagement as other causes, perhaps too, under circumstances somewhat similar to those mentioned by Sir G. Baker: “Æger, inquit, febricitat, lingua subalbida, quâdam mucilagine densè obsita, & si vehementius fuerit excelfactus, nigra etiam, atque sicca: *prosternuntur admodum vires, dissipantur spiritus, nullum non adest febris malè moratæ indicium.*” Sydenham however, affords us no means of ascertaining, whether in this, or any other form, the disease was contagious. Pringle says, that Sydenham makes no mention of contagion and that Willis expressly denies it: and then attempts to account for it by adding that “as the Dysenteric miasma is of a less catching nature, than that of Measles or Small-pox, so in mild Epidemics, its contagious quality might escape their notice.” Pringle seems on this occasion to have made two mistakes, for their Epidemics were far from being of the mild character, he supposed, and in the next place Willis, as we have seen, though he expressly



stands in the same predicament with the last named authors.

Richter, p. 115, in speaking of contagion, observes, “ To those who maintain that  
 “ there is a specific contagion in Dysentery,  
 “ I have nothing to say. If they believe  
 “ that there is in general a specific catarrhus  
 “ miasma, the Influenza has given  
 “ strong proofs of it. It might also, if  
 “ necessary, be asserted, that the catarrhus  
 “ intestinorum is occasioned by a specific  
 “ contagion. But the belief of this miasma  
 “ has not yet made any essential change in  
 “ the method of treating the Catarrh, neither  
 “ would it have any essential influence  
 “ in curing Dysentery: the belief, therefore,  
 “ or disbelief of this contagion is a  
 “ matter of no consequence whatever; but  
 “ I am surprised that it should never appear,  
 “ except at the end of August and September.”  
 “ His entire disbelief of its existence  
 could scarcely be expressed in more unequivocal terms.

Stoll, though he would appear occasionally to have met with that species of the Dysentery, expressly denies contagion in the *Dysenteria incruenta*, yet as expressly states, that it occasionally operated in the production of the *Dysenteria cruenta*.



tery\* which I deem to be contagious, still decides very positively against the idea of contagion in it: p. 327, part III. he says, “Contagium Dysentericum *pauci* in du-  
 “ bium vocarunt, idque effluvium ex uno  
 “ homine expirans, communicari adstanti-  
 “ bus posse *plerique* credunt.” If it be so, he expresses great wonder how physicians, their assistants, and nurses, should escape the disease for so many years. “At probè  
 “ equidem vovi, inquit, Dysentericorum  
 “ dejectionibus aerem, ex quo omnes tan-  
 “ quam ex communi fonte potant, fœdo pu-  
 “ tore corrumpi, morbosque putridos, per  
 “ eminentiam *nosocomiales* appellatos, invi-  
 “ tare; verùm quòd Dysentericorum exha-  
 “ lationes, *eundem* in aliis morbum produ-  
 “ cant, id quidem arbitror observationibus  
 “ adversari, et magni interesse puto, non  
 “ ignorare, Dysenteriam contagio carere.  
 “ Quo enim animo medicus Dysentericorum  
 “ domos, pauperum præprimis tabernas,  
 “ adibit, de morbi contagio persuasus?”

\* This may be inferred from the following passage in p. 268, part III. “Nonnunquam febris *adest originario-putrida*, non multum ab illâ differens, quam secundario-putridam, ex prægressâ nempe biliosâ, fieri supra narrabam.”



Language somewhat similar to this is employed by Dr. A. Wilson, where he declines making any distinction between an epidemic and an infectious disease, as he knows of no criterion to ascertain infection, when a disease is epidemic: he thinks, besides, that the idea of infection ought to be discouraged. This species of reasoning I cannot but deem very reprehensible, from its pernicious tendency, though probably dictated by a very different feeling: for though in the present instance it be true, that the Dysentery is not of itself contagious, yet do I believe that every one will now admit, since we have got better acquainted with the nature and properties of contagion, that it is of much greater moment, not to be ignorant, that a disease either is, or may become contagious, in as much as we may be led by that knowledge to adopt due and efficacious measures of precaution.

Sir G. Baker, whose Epidemic coincides in most respects with that of Huxham, denies contagion in the greater number of cases, though afterwards in speaking of the necessity of a strict observance of cleanliness in this disease, he specifies other cases, where contagion did operate in producing them,



them, but still under such circumstances as must instantly suggest a well-founded suspicion of the presence of Typhus: he observes that cleanliness, while it is advantageous to the patient, is absolutely necessary to insure the safety of the attendants, “ Quam-  
 “ quam enim in ædibus civium locupleti-  
 “ orum rarò propagari visa sit hæc nostra  
 “ Dysenteria, longè alia tamen & miserior  
 “ sors erat immundæ paupertati, *neque om-  
 “ nino dubitandum, quin in infimis de plebe  
 “ familiis, disseminaretur contagio, & pesti-  
 “ lentiaë ritu ab alio corpore ad aliud tran-  
 “ siret.”*

Here we have it distinctly stated of the same Epidemic, that there were some cases contagious, others not; what could create the difference? we never hear of Small-pox being contagious in one instance, and not in another; it then must be the presence of some other disease, capable at least of propagating itself; and that this disease was Typhus, the circumstances mentioned above render highly probable.

Of Willis I need say but little on the present occasion, as I have already anticipated his sentiments on this subject: suffice it to say, that he denies (as he well might) any contagious property in the *Dysenteria incru-*  
 enta,



enta, that is, in the Cholera: and that when he states the *Dysenteria cruenta* in general devoid of contagion, he adds, “*aliquando tamen hic morbus virulentus, & quasi pestilentia, plures interficit, & miasma suum per contagium latè explicat.*” This he states, not so much of the *Dysenteria cruenta* of that year, as of the disease, as it generally appeared in London, and we can well estimate its truth from the accounts of cotemporary writers, for Morton, who describes the disease Epidemic from 1666 to 72, gives good reasons for thinking it highly contagious and fatal in the first years of its prevalence, and much less so in the latter.

I shall conclude this section with a statement of Hillary’s sentiments relative to the causes which produce this disease, who having for a series of years during his residence in Barbadoes, where the Dysentery is almost endemic, kept regular, and very accurate journals of the state of the weather, was therefore well qualified to decide on its influence over that disease:—in p. 203, he says, “I have always found, from the best observations that I could make on the variations of the air and weather, in this island, that if the months of May, June,  
I “ July



“ July and August, were very hot and dry,  
 “ and the following months of September,  
 “ October, and November were accom-  
 “ panied with much rain, so that the air was  
 “ rendered *cool, moist, and damp*, and if  
 “ the intermediate days between the rainy  
 “ days, were very hot, I always observed  
 “ that Dysenteries were very frequent and  
 “ epidemical, and generally more or less  
 “ malignant, as the above-mentioned  
 “ changes of the weather were greater or  
 “ less, more sudden or more gradual, and  
 “ shorter or of longer continuance.\*”  
 “ As then, he adds, p. 205, we have Dy-  
 “ senteries constantly returning every year  
 “ in the West-India Islands, with those

\* That description of the Dysenteric constitution which  
 Grimm has given in treating of the origin of his Dysen-  
 tery, and which afterwards proved malignant, very close-  
 ly coincides with Hillary's.—In P. 147, Grimm says,  
 “ *transiit autem æstas sexagesimi anni in autumnum*  
 “ *perpetuis pluviis, imbribus, nebulisque obrutum, valdè*  
 “ *obscurum, atque ingratum, æstuante tamen Augusti*  
 “ *parte, and mense Septembre. Submaximâ humiditate,*  
 “ *diebus vix triginta frigidioribus interpositis, transiit*  
 “ *autumnus, quin eadem humiditas duravit per omnes*  
 “ *hyemales, & vernaes insequentis anni 1761, menses,*  
 “ *ut hyems nulla, continuus autumnus adfuisse visus*  
 “ *sit.*”

“ rainy



“ rains at that time of the year, may we  
“ not justly conclude, that the Dysentery  
“ is most commonly produced from these  
“ causes, especially as these causes are suffi-  
“ cient to produce it?” Immediately sub-  
sequent to this, he adds, “ It is also proba-  
“ ble that it may be sometimes produced by  
“ *infectious miasmata*, exhaled from dis-  
“ eased bodies, and floating in the air,  
“ which are received into the mouth with  
“ it when we breathe, stick there to the  
“ saliva, and are carried with it down into  
“ the stomach and intestines, where they  
“ produce all the above-mentioned symp-  
“ toms, when they meet with a constitution  
“ fitted by the above-mentioned causes to  
“ receive those infectious effluvia, and to  
“ produce the disease.” “ And thus, he  
“ adds, p. 206, the disease becomes both  
“ epidemical and contagious, though it was  
“ not the latter at its first invasion, or  
“ seizing the first patient. This I have  
“ often observed, especially when *great*  
“ *numbers* have laboured under it at the  
“ *same time*, as often happens among the  
“ *Negroes*.” This last passage very dis-  
tinctly and pointedly refers to that combina-  
tion of Dysentery with Typhus, which alone



I can admit to be contagious; and that the disease attacked in a variety of ways, he himself tells us in p. 207, “ The disease, “ says he, does not always seize the patient “ in the same manner: sometimes it seizes “ them with a Diarrhœa, which is moderate “ the first day or two, but it gradually in- “ creases in all its symptoms, till it be- “ comes a perfect Dysentery. At other “ times it seizes them with an oppression “ and sickness at their stomachs, a gentle “ rigor, with pain in the head, and some- “ times all over the body, which are soon “ followed by a fever.” &c. &c.—And so he proceeds in describing the progress of the fever and Dysentery.—That it was the operation of different causes, which was productive of this variety in the symptoms, must appear more than probable to such as have attended to what has already been said on this subject.—I have been at some pains to shew in this and other instances, that where an author, who in general declares the Dysentery not contagious, states it occasionally to be so, that he then usually furnishes us with grounds sufficiently strong for concluding that Typhus fever was in the latter case always present with it: the point is a material



material one, and I therefore hope to stand excused for dwelling on it longer than by some may be deemed requisite.

I shall now conclude this section with a short notice of a remarkable contrast between the more antient and modern writers, in their opinions, respecting the contagious character of Dysentery: the former seldom, or never doubted of its being possessed of that property, whereas such scepticism has of late prevailed among the latter, that some boldly declare their belief that the disease *never* is contagious: “ I am inclined to assert, says Mr. Milne, p. 143, that this disease is never produced from infection, notwithstanding the high authority which sanctions a contrary opinion, for I have had many opportunities of seeing it in all its stages, and in circumstances most favourable to the communication of contagion, but never could in a single instance, trace the disease to that source.” Language pretty similar to this, is employed by Wade, and others, in speaking of the contagion of Fever and Dysentery. It is language I cannot avoid condemning, because it is plain, they are drawing a conclusion which their premises do not warrant, for though it be

I 3

admitted,



admitted, that the disease as they met it, was not contagious, yet had they no right, in the teeth of opposite assertions, and of as good evidence, to conclude, that the same must necessarily have been the result of the experience of others; That it was, and ought to be different, the following section will assist in demonstrating.

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## SECTION II.

THE COMBINATION OF SIMPLE DYSENTERY WITH TYPHUS OR MALIGNANT CONTAGIOUS FEVER, IS CONTAGIOUS.

IN this section I shall consider the direct evidence of those writers, who appear to have met the disease most generally in combination with Typhus, and in so doing, I trust to be able to adduce them with one uniform and concordant voice, witnessing to the truth of the proposition laid down; of the great likelihood of its truth, we have already had very strong presumptive evidence: for as we have seen that the disease is held by the most respectable authorities to be, if not always, at least very often, contagious:



contagious: and as it has been proved, that it is devoid of this property, both in its simple state, and in combination with intermittent and remittent fever, so must we necessarily infer, that it is to the present combination alone, this proposition can apply: and this inference must appear the more probable, when we consider that one disease in the combination is undeniably contagious. This species of evidence, however strong in its kind, must still rank infinitely beneath that direct testimony, I shall now submit to the judgment of the reader: it will, I hope, be felt by others, as it was by me, to be irresistible.

I shall commence on this, as on a former occasion with Clarke, whose sentiments would appear very fair and decisive: he observes that the Dysentery depends on the same remote causes as the remittent fever, and in *unhealthy* situations is generally at the same time epidemic, and *always contagious*. “ But it may appear, he adds, at “ any season in hot climates, in consequence of perspiration being suddenly “ checked, by night fogs, or rainy weather, “ and by imprudently exposing the body “ after being much heated, to the chilling  
I 4 effects



effects of land winds." p. 321, vol. 2. I am not perfectly satisfied with the expression, "always contagious;" does he mean that the disease is in every instance contagious, or does he mean only, that when epidemic in unhealthy situations, it is then always contagious: now in the first case he would be clearly wrong, and in the second he might be incorrect, unless that epidemic consisted of the combination of the Dysentery with Typhus; besides I know of no disease which is always contagious, and which can at the same time originate from other causes, as well as contagion, except Typhus: so, as Dysentery may arise from the operation of cold, independent of contagion, it may also then become contagious, but only because it can enter into combination with a disease competent to effect these changes.

Degner's testimony is clear and satisfactory as to the contagious character of his Epidemic: he was able to trace the disease from one house over a whole street, and from thence over the remaining part of the town\*:  
he

\* His proofs of this point are contained in the first 40 pages of his work: they are too numerous to offer any detail



he shews that it could not be owing to any particular constitution of the air, not only because all the neighbouring cities remained free from it, “ Sed ne quidem ab initio  
 “ morbi civitas omnibus in partibus infecta  
 “ fuit, sed malum, quasi per gradus, certas  
 “ solummodo plateas occupavit, donec sen-  
 “ sim per *infectionem immediatam* in cœteras  
 “ plateas, ac tandem etiam in vicus, agrum-  
 “ que proximum, translatum fuerit.” p. 67.

What testimony can be more positive than that of Bontius? “ I have pro-  
 “ duced, says he, the examples abovemen-  
 “ tioned out of six hundred, to invalidate  
 “ the testimony of those, who deny the  
 “ existence of epidemic, and pestilential  
 “ diseases in this country. Whatever name  
 “ others may call them by, I care not, but  
 “ *I am certain* that the Dysentery I speak  
 “ of, was contagious, and seized many at  
 “ the same time, and quickly.” He him-

detail of them here. Pringle observes of them, that Deger offers *good reasons* for believing that the fatal Dysentery at Nimeguen, was owing to the infection communicated by one person: yet, says Moseley, “ to me,  
 “ those which Pringle calls “ good reasons” are very  
 “ bad ones, and like all reasons that I have hitherto met  
 with on the same subject.”

self



self laboured under the disease, being, as he observes, first seized with an ardent Fever, and then with a Dysentery which encreased. v. p. 104.

I need say nothing here of Etmuller, of Roederer, or of Grimm, whose sentiments on this subject have been distinctly pointed out in the passages already quoted from their works, for these the reader may refer to the last chapter.

Morton, in his own person, had experience of the contagious nature of the epidemic, he describes, “Nec mihi, inquit, percit contagio, dum mense Augusto sedes Dysentericorum, minus incautè inspicerem, iterumque ex cādē occasione infectus sum.”

Pringle’s sentiments can scarcely be doubted after the extracts already given, and I notice him here, merely for the purpose of pointing out the absurdity of supposing any specific contagion to belong to Dysentery: for as he says, “the contagion passes from one who is ill, to his companions in the same tent; as the straw becomes infectious, though the great source of infection seems to be the privies: as the hospitals likewise spread it,” so it may be asked of what



what nature is this infection? How does it come so regularly to affect the intestinal canals? so conscious was Pringle of the insufficiency of any exhalations from the sick, received in the general mode of contagion, to explain this circumstance, that he seems almost inclined to adopt the agency of animalcula in preference to that supposition, which he says, cannot be maintained, “without proving “at the same time, that when the “blood is thus tainted, the vitiated part of it “by a certain law in the animal œconomy, “must be thrown upon the intestines for “excretion.”

Tissot, whose name I have hitherto scarcely mentioned, furnishes us with a case in point, and with a declaration not less to the purpose: “If the corruption of humours, “which creates *malignant fevers*, be united “with the causes, which produce Dysen- “tery, the Dysentery resulting therefrom, “will, he observes, be malignant;” and he further adds, If there be a disease truly contagious it is this. “I have seen, says he, “within these few months, a terrible ex- “ample of its infection.” The case was that of a young man, who was attacked with a severe Dysentery, truly malignant, which  
in



in a few hours *destroyed all his strength*: he refused all assistance, passed his stools about the house, and died at the end of five days. Six of the family took the disease, though there was no Dysentery prevalent in the neighbourhood, nor was there any one circumstance apparent, to which the disease could be attributed, saving contagion.

Geach, in a short treatise, entitled “Some Observations on the present Epidemic Dysentery,” (at Plymouth, A. D. 1781) contains the following passage, demonstrative of its contagious nature, and of the circumstances under which it became so, though that does not appear to have been its general character: in p. 3. he says, “the disorder till this last summer was chiefly among the soldiers confined to barracks. But I saw then, he adds, and have seen since, that it was very infectious, and in one instance in this town, very alarming, as the patient died in a day or two after the seizure, and had *vibices* very large and very black, followed with such a degree of putrefaction and stench, as to deprive almost instantaneously, two female attendants of their senses, who very soon after became also  
“Dysenteric,



“ Dysenteric, ran through the stages of  
 “ the disorder, till at length one of them  
 “ died, and the other with difficulty reco-  
 “ vered.” It would appear probable that  
 the Typhus was here present; one thing, at  
 least, I have always been able to shew, that  
 when the disease is said to be contagious,  
 there are some symptoms accompanying it,  
 which do not attend the more common  
 course of Dysentery, and which very gene-  
 rally refer, more or less distinctly to some  
 one stage of the malignant contagious fever.

Sennertus de Dysenteria, p. 906, tom.—  
 After speaking of the different causes which  
 give rise to the disease, adds, “ Tandem  
 “ inter Dysenteriaë causas meritò numeratur  
 “ contagium, cùm et antea sæpissimè, et in  
 “ illâ ipsâ Dysenteriâ, quæ anno 1624, hic  
 “ grassabatur, compertum sit, unum ab  
 “ altero infici, & hinc totas familias Dy-  
 “ senteriâ corripì.” Though he thought  
 the disease principally communicated by the  
 excrements, yet after drawing an analogy  
 between it, Ophthalmia, and Phthisis, he  
 adds, “ Quia tamen in Dysenteriâ non  
 “ solum alvus afficitur, ut in Ophthalmiâ,  
 “ & Phthisi, pars una reliquis salvis afficitur,  
 “ sed sæpe tota massa sanguinea inquinata  
 “ est,



“ est, (unde etiam febres Dysentericæ sæpe  
 “ conjunguntur) nec conversationem cum  
 “ Dysentericis satis tutam puto, & Dysen-  
 “ teriam ex lecto Dysenterico communi,  
 “ vel communibus poculis, aut patinis, aut  
 “ aere inspirato attrahi posse existimo, cum  
 “ experientia doceat, non paucos ex con-  
 “ versatione cum Dysentericis in Dysente-  
 “ riam incidisse, qui nunquam in locum  
 “ communem alvum exonerant.” I should  
 not have conceived Typhus to be the agent  
 in propagating Dysentery, had the latter  
 disease been capable of arising from no other  
 source than the excrements; for if it have  
 any specific contagion, it is by that means  
 only I should suppose it capable of spread-  
 ing.\*—Hildams too, in speaking of Dysen-

\* Yet, says Darwin, in speaking of the Dysentery,  
 “ the contagious matter of this disease consists in the  
 “ mucous or purulent discharge from the membrane  
 “ lining the intestines, and not in the febrile perspira-  
 “ tion or breath of the patients: for the fever is only the  
 “ consequence, and not the cause of contagion.” Though  
 it may be admitted that the mucous or other discharge  
 from the intestines may convey contagious matter, as  
 some facts would seem to shew, yet must we notice, how  
 ill-founded the reasoning of this ingenious author is, if I  
 have brought together testimony sufficient to prove that  
 the accompanying *fever* is essentially necessary to the  
 production of its contagion.

tery,



tery, observes, “ Familiaritas quoque & “ conversatio cum ægrotis, & præcipuè la- “ trinarum usus cum iis communis pericu- “ losus est, & facillimè inficit.”

Grainger, in *Historia Febr. Anom. Batav.* p. 18. et seq. gives some account of a contagious Dysentery that broke out among the soldiers, and raged more especially among the fresh conscripts: he describes the symptoms as very severe, and as those of Typhus were also conjoined, he makes the following remark: “ Symptomata quædam “ sanè haud vulgò descripta aderant.”— “ The disease was contagious, he says, “ Nam duos colonos, in quorum stabulo “ Dysenterici cubabant, invasit; immò et “ aliis morbis afflictos, quamvis ab illis se- “ paratos corripuit.”

Roupe, in his excellent Treatise on the Diseases of Seamen, p. 403, makes mention of a violent fever, which raged at Weissemburgh, with different degrees of violence, attended in many with colliquative sweats and petechiæ, but in the *lowest parts* of the town with *colliquative sweats, very fetid, petechiæ, broad livid spots, frequent hæmorrhages, and offensive Dysenteries*, generally fatal in four or five days.—It would seem to have



have been a bilious remittent,\* afterwards converted into a contagious malignant fever, under circumstances alluded to by Dr. Blackbourne, expressly mentioned by Zimmermann, and plainly specified by Rouppe himself: where in p. 96, he speaks of the origin of contagion under filth and want of ventilation: this is what he calls their own contagion, generated by themselves, and not received from others; “ If, says he, there be  
 “ a great number of sick on board, though  
 “ the disorders under which they labour be  
 “ not contagious, but if at such times the  
 “ air between decks be not well purified,  
 “ the sick and healthy kept separate, cleanliness preserved as much as possible; if,

\* That these fevers may suffer such a conversion will appear from the following passage, extracted from Pringle:  
 “ In the autumn of 1757, he says, several soldiers were  
 “ brought into the hospital at Portsmouth with a disorder  
 “ complicated of the *autumnal* and *jail* fever; for when  
 “ these men, upon being seized with the common fever  
 “ of the season, were confined to the holds of the  
 “ crowded transports, their distemper assumed that form.”  
 There is a passage in Zimmermann nearly to the same effect: p. 18, he says, “ Our Dysentery was accompanied with a bilious, or, as it is called, a putrid fever:  
 “ and as the putrid fever was only catching in certain  
 “ circumstances, so our Dysentery was not of itself contagious.”

“ I say,



“ I say, at such times the greatest caution  
“ is not used, the *slightest* disorders will  
“ breed the *worst* of contagions.” And in  
p. 276, he thus speaks of Diarrhœa and Dy-  
sentry; “ but these disorders, as they arise  
“ from a general cause, usually rage epide-  
“ mically in ships, and from being epide-  
“ mical, unless great care is taken, soon  
“ become *contagious*.”

To this long list of authors, many of whom, perhaps, have been unnecessarily, though none uselessly, adduced in support of that great truth I am so anxious to establish, I shall subjoin only two more, and each for a particular purpose: the first I shall produce, not merely for the strength of his evidence in its favour, but because the perusal of his work on the Dysentery struck me indeed with surprise, that it had not long since led others to the same discovery; and the other I shall bring forward for the purpose of presenting to the reader, a view of the sentiments entertained on this subject by our latest systematic writer, and of shewing what little use has hitherto been made of the experience of others in this disease.



Zimmerman's most excellent Treatise on the Dysentery did not fall into my hands for some time after I had put together most of the preceding observations, and drawn from them the conclusions already stated. Of the work itself I shall take but a general survey: some passages I have inserted in their proper place; others I shall here bring together, rather desirous of stimulating the reader to an attentive perusal of that author, than of satisfying him with a mere abstract. He describes the Dysentery as epidemic in the autumn and winter of 1765, and in the commencement of 1766: in like manner about the same time, and under the same constitution of the air, the putrid fever, and particularly the putrid pleurisy, began their ravages, and were extended far and wide. In p. 2. et seq. he details the access and course of the disease, and in chap. II. p. 10, he draws such an analogy between the putrid fever and the Dysentery, as must convince every reader of the truth of Sydenham's well-known observations on the close connection between the epidemic disorders of the same year: for, after an astonishing number of putrid fevers, there followed the  
Dysentery,



Dysentery, attended likewise with a putrid fever. The analogy between them was to be seen in the resemblance of the symptoms in each disorder, of the method of cure that was most successful in both, and even of the effects that followed the errors therein committed, *vid.* p. 11. et seq.—“ It appeared to me, says Zimmerman, p. 20, “ that our Dysentery in general became “ contagious purely through nastiness, and “ the crowding many people together in “ a small space, but was by no means so “ *of itself*; for though many were attacked “ with it at once, this seems to proceed “ from a more universal and widely different “ cause, which operated at once upon every “ one.”—In pages 20 and 21, he makes an accurate statement of that kind of weather which gives rise to Dysentery, and observes with great propriety, that in general it is not the cold that follows on heat, and remains, but that which succeeds heat, and gives place to it by fits, that is to be considered as the cause of Dysentery.

P. 47, he says, “ Experience has shewn, “ that the smell of the patient was least dan-  
 “ gerous,



“gerous, that their breath was worse, and  
“their stools worst of all.”\*

In

\* Dewar, in his Observations on the Diarrhœa and Dysentery, as they appeared in the British army in Egypt, comes to a conclusion, founded on *theoretic reasoning*, nearly similar to this result of Zimmerman’s experience; and yet there is something in it that to me seems strangely inconsistent: p. 97, he says, “As this disease is *often* in a  
“*great measure* local, and is not necessarily connected  
“with a primary fever, I am disposed to conclude, that  
“its contagion seldom or never exists in the matter of  
“perspiration, or operates in consequence of being  
“absorbed by the skin.” Now, both in the premiss, and the conclusion, several things are taken for granted, some of which may perhaps appear incongruous, and others unfounded: in the first place it is stated, and with truth, that the disease is local, but how is this stated? that it is *often in great measure* so: or, that it sometimes is not local, or not *merely* local, because it may be connected with a primary fever; which is almost the same thing as saying, that the Itch, though usually a local, may yet become a general affection, if it happens to be connected with primary fever. I dare say the author could explain his meaning in this passage in a manner more satisfactory: I think I conceive what he would express, but as my object in this Treatise has been to establish, if possible, some accuracy in our ideas respecting the Dysentery, I trust he will excuse me, on the present occasion, for having noticed a little inaccuracy in the mode of expressing them.—I must now advert to the conclusion which has been drawn from this premiss; it takes for granted, that the disease, when local, is contagious, and *therefore* that its  
contagion,



In page 133, he observes that an essential distinction is usually made between a Dysentery with fever, and one without fever, a benignant and malignant, a contagious and not contagious Dysentery: “this distinction however, he afterwards adds,

contagion, unlike the febrile, neither exists in the matter of perspiration, nor is absorbed by the skin.—Now, I think, it has been proved, that the disease, when unconnected with primary fever, is *never contagious*, and therefore that when contagious, its contagion, if that of fever does, may exist in the matter of perspiration, or may be absorbed by the skin. But these are points in the History of Febrile Contagion, very much, and I am inclined to imagine, very justly doubted.—This author adds, “It is probable that it exists chiefly in the stools, and operates in the form of exhalations, by affecting either the tender skin of the anus, and rectum, or the organ of smell and taste.” And further, “It is also probable that the vapour, which proceeds from the lungs, and that which is brought up by the æsophagus in eructations, or even what is secreted in the internal feces, and carried off in vapour with the breath, contains the contagious effluvia of this disease.” I shall only observe of this last passage, that it is somewhat inconsistent to state, that the Contagion of Dysentery may be contained and conveyed in those secretions, by which the febrile contagion is communicated, when the moment before it was denied that it could exist in the matter of perspiration, or be absorbed by the skin, because it was supposed that these two properties belonged to febrile contagion.



“ between a true Dysentery with fever, \* and  
 “ one without fever, appears to me to have  
 “ a very dangerous tendency, and in my  
 “ opinion should be banished from the  
 “ schools of medicine, as it rather deter-  
 “ mines the limits between a Dysentery and  
 “ a Diarrhœa.” A sentiment the reverse of  
 this, is what I should rather have expected  
 from Zimmerman, as nothing more in my  
 mind, was wanting to make this distinction  
 perfectly accurate, but a further distinction  
 of the febrile Dysentery, into different spe-  
 cies, according to the nature of its accom-

\* How are we to reconcile this observation with another which soon follows it: in p. 156, he says, “ All this  
 “ taken together, I give it as my opinion, that the va-  
 “ rious species of Dysentery are to be distinguished, not  
 “ by the difference of evacuated matter, but of the fever  
 “ by which they are accompanied.” The fact, however,  
 is, that he was induced to make the latter distinction, for  
 the purpose of defending Willis and La Moniere against  
 the Breslau physicians, who gave the name of *painful*  
*Diarrhœas* to that disease, which Willis called the *Dysen-*  
*teria incruenta*, but which, as we have seen, should be  
 denominated *Cholera*, for they perceived that the very  
 copious discharges attending the disease must exclude it  
 from the name of Dysentery. Zimmerman supported  
 Willis, and says, (with what justice, has already been dis-  
 cussed) that this disease evidently belonged to the malign-  
 ant species of Dysentery.

panying



panying fever. He is besides incorrect in stating *fever*, to be the ground of distinction between Diarrhœa and Dysentery, as that alone, without other symptoms peculiar to each, would be insufficient for the purpose; indeed Zimmerman, as we have already seen, has occasionally fallen into an error, no way uncommon, of confounding Dysentery with other diseases, to which it bears some affinity.

In p. 136, though he says, that Sydenham's expression of a "Febris introversa" does not perfectly please him, yet he adds, that "this distinction seems to comprehend  
 " the very essence of a genuine Dysentery,  
 " which should sometimes be treated as an  
 " inflammatory, sometimes as a bilious or  
 " putrid fever, sometimes as a fever com-  
 " pounded by both, sometimes as a malig-  
 " nant fever, and sometimes as a bilious  
 " one, accompanied with manifest tokens  
 " of malignity." His own Dysentery, he considered and treated as a bilious, or putrid fever, and accordingly neither bled a vein on the one hand, nor allowed wine nor strengthening medicines on the other.\*

In

\* Zimmerman here seems to have correctly understood the *application* of Sydenham's idea; on this occasion not



In p. 139, et seq. he furnishes the most unequivocal proofs of the circumstances, under which the Dysentery becomes contagious, namely, by a conjunction, malignant, jail or hospital fever: the effects of which combination, he very forcibly describes in its ravages among the English soldiers after the battle of Dettingen. I must refer the reader to this part of his work, as the most decisive on the source of Contagion in Dysentery. I shall only subjoin the following passages as clearly illustrative of his meaning: p. 144, he says, “the benignant species of Dysentery, becomes contagious, malignant, and extremely dangerous, when many sick people are crowded together in a small space, or when peculiar external or internal causes, produce malignity in particular persons:” and in p. 146, he adds, that he does not see “that camp Dysenteries are in themselves more alike correct is another author, in other respects equally so: Blane, p. 449, says, “These two diseases (meaning Dysentery and Fevers) may therefore be considered as vicarious, the one substituting itself for the other, according to particular accidents, and both proceeding from the same general causes; and this is no new idea of mine, but seems to have been Dr. Sydenham’s, when he calls the Dysentery a *febris introversa*.”

“malignant



“ malignant than those that happen in cities,  
“ although in the army and military hospi-  
“ tals, they become excessively malignant  
“ and contagious from several circum-  
“ stances: the same however, takes place  
“ in cities, when a great quantity of people,  
“ attacked with this disease, are crowded  
“ together in a small place, or where the  
“ other different causes subsist, of a pecu-  
“ liar or general malignity.” And in p.  
151, he says, “ *When hospitals are filled*  
“ *with Dysenteric people, some of the assist-*  
“ *ants are attacked only with the Dysentery,*  
“ *and others with the jail or hospital fever.*”

From all these observations, made partly by me, and partly by other physicians, I conclude, says Zimmerman, that the Dysentery is very often only *accidentally* contagious.

Language more clear, or sentiments more decisive and unequivocal could scarcely be delivered; after a perusal of this work alone, who would hesitate in adopting that opinion I have been endeavouring to establish? yet has this work been translated into our tongue for above thirty years, been in general circulation during that time, and recommended from high authority, without having to my knowledge produced such an effect: of which



which a few extracts from the other author, above referred to, will be sufficient to convince us.

The 4th vol. of the treatise on Febrile Diseases, by A. P. Wilson, having lately fallen into my hands, I made the following extracts from the article Dysentery, for a purpose already alluded to.—This author, as I have done, objects to the Pyrexia contagiosa of Cullen's definition, not because Pyrexia is often absent, or that contagion does not properly belong to the disease, but because “ its contagious nature is not always remarkable,” and also because the other symptoms in the definition are sufficient to distinguish it. P. 582, he says, “ the fever is sometimes a Synocha throughout the greater part of its course, more frequently a Typhus, and in some cases it is a well marked Typhus from the first.” In p. 583, he makes the following remark, “ before proceeding further I may observe by the bye, that as the fever in Dysentery is not only sometimes the first part of the disease which shews itself, but even now and then continues for some time before the local symptoms appear, and as the degree of fever often seems proportioned  
“ rather



“ rather to some peculiar virulence of the  
“ contagion, than to the degree of the local  
“ affection, it may seem, that the fever in  
“ Dysentery is regarded as symptomatic of  
“ the local affection with less propriety than  
“ other symptomatic fevers. But it appears  
“ from a variety of facts, that the Contagion  
“ of Dysentery, or the putrid effluvia at-  
“ tending it, may excite a real Typhus *inde-*  
“ *pendently* of any local affection, in which  
“ the latter, frequently does not appear for  
“ some time after the commencement of the  
“ fever, and in some cases \* does not ap-  
“ pear at all: where such a fever therefore  
“ continues for some time before the local  
“ affection shews itself, the case is evidently  
“ to be regarded as a *complication of Typhus*  
“ and *Dysentery*. In the case of Simple  
“ Dysentery, he adds, we shall find suffi-  
“ ent proof of the general affection, depend-  
“ ing on the local, the former being con-  
“ stantly influenced, both with respect to

\* I should like to know on what facts he rests this as-  
sertion; as he appears intimate with the treatise of Zim-  
merman, perhaps he built it on the observation made by  
the latter, in p. 151, and already referred to: “ When  
“ hospitals are filled with Dysenteric people, some of the  
“ assistants are attacked only with the Dysentery, and  
“ others with the jail or hospital fever.”

“ kind



“ kind and degree, by the state of the latter,” &c. &c.—P. 595, he says, “ the fever in Dysentery is not always continued: it sometimes assumes the tertian type, and in many cases remits irregularly:” and in p. 615, he adds, “ the Dysentery, many think, never assumes the remitting form, except when complicated with remitting fever.

Notwithstanding this tolerably distinct view of different combinations, existing, in the Dysentery, this author does not appear to have entertained the most remote idea of the real source of its contagious character, if we are to judge by his observations on that subject: p. 609, he says, “ the disease is *now* so generally admitted to be contagious, *in many cases at least*, that it is unnecessary to offer any quotations in support of that opinion.” But surely it is not unnecessary to offer some for the purpose of shewing what those *many cases* are, where only the disease is contagious, for it is an admission, that there are cases of the genuine disease not possessed of that property. “ Its contagion, like that of most others, he observes, extends but a short way around the sick: its chief source is the excrement.”—He then proceeds in  
stating



stating the sentiments of Zimmerman, but without the least reference to that peculiar combination of Dysentery and Typhus, which he had previously spoken of, or without the smallest ray of light, or of the truth, striking on his mind, notwithstanding the clear decisive language of that author, which it is scarcely possible to peruse without conviction. He even says, in p. 616, “ It is very doubtful whether, (as Zimmerman seems to suppose, and as the great effect of the excrement in propagating the disease, has induced many to believe) Dysentery, like common Typhus, may arise from putrid effluvia alone. The constant affection of the bowels must incline us to believe that there is something *specific* in the Contagion of Dysentery.” By this last observation, he is obviously of opinion, that Dysentery, qua Dysentery, is contagious, and of course, that the *many cases* of the disease, which at least are possessed of this property, cannot owe it to Typhus or any other disease: indeed from the whole tenor of his treatise, he seems to have had no knowledge of that species of contagion, which can propagate as one, two diseases, one of which shall be contagious, and the other, in



in its own person, utterly devoid of that property.

Having taken a pretty extensive view of the history of this disease, as pourtrayed by those authors, whose works I could command, and having also given a fair and accurate statement of their opinions respecting the same, I shall now enumerate their names and arrange them in the order in which they delivered their evidence.

Moseley	}	These authors describe the disease in its simple state, or the intermitten and remittent forms of it: they all, with decision, greater or less, give their voice against the Contagion of Dysentery in any of these forms. Some of these authors, according to the nature of their dispositions, deny Contagion in toto, others are silent on the head, and some again, while they admit that the disease, as they saw it, was not contagious, do not yet deny but that, under particular circumstances, it may become so. This species of disagreement must tend the more firmly to convince us of the truth
Hunter		
Willan		
Akenside		
Cleghorn		
Sydenham		
Huxham		
Richter		
Stoll		
Rollo		
Willis		
Baker		
Hillary		
Milne		



truth of that proposition in which they all coincide: namely,

That the Dysentery, in its simple, its intermittent and remittent forms, is not contagious.

On an opposing list stand names not less respectable than the foregoing:

Clarke  
 Degner  
 Bontius  
 Etmuller  
 Roederer  
 Grimm  
 Morton  
 Pringle  
 Tissot  
 Geach  
 Sennertus  
 Hildanus  
 Grainger  
 Rouppe  
 Zimmerman  
 Rogers\*

These authors generally met the disease in its combination with Typhus; they all uniformly, and without hesitation, pronounce it contagious. Some of them assert contagion to be the sole, others the principal agent only, in propagating the disease: all, however, agree in stating, that it was contagious in the form in which it presented itself to them, namely, in combination with Typhus; whence we must conclude,

\* This last author I have no hesitation in setting down among those who vote for the Contagion of Dysentery; for though he does not plainly say that it is contagious, yet his whole history of the disease speaks it for him.

that



that the Dysentery is contagious only in case of its combination with Typhus, or malignant contagious fever.

Thus then we see that Dysentery can no longer be deemed a contagious disease, while at the same time it must be understood, that it is capable at all times of acquiring that property under circumstances, the existence of which can be easily ascertained; in other words, that the contagion of one disease can so intimately associate itself with another, not possessed of a similar power, as to communicate to such as are susceptible thereof, both diseases, and not that merely, which was in itself contagious. This singular feature in the Contagion of Typhus we shall hereafter have occasion more fully to investigate: at present, therefore, I shall notice the subject no farther, but hasten to conclude this long chapter with an enumeration of the different marks by which each form of the disease may be distinguished.

The Simple Dysentery may be readily known by the presence of all those symptoms detailed in Cullen's definition, excepting



ing the “*Pyrexia Contagiosa*,”\* as it is neither preceded nor attended by fever of *Idiopathic* character: any feverish symptoms present arise from the severity of the disease, and are entirely symptomatic thereof. It is a form of the disease by no means dangerous unless grossly mismanaged, and when art does not interfere effectually to its cure, it most usually becomes of long duration, and very apt to terminate in a chronic state; in all these respects, and in its mode of treatment, it closely resembles the Rheumatism, to which, as we have seen, it is very nearly allied in its general nature and properties.

The Intermittent and Remittent Dysentery is known by all the marks which characterise the simple disease, together with such as indicate the presence of intermittent or remittent fever, either of which may both precede and accompany the disease through its course, and may, whether by the efforts of nature or of art, be removed, leaving

\* On this account Akenside's description of Dysentery is to be preferred to Cullen's definition; it is as follows,  
 “*Qui graviora patitur ventris tormina, simul cum frequente desidendi cupiditate, et cum dejectionibus, vel sanguineis, vel mucosis, (vel utrisque) eum hominem Dysenteria laborare omnes consentiunt Medici.*”



the Dysentery behind. When present they modify that disease so far, that it partakes of such remissions and exacerbations as the fever admits of, and this may generally be known by the same occurring in the current fevers of the day. These forms of the disease are also far from being dangerous, though more so than the last, especially when the fever happens to be of a bad type: both diseases are often cut short by the same means, but when this does not occur, the fever will disappear sooner or later, while the Dysentery continues, of the same character, and of as long and uncertain duration, as in the former case.—But

The combination with Typhus is by far the most important form of the disease: it agrees in every respect with Cullen's definition,\* and is to be distinguished from the other forms by the marks characteristic of Typhus Fever.† It is always preceded by feverish

\* Should we retain Cullen's definition as the definition of this form, would it not be better to substitute *Typhus contagiosa* for the *Pyrexia contagiosa*.

† “ We may, a priori, always suppose, says Zimmermann, p. 170, the presence of a malignant Dysentery, “ where many people sick of the Dysentery are crowded “ together in a small space; but this disorder may like-  
“ wise



feverish symptoms, and its access is generally attended by rigors, nausea, vomiting and *great prostration of strength*; in its progress it is accompanied by the more advanced symptoms of the same fever, and its termination, when unfavourable, (which is too often the case) is marked by all those, usually present at the dying scenes in Typhus: the fatal termination, and crisis fall on the same day, as in that fever, and in every respect, says Grimm, “*Ratio vitam ponendi, omnibus ferè eadem, ac in febris malignis fuit.*” This form is exceedingly dangerous, and is propagated solely by contagion, and in its mode of propagation it precisely resembles fever. When disposed to terminate favourably, the symptoms of Typhus disappear, while those of the Dysentery may

“*wise proceed from many other external, as well as internal causes: its surest pathognomonic signs are, the quick approach of a more than natural weakness; great anxiety about the pit of the stomach, a heaviness in the head, a wild, and yet at the same time a deadlike look, spirits extremely depressed, or a perfect indifference to every thing in the world, frequent slight convulsions, a very weak voice, a great many fainting fits, sometimes a miliary eruption, petechiæ, aphthæ, a very weak pulse, a vast sickness at the stomach, and the other symptoms usual in malignant fevers.*”



continue more or less, for a longer or shorter time; its contagious nature however, I should imagine, ceased with the fever,\* as upon that it originally depended.

The

\* The only seeming objection to this position I could discover, is contained in the following passage, extracted from Lind's first paper on Fevers and Infection, p. 37. "Every one knows, says that author, that the *Camp Dysentery*, and indeed most *Dysenteric fevers* are generally both infectious and malignant: but I had a patient in a chronic flux of two years continuance, which seldom confined him to his bed, and yet he infected with it, almost all persons who used the same privy. This person was lodged in a ward with Rheumatic patients, several of whom complained daily of a severe purging, which they imputed to their medicines; the nurses of the ward became affected in the like manner, who first discovered it to be owing to this patients stools which were slimy and very offensive: upon debarring him from the use of the common privy, this general complaint among them ceased. Cases of a like kind, he adds, have frequently occurred in the hospital."—It does not, however, appear from this relation, that the Rheumatic patients or nurses were infected with Dysentery, for it states, that the former complained daily of a *severe purging*, of such a nature that they imputed it to their medicines; he does not say what course this purging ran with them, or how it terminated, nor whether these *infected* persons propagated the disease further, the probability of which could scarcely be doubted an instant, if it were so certain, that an old chronic case was capable of doing so. It does not appear to me that this general complaint



The importance of the distinctions now proposed, should they be found to have truth for their basis, none will dispute; it must be obvious to every eye. In *theory* they are pleasing and satisfactory, as they furnish us with a ready solution of the many doubts and difficulties, which present themselves at every step we advance, and as they reconcile all those contradictions, and inconsistencies, under which the most respectable writers appear to labour. But it is in a *practical* point of view that these distinctions claim their greatest meed of praise: in that respect their utility is of the first moment, and their advantages incalculable, more especially, as we shall find, that these distinctions, at least some of them, apply to other diseases, of importance little short of Dysentery. By attending to these distinctions we are at once admitted to an accurate and clear view of the disease before us, and are no longer embarrassed with vague notions, uncertain opinions, and indecisive practice.

complaint among the patients and nurses was Dysentery; it was more probably a Diarrhœa, arising from some cause common to them all: for infection from the dysenteric patient must produce a disease like to itself in every respect, which that certainly was not.



We are taught by them, in one case, not to entertain unnecessary fears of Contagion, nor in another to deride them as unfounded, but are enabled to adopt precautions when requisite, and reject them when needless. According as they direct, we must guard our prognosis, and guide our practice, for in the latter case, we are, by means of these distinctions, led at once to decide upon and adopt the most proper and vigorous measures.—This is but a short and imperfect sketch of the many advantages resulting from the distinctions which have been proposed; it will, I trust, be sufficient to convince the reader of the great importance of deciding on their truth.



## CHAPTER V.

## TREATMENT OF DYSENTERY.

## FURTHER PROOFS

HENCE DEDUCED OF THE TRUTH OF FORE-  
GOING CONCLUSIONS.

IN undertaking to speak of the treatment of a disease, so complicated as Dysentery has often appeared to be, it would be presumptuous in any man to attempt so comprehensive a subject on the ground of his own limited experience, and it would be equally useless, as it must be tiresome, to go into a full detail of that of others. As, hitherto in conducting this treatise, it has been my object, not so much to give a regular or systematic account of that disease, which forms the chief subject of it, as to confine myself to the establishment of certain propositions which relate to it, so, on the present occasion, it is my intention not to enter into any minute or detailed history of the treatment necessary in the disease,



but merely to take such a view of the principal remedies, from time to time exhibited in it, and of the sentiments delivered by various authors of respectability concerning them, as shall be sufficient to enable us to form some judgment whether, and to what degree, their practice was influenced by the different combinations of the disease, and thence to estimate how far the truth of the propositions is to be affected by their opposing statements. After we have considered the sentiments of authors respecting individual remedies, as applicable to different stages or states of the disease, and drawn from thence such inferences as they may warrant, I shall conclude the chapter with a concise view of that plan of treatment which I should judge best adapted to each form of the disease.—I shall commence with that class of remedies called Evacuants, under which head I shall principally consider Venæsection, Emetics, Purgatives, and Sudorifics: yet, as these curative means, though reduced under the same general title, differ essentially in their immediate, and as to degree in their ultimate effects, so it may be expected that an author, who shall recommend one, may perhaps condemn another,  
omit



omit a third, and place restrictions on a fourth.

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## SECTION I.

### EVACUANTS.

IF, from the survey which has been taken of the most reputable writers on Dysentery, I have been justified in the distinctions that have been made, and the conclusions that have been drawn, the truth of them must be perceptible in the difference of that treatment, which each practitioner found the most successful: and if any difference be at all obvious in the general plan of treatment, it is impossible but it must exist in a much greater degree in relation to the class of Evacuants, and more especially in reference to one article in that class, with which therefore I shall here begin, as it a remedy that usually takes the lead of all others, whether it be employed with propriety or otherwise.

And first, of *Venæsection*. Respecting the necessity or advantage of this remedy in the Dysentery, we find considerable variance



ance of opinion to exist, and though we may observe a great majority of practitioners employing it in the early stages of the disease, we are not, on that account merely, to argue that it was necessary or advantageous, unless they specify certain evils or inconveniencies attending the neglect of it: whereas such authors as condemn the practice, usually do so on account of positive mischiefs having been observed to follow its use; for this reason more weight must attach itself to the condemnation of the latter, than to the silent admission or doubtful praise of the former.—The opinions of practitioners respecting Venæsection will be found to vary according to the circumstances under which they met the disease: one class consider V.S. highly necessary: a second deem it useless: and a third look on it as injurious. We shall be at no loss to discover that such as describe the disease in its simple state, or in combination with intermittent and remittent Fever, form the two first classes, while they who met it in combination with Typhus constitute the third.

“ The appearance” of inflammation in the bowels on dissection, says Hunter, “ would seem to shew the propriety of this evacuation,  
“ tion,



“ tion, yet must it be allowed, that there  
 “ may be inflammation, that is, redness,  
 “ swelling and pain in a part for which it  
 “ would be highly improper to let blood, as  
 “ is the case in all erysipelalous inflamma-  
 “ tions;” it is a question only to be deci-  
 ded by experience, and with respect to ex-  
 treme cases, Hunter properly observes, “ that  
 “ in slighter cases or when the disease is  
 “ treated early, other remedies are so ef-  
 “ fectual without it, that recourse is never  
 “ had to bleeding; and when the disease is  
 “ more violent, the strength of the patient  
 “ has been reduced so much of a sudden,  
 “ that one would scarcely dare to make use  
 “ of that evacuation.”

Sir George Baker's sentiments are decided-  
 ly in favour of this operation: “ Sanguinis  
 “ detractioem, inquit, supervacuum fore,  
 “ medicis insedit diu, invereravitque malè  
 “ fundata, malè ominata opinio. Id verò  
 “ auxilium inter initia, maximè si signa in-  
 “ flammationis aliqua adessent, nunquam  
 “ alienum deprehendi.” He even speaks of  
 its repetition under particular circumstances,  
 and concludes with this observation, “ Ne-  
 “ que prudentis est ac circumspecti medici  
 “ timere,



“ timere, ne imbecillum reddat ægrotantem,  
 “ cùm capitis res agatur.”

Hoffman uses language as strong: “ Mul-  
 “ tiplici ductus experientiâ statuo, quòd in  
 “ subjectis valdè plethoricis, diætæ vinosæ  
 “ adsuetis, si Dysenteriâ cum febre con-  
 “ tinuâ corripiantur, omnino perquam sit  
 “ necessarium venam ab initio morbi sec-  
 “ are.” Of these two last authors however,  
 we observe that neither of them positively  
 recommend venæsection, simply because the  
 disease is Dysentery. The former never  
 found the operation *amiss*, especially if *any*  
*signs of inflammation were present*, that is,  
 when symptoms independent of that disease,  
 arose; and the latter found it so necessary  
 only in subjects very liable to inflammation.

Dr. Rollo thinks venæsection seldom or  
 never necessary in the Dysentery, as it is not  
 at first necessarily connected with inflam-  
 mation, which when it occurs is, he says, of  
 the erythematic kind, is superficial and pro-  
 duced by no inflammatory diathesis, but by  
 causes immediately acting on the interior  
 surface of the intestines. *vid.* p. 46.

Moseley recommends V.S. under two dif-  
 ferent circumstances: “ There are but few  
 “ instances, he says, where it may not safely  
 “ be



“ be done in the beginning of the disease, observing only, “ non quæ ætas sit, sed “ quæ vires sint.” “ The necessity is obvious, where the patient is plethoric, with “ much fever, full pulse and severe pains.” v. p. 247. He afterwards adds, in p. 282, “ that when the patient, after taking his “ diaphoretic medicine, instead of sweating, “ becomes restless and hot, there V.S. or “ an emetic is necessary, which never fails “ to dispose the body to a sweat.” A very small quantity of blood, however, is sufficient for this purpose: this also is often the case in Rheumatism.

Tissot says, that sometimes the Dysentery begins with an inflammatory fever, the pulse hard and full, with violent pain in the head and loins, and tense belly; in which case the patient should be bled; but when the Dysentery is united with a putrid fever, he does not mention this remedy, and when joined with a malignant fever, he cautions against its use.

Hillary thinks it always necessary to take away *some* blood, more or less, as the nature of the fever, the strength, quickness and fulness of the pulse do indicate, and the strength and other circumstances of the patient will



will permit. Sydenham generally bled, and Stoll used V.S. occasionally. Richter makes no mention of it.

“ In the inflammatory Dysentery, says  
 “ Zimmerman, p. 179, Venæsection in the  
 “ beginning is a principal article: and there  
 “ is no occasion to fear repeating it, if the  
 “ patient has still strength, and is not too  
 “ much exhausted by copious stools: it has  
 “ sometimes an astonishing quick and good  
 “ effect”.—In p. 160, he says, “ this spe-  
 “ cies of Dysentery shewed itself in the  
 “ village of Viterne, in Lorrain; it came  
 “ on with a breaking of wind upwards, and  
 “ a very violent pain in the stomach and  
 “ bowels: on this ensued a fever, and soon  
 “ after, frequent dysenteric stools, with a te-  
 “ nesmus and unquenchable thirst, and such  
 “ an inflammation from the œsophagus down-  
 “ wards to the anus, that the patients thought  
 “ their insides were on fire; the tongue near  
 “ the œsophagus was inflamed and black:  
 “ if the patient vomited in this condition,  
 “ he died on a sudden. In the space of ten  
 “ days fifteen persons died of this distemper,  
 “ described by Dr. Marquet, Dean of the  
 “ College of Physicians at Nancy. Some  
 “ who were seen walking about the streets  
 “ at



“ at five o'clock in the afternoon, were  
“ seized with it, and died at ten o'clock at  
“ night.” I have here given at full length  
the description of that disease, which Zimmerman and others have called the Inflammatory Dysentery, because I think it carries with it sufficient evidence to prove that it was not any form of the Dysentery whatever. If it be Dysentery, it certainly differs most essentially from any epidemic of that disease on record, in its mode of attack, in its symptoms, and rapid fatality. If then it disagrees so materially from the disease as to give it no claim on the name, we should surely assign it the title of that one with which it would appear to coincide in every necessary respect: for my part I would denominate the disease an Inflammatory Colic.\* Independent of this species of the Dysentery, Zimmerman makes no mention of Venæsection, except to condemn it more especially in the malignant form of the disease: “ I, for

\* Besides the great and striking advantages of V.S. in this disease, Zimmerman adds, in p. 181, “ that emetics  
“ are in it a deadly poison: purges also by their irritation do not do less mischief, as they increase the inflammation.” This observation furnishes further arguments against calling the disease a Dysentery.

“ my



“ my part, says he, entirely reject V.S. in  
 “ the malignant Dysentery, especially as I  
 “ have already found it unnecessary in the  
 “ bilious \* Dysentery :” and before that, he  
 observes, that the sick have been seen to die  
 in the most deplorable manner upon the use  
 of this remedy.

Pringle says, “ that it may be proper to  
 “ begin with Venæsection, though Dysen-  
 “ tery *of itself* may not require that evacu-  
 “ ation, but from the accompanying inflam-  
 “ matory symptoms, it is frequently indis-  
 “ pensable, and always conducive to the  
 “ cure.”—He cautions strongly, however,  
 against its use, or repetition, in so putrid a  
 disease, except in the winter, or vernal Dy-  
 senteries, as being of a more inflammatory  
 nature; in these he found V.S. and rhubarb  
 sufficient without vomits, for then the sto-  
 mach is less disordered. Pringle, we may  
 remember, refers the treatment of the *malig-*  
*nant* Dysentery to that of the hospital or jail

\* He says, p. 186, “ that Venæsection may, and  
 “ should be, omitted in a Dysentery, that is merely at-  
 “ tended with a bilious fever; but that there is likewise  
 “ not the least reason to blame the physicians, who make  
 “ use of it in complicated cases.”

fever :



fever: in it therefore, V.S. must be inadmissible.

Degner thus speaks of V.S. “ Venæsec-  
 “ tioni, in nostro morbo nullus erat locus,  
 “ sive præservationis sive curationis gratiâ  
 “ administratæ, vires vitæ frangit, naturam  
 “ in motibus suis salutaribus conturbat,  
 “ unde vomitus cruentus, & mors venæsec-  
 “ tionem citò sequebantur.” P. 267.

“ When the disease, says Clarke, is at-  
 “ tended by a fever of the inflammatory  
 “ kind, no evacuation is better calculated  
 “ for the relief of the patient, or better  
 “ adapted for restraining the hæmorrhage:”  
 And yet does he afterwards add, “ I do not  
 “ remember to have met with above a case  
 “ or two, which *seemed* to require bleeding,  
 “ and the operation though performed early  
 “ in the disease, did not in the *least* relieve  
 “ the patient; but when the flux is of a  
 “ chronic kind, or accompanied with a low  
 “ fever as is most usual in hot climates,  
 “ bleeding would only serve to impair the  
 “ patient’s strength, and if not immediately  
 “ fatal, would at least precipitate his fate.”

Even local blood-letting would appear to  
 be of little service in Dysentery, and there-  
 fore objectionable, as being a means of a



further reduction of strength in a disease highly debilitating: since Willan declares that the repeated application of leeches to the abdomen did no good. The sentiments of various other authors might be adduced under this head, but it would be tedious, and must appear unnecessary after those already quoted, for they, I trust, have sufficiently shewn, that generally speaking there is little occasion for V.S. in any form of the disease, but that in the combination with Typhus, it is utterly inadmissible. I shall therefore conclude this article with some excellent observations on the employment of this remedy in Dysentery, as extracted from Wilson's treatise on Febrile Diseases, P. 631, he says, " The reader will find many speak-  
" ing of blood-letting at the commencement  
" of Dysentery, as necessary, in short, as a  
" remedy, to be had recourse to, if all the  
" remaining strength of the patient will  
" bear it. Such writers speak, as if they  
" expected from blood-letting some essen-  
" tial change in the state of the local affec-  
" tion, and therefore insist upon its employ-  
" ment, wherever the general state of the  
" system admits of it; and if an inflamma-  
" tion of the bowels always attended the  
" commence-



“ commencement of Dysentery, this expectation would be just. But this is rarely the case. Inflammation, when it does attend Dysentery, is the consequence, not the cause of the disease, and therefore seldom supervenes early. Besides those who recommend an indiscriminate use of the lancet in this disease, mention symptoms as warranting its employment, which do not indicate inflammation.—What advantage then do we derive from blood-letting at the commencement of Dysentery, except where the excitement runs so high, as to threaten immediate danger, or much subsequent debility? Has it been found particularly powerful in allaying the pains, in removing the peculiar state of the intestines in Dysentery, or in promoting the evacuation of the natural fæces? If not, what compensates for the debility it occasions? Instead therefore of letting blood in all cases where the strength can bear it, it will be found a maxim better supported by experience, to *avoid* it, wherever the symptoms can be otherwise allayed.” In the propriety and truth of this last observation I fully coincide, inasmuch as I am perfectly satisfied that



mismanagement alone can, and that in a very small degree will be sufficient to render the operation necessary in that peculiar state of diseased action which occurs in the Dysentery.

Secondly, *Emetics*.—The articles of this powerful class have not been neglected by practitioners in the Dysentery: indeed there is scarcely an article of the class, which has not been at one time or other recommended to the public with extravagant praises, or a single practitioner, who has not been more or less devoted to some one favourite among them, to the exclusion of the others. Though this circumstance alone is sufficient evidence of the great efficacy of emetics, when properly administered in the disease, yet should we cautiously guard against an indiscriminate use of them in every state or stage of a malady so diversified as Dysentery. The articles of this class, according to their nature or mode of administration, may be made to operate in any one or several of the following ways: and the more compound their operation, the more effectual in general will it be found to be; they may unload the stomach of its contents, move the intestines gently downwards, and promote or effect a

determination



determination to the surface: each of these effects may be advantageous in different forms or stages of the Dysentery; in some forms of the disease it is peculiarly necessary to unload the stomach, in all to move the intestines downwards, and in particular stages to promote a diaphoresis, though it be not alike advantageous or safe in every form, that art should interfere for this purpose. Some articles in the class of Emetics possess superior efficacy in producing one or more of the effects mentioned, and upon their possession of these, and not of any specific power, must their reputation in the Dysentery rest. On a review of the treatment adopted by different practitioners in this disease, we shall find that Emetics have been employed by all, by some with the sole intention of unloading the stomach, by others with a view to exciting a diaphoresis, and by others again with the design of clearing the bowels of their contents, as well as of answering either or both of the two former purposes: they are most beneficial in the intermitten and remittent forms in all these different respects: in the combination with Typhus they are often necessary to unload the stomach, while in the Simple Dysentery



they are seldom required for this purpose; in the first case they may be more freely used and frequently repeated, but in the second they must be employed with more caution.

Moseley, in his West-India practice, after bleeding began with a vomit of Ipecacuanha to relieve the stomach from a load of acid, porraceous and bilious impurities, but after this effect, the great expectation he entertained from vomiting, was the determination made thereby to the surface. In London however, he says, it is his common practice to order the patient to bed on taking the Ipecacuanha, and direct that the operation of sweating, rather than vomiting, should be promoted. This difference in the treatment naturally arose from the variations of the disease in the West-Indies and London.

Zimmerman gave emetics even at a time when the stools were very bloody, as he saw that less blood came away afterwards; nature herself, he observes, pointed out this way of evacuating the putrid matter, as almost all that were taken with the Dysentery, had at the beginning a continued propensity to vomit: many vomited and that with benefit. He always omitted the emetic, when there was the least suspicion of inflammation. He  
says



says that Eller found no evacuation so conducive towards the cure of an Epidemic Dysentery, as vomiting, given at repeated times thoroughly to expel the irritating bilious matter.—Zimmerman observes in p. 236, that “in the malignant Dysentery, evacuations must sometimes be entirely omitted: sometimes Emetics are noxious at the beginning, though very often it is necessary to give a vomit first, and afterwards purges.” But experience alone can be our guide in distinguishing these cases.

In general Emetics are most effectual and have the best effects, when they operate likewise by stool. Pringle in imitation of Eller's practice managed the Ipecacuanha, so as to effect both purposes, for he gave it in small doses repeated several times in the day, till a vomiting or purging ensued; fifteen grains administered in this manner procured a larger evacuation, than thirty grains given at once. This practice however, he laid aside from the great sickness which generally accompanied the operation, though he declares, notwithstanding his partiality to the vitrum antimonii ceratum, that he is not yet clear, whether it be not the surest method of cure. These small doses however,



will on many occasions produce no vomiting where it is very requisite, namely, when the stomach is loaded with a quantity of pituitous matter; here we must employ the Emetic tartar alone, or in combination with the Ipecacuanha, as this remedy is more powerful in clearing the stomach or intestines of their contents. Akenside's mode of giving the Ipecacuanha was still more in the extreme, for he gave but one grain every six hours, and on it alone, excepting V.S. and an Emetic, he relied for the cure of Dysentery.

The great efficacy of Ipecacuanha, however administered, has by some been attributed to an antispasmodic, and by others to a purgative power: Friend more justly gives the credit to the diaphoresis consequent on its use: he says, "*Radix Ipecacuanha* " *præter vim vomitoriam quam obtinet,* " *uberrimum sudorem excitare solet, atque* " *in hoc (quantum ego conjecturâ assequi* " *possum) principuè consistit egregia illa* " *in Dysentericis affectibus virtus quam* " *sibi præ aliis vomendi instrumentis vindic-* " *cat.*" Piso, who bestows the most extravagant praises on this remedy, attributes part of its good effects to an astringent quality:



quality: “ Quippe præterquam, inquit,  
 “ quod tutè et efficaciter tenacissimos quos-  
 “ que humores per ipsam alvum, sæpissimè  
 “ autem per vomitum ejiciat, & a parte  
 “ affectâ derivet, vim quoque astrictivam  
 “ post se relinquit.”\* For this reason and  
 for want of sufficient powers, Sir G. Baker  
 prefers the Emetic tartar to it, as that re-  
 medy surpasses it in emetic and diaphoretic  
 effects: it also possesses this great advan-  
 tage, that after clearing the stomach, its  
 powers do not cease there; but are effectual  
 in producing a full evacuation of the intes-  
 tinal canal. It was no doubt owing to its  
 power of producing these two important ef-  
 fects, that the vitrum antimonii ceratum  
 obtained its great celebrity in Dysentery,  
 and would still hold a pre-eminent station,  
 but that the violence and uncertainty of its  
 operation rendered its exhibition at times  
 dangerous or nugatory.

\* Blane gives testimony of the good effects of Ipecacu-  
 anha, without specifying the mode in which it acted:  
 “ the best medicine, he says, in the day time, we found  
 “ to be small doses of Ipecacuanha alone, twice or thrice  
 “ a day.” To athletic seamen he gave it in two grain  
 doses, but one was sufficient for the more delicate consti-  
 tutions of private practice.

Thirdly,



Thirdly, *Purgatives*—These, though less obviously indicated in the Dysentery than emetics, are yet found more essentially necessary to a successful management of the disease. When practitioners permitted their judgment to be guided by the appearance, and not by the reality of things, the frequency of stools lead them to entertain the falsest notions, and adopt the most erroneous and destructive practice: hence arose the early use of astringents and opiates in the Dysentery, and hence the similarity of treatment in that disease, and Diarrhœa.\* But we have now procured a greater insight into the real nature of this disease, and in proportion as we advanced in our knowledge of it, have purgatives been advancing in estimation: they are now as universally employed as they were before uniformly neglected, and the question no longer is, shall purgatives be given, but what purgative, and in what manner.

\* We may observe the influence of this doctrine, in all the old, and even many modern works, on the *Materia Medica*: in these we can scarcely find an article praised for its efficacy in Diarrhœa, that is not said to be equally good against the Dysentery.

Hunter



Hunter always began by giving as early as possible a brisk purgative, either the magnesia vitriolata or Glauber's salt: its operation was assisted by drinking plentifully of diluting liquors, till a full and copious evacuation was produced. Sir G. Baker speaks thus highly of the magnesia vitriolata, "Nil  
" fere quidpiam, inquit, aut certius aut  
" citius alvo moranti calcar addit." With respect to purgatives in general, Hunter observes, that though various articles of this class are recommended by different writers, it is yet probable, there is nothing specific in any of them, and that they are more or less beneficial, only as they possess in a greater or less degree, the power of operating easily, speedily and effectually; and some, experience often shews to agree with particular constitutions better than others. After a favourable operation of the purgative, he gave an opiate at bed-time: the former in almost all cases procures a truce with the disease, and the latter prolongs it; but it is in slight cases only, and at the commencement of the disease, that one dose of physic is sufficient to stop its progress: a respite is in general all that is obtained: for on the recurrence of the symptoms,



symptoms, the same remedies are to be repeated. The sick are not weakened, he says, by the operation of purgatives, at least so long as they procure relief from the griping pain; and Monro, expresses his astonishment at the little loss of strength, which his patients suffered on being so frequently purged, for he found that a great part of the cure depended on the frequent repetition of the gentle purges he gave in the beginning, and he observed that the patient, instead of being weak upon it, appeared to acquire a greater degree of strength and cheerfulness from the relief that followed the operation of each purge; and indeed says Zimmerman, “the truth of this great  
“medical maxim appears evident with re-  
“spect to purges in the putrid Dysentery,  
“that in it, no other medicines strengthen  
“the patient, than such as diminish his  
“disorder: and that he is very often  
“strengthened most when he thinks he is  
“most debilitated.” But there is a period, says Hunter, beyond which purgatives cannot be continued with advantage, when the disease has been violent, the purgatives frequently repeated, and the symptoms still recur, while the strength is greatly im-  
paired.



paired. In this situation, he gave a strong decoction or infusion of the Peruvian bark and Camomile tea, with so much Rhubarb in it as would procure two or three copious discharges from the bowels in the four and twenty hours.

“ The bloody stools did not hinder me  
“ from giving purges, says Zimmerman, as  
“ I saw they cleared the bowels of the acrid  
“ matter, and that no more blood appeared  
“ in the stools, as soon as this matter was  
“ quite evacuated; and I gave them, he  
“ adds, as long as there were any indica-  
“ tions of an acrid putrid matter in the  
“ bowels, without inflammation or suppura-  
“ tion.” v. p. 39.—He always chose them  
of the gentle and acid kind, as strong pur-  
gatives in the Dysentery always occasion an  
intolerable colic, and greatly weaken the  
patient.

Pringle declares, the necessity for conti-  
nuing the purges is rather to be determined  
by the pertinacity of the gripings, and  
bearing down, than by the blood, that ap-  
pears in the stools: and he thinks it impos-  
sible to effect a cure without copious eva-  
cuations of this kind; he therefore advises  
us, not so much to attend to the dose, as to  
the



the effects, which are to be judged of, not by the number, but by the largeness of the stools, and the relief which the patient experiences from the gripings and tenesmus.—“Purgatives and opiates alternately, says Willan, was the only mode of mitigating and shortening the disease; the operation of the purgatives was attended with a great, though temporary aggravation of the pain and straining, so that the patient could not be induced to take the same medicine twice.” His purgatives were the Magnesia vitriolata, oleum Ricini, & Calomel: and these it was necessary to give in succession, or in different forms. Strong doses were likewise requisite in order to produce stools of the usual smell and consistence: and unless this effect took place, the patient was made to suffer without any advantage. Neither did opiates afford rest or the least alleviation of pain, if not prescribed in the strongest form: and after the purgative, it was necessary to give a draught early in the evening, and repeat it every three hours, in order to secure some quiet during the night, and a respite from the morning exacerbation. The opiates he gave every night, but found that the purgative could seldom



seldom be borne oftener than every second or third day.\* Zimmerman in p. 242 and 3, though he admits the great benefit derived from emetics and purgatives in the malignant Dysentery, yet cautious against their indiscriminate or repeated use in that form of the disease. With respect to the choice of purgatives, he seems on the whole from his own experience and that of others, to decide in favour of the sal catharticum amarum, especially in combination with manna and oil, which operated without pain or anxiety, evacuated much better, and gave more relief, than any other. Rhubarb, formerly the favourite purgative in Dysentery, has long since given place to others more active and efficacious: perhaps however, it

\* “ It is, says Dewar, when Dysentery proves tedious  
 “ and obstinate, and is attended with many varying  
 “ symptoms of debility, that all our assiduity is requisite  
 “ to prevent the patient from rapidly declining.—We  
 “ must take care to keep the bowels moderately open,  
 “ but must avoid the most trifling excess in our laxatives :  
 “ and though there should be some tendency to costive-  
 “ ness, if there is no pain in the bowels, it is unnecessary  
 “ to solicit a motion oftener than once in two days. The  
 “ intestines will improve in strength by being seldom ex-  
 “ cited, provided the fæces do not accumulate in such  
 “ quantity, as to produce obstruction or annoyance.”

does



does not merit all the reprobation it has received, and though it must rank as inferior in virtue to many others, yet should it not therefore be deemed an improper remedy in the Dysentery. Degner employed it in the state of watery tincture, and if we are to judge from his declarations, and his language, when he calls it, “divinum potius quam humanum remedium,” we must suppose with considerable advantage. Zimmerman too made use of it when other purgatives would not keep on the patient’s stomach; then, he says, the tincture of Rhubarb given in great quantities had something very excellent in it, as the stomach bears it very well, as it often takes off the vomiting, and as it *at last* puts an end to the disorder, though not so speedily as other purgatives. “Others, say Blane, as well as myself have made a practical comparison of the saline purgative, with that composed of Rhubarb and Calomel, and we gave the preference to the former, as more easy, speedy and effectual, especially in the first stage.” He adds however, that cases may occur where the other would be more adviseable, as where there is a sense of weight about the stomach, arising most probably from the biliary organs being



being clogged with bile, and which emetics have failed to remove: then the Calomel has the best effects.

“ Upon the whole however, says Wilson, p. 649, no other cathartic has been so celebrated in Dysentery as Ipecacuanha given in small doses to prevent its proving emetic. From the very many trials I have made with it, it appears to me to be the best of all cathartics in Dysentery: and this probably, in part at least, depends on the relaxation it induces on the skin, which is always accompanied with a tendency to a similar relaxation in the alimentary canal.” He adds, that the *Antimonium tartarisatum*, though an excellent remedy, yet seems upon the whole much inferior to the latter in relieving that peculiar state of the bowels, which appears to constitute the disease.

I shall conclude these remarks on purgative medicines, merely by noticing that the testimony of the most judicious practitioners is very decidedly in favour of such of them, as may be made occasionally to act as emetics, and more especially of such as have a tendency to act on the skin: such purgatives as these may be almost relied on for the



compleat cure of Dysentery, independent of other aid; whereas such as operate solely on the intestines, though very effectual in relieving the disease, fall far short of the former, as Anti-dysenteric medicines. We shall be the more confirmed in this opinion, after we have considered the various testimonies in favour of

*Sudorifics.*—The remedies already treated of, Venæsection, Emetics and Purgatives, though each very efficacious in their *direct* operation to afford considerable relief in the Dysentery, are yet, if carried no further, very far short of effecting a speedy, compleat, and permanent cure of the disease. This office, with the aid of the former, peculiarly belongs to that class of medicines now to be spoken of; of the truth of this position, I do not fear being able to produce testimony, such as shall satisfy any reasonable mind. And in the first place I would ask, what are those medicines which have at different times acquired such celebrity in Dysentery? are they not such, as besides their emetic and purgative effects, exert a more uniform and direct influence on the vessels of the skin? are not Ipecacuanha, Tartar Emetic, and the Vitrum Antimonii Ceratum,



Ceratum, the only articles, which have with any justice, laid claim to the title of specific remedies in the Dysentery? so far we have presumptive evidence in our favour, I shall now adduce more positive testimony in its support.

“ This early and reasonable measure, says  
 “ Blane, (meaning a brisk purgative) will in  
 “ many cases, put a stop to the disease, *es-*  
 “ *pecially*, if the patient is thrown into a  
 “ sweat immediately after the bowels have  
 “ been thus thoroughly evacuated. It is of  
 “ great service in this disease to promote free  
 “ perspiration, and even a plentiful sweat,  
 “ which may be effected with great advantage  
 “ by giving, at bed time, a medicine com-  
 “ posed of Opium, Ipecacuanha, and a little  
 “ Neutral Salt, accompanying it with plenti-  
 “ ful warm dilution. Nothing tends more to  
 “ relieve griping and tenemus, than a gene-  
 “ ral warm moisture on the skin. The Ipe-  
 “ cacuanha, which is an ingredient in this  
 “ medicine, is one of the *best* Anti-dysen-  
 “ teric remedies we know; the opium pro-  
 “ cures rest; and this joined to the sudorific  
 “ effect of the whole, not only gives a tem-  
 “ porary relief, but tends to carry off the  
 “ disease.” *vid.* p. 456.



“ I have with great pleasure seen, says  
“ Hillary, that giving the Ipecacuanha in  
“ small doses, viz. *three* grains, when the  
“ patient was reduced so low that he could  
“ not bear any further evacuations, and re-  
“ peating it every three hours, till he has  
“ taken four doses, and increasing the last  
“ dose to six or seven grains, it has after  
“ giving a gentle puke or two, almost sur-  
“ prisingly restrained the flux, and brought  
“ on a fine free Diaphoresis all over the  
“ body, which was continued for some hours  
“ by drinking small warm liquids; and the  
“ delirium, tremors, and all the other bad  
“ symptoms, went off, and the patient has  
“ from that time soon recovered, by only giv-  
“ ing a few doses of a restringent diaphoretic  
“ opiate, and was thus as it were, snatched  
“ from the jaws of death.” *vid.* p. 214.

Moseley is well known as one of the most strenuous advocates for the sudorific plan of treatment in Dysentery; perhaps he is too much attached to it, to the neglect of other efficacious means: it is however, sufficient for my purpose to shew that from his experience of the plan, he was justified in his partiality towards it. It was his practice after the emetic, and opiate, to empty the  
bowels,



bowels, *but with caution*, in case the patient is weak, and in such a manner as not to increase the determination of blood there and divert it from the surface, for then we lose the ground gained by the vomit, and counteract the principal design in giving it. An Antimonial that acts much on the skin, and purges at the same time, is what Moseley depended on. James's powder he found admirably calculated to answer this intention; it possesses in his estimation this great advantage, that though it shall effectually cleanse the primæ viæ, properly given, it never fails to excite a plentiful sweat, and its effects terminate on the skin.

The sudorific process once begun, must be kept up by uniting an opiate with a diaphoretic, and administering them occasionally. Laudanum and Antimonial Wine combined, causing little irritation, and being a pleasant and certain diaphoretic, will answer this purpose. Laudanum is necessary to Antimonial or other emetic medicines, where a sweat is intended, in order to take off their irritation; by which means their doses and effects may be greatly extended. When this process is successfully continued, all the symptoms grow milder, and if the



patient breaks out into a rash or efflorescent eruption, the disease will soon be removed; even the first stool, after sweating was raised, has been less bloody, and the third or fourth often scarcely tinged. *vid.* Passim, p. 248, and seq. for a more full detail of this process and its advantages, and for every direction necessary to insure its success.

Tissot says that he has cured many Dysenteries, by ordering the sick no other remedy, than a cup of warm water every quarter of an hour. It is likely that in these cases it operated by producing Diaphoresis.

La Moniere, Fabricius Hildanus, Baglivi, and others, all bear witness to the good effects, consequent on sweating: and Richter says, there was no symptom more favourable than a moist skin, and that the cure of Dysentery does not so much depend on vomiting and purging, as upon allaying irritation, and upon perspiration being gently increased. Besides these, I might adduce various other testimonies in favour of sudorifics, but shall satisfy myself with speaking of a few remedies, found by experience to be possessed of considerable efficacy in this disease, the great tendency of each of which must have been that, of inducing a considerable



derable relaxation on the surface of the body, and consequently disposing it to a free discharge. I shall begin with the *Warm Bath*: This is a means of relief hitherto much overlooked in the Dysentery, and yet it is one, that from its general nature and operation, and from the few trials, which have been made of it, I should be disposed to consider, as bidding very fair to stand foremost among the most effectual remedies against this disease. The obvious tendency of the warm bath to remove all constriction from the surface, and thereby to relieve internal pains, more particularly of the abdomen, should have long since led to its employment; and the positive benefits derived from its use would have been the best arguments in favour of its continuance. The analogy of other diseases should give us every hope that the remedy would prove beneficial: the little experience we have of it, confirms these hopes in the strongest manner. Baker, whose name I have mentioned more than once, with the respect and authority it deserves, says, that he made but one trial, “ Si quid, et in hoc affectu valere  
 “ posset: Experimentum autem fœliciter  
 “ atque ex voto cessit; ægrotanti, quem per  
 N 4 “ universam



“ universam Hebdomadam malè habuerat  
 “ Dysenteria, in aquam demisso, dolor ilico  
 “ omnis conquievit, *alvus largè resoluta est:*  
 “ facilis inde ad bonam valetudinem recur-  
 “ sus.” Besides the immediate ease from  
 pain which was experienced in this case, I  
 wish particularly to remark another impor-  
 tant advantage gained by it, the “ *Alvus*  
 “ *largè resoluta:*” for it is in this point of  
 view that I consider the warm bath as a most  
 effectual and valuable remedy: it acts im-  
 mediately on the skin, and remotely on the  
 intestines, relieves all their uneasy sensa-  
 tions, and removes that condition of them,  
 which counteracts the free expulsion of their  
 contents. I have understood from undoubt-  
 ed authority, that the warm bath was exten-  
 sively employed by one of our surgeons in  
 the last unfortunate expedition against Hol-  
 land, and that his success in the disease was  
 very considerable from that circumstance  
 alone; the effects which he experienced from  
 its use, were immediate relief from the tor-  
 mina and tenemus, and a great facility of  
 cleansing the bowels by the mildest laxatives,  
 the difficulty and pain of doing which, first  
 led him to the trial. The great advantage  
 derived from it, consisted in curtailing the  
 duration



duration of the disease, and in procuring a considerable amelioration of all the symptoms during its further continuance.

Blane says, that he has observed great benefit from the use of external remedies, and that, perhaps, these have been too much neglected. The warm bath, he adds, is of great service, especially where the gripes and tenesmus are severe, and where the fever has been taken off by previous evacuation. Fomentations also, or warm applications of any kind to the abdomen give temporary relief. In private practice, he says, he has afforded considerable relief by fomenting the anus with hot water, or decoction of Camomile flowers, with some Laudanum sprinkled on the stupes. This, he adds, has so sensible and sudden an effect in allaying the agonies of tenesmus, that any patient who has once experienced the good effects of it, will not fail to call for its repetition upon a recurrence of the same sufferings.—Indeed, there is scarcely a practitioner, that does not speak of the great benefit derived from fomentations: and if these do so much, what must we expect from the warm bath?—“Tepid baths could not well be employed  
“ in



“ in the country, says Richter, however,  
“ much I expected they would be of service.  
“ I tried them twice in the hospital with *evident*  
“ benefit. Warmth in general was of  
“ great use: the disease was frequently fatal  
“ among the country people, from their  
“ often getting out of bed with bare feet on  
“ stone floors.”

Moseley says, p. 319, that Alexander Tralianus recommends warm baths as useful in the beginning of the disease; and in p. 321, that Avicenna observes, sleep to be of all things most beneficial to people with fluxes; and that he advises baths and frictions with warm oils to open the pores, and bring the humours to the surface of the body. Dry-cupping the belly also, the latter says, has often removed fluxes and excoriations of the bowels in four hours; and that he had himself experienced it.

Dr. Rollo observes, that the most important benefits have been derived from warm bathing and fomentations; and Rush mentions, in his *Medical Observations and Enquiries*, that the warm bath was found, in many cases, to be an effectual remedy for an obstinate Diarrhœa, consequent on Dysentery.



sentery. Rollo says, the degree of heat should not be much *above*\* blood warm.

After the testimonies I have adduced in favour of this remedy, we can scarcely hesitate a moment in giving it our warmest approbation. It is perhaps, after an emetic, one of the first remedies I would adopt in the commencement of Dysentery, and also one of those upon which I would principally rely during the course of the disease. It would constitute a powerful auxiliary to Moseley's sudorific plan, and it would render essential assistance to the operation of purgatives; for these we have seen, when exhibited alone, to create a temporary aggravation of all the symptoms, and afterwards only procured a short truce with the disease, which was sure soon to return, almost as severe as ever. The warm bath properly employed is calculated to render

\* The degree of heat best adapted to the Dysentery is a point not mentioned by any other author, who has spoken of the remedy. It is a point of some moment, and therefore I take notice of Dr. Rollo's injunction—in my opinion the heat should be rather *below* than above that of the human body: perhaps somewhat between 92° and 96°—for the most correct observations respecting warm baths, consult Marcard sur l'Usage des Bains Tiedes, &c.—it is a translation from the German.



the operation of the purgative both easy and effectual, as it guards against a relapse.\*— It is indeed a remedy, which, when in our power, should never be omitted :

*Æquè pauperibus prodest, locupletibus æquè,  
Æquè neglectum pueris, senibusque nocebit.*

I shall next speak of a practice, of late first recommended by Dr. Whyte, and the great advantages of which are strongly insisted on by Mr. Dewar: I mean swathing the abdomen with flannel, the beneficial effects of which appear, from that gentleman's account of it, to be far greater than one would at first expect. He describes, p. 112, the particular manner in which he employed it, and says, that “ when begun  
“ early, and well attended to, not neglect-  
“ ing the usual collateral means, it seldom  
“ fails to effect a cure: it acts partly on  
“ the same principle as the tepid bath,  
“ which is one of the best remedies for in-  
“ flammatory diseases attended with slight,  
“ but constant internal pain, and proceed-

\* It must be observed of the warm bath, and of sudorifics in general, that greater care than usual is required during their use, to guard against cold, as the body, while under their influence, is very susceptible of its impressions.



“ ing from cold: the flannel bandage, he  
“ says, possesses all its virtues, and is free  
“ from its disadvantages. By wearing it,  
“ the patient carries constantly along with  
“ him a bath of the best temperature, inva-  
“ riable in its heat, one which will, on no  
“ occasion, weaken him by profuse perspi-  
“ ration, and which, being never laid aside,  
“ till it is no longer necessary, cannot ex-  
“ pose him to the effects of cold by a change  
“ of temperature.”—The effects which im-  
mediately follow the application of this  
swathing he describes to be—1st, It re-  
moves that local torpor of the abdomen,  
under which a dysenteric patient often la-  
bours: 2d, It obviates rawness and griping:  
as the flannel bandage prevents the impres-  
sions of cold, which form the chief cause of  
these uneasy sensations, it does not fail to  
correct them: 3d, It removes dejection and  
languor; as the patient soon feels himself  
invigorated, and better fitted to relish the  
enjoyments of life: 4th, It corrects that  
dyspnœa, which is so often the consequence  
of dysenteric debility. The ultimate good  
effects of the flannel bandage are, he says,  
an increase of general strength, and a heal-  
ing process in the intestines, proceeding  
from



from an improved state of sensation in those organs. He does not, however, attribute all the good effects of this practice to the mere operation of its uniform temperature, but says, that when the bandage is first applied, much of its efficacy arises from its pressure: for pressure, even with the palm of the hand, often gives a temporary relief to the bowels; \* still, however, the warmth produced by the bandage is of more lasting benefit, and should be kept up without intermission, till the Dysentery is cured. As much of its good effects depends on a proper application of the bandage, Dewar gives particular instructions for that purpose, for which see p. 115.—The administration of a purgative in the beginning of the disease is attended with the most striking advantage in its operation, from being accompanied

\* We cannot be surprised, that in Dysentery much benefit should be experienced from the bandage on the principle of its pressure: for as we know, both from the sensations which are felt by the patient, and from every observation we can make on the disease, that irregularly convulsive, or spasmodic motions of the intestines give origin to its most distressing symptoms, so it is plain that nothing can obviate these symptoms sooner than such means as will restrain these motions, though other assistance may be necessary, entirely to remove them.

with



with this species of swathing; for when the body is thus bound up, the cold air is entirely excluded, and therefore the bowels are solely operated on by the force of the medicine: the excitement consequently which they receive, being regular and uniform, evacuates them more completely, at the same time that it has the best tendency to restore their healthy functions. I have dwelt thus long on the practice of bandaging the abdomen, because I conceive it deserving of the recommendation it has procured from this author: where the warm bath cannot be employed, it would form the best substitute; but previous to its application, I would, if possible, first immerse the patient in a warm bath, or, at least, use extensive and long continued fomentation; in any case I would never neglect the practice of swathing, but am satisfied that its effects must be much more decisive, when preceded and occasionally accompanied by the warm bath or fomentations.—The principle and advantages of this practice would seem not to be unknown to Ætius, when he says, “It is wonderful what  
“ good effects are produced by such plaisters  
“ as are used to bleeding wounds, after the  
“ inflammation is abated, and such as are  
“ used



“ used for fractures, applied round the whole  
‘ belly and loins to the back.” *vid.* Mose-  
ley, p. 314.

Besides the warm bath, and swathing, I am inclined to rank among the list of diaphoretic medicines, an article, which as a remedy in Dysentery, has received its share of praise. It is admitted to be an all-powerful agent in various maladies, in some of which, from the obscurity in which its *modus operandi* lies buried, it is said to act as a specific, in others as a deobstruent, stimulant &c. &c. I need scarcely say that on this occasion, I allude to Mercury, which by many is said to possess a specific power over the Dysentery, though by others its beneficial influence is attributed to its purgative effects; I am disposed to resolve its specific power into its action as a diaphoretic, though on this head no positive proofs can be given: indeed, such could not, in the solution of a question of this kind, be reasonably expected.

Dr. Clarke, was among the first to propose Mercury as a specific in Dysentery, and that not only in its acute, but chronic state; he declares himself from experience thoroughly persuaded that it is possessed of powers to remove inflammation and ulceration of the  
intestines,



intestines, which he says, are the chief causes of death in this distemper. To those in the chronic stage, he gave small doses of Calomel with Opium, every night, and sometimes in the morning, with a purge at intervals. Calomel in almost every instance in which it was exhibited, soon subdued the disease, or reduced it to the nature of a Simple Diarrhœa. In the acute stage, he says, he was doubtful, whether to attribute its effects to its purgative or mercurial quality, but that he was soon convinced its virtues were owing to its mercurial powers, on finding in two patients, to whom he administered three grains of Calomel with Opium, every four hours, that when the gums became tender, the gripes and tenesmus were instantly relieved, natural evacuations followed, and health speedily restored without any other medicine. In obstinate cases, he adds, the disease will seldom abate much of its violence, till some degree of tenderness be perceived in the mouth: that still however, care must be taken not to induce much salivation, as that would prolong the recovery. In the acute stage of the disease, he preferred Calomel on account of its laxative qualities, and to render it more certainly so,



and likewise to *determine it to the surface*, he at first combined it with a small portion of Tartar Emetic: but more lately, he added no other medicine to it, than Opium. In the chronic stage, he gave the Calomel in much smaller doses, or if it proved too laxative, he substituted pills of the quicksilver, as extinguished in mucilage, and directed besides, a pill of one grain or two of Ipecacuanha, and half a grain of Opium to be taken every morning.

Before taking notice of the cases which Clarke has given us, as illustrative of this practice, I have been particular in stating the whole of that treatment for the purpose of remarking, what must be obvious to every one, that all those articles, with which he combined the Calomel, or administered at the same time with it, are possessed of no inconsiderable diaphoretic powers, and therefore that any tendency, which Mercury may have to act on the skin, must be greatly promoted by their assistance. We may also perceive on looking over the cases, that he was very constantly in the habit of giving, independent of the former articles, the Pulv. Ipecacuan. comp. one of the most powerful sudorifics, we are in possession of.

In



In offering any remarks on the treatment by Mercury, or on the cases brought in support of it, I do not mean in the smallest degree to reflect on the practice itself, for its singular and striking success must be admitted to rest on the most indubitable evidence: my sole object in so doing, is to attempt some rational explanation of the manner, in which it exerts such beneficial influence over the disease; for no one can believe that it cures Dysentery as it does Pox, by a *specific* power, or that it is the same property in that medicine, which destroys the diseased action in both: at least, I will say, that I never could bring my mind to give assent to the proposition, though at the same time I did not entertain the smallest doubt, that “the  
“ distemper would either be speedily remo-  
“ ved, or become extremely tractable, as  
“ soon as that medicine occasioned the  
“ slightest tenderness in the gums.” Should I succeed in making it appear at all probable, that Mercury owes its reputation in Dysentery to the power it possesses of raising and keeping up a steady, permanent, and uniform diaphoresis, (the great efficacy of which in that disease we have just witnessed) let no one object to this enquiry on the



ground of its inutility of the folly of wasting time in disputing about the *modus operandi* of a medicine, concerning the efficacy or administration of which, there is no question: "*Causa latet, res est notissima.*" To such I would only answer, that if there be any fair grounds for maintaining the opinion I have advanced, it must be of some moment not to be ignorant of it, as we might be induced thereby not only to assist the effective action of the remedy, but also be enabled to avoid such circumstances as might tend to counteract it: for if I am not much mistaken, I have seen cases, where Mercury, though carried its full length, failed in producing its usual beneficial effects, because attention was not paid to such circumstances.

It is to be observed that the Mercury, though as a purgative it might afford occasional relief, yet never induced any essential change in the state of the disease, till such time as the gums became tender, when, with almost invariable uniformity, the gripes and tenemus permanently cease, and natural evacuations follow, or are easily procured by any gentle purgative. This circumstance is of course adduced as the strongest and most decisive argument for the specific power of  
the



the medicine; but we may perhaps be inclined to doubt this conclusion, when we reflect on the obvious changes induced by the action of Mercury: in the interval between its first administration, and its affecting the gums, its stimulating power is felt in a quickened circulation, and often in feverish heats: during this period it is rather hurtful in Dysentery, unless it operates by stool, and other means are found necessary to check the progress of the disease, and alleviate the sufferings of the patient; but when pushed so far, that its action is perceived in the mouth, then the disease most surprisingly vanishes; but what had been the changes previously induced? The stimulating effects of the remedy have ceased, and a general relaxation (if I may use the word) has taken place: all the secretions and excretions are increased, and that by the skin in a remarkable manner. It is also to be noticed that the sooner these effects are produced, and with the smallest quantity possible, the better; and that there is no necessity for keeping up the action of the remedy on the gums for any length of time; a circumstance essentially necessary in the treatment of Pox, where the specific and not the



diaphoretic power of Mercury is required. That such a state of the skin, as I have described, precedes or accompanies this favourable change in Dysentery will appear from a few of those cases, which Clarke has detailed: and let me observe here, that where this effect is occasionally pointed out as consequent on the use of Mercury, by an author, who did not look upon that effect as at all essential to the beneficial operation of the remedy, we may reasonably suppose, that unless it occurred in those instances in a remarkable degree, it would not have been noted by him; and *therefore* that we are not to conclude, it did not take place at all, in those cases, where all mention of it is omitted.

This very intelligent and accurate writer has described eleven cases of the acute disease, and one of the chronic at full length: in the first three there is no allusion whatever to diaphoresis as consequent on the use of Mercury, or as simultaneous with a favourable change in the disease: but in the fourth case of Christian Hall, he says, “ she  
“ continued regularly in the use of these  
“ pills (Calomel and Opium) till as many  
“ were taken as contained fifteen grains of  
“ Calomel.



“ Calomel. Her disease then yielded: her stools became natural, and she was in a *constant uniform perspiration.*” In case sixth, which was complicated with a low remittent fever, the patient after taking about half a drachm of Calomel, is said to have sweated during the whole night, and on the subsequent day, to have passed eight bilious offensive stools, but without any tenesmus; on the day after, the Dysenteric symptoms totally disappeared.—Case the eighth was exceedingly severe, and somewhat advanced before he visited her: on giving the Calomel and Opium as usual, the patient is described, but previous to the affection of the mouth, to have got into an universal sweat, in consequence of which, the evacuations became feculent and bilious, and the gripes and tenesmus were much relieved: the case however, being unusually severe and protracted, a further continuance of the remedy was necessary, till her mouth was considerably ulcerated, when the painful complaints of the bowels disappeared. The eleventh case was also more than usually obstinate: this patient is described on the 15th, to have been somewhat easier, after having taken thirty nine grains of Calomel, and the mouth being



made a little tender. On the 16th, she is said to have had eight stools during the last sixteen hours, most of them feculent and intimately mixed with green bile. *She was also in an universal warm sweat.* And on the Calomel and Opium being still continued, till she had taken seventy grains of the former, her stools are then said to be commonly feculent, and she herself to be generally *in a moderate perspiration.*

Such is the evidence afforded by Clarke's cases: I do not adduce it as proof approaching to any certainty, but with that, which is to follow, it will, I trust, give some colour of probability to the supposition advanced relative to the modus operandi of this remedy in the cure of Dysentery. Milne, who explains all the phenomena of Dysentery, upon the supposition of stricture, and consequent over-determination of blood to the contracted and excited parts, thinks Calomel and mercurial frictions perfectly adapted to obviate both, removing the stricture by a purgative operation, and the over-determination of blood, by means of a mercurial action upon the system at large, as otherwise the practice would answer no purpose. He gave the Calomel purgatives in  
quantities



quantities of from eight to twelve grains, generally the latter, to be sure of its effects. Not one dose however, probably not two, perhaps not three, not even more, will be found sufficient; so that it is therefore to be given dose after dose without interruption, that is, night and morning, accompanied by mercurial friction of the abdomen, until the mouth becomes sore: by this time, the disease very often yields, so that, as he says, Mercury must be possessed of a powerfully antispasmodic or relaxing quality. Occasional purgatives of Rhubarb or Glauber's Salts should be employed till the bowels are perfectly regular in their action: more Calomel will hardly be requisite, but the mercurial frictions may be continued, if the affection of the mouth will admit; he adds, that the sooner the mouth is affected, while purging is had recourse to at the same time, the greater will be the certainty of a speedy and permanent cure.—Such is a general outline of the treatment adopted by this writer, than which, he is inclined to assert, none will be found so efficacious. That the remedy acted as a sudorific in his hands, he does not assert, neither does he furnish us with the detail of any cases, whence such an effect



effect might be inferred; but there is a paragraph in p. 147, of his work, from which I should conclude that it must very frequently have operated in that way, and at the very time the mouth was made sore: “ Would nauseating doses of Emetics, he asks, if given in this disease, act in conjunction with Mercury? would they render *its effects* more powerful? Nausea produces costiveness—so does the *mercurial action*, by *determining powerfully to the surface*. Nauseating medicines, he says, appear to produce a more equable circulation, and to diminish any increased flow to a particular part: but probably their effects ought to be longer continued, and more constant than is generally adopted in practice. *A more or less powerful operation* is to be expected from Mercury according to the affection of the mouth.” From the whole of this passage, I should be inclined to infer, that the *mercurial action* pretty uniformly operated with him in *determining powerfully to the surface*.

Such is the whole of the evidence I have been able to collect in favour of this conjecture, for as such only I advance it. I have assigned my reasons for doubting that Mercury



cury could act as a specific remedy in the disease: the well known efficacy of sudorifics in it, and the appearances, which seemed to indicate that this medicine had operated not unfrequently in that way, have induced me to rank it among the remedies of that class. But whether my conjectures be well or ill founded, still will the reputation, which Mercury has so deservedly obtained against Dysentery, remain perfectly unaffected by them.

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## SECTION II.

### CORROBORANTS.

HAVING undertaken to speak of the Treatment of Dysentery, principally with the view of establishing those positions which had been advanced respecting that disease, and having already seen how far the employment of Evacuants warranted us in maintaining them, I shall proceed to take into consideration various testimonies in favour of the exhibition of certain remedies possessing a tendency somewhat opposed to the former: these I shall rank together under the general title of Corroborants; and  
of



of these Bark and Wine shall engage the greater share of our attention, as they constitute the principal and most obvious articles of the class, and as the judgment we shall form on them will of course decide our sentiments respecting the remainder.—And

1st, *Of the Bark.*—This remedy has been employed against disease, in general with some one of these views: to put an end to such as are possessed of the intermittent or remittent character, and to obviate acute or chronic debility. It has been pretty extensively exhibited in the Dysentery in its various forms and stages; of course with different views, and with variable success. On an examination of evidence, I imagine we shall find, that in the intermittent and remittent forms of the disease, it has been administered with the first intention, and with considerable success, that in the Malignant Dysentery it has been used to obviate the acute debility so constantly present with that form, and that it has also been employed to remove that state of chronic weakness, so uniformly consequent on the disease in general.

The person who seems first to have used the Bark in Dysentery was Morton, the unjustly



justly neglected Morton: he successfully employed it in conjunction with Opium, not only in those Dysenteries with the most regular remissions and exacerbations, “sed et in ipsis quotidianis Dysenteriis,” as he expresses it, “quoties scilicet et quamdiu se de genere  $\Sigma\upsilon\upsilon\epsilon\chi\epsilon\omega\upsilon$ , ex quantulacunque alterante symptomatum exacerbatione & remissione, et non *malignas* esse proderent, pharmaci hujus polychresti vires experirer, neque eventus, mehercule, spem meam unquam fefellit.” No language can be more precise, or meaning more distinct, respecting the circumstances under which he administered the Bark: one exception that he makes may at first sight appear rather singular, as it would seem to be the case, where we should suspect the aid of the Bark most requisite; the exception is conveyed in the words “et non *malignas* esse proderent:” then he says, he would not try the powers of this valuable remedy: What could be his reason for this, or what his meaning in the passage? I should imagine that the “non *malignas*” referred to the combination of Dysentery with Typhus, over which the Bark possesses no such powers as over the intermittent and remittent forms: for the exhibition of that

article



article has the Fever in general, and not the Dysentery in view, though from the strong connection between them, in destroying one it may put an end to both.

Morton's example and practice has been imitated by several: and with a result agreeable to his promises and their wishes. Dr. J. Hume observing, that towards morning the sick were always remarkably easy, and free from griping or much purging, and that towards noon all the complaints, with tormina and thirst returned, made trial of the Bark, which he gave in substance to the quantity of a drachm or more at a time. When the complaints returned, he desisted from the Bark, and gave the usual anti-dysenteric remedies for the remainder of the day, not sparing opiates at night. The success of this treatment exceeded his most sanguine expectations. Millar, Lind, and Hunter recommend similar practice, and in p. 52 of this treatise I have mentioned the use which Cleghorn made of the Bark in the Intermittent Dysentery.

Reide, after a cathartic, opiate, and sudorific, gave the Bark, first in decoction, with a few drops of the Tincture of Opium, to prevent its running off too quickly by  
the



the bowels; according as the bowels acquired strength, the powder was mixed with the decoction, and in the decline of the disease the Bark was given entirely in substance. It was prescribed, he says, in near 300 cases of the disease during his residence in the West Indies, and the only case of the disorder which proved fatal was one, where it was not employed.

“ When the Dysentery assumes the intermittent form, says Rollo, the return of the paroxysms is to be prevented by an early exhibition of the Peruvian Bark, in as large doses as the stomach will bear. If the disease is more continued, but distinguished by alleviations and exacerbations, the Bark is to be used during the former, and in the latter the other remedies and means of treatment are to be adopted. By a timely exhibition of the Bark in the first alleviations of the disease, after the necessary evacuations have been made, a favourable turn generally takes place.” *v. p. 69.*

Clarke, who was such an advocate for the Bark in every species of Fever, says, that it seems to be well adapted for the cure of this disease, especially when it depends upon the  
same



same causes which produce Remittent Fever. “ In the Putrid Flux of Bengal, he says, “ no medicine was attended with more wonderful effects: it was found as necessary “ a part of the cure as vomits and purges; “ yet in several cases the bowels were so ir- “ ritable, that notwithstanding the use of “ opiates, the medicine was speedily car- “ ried off by stool, and the patients in a “ manner half corrupted, fell victims to the “ disease. Since that, however, he adds, “ I have prescribed it in the Straits of Ma- “ lacca, in China, and in England, with- “ out producing any good effects: it seem- “ ed, on the contrary, often to aggravate “ all the symptoms, and was *never* attended “ with the least advantage till the disease “ was overcome, and nothing seemed want- “ ing to complete the cure, except bracing “ the relaxed intestines.” Now in the Ben- gal Dysentery the disease often partook of considerable remissions, and hence the good effects of the Bark in it; it is not in Dy- sentery, as Dysentery, that it is serviceable: therefore when he tried it in that disease elsewhere, and probably then not attended by remissions, it was of no advantage what- ever,



ever, but seemed rather to aggravate all the symptoms.

Lautter (as quoted by Wilson) in his *Historia Med. Bien.* observes, that when the fever remitted, the urine depositing a lateritious sediment, however, irregular, or short the remissions, he immediately had recourse to the Bark, which he gave in substance. Scarcely, he says, had the patient taken half an ounce, when the stools became less frequent, the griping was allayed, the tenesmus, which formerly baffled all means that could be employed, almost wholly ceased, and the pulse at length lost its unusual frequency.

I cannot proceed further with this subject, without reverting to those observations of Sydenham's respecting the Epidemic diseases, which I have given in p. 40 and 41, but in particular to that which states, that "these  
" self-same diseases (Pleurisies, Anginas,  
" Dysenteries &c.) which thus appear as  
" Intercurrents, may exist, only as mani-  
" fest symptoms of the reigning fever, and  
" are *then* to be treated, not as essential  
" diseases, but by the method that fever  
" requires, adapted to each particular case."  
I need scarcely remark how fully this obser-  
P vation



vation is justified by every statement that has been made in this section.

“ This disease, says Tissot, is sometimes  
“ combined with an Intermitting fever: in  
“ which case, the Dysentery must be re-  
“ moved first, and the Intermittent after-  
“ wards. Nevertheless, if at each access,  
“ the fits of the fever have been very strong,  
“ Bark must be given as before directed.”

“ With respect to tonic medicines, says  
“ Wilson, p. 640, the tendency of this dis-  
“ ease to inflammation has deterred many  
“ from employing them at any period, and at  
“ an early period they are generally hurtful,  
“ even where the debility is considerable.  
“ When Dysentery, however, has been of  
“ some standing, and has occasioned much  
“ debility, or is complicated with Typhus, or  
“ with intermitting and remitting fevers,  
“ the Bark seems often to have proved highly  
“ serviceable. Sir J. Pringle recommends it  
“ with the Serpentaria, or the Serpentaria  
“ alone, when the fever is of a malignant  
“ nature.” That it is also often highly use-  
ful in Dysentery of long standing, and which  
has occasioned much debility, although it  
be not complicated either with Typhus or In-  
termitting Fever, appears, he says, from many  
observations.



observations. In such cases, Akenside gave it combined with a Cathartic.

That the Bark is beneficial in the combination with Typhus, though by no means so successful as in the intermittent and remittent forms, appears from what Zimmerman states in p. 250. He says, that “when the patient’s pulse sinks, his strength brought down, and he himself oppressed with anxiety, the disorder then requires all the same remedies, that are necessary in malignant fevers.” The Bark, he observes, claims a place here above all the rest; and though he does not state that it has been given with compleat success, yet he says, that it was found a better medicine than any other: to prove this, he adduces the experience and practice of De Haen, Monro, Dr. Medicus, and Tissot.

These statements respecting the use of the Bark in Dysentery, will, I trust, be found consistent with the observations made at the commencement of this section; the result, I doubt not, will be little less satisfactory, after we have considered the testimonies which are in favour of the exhibition of

*Wine and other Cordials.*—If the view which has been taken of this disease be just,



it must be obvious, that wine is inadmissible in the Dysentery itself, and that some essential change must be effected in its circumstances or condition, which can at all warrant the use of that remedy. We have seen that venæsection, though not absolutely necessary in the disease, may yet often be employed without injury, and frequently perhaps with benefit: we have besides seen that other evacuations were indispensable requisite to its cure, and that in the debilitated state, consequent on the disease, only, was assistance called for to support the patient's strength; in such cases as these Wine cannot fail of being highly detrimental when administered during the height of the disorder; accordingly we find various authors, and Zimmerman in particular exclaiming against its use, and detailing numberless instances of its mischievous effects: "Aromatics and Wine in general, says the latter, " p. 112, excite in Dysenteric persons a " dangerous irritation in the bowels, increase " the pain, fever and strangury: and when " they operate as astringents (which however, does not often happen) they produce all the bad effects of those dangerous " medicines: and Wine in particular excites " a very



“ a very dangerous sensation of anxiety in  
 “ the pit of the stomach, that very often ac-  
 “ companies an inflammation of the bowels,  
 “ or precedes it, or a gangrene, but which  
 “ ought not to be confounded with that  
 “ oppression of spirits, that appears directly  
 “ at the beginning of malignant Dysen-  
 “ teries. Brandy is here an absolute poison,  
 “ and all these remedies occasion, even in  
 “ convalescents, a most dangerous relapse  
 “ of the same disorder.” Little further need  
 be said to prove the pernicious tendency of  
 such practice in the Dysentery in general:  
 let us then endeavour to ascertain the parti-  
 cular cases where it may be required, and  
 the circumstances which then render it ne-  
 cessary.

“ In the advanced stages, says Wilson,  
 “ p. 643, when the debility is considerable,  
 “ or earlier if Typhus attends; Wine has  
 “ been found eminently useful. Dr. Brock-  
 “ lesby sometimes allowed his patients a pint  
 “ and a half of Port Wine, or even more,  
 “ every twenty-four hours. But some, he  
 “ says, object to the early use of Port Wine,  
 “ on account of its astringency; and Zim-  
 “ merman and others to the use of all kinds  
 “ of Wine and distilled Spirits at *every*  
 P 3 “ period



“ period of the disease. It is probable, he  
“ adds, upon comparing what Zimmerman  
“ says of these remedies, with what is said  
“ of them by others, that he did not distin-  
“ guish with sufficient care, the cases in  
“ which they should be employed.” I quote  
the latter part of this passage, because I am  
happy in having an opportunity of contra-  
dicting it; for I should indeed be sorry that  
a reflection of this kind could with justice be  
thrown on a practitioner so eminent for his  
integrity, discernment, and observation; at  
the same time I am perfectly satisfied that  
Dr. Wilson believed he had good grounds  
for what he asserted: in that case however,  
he could not possibly have read all Zimmer-  
man has delivered on the treatment of *malignant*  
*Dysentery*, and in particular, that re-  
mark of his in p. 245, which expressly states  
that “ Wine does as much *good* in this  
“ species of *Dysentery*, as *harm* in the  
“ others.” In support of this assertion he  
adduces the authority of Pringle, Monro,  
and Van Swieten: the former, he says, will  
have the malignant *Dysentery* treated in  
general like a malignant fever, in which  
nothing could exceed the effects of Wine  
with such patients, as were weak and de-  
prived



prived of all strength: he therefore advises the use of that remedy in certain circumstances in this Dysentery: he allows it in general in this disorder, when the patient's strength is decreasing, and his voice low and weak; but says at the same time that we can never be absolutely sure of the effects of Wine till we have tried it.

On the same principle that he forbad Wine in the Bilious Dysentery, did Zimmerman interdict the use of flesh-broths, which, though allowed by Degner in the Epidemic of Nimeguen, are yet, he says, contra-indicated in that species, as they promote putrefaction. Still, however, does he pronounce of them as he did of Wine, that in the Malignant Dysentery “ these broths  
 “ should form the patient's only food, with  
 “ a view to keep up his strength: for the  
 “ degeneracy of the humours in Malignant  
 “ Fevers seems to differ from the degene-  
 “ racy of the same in the bilious species,  
 “ not only in degree, but even in characte-  
 “ ristic symptoms; he adds, that the great  
 “ difference between the medicines employ-  
 “ ed in bilious and malignant Dysenteries  
 “ shews the necessity for making this dif-  
 “ ference in diet, especially as it is of great  
 P 4 “ importance



“ importance to keep up the patient’s  
“ strength in the latter, and this quality  
“ incontestably belongs to chicken-broth,  
“ though in other cases it has manifestly a  
“ contrary effect.” v. p. 244. I need here add  
nothing further respecting the exhibition of  
Wine in this disease, as it is obvious that no  
authors, except such as had met it in com-  
bination with Typhus, at all employed or  
advised the remedy. I shall therefore con-  
clude this article by referring to p. 70 and  
71 of this Treatise for an account of that  
treatment, which Rogers found it necessary  
to follow in the malignant Dysenteries of  
Cork.

With respect to other cordials, Zimmer-  
man says, that Camphor deserves a place  
next the cortex, for raising the patient’s  
forces in the malignant Dysentery: and  
adds, that it may be conveniently joined  
with the Extract of Bark, and even with  
Ipecacuanha, but must not be given in large  
doses. Dewar says, that Camphor forms  
one of the most useful articles in the class of  
cordials. P. 129, he observes, that “ if re-  
“ peated with proper frequency, it has a  
“ more lasting effect than Wine, in relieving  
“ the patient from sinking of the heart, and  
prostration



prostration of strength, and produces an agreeable sensation of warmth in the belly."

Where the Bark and Wine fail in supporting the patient, Zimmerman would seem to recommend Musk on the authority of Dr. Munro, and Extract of Saffron under the sanction of Bontius, as from the exhibition of the former in malignant Fever the patients were the next day better, their skin was moistened, their pulse rose, and the feverish symptoms went off by degrees. This, however, he says, he relates "only with a  
" view to mark out, with one stroke of his  
" pen, the perfectly peculiar nature of a  
" malignant Fever, for the use of such as  
" make a hotch-potch of all fevers, and  
" then attack it with the like hotch-potch  
" of medicines."

Before I proceed to state that mode of treatment, which, from the review that has been taken of the remedies more usually employed in Dysentery, I would conceive best adapted to each particular form of the disease, it may not be amiss previously to take some notice of two other articles, which, when administered under proper indications, may be found possessed of considerable advantages; the articles I mean are Opium  
and



and Blisters: and as one or both of them may in some degree act indirectly as Corroborants, they may, without great impropriety, be treated of under that head.

*Opium.*—The most uniform and obvious effects of this remedy, when employed in Dysentery, are the temporary alleviation or cessation of its distressing symptoms, and the production of costiveness: if the former object could be obtained without the latter, it would be an invaluable medicine in this disease, but as the one cannot be obtained by its means without the other, few articles in the *Materia Medica* have done more unspeakable mischief than the intemperate or untimely use of Opium in Dysentery.—There are now few authors who do not strongly caution us against it, under different circumstances of the disease, and though they in general agree that it must be avoided in the beginning, or before evacuations have been employed, they are not so unanimous as to the extent, or the object for which it should be administered. I shall not therefore, as it must appear needless, adduce any authorities against the use of Opium in the early stage of Dysentery: it stands on the same footing in that respect with



with astringents, and both cannot be too strongly condemned at that period. Zimmerman, in p. 205, has brought together the testimonies of various authors to that effect, and I should not here have taken any notice of that circumstance, but that he appears to reflect on Sydenham for having sanctioned by his authority the improper use of Opium, and its preparations, from which, he says, Sydenham does not seem to apprehend much danger. This censure could only arise from want of attention to the different occasions on which he administered that remedy, and to the different conditions of that Epidemic he described. In the *fully-formed* Dysentery, according to the indications laid down by him, he bled, and then purged repeatedly for several days, with an opiate after every purge, but if the disease proved so obstinate as not to give way to this treatment, he gave the opiate every morning and evening till it went off, and sometimes in larger doses every eight hours, if the former dose proved too weak to stop the flux: he adds, that he has not hitherto found the least inconvenience from so frequent a repetition of opiates, whatever mischief, inexperienced persons might groundlessly apprehend



hend from them—he further says, that when the flux amounts only to a *simple looseness*, it will be sufficient to give half a drachm of Rhubarb every morning, and an opiate the following evening. He also says, that during the first autumn of the Epidemic, wherein the Dysentery and dry Gripes prevailed, and in which the former was of a more spirituous and subtile nature than in the subsequent years, he found purgatives less effectual, but that the frequent repetition of *warm* glysters, and large dilution with *cold* whey, were attended with complete success in both diseases, for the patient, on being immediately put to bed, soon fell into a spontaneous sweat, which was kept up for four and twenty hours, but not provoked by medicine. These two last observations will tend to shew the very different conditions of the Dysentery in this Epidemic, or perhaps the too great looseness in which he employed that term; therefore we cannot justly censure him when he remarks, that “ though in those years, wherein  
“ the Dysentery raged so epidemically, eva-  
“ cuations were *absolutely* necessary before  
“ having recourse to Laudanum; yet in any  
“ constitution, which has *a less tendency* to  
“ this



“ this disease, they may be safely omitted,  
“ and the cure completed by the shorter  
“ method, namely, by the exhibition of  
“ Laudanum alone, in the manner above  
“ directed.”

The author next to Sydenham, who appears to have made most use of Opium, is Richter: in his Epidemic there was a great overflow of bile and considerable irritability of the stomach, so that other medicines could not well perform their office: in particular, emetics and purgatives often only added to these symptoms. “ The cure of  
“ the disease, he says, principally depended  
“ on allaying pain and irritation, and on  
“ raising a gentle diaphoresis. Opium and  
“ Antimony effected this: I can affirm, that  
“ by the use of Opium, the bilious symp-  
“ toms disappeared, and the patient was  
“ cured without evacuation.” Opium, he adds, did not bind the belly: it lessened the number of stools, and made them stercora- ceous. I have cured many also, he says, by emetic and purgative medicines, *without evacuation*. Emetics in small doses had evidently as great an effect, frequently even greater than in full doses, upon the pains and stools. From all which he thinks it  
appears



appears probable, that the cure of Dysentery does not chiefly depend upon vomiting and purging, but upon allaying irritation, and upon perspiration being gently increased. He does not, however, reject Emetics and Purgatives altogether, but gave them at the beginning of the disease, when there was any indication for them. The former, he says, evacuate the bile, an accessory irritation, diminish the spasm in the intestines, and promote a gentle perspiration. He gave the preference to Ipecacuanha, which seemed to him to have more effect upon the pains than Tartar Emetic. He was particular in employing cathartics with the country people, with whom he always suspected an accumulation of accessory stimulus in the intestines: "I affirm, says he, " that no purgative operates so powerfully, " and at the same time so gently, as Calomel. It even appeared to me to have an " essential influence on the disease itself; " most purgatives increase the pains: Calomel frequently diminished them remarkably." After the primæ viæ were once emptied, if the fever was inconsiderable, he gave Tinct. Thebaic. cum Vin. Antimon. Huxh. or the Extract. Opii cum Ipecac.—

Opium,



Opium, he remarks, when given but seldom, produces only a short and transitory relief: it must be given *constantly*, and *for a continuance*, to cure the disease radically.— This language, and this practice, would appear quite adverse to the experience of this remedy in the hands of other practitioners; but we may, I believe, reconcile them, if we attend to the effects which it produced according to the mode of administration. With the generality of practitioners, who speak unfavourably of Opium, it had been given early, and before proper evacuations were employed, and then, after a treacherous calm, induced a more dangerous relapse: now Richter generally premised evacuations, and then gave his Opium alone, or combined with other articles, and persevered in their use, till *Diaphoresis* was excited: and on this I conceive the difference of result to have depended. That this was the usual operation of Opium in Richter's hands will appear from the following passage: “ Opium  
“ was *constantly* of the greatest use when  
“ perspiration was obstructed:” and again,  
“ a soft pulse and a moist skin were the  
“ chief signs of its good effect, and of a  
“ certain



“ certain amendment.”\*—In defending Richter on the present occasion, I do not wish to hold up his practice for imitation, but would adduce it as a further proof of the efficacy of Sudorifics in the disease, of which, however, there are several more safe in their exhibition than Opium; the latter, Moseley says, may be given in small doses with safety, and without any of its usual inconveniencies, while the patient is sweating.

“ The real use of Opium, says the last  
 “ author, is to arrest the hurry of the dis-  
 “ ease, to procure time to put some rational

\* That other practitioners have also experienced this same effect from Opium, will appear by the following passage from an author, whom I had no opportunity of consulting till I had written the greater part of this chapter: though he condemns the premature use of astringents, opiates, and sudorifics, yet he says, p. 362, “ Opiata,  
 “ non tantum astringenti, et soporiferâ quâ pollent vir-  
 “ tute, sed etiam plenior per cutis poros diaphoresin  
 “ invitando, et humores ad corporis peripheriam ab in-  
 “ testinis invitando, ad Diarrhœæ & Dysenterix cura-  
 “ tionem conferunt.” *vid.* O’Connell de Morb. Acut. & Chronic. quorundam Obs. Medic.—Indeed abundant proofs are not wanting of the diaphoretic quality of Opium: we know how powerfully sudorific it is in combination with Ipecacuanha.

“ mode



“ mode of cure into execution, to take off  
“ the irritating quality of other medicines,  
“ to give them their intended effect, and to  
“ ease those tormina, which are sometimes  
“ intolerable.” Hunter points out another  
material advantage attending its use: “ If  
“ the griping and other symptoms, he says,  
“ are relieved by the physic, an opiate never  
“ fails to do good, by prolonging the truce:  
“ and it is of importance in a disease that  
“ so severely harrasses and debilitates the  
“ sick, to procure even a temporary relief to  
“ their sufferings, whereby they are better  
“ enabled to bear the operation of medicines  
“ afterwards necessary, and to support them-  
“ selves against disease.”

Blane observes that opiates are least ad-  
missible in the beginning, where evacuation  
is the principal object; but as the disease  
advances they become more and more allow-  
able and useful. The cautions necessary in  
their administration, he states to be: 1st.  
To premise sufficient evacuation, blood-let-  
ting when required, but more especially  
purging. 2d. To obviate the effects which  
an anodyne has of causing a retention of the  
contents of the intestines; and 3d. To pre-  
vent feverish heat, and delirium. This he

Q

effected



effected in the first stage of the complaint, by combining it with Ipecacuanha, and a little Neutral Salt; and in the chronic stage by joining it with a few grains of James's powder, or Vitrum Antimonii Ceratum, as in the latter form it is not so strongly sudorific: that being an effect we do not so much require in the chronic as in the acute state. He says, that there is a very observable difference, in some cases, between Opium given in a liquid, and in a solid form: the former being much more certain in its effects, when the intention is to procure speedy, and effectual ease; a clean tongue he adds, as denoting the absence of fever, is one of the symptoms that chiefly justifies the use of opiates.

Wine, says Rollo, is only admissible in cases of extreme debility, and even in these, Opium often proves the best and safest cordial. “ Yet, observes Dewar, this powerful  
“ stimulant, if too soon and too frequently  
“ repeated, loses its effect, and only exhausts  
“ the strength. The excitement, which it  
“ produces is one of those occasional expedi-  
“ ents by which we procure for our patient  
“ a temporary suspension of his sufferings,  
“ and thus enable him to accumulate a little  
vigour:



“ vigour: but its frequent repetition does  
“ not produce such a steady stimulant effect,  
“ as can entitle it to be prescribed for a  
“ tonic course of any continuance.”

We see in general how cautious practitioners have been of administering this remedy in the more early stages of Dysentery, and we may observe in some degree a partial analogy between this disease and Gonorrhœa, not only in some points of their treatment, but also in the pernicious effects occasionally experienced in both from the premature use of opiates and astringents. In each disease there is an increased discharge, which a false indication, arising from an erroneous conception of its nature, would deem it right to put a stop to, and accordingly has recourse to opiates and astringents for that purpose, the exhibition of which at an early period with such a design, is too often attended with consequences equally detrimental and irreparable; whereas the object in both should be, at first to encourage by appropriate means a discharge, which will in time bring about a natural solution of the disease. This discharge, however, being apt to continue after a cessation of the original disease, being then no longer salutary,



may be prudently suppressed, and its further duration checked with safety and advantage. That this is the case with respect to Opium in Dysentery, the use which Sydenham made of it after full evacuation, has sufficiently testified, and Sir George Baker's declaration is not less to the point; "Quam-  
" primum, inquit, alvi excretiones natu-  
" rali se more quodammodo haberent, jam  
" tandem Laudanum illud Sydenhami, non  
" modò *tutum* fuisse confiteor, sed etiam  
" aliquandò ad consummandam curationem  
" magnopere *necessarium* contendo."

I shall now conclude the subject of Opium with a single remark, which, I hope, will not appear devoid of foundation, after what has been said on the general treatment of the disease: namely, that there will be but little occasion for this remedy in the acute state of Dysentery, provided the warm bath, and swathing can be employed: for these conjointly possess every possible advantage that can be derived from the use of Opium, without being attended by any one of its inconveniences, or by a single consequence, which could contraindicate their employment in this disease; but that, if these cannot be had at this period, it will be almost impossi-  
ble



ble to avoid calling in Opium to our assistance in the earliest stages; in this case we must be guided, as much as may be, by those practical cautions already delivered.

*Blisters.*—These in general seem to have been employed in Dysentery for a symptom, not necessarily belonging to the disease, though perhaps by mismanagement, or otherwise easily excited. “ If there was a fixed  
“ pain in the belly, says Richter, or if the  
“ pains were constant, so that the patient  
“ was not free from them, even when he  
“ was not at stool, I applied a blistering  
“ plaster to the abdomen with the best  
“ effects.”—“ Sometimes, says Dewar, there  
“ is a fixed pain in one part of the abdomen,  
“ which refuses to yield to our common in-  
“ ternal remedies. This indicates a tend-  
“ tency to a more active inflammatory action  
“ in a particular part of the intestinal canal.  
“ It is to be obviated by frequent fomenta-  
“ tions, and to these, if taken in time, it  
“ generally yields. When it does not, it  
“ will often be relieved by the application  
“ of leeches, and still *more frequently* by a  
“ large blister. If it resists these remedies,  
“ we must determine on general blood-let-  
“ ting, provided the patient’s strength will



“ at all admit of it.” It is seldom however, there can be occasion for the latter expedient, blisters are in general so efficacious for the purpose. When the fixed pains of the abdomen did not yield to fomentations or demulcents, Pringle applied blisters with the greatest success, to the part affected; and in some cases, in which the pain was quite intolerable and accompanied with fever, Monro found himself obliged to breathe a vein, and sometimes to lay a blister on the part.

“ Blisters, says Zimmerman, p. 211, not only act as palliatives in the Dysentery, but likewise contribute towards the cure; being of the utmost use in extraordinary cases of this disorder, as well as in the immoderate Diarrhœas attendant on putrid fevers, and indeed in general in all obstinate alvine fluxes.” He mentions that in his Epidemic there were some slight Dysenteries among children, extraordinary obstinate, and sometimes very tedious, and adds, that though vesicatories did not always succeed, they yet merited the preference above all other remedies in these obstinate cases. Tissot, he says, ordered them to be laid on eleven children: on one they had no effect: with another they had a visible,



" a visible, but transitory success: with all the rest, they did more towards the cure, than all the other remedies employed for that purpose. Tissot commonly ordered them to be applied to the calves of the legs, and when the belly was distended, to the nape of the neck; but " I, for my part, says Zimmer-  
" man, in this case, laid them upon all three  
" places at once."\*

These observations apply to the use of blisters in the Dysentery in general, whenever symptoms of fixed pain or local inflammation should arise; in such cases their great utility is not questioned by a single practitioner: and, I should imagine that under circumstances such as these, their beneficial effects must be proportioned to the size of the blister, and its proximity to the part affected; indeed, it may be observed here, that vesicatories to the abdomen seem better qualified, and more strongly indicated than venæsection, to relieve or finally reduce that species of inflammation which belongs

\* Dewar relates, p. 149, that one of the French physicians in Egypt says, " a large blister applied to the abdomen, gave almost instantaneous relief, in several desperate cases of Dysentery, and put the patient's life out of danger in a few days."



to the Dysentery: and the analogy with other diseases would, in one respect at least, very highly countenance their use, for we know, in some species of Colic, with what difficulty stools are often procured, and we have also known with what facility they followed, the moment a blister applied over the abdomen, began to operate. On this ground alone, I would not hesitate in employing them, in cases where the warm bath and swathing could not be applied, or where the latter should fail in rendering the operation of purgatives both easy and effectual: though that, I am inclined to imagine, must be a rare occurrence.

There is yet another case, and another manner, in which Blisters seem to exert a very beneficial influence in the Dysentery: I mean in the malignant form of the disease, or in its combination with Typhus; and here perhaps they operate more on the system at large than on the local disease: indeed in this respect they must stand as much indicated in Malignant Dysentery as in the Malignant Contagious Fever. “The  
“honour of having first revived the use of  
“vesicatories in Malignant Dysenteries,  
“belongs, says Zimmerman, to two physi-  
“cians,



“ cians, illustrious for their inventions in the  
“ practice of physic, Dr. Hirzel and Dr.  
“ Tissot. Dr. Hirzel began with a woman,  
“ that in the Malignant Dysentery had con-  
“ vulsions and fainting fits every quarter of  
“ an hour, and during the intervals lay in a  
“ perpetual delirium; he delivered her from  
“ this dreadful disorder chiefly by means of  
“ Blisters; and Dr. Tissot saw in many  
“ cases, his patients’ stools and anxiety di-  
“ minish, and their strength increase, as  
“ fast as these plasters operated; he there-  
“ fore never neglects this auxiliary in Ma-  
“ lignant Dysenteries, except a great deal  
“ of pure and dissolved blood comes away  
“ with the stools.” *vid.* p. 249 and 50.

I have now concluded the observations on those remedies, which are most usually employed in Dysentery. The subject has carried me farther than I had expected, or originally intended, but as it is an object of the first importance, I trust I shall stand excused for having presented, though at some length, to the reader, a general view of the practice adopted in this disease by the most respectable practitioners; if the variations in their modes of treatment, have in any degree conduced to give further stability to  
those



those propositions, with which I had set out, my purpose is answered; but a more useful end may be obtained for the reader, by its enabling him to judge with precision and accuracy, of the proper application of each remedy to the different forms and stages of the disease. I shall therefore hasten to conclude this long chapter, with a concise view of that plan of treatment, which from the survey just taken, I would conceive best suited to the disease in its more simple and combined states.

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### SECTION III.

#### TREATMENT PECULIAR TO DYSENTERY, IN ITS SIMPLE AND COMBINED STATE.

*And first,* of the Simple Dysentery. The treatment of this form will seldom present much difficulty to the practitioner, if the disease be taken early into hand, if his remedies be well-chosen, and seasonably applied. Of itself it will scarcely require Venæsection, though in subjects of strong inflammatory diathesis, it may be employed with safety, and perhaps with advantage.

Emetics



Emetics are seldom demanded on account of any great nausea or sickness at stomach, or for the purpose of unloading that viscus. They may however, though not in the first instance, be administered in conjunction with other remedies, either with a view of clearing the intestines, or of promoting a Diaphoresis.\*

Purgatives in every form of the disease are essential to a cure, though in some more than in others, as fecal accumulation is very apt to occur repeatedly in particular forms. The object to be had in view in employing or selecting them, should be, to clear the intestines of their contents effectually and speedily, and at the same time with as little irritation as possible: this latter point is effected by the previous use of the warm bath, fomentations, swathing, or Opium, while they promote, with the exception of the last, a quick and compleat evacuation. After a first

\* The strongest testimonies seem more in favour of Ipecacuanha than Tartar Emetic: as being milder, more manageable, and ultimately more efficacious: it has more powerful effects, also in combination with Opium. Where the object is merely to unload the stomach or biliary system, these two emetics may be employed together with most advantage.

evacuation,



evacuation, purgatives may still be necessary, and should be persevered in according to the indications for their use.\*

After the bowels have been once effectually cleared, then is the time to raise, and keep up a gentle diaphoresis, which alone can prevent a return of the tormina and tenesmus, and in conjunction with other means, finally put a period to the disease. There is no necessity for carrying this plan any great lengths, but it is absolutely requisite to keep the skin moist for some time, and carefully to avoid all impressions of cold.†

As

\* The Purgatives to which experience has given the preference, appear to be the Sal Cathar. amar. Oleum, Ricini, and Calomel. Of the Oleum Ricini, I have hitherto said little, but by many it is praised very highly for its mildly-purgative operation, and its anodyne effects, in several instances easing the pains, as soon as taken. Rhubarb, though inadvisable at the commencement, yet seems to be of great service towards the end of the disease.

† The Sudorifics which have been most advantageously employed in Dysentery, are the Ipecacuanha, Pulv. Ipecac. Comp. Tartar Emetic, Pulv. Antimon. Opium, &c. and these have their beneficial operation much increased when used in combination with the warm bath, swathing, fomentations &c. Mercury, though not the quickest in its effects as a sudorific, is yet the most efficacious in keeping  
up



As a means of relieving the tenesmus, and other uneasy sensations, clysters are strongly recommended, as being possessed of considerable influence in allaying them; they, no doubt, do some good, when composed of proper ingredients, and well administered. It must not be concealed however, that this remedy is far from being unobjectionable: at least it appears that the benefits derived from their use, are more than counter-balanced by the disadvantages attending their frequent repetition.—“ Neither did  
“ emollient injections, says Richter, do by  
“ far so much good as I expected: they for  
“ the most part came off again very soon  
“ without any effect: they often increased  
“ and renewed the pains.” This however, he in part attributes to the clumsy manner in which they were administered.—Blane

up that state of the skin, which is so requisite to prevent relapse. There is also another advantage attending its use, which does not so much belong to any other of this class, and this is, that patient and attendants are more careful in guarding against cold while under its influence than they would be under the operation of other medicines. Mercury, though it may be but seldom required in the Simple Dysentery, yet in more obstinate cases should be administered in the manner already detailed.

says



says of them, p. 466, “ I was at first tempted  
 “ to think that a *very frequent* injection of  
 “ such clysters would be very useful, by  
 “ cleansing and soothing the colon, and  
 “ rectum, so as to prevent further *exulcera-*  
 “ *tion*, and dispose the parts to *heal*. But  
 “ besides the objection arising from the ten-  
 “ derness of the intestine, I found that if  
 “ they were given oftener than *once* a day,  
 “ they rather increased the uneasiness, and  
 “ made the patient feel languid and ex-  
 “ hausted.” He adds, however, that when  
 not abused they are of the most eminent ser-  
 vice in this and other complaints. The testi-  
 monies of other authors are not less decisive:  
 “ Clysteres, says Baglivi, *copiosè* præscripti,  
 “ quandoque exasperant morbum et exul-  
 “ ceratis intestinorum fibris majorem orgas-  
 “ mum excitant. Dentur igitur rarius & in  
 “ parvâ copiâ.” And O’Connell, to whose  
 opinion, I have once before referred, declares,  
 “ Enematum quorumcunque repetitionem  
 “ in hoc morbo frequentiore, noxiam  
 “ semper & infaustam observavi.” This  
 same author makes a distinction in the mode  
 of administering an emollient and restrin-  
 gent injection, which may be worth attend-  
 ing to: “ Enemata restringentia, he says,  
 “ parvâ



“ parvâ tantùm singulis vicibus quantitate,  
 “ injicienda volo, ut facilius retineantur;  
 “ quia experientiâ sæpius repetitâ dedici,  
 “ enemata in hoc morbo ultra trium vel  
 “ quatuor unciarum quantitatem injecta,  
 “ nec facilè nec diù retineri posse.” “ En-  
 “ emata antem emollientia, & humorum  
 “ acrimoniam obtudentia, largiori paulò  
 “ quantitate injici possint & debeant; quia  
 “ ob diversas prorsus intentiones, hæc &  
 “ ista subministrantur &c.”

These observations relative to clysters are applicable to all the different forms of Dysentery, and should therefore be always had in mind. With respect to drinks, it would appear that warm water, or whey, is as beneficial in the early stages of the complaint, as any more emollient or mucilaginous liquors. Towards the decline of the disease, however, when the intestines more particularly require something to sheath their tender surface, preparations of a more oily nature are admissible, and are attended with much benefit. Sir G. Baker says, he found nothing of so much advantage in the decline of the disease, as a preparation of cow's milk, boiled with fresh suet, and to which some share of starch is added; he observes also, that melted butter

is



is a remedy long in use among the Irish against Dysentery, and therefore, probably not without its advantages.

Evacuants, it must be obvious, are much more required in this form of the disease, than Corroborants of any kind: indeed any of the latter, but Bark and Wine in particular are only admissible during the state of convalescence, and then weak preparations of them. With respect to articles of diet, regimen, &c. these points are so fully considered by most authors, that it would be a needless waste of time, to think of detailing them here. I shall only observe that attention to these minute particulars is of more consequence to the recovery, and continued health of the patient, than might at first view be conceived: nothing is so easily produced as a relapse, nothing with more difficulty recovered from:

—————“ *facilis descensus Averni*  
“ *sed revocare gradum, hic labor, hoc opus.*”

In fine, the means I would generally adopt in the Simple Dysentery are, the warm bath, swathing, a brisk purgative, after it an opiate, and then a course of gentle sudorifics, with the occasional employment of warm bath  
and



and opiates. These would constitute the chief remedies, though various others might be required, if indicated by circumstances already detailed.

*Treatment of Intermittent and Remittent Dysentery.*—In the management of these forms we should constantly hold in remembrance, those observations which Sydenham has left us, relative to the nature and genius of certain diseases, that occur sometimes as symptomatic, sometimes as essential diseases of the season. When they appear in the former character, and the means of knowing it have already been mentioned, they can only be treated with success by that method, the fever with which they are combined, requires, adapted to each particular case. The truth of this proposition has been evidenced from various quarters: Sydenham, Morton, Pringle, Cleghorn, Clarke, Zimmerman and various others bear abundant testimony in its favour. Accordingly it must be the business of the practitioner, carefully to attend to, and make himself acquainted with the nature and treatment of the reigning fever of the season, which he must expect to vary much in different times and places: and when he is in possession of this knowledge, he must

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apply



apply it to the particular disease, under such restrictions as its peculiar character may require. We have already seen how much Dysentery is influenced by the fevers of the season: the intermittent and remittent forms are instances of it. We must therefore endeavour to learn from nature, and not from books, what plan should *particularly* be followed in the treatment of these forms; we are not however to despise the information which may be derived from the latter: they will at least shew us, what success others have had in similar cases by pursuing the general plan laid down: we must imitate their example, but not their practice. It cannot therefore be expected of me here, to speak on this subject, but in a very general manner, more especially as I have been already very full on the use of those remedies, which are usually exhibited in such cases.

In the treatment of Intermittent Dysentery, I have already detailed the steps which were adopted by Cleghorn and Rollo: Tissot gives nearly the same advice, only in a more general way: "This disease, he says, is  
" sometimes combined with an Intermitting  
" Fever; in which case the Dysentery must  
" be



“ be removed first, and the intermittent  
“ afterwards. Nevertheless, if at each ac-  
“ cess, the fits of the fever have been very  
“ strong, Bark must be given as directed  
“ for that fever.”—Emetics and purgatives  
may be indicated the more, from the presence  
of the fever, and the propriety of venæsec-  
tion must depend a good deal on the charac-  
ter of the fever, and of the patient, but  
more especially on the season of the year, as  
Vernal Intermittents not only bear, but often  
require bleeding, before the Bark will take  
due effect.

In the same manner with respect to the  
Remittent Dysentery, the propriety of this  
operation must depend much on the same  
circumstances, though from the greater  
tendency of this form to generate symptoms  
of putridity, and to acquire a contagious  
character, it must be less admissible than in  
the former, and the testimonies of various  
practitioners have shewn that it has been  
seldom necessary.

Emetics are highly necessary, and very  
advantageous in clearing the stomach, and  
primæ viæ of their morbid contents, and a  
repetition of them may be required for the  
same purpose: along with them a temperate



use of opiates may be attended with beneficial effects in allaying the great irritability of the biliary system, and the irritation which its disordered secretions give to the stomach and intestines, and which the exhibition of an emetic often tends to increase.

Purgatives are still more loudly demanded, as being indispensably requisite to evacuate the bowels of an inconceivable load of bilious and other impurities, which are constantly accumulating, and which should never be permitted to remain, while means can be had of expelling them. Calomel purges seem the most powerful and efficacious for this purpose, and should, while the necessity continues, be unremittingly administered with occasional doses of more gentle laxatives. If employed only during the time such necessity lasts, the patient, instead of being weakened, will daily gain strength.

With respect to the general and full employment of the sudorific plan in the Remittent Dysentery, it appears to me rather objectionable, at least not near so much warranted as in the simple disease. How far the warm bath may be admissible in this form, I know of no facts which speak with any decision; swathing I should deem innocent



cent as to the fever, and very beneficial as to the Dysentery. This much I would say, that every means, consistent with the nature of the fever, of keeping the skin moist and perspirable, may be employed with safety and advantage. But there is yet one article of this class, that seems to possess powers, peculiarly adapted to the circumstances of this combination: for whether viewed in the light of a purgative, sudorific, or deobstruent, no remedy can rival Mercury in this form of the disease; it acts against it in a treble capacity: as a purgative it effectually clears the bowels of their morbid contents: as a sudorific it materially influences their action, and tends to make it regular: while as a deobstruent, it possesses a powerful operation over the biliary system, which in this form is more or less deranged. It is a remedy I would never fail of exhibiting for some or all of these purposes, unless by a timely and expeditious use of the Bark, I was enabled at once to put an end to both diseases. That this will often happen when it is administered under the circumstances pointed out by Morton, we have his own testimony, and that of many others to convince us: to his former declaration on this



subject, I shall here subjoin the following passage from the same author: “ Et nullus  
 “ dubito, inquit, quin Diarrhœa quævis  
 “ Epidemica & quæcumque alia cum febre  
 “ juncta, imo febres castrenses cum hujus-  
 “ modi symptomatibus ut plurimum sociatæ,  
 “ hâc methodo compendiariâ multo certius  
 “ sanari possint, quam magno illo apparatu  
 “ Rhabarbari, &c; Atque equidem ut a  
 “ verâ & genuinâ ideâ hujus morbi formatâ,  
 “ curativæ indicationes in posterum certio-  
 “ res desumantur, mihi in voto est, ut  
 “ communi medicorum consensu, Diarrhœa  
 “ & Dysenteria castrensis, seu quæcumque  
 “ alia Epidemia, nomine proprio *Συμμεχέος* spu-  
 “ riæ et colliquativæ, posthâc designetur.”

It is indeed a very curious circumstance in the history of these compound diseases, that means peculiarly adapted for the removal of one, seldom fail of producing a like effect on the other. Thus with respect to Dysentery, the Bark produces no one salutary effect on it, when alone, but when combined with the intermittent or remittent fever, it generally succeeds in curing the former, by the powerful influence it possesses over the latter. Sometimes the reverse may take place, that is, by directing our  
 means



means against the Dysentery, we may be successful in removing the fever. Thus Zimmerman, who followed that plan, says, p. 55, “as fast as each symptom of the Dysentery decreased, and at length vanished, I perceived that the fever in like manner decreased and vanished.” Should the Bark however, only disperse the fever, we have got rid of one difficulty, and may attack the Dysentery by those means already directed, and with better prospect of success.

In the Remittent Dysentery, Wine and Cordials are very pernicious, and should only be allowed in very moderate quantities during the state of convalescence; if the disease have been properly treated, even then there will be in general but little occasion for strengthening medicines, as the patients will recover of themselves: when they were necessary, Zimmerman took care to select such, as while they strengthened, did so without heating, and promoted some degree of evacuation. Copious drinking of warm water, or of warm whey, the use of acid beverages, and in particular, of ripe fruits, seem to be highly serviceable. “The regimen, says Clarke, ought to be much the same as in remittent fever: and when the



“ disease is accompanied with putrid symptoms, nothing will be found to answer better than ripe fruits.” When he could not procure these for the men, he gave them vinegar in their drinks with advantage, and never found it to increase the griping pains. Tissot bears the strongest testimony in favour of fruits in the Dysentery, and Sir. G. Baker, makes the following very striking remark: “ Qui fructus aut æstivos aut autumnales *immodicè* assumpserant, vel nullâ tentabantur Dysenteriâ, vel si tentarentur, *levissimè* ægrotabant.” Zimmerman says, that acids were of great use, and that he allowed those who were getting well as much boiled fruit as they chose, with lemons and lemon juice.—Nothing further need be said to justify their use, or to shew how ill founded that prejudice is, which pronounces them noxious in the Dysentery.

With respect to air and cleanliness, it is extremely necessary in this form of the disease to attend to both, from the facility with which it appears to pass into the malignant or contagious variety; for it may be remembered that a great majority of those authors, who describe the remittent Dysentery, make mention of its having become  
contagious



contagious from inattention to these two particulars. Zimmerman says, he was always extremely careful to have the air fresh in the chambers of the sick, and that he looked upon cleanliness as a thing of the utmost importance in the cure of the Dysentery. Experience has daily shewn the necessity of attending to these rules; but in doing so, we must cautiously guard against too free an exposure of the patient to cold air, which in this complaint must do more harm, than fresh air can do good.

*Treatment of the Simple Dysentery, in combination with Typhus, or Malignant Contagious Fever.*—In the management of the Intermittent and Remittent Dysentery, we have seen the great necessity of directing a great share of our attention to the fever which attends; this necessity exists in a still greater degree, in respect to that form of the disease I am now about to speak of. I may here introduce this subject with a very appropriate observation of the intelligent Tissot: “The cause of malignant  
“ fevers is, he says, not unfrequently com-  
“ bined with other diseases, whose danger it  
“ extremely increases. This may be known  
“ by the union of those symptoms, which  
“ carry



“ carry the marks of malignity, with the  
“ symptoms of the other diseases. Such  
“ combined cases are extremely danger-  
“ ous; they demand the utmost attention  
“ of the physician; nor is it possible to  
“ prescribe their exact treatment here, since  
“ it consists in general of a mixture of the  
“ treatment of each disease, though the  
“ *malignity* commonly demands the greatest  
“ attention.” *vid. Art. of Malignant Fevers.*

The justice of these observations will be manifest on a survey of that plan of treatment which has been found requisite in this combination, and more particularly when we remember that Pringle almost identifies its treatment, with that of malignant fever.

Zimmerman seems to me to be the only author who has spoken at any length of the method of cure appropriated to this combination, and as he appears to have treated of it with much accuracy and discrimination, I shall not hesitate in repeating the most valuable of his observation. In the first instance he remarks, that *a pure air* is above all things requisite, as in general the greatest danger proceeds from an impure air, which can never be made amends for either by diet or medicine. Cleanliness likewise  
in



in all respects is of extreme importance; for if all this be not attended to, the malignity will spread, a great number will die, and even the most powerful remedies will be without effect. He afterwards observes with great truth, that “even in those Epidemics  
“of the Dysentery, which are indisputably  
“benignant, as well as in Epidemics of the  
“malignant fever, there are always here  
“and there malignant Dysenteries, in which  
“the observance of these rules is of the  
“greatest importance.” *Vid.* p. 233, et seq. In reviewing the history of this disease, we cannot but acknowledge how well supported he is by facts in this last observation: if well attended to, these rules would seldom permit the Dysentery to rage malignant and contagious.

Respecting the employment of evacuations, I have already at full length detailed, how far each article of that class was admissible in the malignant Dysentery: little therefore need to be said here regarding their use; and though the greatest judgment and skill be requisite in balancing any contraindications, which may be expected not unfrequently to arise in so dangerous a complication of disease, yet should we always  
hold



hold Tissot's advice in remembrance, *rather to incline* to the symptoms of malignity, and let them occupy the greater share of our attention, without overlooking the reasonable claims which the Dysentery must make on it. In strong confirmation of the propriety of this advice, we find venæsection almost unanimously rejected, too often from the sad experience of its pernicious effects; we find too that emetics and purgatives, though they must and may be used, with considerable advantage, yet demand more caution in the mode and extent of their administration in this, than any other form of the disease; and as to sudorifics they would appear not very admissible; we have now for some period of time been acquainted with the mischievous consequences of forcing sweats in fever, and as it is the fever, which here constitutes the great source of danger, so the sudorific plan, however, advisable against the Dysentery, must be given up as strongly contra-indicated by the fever. Degner thus cautions against its employment in this species: "Sudorifera cautè  
" *adhibenda sunt: sudor enim arte prolectus*  
" *minime eundem effectum salutarem habet,*  
" *ac spontaneus, quo natura materiam hu-*  
" *moribus*



“ moribus insinuatam aptè e corpore elimi-  
“ nare novit.” If the disease should appear  
to incline to this evacuation, we need not  
hesitate in promoting it, without carrying  
it too far, which might tend to debilitate the  
patient, and thus do more harm, than it  
could otherwise be of service. All means  
are not alike for this purpose: the gentlest  
are to be preferred: Ipecacuanha is perhaps  
the remedy which may be employed with  
most safety and advantage: “ It is without  
“ doubt, says Zimmerman, the principal  
“ remedy in this species of Dysentery;”  
and he directs that it should be given by way  
of emetic directly at the beginning, and  
then, after having purged the patient by  
gentle means, to recur again to its use in  
very small doses, to be taken every two  
hours in chicken or veal broth.

How far the warm bath, swathing and  
mercury may be admissible in the malignant  
Dysentery, I cannot take upon myself to  
say, nor do I know of any facts, which  
would authorise me to pass an opinion on the  
subject; if however, I might be allowed to  
conjecture on a question that can only  
be decided by experience, I should conceive  
that the last article must be improper, and  
that



that two former may possibly be of service, without aggravating any of the symptoms of fever: the temperature of the bath should however, be carefully attended to, and ought probably be much lower than  $96^{\circ}$  of Far<sup>t</sup>.— But whether the cold or warm effusion, as employed by Dr. Currie against Typhus fever with so much success, presents any prospect of doing good in the combination of that fever with Dysentery, is a point, upon which, without some experience, I can, still less, venture to hazard a conjecture. It would appear from the trials which Dr. Currie made of these remedies in an Epidemic Dysentery at Liverpool, in 1801, that much is not to be expected from them. This disease prevailed in company with Scarlatina Anginosa and Typhus, which latter, he says, burst its accustomed boundaries, and extended into the habitations of the opulent; the Dysentery would seem from the description he has given of it, often to have partaken of the symptoms of the latter disease, was unusually fatal, and its treatment, very difficult. The heat, he adds, in this disease, after the appearance of the dysenteric symptoms rose to  $102^{\circ}$  and  $103^{\circ}$ , the tongue became furred, the skin dry, and the pulse from 100 to 120,



in the minute. " I did not try, says Dr. Currie, the cold effusion or the application of cold in any form, having learnt by experience, that it does not succeed in fever with affections of the bowels. I tried, however, the tepid effusion in a few cases, and though with abatement of heat, with no lasting benefit. The patients complained of the fatigue and pain of moving, and of the chilling effects of the remedy, which was therefore abandoned." *v. p.* 397 et seq.—Dr. Currie seems to be the only person who has experimented on this subject, and his experience and opinions are deserving of every credit, that can be due to candour, impartiality and judgment.

But the influence which the presence of Typhus fever exerts over Dysentery, is in no respect so strongly manifested, as in the necessity, which it too frequently produces for the exhibition of various cordials, tonics and corroborants: and in this necessity it differs from all other forms of the disease. Under the section of Corroborants, I have brought together the evidence which testifies this necessity, and the advantages derived from complying with it: to that head therefore



fore I shall refer for the mode of administering them, and for the occasions which demand their use: indeed it may generally be observed, that they are the same, as may occur in the malignant contagious fever, and that whatever indicates their use in the latter, calls still more loudly for their exhibition in the Malignant Dysentery, in which, after we have obviated by various means the most urgent symptoms of the flux, the debility that ensues in consequence of such evacuations, must be proportionably greater.

Something more might be added on this head, but as I have reason to fear I may already be thought too tedious, I shall conclude the observations I had to offer relative to the Dysentery, by again recurring to a proposition, which *in my mind* has been supported by proof, approaching to demonstration. But perhaps in this I may have deceived myself, and often seen arguments in its favour, where none such existed, or such, as none but prejudiced eyes could have perceived; should this have unfortunately been the case, I hope they are still open to conviction, and as truth has been the object at which it was my wish to arrive, I trust I shall not be found backward in assenting to it,



it, under whatever shape it may appear. It has been advanced that *Dysentery was not contagious*, but that *it might be communicated by contagion, in consequence of its combination with a disease possessed of that property*. The whole of this proposition has, in my estimation, been established by reasoning *a posteriori*; that the first part of the proposition was true, and the remainder therefore highly probable, might, I also think, be inferred *a priori*: if so, we need not then be surprised that the former train of reasoning should have led to a similar conclusion. For it may be argued thus: If *Dysentery* be in itself contagious, it must range under some one of these species of contagion, as this, we know, to be communicable in three ways only, either by invisible particles, in a visible form, or in both ways: an instance of the first we have in Typhus, of the second in Itch, and of the third in Smallpox. We find the diseases, which arrange themselves under these different species of contagion, possessed of strongly distinctive, and characteristic marks, bearing a striking analogy to each other, and differing very essentially from those of any other division. *Dysentery* then, if contagious, must possess



some characters in common with that class of contagious diseases to which it may belong. Let us examine, whether on this ground it be entitled to a place among any of them; for this purpose it may be best to consider its claims to contagion in the retrograde order; and first, of its title to the third species: Under this head, few I believe, will think of placing it, as the diseases, which rank under that class, are the genuine Exanthemata, possessed of very peculiar characters, to none of which Dysentery can lay claim, for it is unattended by eruption, by fever of definite duration, and may be taken on exposure to its cause, any number of times.—It cannot range itself under the second species of contagion, along with Itch, Syphilis, Gonorrhœa, for these diseases are communicable by direct contact only, and their contagion does not induce fever; now Dysentery is often attended by fever, and if it is possessed of contagion, it is capable of acting without the necessity of direct contact,\* neither can it be exhibited

\* Whether Dysentery be communicable by direct contact of intestinal excretions to the anus, I cannot take upon me to decide: we know that in general its contagion is propagated and received in all respects like that of fever



ted in a visible form. Nor can Dysentery be of *itself* ranked under the first species of contagion, for the diseases of this class are always attended by fever, and are of definite duration: Now Dysentery is at times unaccompanied by any fever, and as is known to every one, it is a disease of no definite duration, continuing often for weeks, nay, months or years.—Thus at least would theory alone induce us not to doubt, what facts and the best authorities have established, namely, that Dysentery can no longer hold a place among diseases *originally* contagious, but that it must form one of a distinct order of communicable diseases, which may become *secondarily* contagious,

fever, but whether we can rely on the following observation of Hælidæus Padoanus, I am uncertain and will leave for others to pass judgment on: “Eo malo, inquit, sæpius  
 “videas corripì, quibus clyster infunditur, instrumento  
 “non benè abluto, quo antea Dysentericus usus fuit; in  
 “sedili etiam seu loco excretionis contagii vestigia ali-  
 “quando remanent.” This observation is quoted by Moseley, p. 354, and though he makes no remark on it, we may easily conjecture his sentiments. I confess I was inclined to doubt the fact, but am less so at present, since it would appear that the contagion of Hospital Gangrene may also be communicated by direct contact. But of this, more hereafter.



or are propagated by means of a disease, possessed of contagion, while they themselves are devoid of that property. Whether this compound contagion operates in the production of other diseases, besides Dysentery, shall be the object of our enquiries in the subsequent chapter.



## CHAPTER VI.

## DISEASES,

## ANALOGOUS TO DYSENTERY

## IN THE

## SOURCE OF THEIR CONTAGION.\*

Notwithstanding the great support, which an investigation of this kind, must receive from the firm grounds on which we have established certain propositions relative to Dysentery, yet do I enter *now* upon it with

\* It had been originally the author's intention, (as may be seen by the Prospectus of this work, long since in circulation,) to have introduced here, previous to this investigation, a general review of the whole class of contagious or infectious diseases, to have pointed out the circumstances in which they seemed to agree or differ, and afterwards to have attempted some classification of them; for this part of the work he had prepared a variety of materials, but as his observations on Dysentery have far exceeded the bounds he had at first prescribed to himself, he thinks it more prudent to suppress them on this occasion, till he shall have better ascertained the feelings of the public towards the present publication.



some degree of reluctance, because I am conscious it is much more imperfect than could be wished, or than it might be, had more time and attention been bestowed on it. With respect to the diseases to be spoken of under this head, on some of them, at present, I can offer little more than plausible conjectures: respecting others I can offer something like proofs: of all, perhaps, I may at a future day be enabled to speak with more confidence. Let not the reader judge the whole foundation weak, because all parts of the building are not of equal strength. I have felt it my duty to state to him all that I know of this subject, and if I have not toiled in vain, to lay open to his view an highly interesting prospect, which from its great extent may not be equally visible or pleasing in all its parts, let an indulgent eye traverse the whole, and with grateful partiality endeavour to rest on the fairer and brighter objects.

In the case of Dysentery we have seen evidence of a very singular truth, that a disease, which in the generality of instances is not contagious, may yet become so, from a coincidence of certain circumstances; singular it must appear at present, but, as we advance in this investigation, perhaps it may  
be



be thought less so, for we may observe with Pringle, that “in the first steps of our enquiry, we meet indeed with great variety and obscurity, but as we penetrate further into nature, we find so much analogy amongst her works, that we are forced to acknowledge, and to revere her simplicity.” “Facilè enim persuademus, omnia ad certas & perpetuas fieri leges, nihilque in totâ rerum naturâ casu quodam fortuito, et extra causas suas contingere.”\* It is to be hoped that the truth of those observations will be confirmed in the present instance: and as so much of the obscurity which enveloped this subject, has been dissipated in regard to the Dysentery, it may be expected that the simplicity of nature’s workings will be seen in the analogy, which it bears to other diseases. After I had satisfied my mind relative to Dysentery, two queries naturally followed; Are there any other diseases, besides it, which at times are contagious, at times not? And if there be any such, do they derive their contagious character from the same source that Dysentery does? The following diseases may, I be-

\* Roederer de Morbo Mucoso.



lieve, answer in the affirmative to the first question: namely, Catarrh, Cynanche, Ophthalmia, Erysipelas, Ulcer, and Peritonitis: these, when contagious, are generally denominated, Catarrhus Contagiosus, or Influenza, Cynanche, or Angina Maligna, Ophthalmia (Ægyptiaca) Erysipelas, Vesiculosum, Typhodes, vel Contagiosum, Malignant Ulcer, or Hospital Gangrene, and Puerperal Fever. The other question I will venture to answer generally, by declaring my belief, that every disease, which at one time can be contagious, and at other times is not, acquires that property only in consequence of its combination with Typhus Fever. We shall consider these diseases in the order in which they have been enumerated.\*

\* There is, however, no intention of attempting a detailed history of any of them, but merely to refer as concisely as possible to such circumstances, as more properly belong to this investigation.



## SECTION I.

## CATARRH.

THIS disease is ranked by Cullen in the same order with Dysentery, and perhaps had a greater right to be classed with it, than he was aware of. His definition of it is however more guarded, for as I have already mentioned, he only states it to be, "Pyrexia *sæpe contagiosa,*" and though he unhesitatingly admits in Catarrh, what he so much doubted respecting Dysentery, yet he does not offer the slightest conjecture, how a disease can be propagated by an agent, so different from that, by which it is usually spread. In offering my opinion that its contagion is owing to Typhus, I can by no means pretend to support it with direct or positive proof: a few circumstances in the general history of that disease have induced me to adopt the opinion.—From the various accounts we have on record relative to Catarrh, it would appear to resemble Dysentery very much in several particulars; it often exists sporadically, with or without much fever, and is then never suspected of contagion;



gion: it frequently prevails epidemically, in combination with Remittent Fever, and is then very often erroneously denominated Influenza; for this is another form, in which it spreads epidemically, is propagated by contagion, and when it does occur, is very general. It is not perhaps an easy matter to discriminate the latter cases, and I confess that though I have seen some grounds for adopting this distinction, I am not able at this present moment clearly to distinguish those Epidemics that were contagious, from those that were not, neither can I precisely state those circumstances, which form the criterion. The true contagious Catarrh would often seem to range the whole habitable globe, and rages more universally than any other disease\* we are acquainted with: and in these cases it seems principally to be characterised by symptoms of debility.

“ Far more prostration of strength, says  
 “ Dr. Hamilton, was united with it, than  
 “ we ever find attend Catarrhs from cold  
 “ alone; and I am led to think this forms  
 “ one of its chief distinguishing symp-

\* We have one instance on record where the Dysentery is said to have prevailed over all Europe, in the year 1538. *Vid.* Fernelium de Morbis Pestilentium, cap. 13.



“toms.”\* He afterwards adds, that few diseases, putrid fevers excepted, ever produced loss of strength, and debility more suddenly than this. Dr. Hamilton mentions many facts to prove its contagious nature, and so also does Mr. Chisholm in the Medical Commentaries for 1790; they both represent its mode of seizure and of spreading as similar to that of other contagious diseases: and, they, as well as most others, insist on the general inutility or mischief of venæsection. “The fever, says the latter gentleman, sometimes appeared in the continued form, but most generally in that of a remittent, or intermittent; and when in the latter, the paroxysm always came on in the evening, and continued all night. And although the catarrhal and other symptoms continued all day; yet they always, particularly the cough, seemed to increase in violence, when the exacerbation of the fever took place.” “There were also, he says, some instances of Putrid Fever shewing itself, under the resemblance of this Epidemic. Some cases

† *Vid.* 2d vol. of the Mem. of the Medical Society of London, P. 447.



“ in particular, partook so much of the  
“ putrid diathesis, as to render the situation  
“ of the patients extremely dangerous. In  
“ these, the catarrhal symptoms were very  
“ evidently marked, and occasioned the loss  
“ of two patients, by their preventing me  
“ from attending to those of putrescence.”

In the Peripneumonies and Pleurisies also which soon took place of this Epidemic, he describes symptoms peculiar to Malignant Fevers: more especially that uncommon sensation of heat, which is felt on touching the skin of patients labouring under such fevers. They resembled, he says, that fatal Epidemic described by Dr. Cleghorn. Something similar to this also occurred with the Epidemic cough, of 1675, described by Sydenham; a *symptomatic* Pleurisy accompanied the fever, which prevailed at the time the cough began, and this did not bear repeated bleeding, as the essential Pleurisy does, because the fever of which it was a symptom, did not bear bleeding, therefore it must prove prejudicial in the Pleurisy which goes off with, or lasts as long as the fever. In speaking of this Pleurisy it is not a little singular that he takes occasion to explain what is meant by Malignant Pleurisy,  
and



and by malignity in Epidemic diseases in general. Now the Malignant Pleurisy is such as will not bear bleeding; and this, he conceives to depend on the fever, with which it is accompanied; for it sometimes, he says, happens, that the peculiar Epidemic fever of the season, from some sudden alteration in the manifest qualities of the air, readily throws off the morbid matter, upon the pleura and lungs, whilst the fever, *notwithstanding*,\* continues exactly the same; and therefore that the propriety of V. S. must be judged of by the genius of that fever. Malignity he conceives to consist in some hot, and spirituous particles, which act chiefly by way of assimilation: “ Thus, he says, “ fire generates fire, and a person seized “ with a malignant disease, infects another “ by an emission of spirits, which soon assi- “ milate the juices to themselves, and

\* This we may perceive to be language exactly similar to that which he uses respecting Dysentery; for that he calls a febris introversa though it appears from his own account, that his febris dysenterica, still, *notwithstanding*, coexisted with this febris introversa. It plainly shews, whatever his language or meaning might be, that such diseases only consisted of a combination of the fever of the season, with local affections of the chest, or some of the abdominal viscera.

“ change



“change them into their own nature.” Are we not from this to conclude that Sydenham was well acquainted with the contagious property of malignant diseases, and that he looked upon the Malignant Pleurisy of course to be possessed of that character? Indeed facts do not seem wanting to shew that Pleurisy and Peripneumony have been propagated by contagion. We know them at the present day, often to exist in fatal combination with Typhus Fever,\* and we find

\* Huxham, in his 2d vol. De Aere & Morb. Epidem. furnishes us with some important details respecting such a combination: p. 128, he says, “Gallos inter, & Hispanos *captivos* sævit admodum Febris quædam *catarrhalis maligna*, jugulatque per plurimos: accedunt sæpe petechiæ, haud raro Parotides, sæpe pustulæ urentes aquosæ;” and again, in the report of the next month, he says of this fever, “Jam grassatur maxime & per plurimos *captivorum* mittit ad Inferos; etiam nunc haud raro Nostrates, *qui cum his multum versantur*.” And again he reports, “Sævit usque, immensamque stragem edit; ministrantes aniculas, imo et quosdam Chirurgos nunc invadit, etiam et occidit.” In some months after, he states the return of such another fever, and at the same time describes two very different species of Peripneumony, one prevailing principally among the rustics, and requiring large and repeated bleedings, while the other would not admit the loss of one-fourth of the quantity, “*sine summa virium ruina*.” In p. 144, he discriminates



find many of the more ancient authors speaking in familiar terms of the Putrid and Contagious Pleurisy or Peripneumony. Short mentions from Johan. Cole de Billona, that a hot air, cloudy and moist atmosphere, had continued for some years, and that Malignant contagious Peripneumony followed in all Europe. Webster, from whom I have taken this statement, makes mention of the symptoms which attended it, and which were those of the most virulent Typhus, for it was Epidemic about the same time with the Plague; Venæsection was certain death. “ It will be found, says the latter writer, “ that Malignant Pleurisy and Peripneu- “ mony usually form a part of that series of “ diseases, which occur during a period of “ general contagion. When Plague, and “ Yellow Fever occur in summer, in “ northern climates, Pleurisy and Peripneu-

minates between them, and in p. 150, he states thus, “ Febris putrida, nervosa, maligna usque grassatur, plu- “ resque opprimit: Et Peripneumonix quoque multæ “ *mali admodum moris*; hæ post tres quatuorve dies, in “ malignam nervosam, aut petechialem degenerant, vires- “ que summopere deprimunt.—Vix sanguinis missionem “ adeo admittunt, multò minùs repetitam aut largam.”— These passages need no comment.

“ mony



second is, the little mortality attending the contagious Catarrh, compared with contagious Dysentery: no doubt, the contagious Catarrh is much more dangerous than the common Catarrh from cold, but its fatality is not in that proportion we would expect, were it in combination with Typhus. Perhaps I would have done better, had I omitted Catarrh in this chapter, and substituted Pleurisy and Peripneumony in its place.

held, many labouring under the Influenza, still attempt going about their usual business, and often perform long journeys: and from the great multitudes they are conversant with, are enabled to spread the disease far and wide. We are always able to trace the progress of the disease from one town to another by contagion: we can therefore only explain the greater diffusion to the greater facility of commerce with the sick, such as does not occur in diseases of a more malignant type. Something analogous to this we have in the case of Pertussis, which is more rapidly diffused than other epidemics, from the patients being able to go abroad, and under no necessity of confinement.



## SECTION II.

## ANGINA MALIGNA.

THE real nature of this disease has been the subject of great dispute and uncertainty, in consequence of those controversies which have long agitated the medical world, relative to the contagion, and varied forms of Scarlatina. But as it now appears to be a fact, established on the most indisputable grounds, that the Scarlatina Simplex, Anginosa, and Maligna, all originate from the same contagion, I shall not waste more words on the question, but will endeavour to point out, on the authority and observations of an author of great experience and judgment, the existence of a species of Angina, which owes its contagious character to a source similar with that of Dysentery.—Willan, in p. viii. of his preface to the Reports on the Diseases of London, observes, how singular it is that Fever should unite itself with other complaints, and be propagated by infection under a *double form*.—“ I may mention, he says, as an instance, “ the combination of an ulcerated sore



“ throat with malignant fever. To this  
“ combination alone, which *often* occurs,  
“ and is very contagious, the title of An-  
“ gina Maligna would have been most pro-  
“ perly applied. Medical writers, by not  
“ distinguishing it from the Scarlatina An-  
“ ginosa, have been led into obscurity,  
“ and made a foundation for some needless  
“ controversies.” In p. 131, he mentions a  
case of the disease, which, along with the  
usual symptoms of a *Malignant Fever*, ex-  
hibited deep ulcerations of the tonsils, and  
adjoining parts, covered with ash-coloured  
sloughs, and surrounded by livid edges.—  
Nevertheless, the fever ceased, and the ul-  
cerations were healed in about eight days,  
by the use of a valuable bark, lately im-  
ported from the Colony at Sierra Leone. By  
this description we may plainly see how this  
Angina Maligna differs from the Cymanche  
Maligna of Cullen, and though he makes it  
a different disease from Scarlatina Anginosa,  
yet do they evidently appear similar from  
the scarlet eruption which always attended  
it. I shall finish this article by another quo-  
tation from Willan’s third order on Cuta-  
neous Diseases, where, in treating of Scar-  
latina, he observes, p. 333, “ It may there-  
“ fore



“ fore be concluded that no British author  
 “ has yet described any epidemical and  
 “ contagious sore-throat, except that which  
 “ attends the Scarlet Fever. The title “ An-  
 “ “ gina Maligna” would have applied with  
 “ equal, if not with more propriety, to the  
 “ sore-throat connected with a different spe-  
 “ cies of contagion, viz. that of the Ty-  
 “ phus, or Malignant Fever, originating in  
 “ the habitations of the poor, where no atten-  
 “ tion is paid to cleanliness and ventilation.  
 “ The fever and sore-throat are sometimes  
 “ communicated together, and the disease,  
 “ thus complicated, does not become epide-  
 “ mical like the Scarlatina, nor is it at-  
 “ tended with any eruption except pete-  
 “ chiæ. It is often fatal, but not at so  
 “ early a period as the Scarlatina Maligna :  
 “ it may also be repeatedly received, whereas  
 “ the Scarlatina occurs but once in the  
 “ same person.”

Dr. Willan, in a note, refers to two other  
 authors as having spoken of this combina-  
 tion, but whom I had no opportunity of  
 consulting: these are Petr. a Castro de  
 Febr. Malig. punctulari (§ xxii. p. 232)  
 Norimbergæ, 1652: and Ramazzini, Con-  
 stit. Ann. 1691-4, § xx. From the former



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he quotes the following passage: “ Solet  
 “ præterea pestilens hujus morbi malignitas  
 “ fauces invadere & anginosos effectus effi-  
 “ cere, tonsillarum tumores, cancrosa ul-  
 “ cera, aphonias, gulæ resolutiones, & si-  
 “ milia.”

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### SECTION III.

#### OPHTHALMIA.

RESPECTING the contagion which occasionally operates in the production of this disease I shall say very little, as I would rather refer the reader to the same source whence I have derived my greatest information. In Mr. Power's Treatise on the Egyptian Ophthalmia, he will find the best detail of evidence to prove it contagious, and also the most probable account of those circumstances under which it becomes so. Mr. Power regards the *Putrid Virus* as the great source of disease in Egypt, and is of opinion, that this virus, which, partially applied, produces Ophthalmia, may, under a change of circumstances, give origin to the Plague, Eruptive Fever, or Dysentery. He  
 also



also concludes, from the phenomena this Ophthalmia presented, and from the mode of treatment which alone was found successful, that it was contagious, and existed in combination with Typhoid Fever. It is probable that this *putrid virus* has some similitude, if not the same, with the Contagion of Typhus: this may be thought more likely, when we consider, by and bye, what he says of Hospital Gangrene; in the mean time I must refer to the work itself for a more satisfactory illustration of this subject, and shall conclude by remarking, that it must appear not a little curious that the more ancient medical authors never entertained the least doubt of the contagious nature of this disease, no more than they did of Dysentery. Could it be owing to the same cause that they have both ceased very generally to assume that character?



## SECTION IV.

## ERYSIPELAS.\*

With respect to this disease, Willan, p. viii. of his preface, very properly remarks, that though usually ranked among the Exanthemata, it has little affinity with the other diseases, arranged under the same order. In p. 37, he observes, that this disease was divided by the ancients into Erysipelas Phlegmonodes, and Erysipelas Œdematodes. For the former they properly direct blood-letting, purgatives, and a cooling diet. The latter, with less redness, exhibits more tumefaction. It is attended with a quick but weak pulse,

\* A distinction may be made between the diseases already spoken of and those which follow, as all of the former, Dysentery, Catarrh, Angina, and Ophthalmia, may prevail epidemically, and be widely diffused; whereas those yet to be treated of, Erysipelas, Malignant Ulcer, and Puerperal Fever, seldom extend their influence beyond the walls of hospitals, or other places where the sick may be crowded together. The former require for their production certain states of the atmosphere, which act as remote causes: while the latter require *certain* predispositions on the part of individuals; hence the sphere of action in the latter case is limited, in the former much less so.

with



with delirium or coma, and has a tendency to gangrene, which cannot always be prevented by the use of Peruvian Bark, mineral acids, and a cordial regimen. The œdematic or gangrenous form of the Erysipelas is, he says, the most usual in London. And he further observes, in p. viii. of the preface, “ The form of it, entitled Erysipelas  
“ Phlegmonodes, does not seem communi-  
“ cable by contagion: however, when the  
“ fluid contained in its vesicles is inoculated  
“ into the arm, it excites a diffuse inflam-  
“ mation and swelling, with a slight degree  
“ of fever. The œdematic, or gangrenous  
“ forms of Erysipelas, may be combined  
“ with *Malignant Fever*, and *thus* commu-  
“ nicated from one person to another.” Instances of this, he adds, have occurred frequently in hospitals, the complicated disease spreading through a whole ward.

Dr. Wells, in the second volume of the *Transactions Medical and Chirurgical*, gives some observations on, and a few cases illustrative of, the contagious nature of Erysipelas. The fact of contagion he seems to prove in a manner satisfactory enough, but we have very little room left for judging of the circumstances under which it originated:

the



the cases are not detailed with much minuteness, and it is only in one instance you can collect the presence of Typhus, and on this occasion, he says, " I found the patient labouring under the ordinary symptoms of what is commonly termed a low fever, when it is likely to prove fatal." The disease, as we might expect, terminated unfavourably with several of his patients, and the tonic plan was alone successful. He mentions some instances where the disease seemed to spread by contagion in the hospitals of London and Edinburgh. I have not been able to collect any other facts to the point on this subject.

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## SECTION V.

### MALIGNANT ULCER, OR HOSPITAL GANGRENE.

WHEREVER this disease has appeared, its destructive influence has imperiously demanded attention, and its singular nature has uniformly excited curiosity ; yet, notwithstanding the union of motives so powerful in urging to a full investigation, and complete discovery



discovery of its character and genius, has it still remained overshadowed with much obscurity; and though the disease has been of no very unfrequent occurrence in large ships, transports, and in hospitals, in consequence of which we have learned, by fatal experience, that it is highly contagious, and have ascertained, through the same medium only, the best means of treatment and of prevention, still may I venture to assert, that *as yet* the source of its contagion has never been pointed out. But that it is contagious at all, some gentlemen undertake to doubt, and under that doubt to deny. Dr. Trotter, in particular, who has given us detailed reports of this disease under the name of Malignant Ulcer, seems to me to fly in the face of the whole of that evidence laid before him by the surgeons of the fleet, when he doubts in the least, much more when he indirectly denies the conclusion that unavoidably followed from it. “With respect  
“to its contagious nature, says he, I do  
“not mean to enter into any dispute: I  
“have not seen or *heard* any thing that can  
“entitle it to the term Infectious; and to  
“admit this as a part of its history, is at  
“once to stifle enquiry.” And again,  
he



he says, in p. 489 of the 3d vol. “ Mr. “ M'Dowal speaks of the infectious nature “ of this Ulcer. This language has been “ employed by others, and is an easy way “ to get quit of a difficult question.” And though he admits that some *strong* facts have been brought forward, that would seem to countenance the contagious nature of the sore, and though he has *no doubt* that Inoculation might ingraft a disposition to this gangrenous ulceration in another person, yet is he inclined to doubt its contagion, and conceives that a disposition of body, obtained by peculiar diet, and modes of living, explains the fact, without having recourse to any occult causes; and thus, to these *strong* facts, and *his own certainty* of the powers of Inoculation, he opposes “ a *sus-* “ *picion*, that a long and excessive use of “ spirituous liquors most frequently pre- “ cedes its appearance.” That such a cause would be equal to the production of Hospital Gangrene he thinks likely, because “ the “ stimulus of ardent spirit, in certain consti- “ tutions, covers the face, nose, &c. with red “ and irritable spots and eruptions; it taints “ the fluids, impregnates them with fœtid hy- “ drogenous gas, which exhales from the “ lungs,



“lungs, and is known by its bad smell; it  
“excites to excess the moving fibres, and  
“disposes them to Gangrene with more ra-  
“pidity than any other ordinary stimuli.”  
I forgot to mention that he also looks upon  
a long residence in warm climates as often  
forming part of this disposition to the Ma-  
lignant Ulcer. But Dr. Trotter furnishes  
facts sufficient to overturn both suppositions,  
for the great diffusion of the disease, when  
it once got footing in a vessel, was sufficient  
to shew, how little necessary the use of ar-  
dent spirits was to give it origin, and he him-  
self observes, p. 212 of the 2d vol. that  
“peculiarities of constitution have not been  
“remarked in our patients, and men who  
“had *lately* come to sea, were equally suf-  
“ferers.” Had Dr. Trotter recollected that  
this disease often originated in crowded and  
ill-ventilated hospitals, and with equal fata-  
lity spreads among those addicted to spirits,  
and those who are not, and that few of such  
patients have ever visited a warm climate,  
he would not have quitted the field of prac-  
tical inference for that of theoretic specula-  
tion. I have indulged in these remarks on  
Dr. Trotter’s opinions of Malignant Ulcer,  
because, as “the prevention of so horrid a  
“malady



“ malady becomes of the most serious con-  
“ sequence,” therefore a detection of error,  
or of the truth, must be of the utmost im-  
portance.

Very differently has another gentlemen  
decided on this subject; and he had at least  
equal opportunities for observation and in-  
formation. Dr. Blane says, he was led into  
an opinion that Ulcers were infectious, from  
observing that some ships were much more  
subject to them than others, though in the  
same circumstances in point of climate, vic-  
tualling, and the duties of the service.  
Some facts, he adds, which have occurred  
in this war, have put the question beyond  
all doubt. The truth of this position he  
evinces by various proofs adduced in p. 506  
et seq.—Proofs similar to them may also be  
found in Dr. Trotter’s *Medicina Nautica*,  
2d and 3d vols. Art. Malignant Ulcer.—I  
shall not dwell on the detail of these, but  
will now mention a few of the phenomena  
which the disease presents. “ The propen-  
“ sity to this complaint was such, says Dr.  
“ Blane, that the smallest sore, whether  
“ from a hurt or a pimple, fell into the state  
“ of an ulcer. Blistered parts were also af-  
“ fected in the same manner. Sores which  
“ seemed



“ seemed to be in a healing state, would  
“ suddenly become gangrenous. The men  
“ who slept near the ulcerated patients were  
“ most apt to be seized by them, as also the  
“ centinels and nurses who were about them.  
“ The incisions of those who underwent sur-  
“ gical operations, and were placed among  
“ them, assumed the same ulcerous state;  
“ while those who were placed in a remote  
“ part of the ship healed in a kindly man-  
“ ner. These ulcers were attended with  
“ symptoms of the most virulent and ma-  
“ lignant kind. They began with violent  
“ inflammation, which suddenly terminated  
“ in mortification; destroying in a short  
“ time the fleshy parts, so as to expose the  
“ bone, which soon became carious. They  
“ had all the characters of the worst sort  
“ of Scorbutic Ulcers, but they took place  
“ in constitutions in which there was *no*  
“ *other* symptom of scurvy, nor did they  
“ yield to lemon juice.”—Dr. Trotter, p.  
177, 2d vol. mentions, that “ contused  
“ spots, even where the cuticle was not  
“ broken, were not exempted from the ge-  
“ neral tendency to ulcer. But parts that  
“ had been burnt or scalded, above all  
“ other accidents, most quickly assumed  
“ the



“ the nature of this horrid sore ; spread and  
“ inflamed more rapidly, and in the end  
“ put on the most formidable appearance.”  
And again, he adds, p. 178, “ Although for  
“ the most part these ulcers spring from  
“ some external injury, yet we have met  
“ with a number of cases, where neither  
“ wound, puncture, scab, or contusion,  
“ could be said to have first taken place.”  
He then describes the manner in which the  
disease in these cases formed, till it assumed  
every characteristic symptom with conco-  
mitant fever and subsequent ulceration. It  
would appear that sores, possessing a spe-  
cific action as venereal, scrofulous, and va-  
riolous, were not liable to the attacks of this  
disease. It was observed also, both in the  
ships and at the hospitals, where this species  
of ulcer prevailed, that the hands of those  
who dressed them, when the skin was broke,  
fell into the same sort of ulcer. The prac-  
tice usually adopted with any success, was  
an emetic and purgative during the inflam-  
matory stage, with wine, cordials, and nou-  
rishing food, the moment gangrene appear-  
ed,\* after which the patient required the  
most

\* Stimulant and antiseptic applications were also made  
use of.—But my business here is not to speak of the treat-  
ment



most generous diet. These articles, says Dr. Trotter, meaning wine, cyder, and porter, seemed to snatch some from the very verge of dissolution: and a cool atmosphere conferred the highest benefits. “ The primary objects of attention, says Blane, “ should be ventilation, cleanliness, and “ separation. It is very rarely that this infection exists but in *large* ships, or in “ the wards of hospitals, where there are a “ *considerable number* in *one* apartment, “ producing a concentrated effluvia, and the “ most important point is, that there should “ be as few as possible within each other’s “ atmosphere.” The necessity of pure air he inculcates in the strongest manner, both as a means of prevention and cure.—I have been thus full in the detail of particulars from these authors, that we may have the less difficulty in establishing a point I shall, by and bye, insist much on. Dr. Trotter looks upon this as a non-descript disease, though he says we cannot assert it to be a new one; there are certainly no professed

ment required in the disease, but merely to notice so much of it as may tend to illustrate its nature, and lead to some discovery of the source of its contagion.



treatises on the subject, but we shall see very shortly that it has been often noticed and described by other authors of a more remote date. The disease often appeared in the Hotel Dieu at Paris, and I believe there are accounts of it in the Memoirs of the Academy of Sciences; but I have not been able to procure access to them. It has also often occurred in the Royal Infirmary of Edinburgh, and has uniformly arisen from filthiness and want of ventilation, for since these causes have been obviated, the disease has not shewn itself there. In that hospital its action produced the same phenomena as on board ship, and its contagious nature was too often satisfactorily exhibited. Its contagion was communicated by sponges, sharpees, by the indiscriminate use of bandages: at times also it seemed to be spread by the air of the ward, and its influence would then seem to act within three days.\*

\* These observations relative to this disease, as it appeared in the Royal Infirmary, I record on the authority of a gentleman, whose accuracy and judgment may well be relied on. Mr. Thompson, one of the surgeons of the Infirmary, and lately appointed Professor of Surgery, is the gentleman whose goodness favoured me with these and some other facts.



The symptoms of the disease were local and general; the constitutional symptoms usually preceded: they were those of *debility*.

With respect to the nature of the contagion, which produces the Malignant Ulcer, no one has hazarded an opinion; from the place in which I have undertaken to treat of it, the reader must of course have supposed, before I had time to inform him, that I consider Typhus as the source whence it derives its origin: and in support of this opinion the history of its symptoms and treatment, the places of its prevalence, and the means of prevention, furnish strong *presumptive* evidence. To these, as they have been stated, I shall refer with this single remark, that it is necessary for the explanation of the various phenomena of this disease to suppose, that its contagion, whatever it be, must act in a manner similar to that of Typhus, that is, on the system at large, and not on any part of it *primarily*, because it appears that sometimes there was no ulcerated part whatever on which it could exert an immediate influence, but that, after operating on the system, it then produced its appropriate effects on such parts as were possessed of weak powers of life: and that such was the na-



ture of its action will, I think, appear from the circumstance of its being stated, that it was very uncertain whether that action was at first general or local. Now this apparent uncertainty is in my mind the thing which at once decides the question in favour of its general action. For if we suppose that the contagious matter is generated in the ulcer alone, and that its action is of course local, what should follow? Why, that it would scarcely be possible this uncertainty should exist, for if its influence be first exerted locally, it is not possible that the constitutional symptoms should appear as early as the local ones, which we know to be usually the case; for what is the fact when we inoculate small-pox matter? Local symptoms always precede the constitutional ones for a given time, and when it is taken without the insertion of its virus, constitutional symptoms ever precede any eruption. Besides, such a supposition will not explain how the gangrene can arise without a previous ulcer, or how it can act on wounds that are constantly protected from its direct influence. And why should it excite such high and dangerous fever? Such is not the consequence of any of those contagions, whose ac-  
tion



tion we know to be local, as of Chancre, Itch, &c. But if we suppose that the contagion productive of Hospital Gangrene first acts constitutionally, and then locally, we can readily explain all the phenomena of the disease,\* and why all this uncertainty attaches to the question. The surgeon on going his rounds perceives that wounds which on the last inspection appeared healthy, are now assuming the characteristic features of Hospital Gangrene: the local symptoms sufficiently assure him of this, and on turning his attention to the general condition of his patient, he finds

\* I have already mentioned, with respect to Dysentery, the doubts I entertained of any local contagion being generated in the intestines, such as could be communicated by a glyster pipe. I also stated that the analogy, which seemed to shew that a general and local contagion existed in Hospital Gangrene, made me hesitate in deciding against it in Dysentery. It has seemed to many that the contagion of Hospital Gangrene was spread by the common and indiscriminate use of Sponge, Sharpee, &c. But this always took place in the foul, ill-ventilated wards, where the contagion had originated: and therefore such a mode of communication must appear doubtful. I have heard that Saussure has induced the disease by actual contact, but as I am ignorant under what circumstances this was done, I cannot say, whether it be liable to the same or other objections. It is a point, as yet not decided to my satisfaction.



him labouring under such constitutional symptoms as fully confirms his fears. These constitutional symptoms always co-exist with the altered appearance of the wound, and might perhaps have been found to precede it, had the surgeon been as much in the habit of attending to the general as the local affections of his patient; but the former he is too apt to overlook till forced on his observation, and hence he is led to conceive it uncertain whether the constitutional or local symptoms precede. Besides the reasons already assigned, one consideration more may induce us to decide for the former, when we reflect in the first place, that the local symptoms are scarcely ever known to occur, without the presence or precedence of those which are constitutional; and in the next place, that Contagion, when received into the system, does not act instantaneously, but requires an interval of time, more or less, for the production of its appropriate effects: therefore, if we can suppose for a moment that the Contagion of Typhus, acting under particular circumstances, and on individuals possessed of a predisposition to the disease, be capable of exciting Hospital Gangrene, is it not likely, I say, that the  
action



action of that Contagion should be perceived as early in the altered condition of the wound, as in the production of its more peculiar symptoms? It is not unlikely, if both were equally attended to, that we would find these effects nearly simultaneous; I have already offered some reasons for explaining why the local appearances have in general been first noticed.

As this disease then is propagated by Contagion, and as that Contagion, if it acted *primarily* on the wound, should not necessarily, or rather could not, produce general symptoms immediately co-existent with the local ones, and if it acted, like Typhus, on the system at large, must of course induce local and constitutional derangements nearly at the same instant of time, is it not probable that this Contagion, if not dependent on that of Typhus, at least strongly resembles it? \* That it is the Contagion of Typhus,

\* Let any person, in the least conversant with those striking appearances, which patients labouring under Typhus Fever present to the eye, visit the wards where Hospital Gangrene rages, let him know nothing of the existence of that disease, but let him merely inspect the countenance, and the tongue of such as are suffering from it, and he would directly pronounce them the victims of



phus, acting under peculiar circumstances, the following facts may, perhaps, render more than probable.

In treating of the Malignant or Contagious form of Dysentery, which in my mind has been clearly proved to consist in a combination of that disease with Typhus Fever, I had occasion to quote Roederer, as having described that combination under the name of "Febris mucosa acuta maligna"; it principally prevailed in the camps and military hospitals of a town that was besieged and reduced to great distress from a scarcity of provisions, and the immense multitudes crowded within its walls. The hospitals were literally crammed with the sick, and the whole town was little more than an hospital. There were many surgical and other patients in the hospitals, besides those labouring under Dysentery, and as this latter disease spread among them, he describes the effects it produced on the various maladies that thus became combined with it. In page 90, there is the following remarkable passage: "Vul-

the fever. But the attention paid to the local disease, diverts our reflections from such particulars, and blinds us to the inference that would otherwise naturally follow.

" nerati



“ nerati quovis modo omnes fere, liceat an-  
 “ tea fuerint sanissimi, *ex mucosi Epide-*  
 “ *mici connubio*, citiùs lentiùsve succubue-  
 “ runt. Pro gravitate vulneris accenditur  
 “ febris acuta, *ex inflammatione* paratur pus  
 “ mali moris, fœtens, ichorosum, sequitur  
 “ *gangrena*, *juncto simul* symptomatum  
 “ febris mucosæ, acutæ malignæ, plus mi-  
 “ nus biliosæ, inflammatoriæ, putridæ *satel-*  
 “ *litio.*” Nothing can be more precise than  
 this language ; nothing more clear, than the  
 supervention of Hospital Gangrene, on the  
 wounded patients being attacked by contagi-  
 ous Dysentery. But he details more of its  
 effects, and in p. 91, observes, “ Eadem  
 “ fere sors fuit præfectorum in urbe ex vul-  
 “ neribus ægrotantium, ac militis gregarii  
 “ in nosocomiis.” We may remember that  
 the air of the whole city was little superior  
 in purity to that of the hospitals. “ Non  
 “ temerè quisquam vel lene quoddam vulnus,  
 “ per se in corpore sano facilè coaliturum,  
 “ sive ex infortunio, sive operatoris manu  
 “ inflictum, sustinuit, quin *accedente febre*  
 “ *mucosâ*, in discrimine vitæ fuerit versatus.  
 “ Sub operatione, aut ex venâ sectâ profluit  
 “ sanguis tenuis, ingrâtè ruber, levitè fus-  
 “ cus, laudabili indole gelatinosâ, glutine  
 “ et



“ et consistentiâ destitutus, diffluens, justò  
 “ dilutior: ægrè coit in placentam, multo  
 “ sero circumfusam, tenui crustâ inflamma-  
 “ toriâ tectam. Ipsa vulnuscula phlebo-  
 “ tomo inflictâ ægrè consolidantur, & post  
 “ plures dies, madida labia vulnusculi adhuc  
 “ dehiscunt.” These passages need no com-  
 ment: I may safely commit them to the  
 reflections of the reader; I cannot however,  
 avoid noticing the singular combination of  
 disease that occurred in this case: two local  
 affections, Dysentery and Ulcer, neither of  
 them contagious, are spread by contagion  
 in combination with Malignant Fever; thus  
 presenting a very singular fact in the annals  
 of medicine, and equally demonstrative of  
 the common source whence these diseases de-  
 rive that property.

Without further circumlocution I shall  
 detail the subsequent facts, which though  
 they do not so plainly and directly announce  
 the existence of Typhus in *combination* with  
 Malignant Ulcer, yet clearly infer the pre-  
 sence and probable co-operation of that dis-  
 ease. Power, whom I have already referred  
 to on the subject of Ophthalmia, mentions  
 that the Plague appeared so frequently at  
 Rosetta, in common with Ophthalmia and  
 other



other diseases, that it was found necessary to establish a separate hospital for the patients labouring under the former, and to prohibit the admission of a single patient into the general hospital, till a scrupulous examination of the symptoms had previously taken place. Under these inspections, he observes, the great variety of diseases that appeared, compared with the similarity of the circumstances in which they were contracted, afforded a striking proof of the general prevalence of the *putrid virus* in the atmosphere, whilst a general tendency to *putrefaction* rendered its constant existence, and increased acrimony within the hospitals equally evident; the smallest sore, arising from a common pimple, would, he remarks, often spread to an alarming extent, and the wounds inflicted by the surgeon, were, he says, alike fatal as the original sores, and almost uniformly assumed the same appearances, to remove which the surgeon had in the first instance interfered—*vid.* p. 25, 4th section. These are obviously the characteristics of Hospital Gangrene in company with the Plague.

Lind furnishes us with a fact still more to the point; in his Observations on the Diseases  
of



of Hot Climates, he describes, (part I. chap. III. sect. III.) the disorders most usual in Batavia, a place notorious for its unhealthiness; and on this occasion he records the great mortality that prevailed on board the English ships which touched there: these suffered more, he says, by the *malignant* and fatal diseases of that climate, than in any other part of India. The Fever so prevalent at that time, he describes to be of the remitting kind, exceedingly mortal and malignant: it raged not only in the ships, but through the whole city, which afforded one scene of disease, desolation and death. At that time, a slight cut of the skin, the least scratch of a nail, or the most inconsiderable wound, turned quickly into a *putrid spreading Ulcer*, which in twenty four hours consumed the flesh even to the bone. This fact he recounts as very extraordinary, and scarcely to be credited upon a single testimony, yet on board the ships, he says, they had the most fatal experience of it. The fever, whose influence was on this occasion so destructive, he believes, and proves to be infectious in another part of his treatise. *Vid.* part II. chap. I. sect. V.

The



The conjunction of a bad state of Ulcers with the prevalence of Malignant Fevers is occasionally noticed by different authors. Thus Webster, in p. 335, of his 1st vol. on Epidemic Diseases, mentions on the authority of another historian, that in Italy in the year 1694, “ the Spotted Fever, and in some “ places Dysentery were very mortal. “ Wounds degenerated into ulcers, and “ blisters were followed by mortification, “ which proved fatal to many.” Thus too, of a Plague at Venice it is mentioned, that it absorbed all other disorders in its pestilence, *even* wounds and ulcers.

Pringle also mentions a circumstance that very probably refers to this disease: he says, that among the soldiers, who had embarked, the fever during the voyage having acquired new force by the confinement of the air, by the *mortifications*, and other putrid effluvia, it became so virulent, that above half died in the boats, and many of the remainder soon after their arrival.

I shall conclude this detail of facts relative to Hospital Gangrene, with one of more interest and weight than some of the latter: it is extracted from Ramazzini, and will be found in p. 638, of his 40th chap. de morb. castren.



castren.—In this chapter, he principally quotes the observations of G. Erric Barnstoff, who attended the German armies during five campaigns in Hungary, a country at that time noted for the great fatality of its disorders. He states, Malignant Fevers, Dysentery, and wounds to constitute the chief diseases of the army, and after speaking of the two former, he subjoins the following remarks respecting the latter:

“ Quoad unitatis solutæ morbos, vulnera  
 “ scilicet, quod sæpe evenit, rem notatu  
 “ dignam observavit vir illustrissimus inesse  
 “ iis nescio quid *castrense malignum*, quam-  
 “ vis vulnera essent levissima, nec mali-  
 “ quicquam de illis liceret suspicari; obser-  
 “ vavit enim in diuturnis arcium obsidioni-  
 “ bus, vulnera omnia cum contusione, ve-  
 “ luti sclopetorum, ac præsertim in capite,  
 “ utut leviuscula, curatu esse difficillima,  
 “ et licèt summâ delegendiâ tractata, cum  
 “ summo tamen chirurgorum dedecore per-  
 “ sæpe lethalia fuisse, superveniente nimi-  
 “ rùm *inflammatione*,\* ac postmodum *gan-*  
 “ *grenâ*,

\* As inflammation is found so generally to precede the appearance of the Gangrene, it may be objected against the cause I have assigned for the production of this dis-  
 ease



“ *grena*, ut locus interdum suspitioni fuerit,  
 “ hostes obsessos veneno plumbeas glandes  
 “ armasse. Verùm a desertoribus habitâ  
 “ notitiâ, idem fatum quoque ex susceptis  
 “ vulneribus obsessos obiisse, de malignitate  
 “ per aerem vulneribus communicatâ, dubi-  
 “ tari cœptum, ideoque bezoardica, terrea,  
 “ et absorbentia cum cephalicis vulnerariis  
 “ præscribendo, feliciores vulnerum insti-  
 “ tuebantur curationes, non omissâ interim  
 “ vulneratæ parti, remediorum ejusdem in-  
 “ dolis applicatione.” In this case, the be-  
 sieging and besieged suffered the same fate,  
 and as the former had been long molested by  
 Malignant Fevers and Dysentery, it is still  
 more likely that the latter could not have

ease, that it is not likely the influence of Typhus would  
 be evidenced in an inflammatory process. To this it may  
 be replied, that Typhus often commences in a similar man-  
 ner with symptoms of great excitement, and is then de-  
 nominated Synochus by nosological writers; sometimes  
 no such action takes place, and we find cases of the  
 Malignant Ulcer to answer to it: thus Mr. Edwards, in a  
 letter to Dr. Trotter, p. 474, of the 3d vol. observes,  
 “ From the debility constantly ensuing, I was afraid to  
 “ use general bleeding, recommended by you, nor did I  
 “ venture upon cupping, although, I think it might be  
 “ often useful, yet I have seen many cases spread, *with*  
 “ *but very little inflammation attending them.*”

escaped



escaped the same enemies, and that these diseases must have been to blame in the production of Malignant Ulcers, will, I trust, appear as well from this, as the other statements, which have been made.

Such are the proofs which alone I can at present adduce in support of the opinion I have advanced relative to the contagion of Hospital Gangrene:\* though few in number, they are not deficient in weight, and should at least suffice to attract farther attention to

\* When I began these observations on this disease, I conceived myself the first person who had ever entertained, or at least publicly maintained these sentiments; *perhaps*, however, Dr. Jackson, in his outline of the History and Cure of Fever, may be entitled to any credit due to the original proposer of the idea, if he can be supposed, in the following passage, to have advanced opinions similar to mine: p. 328, he says, “ Blotches on the skin and sore legs frequently appear in crowded barracks, in ships, or hospitals; they depend *evidently* upon a cause of febrile contagion; but the precise *state* or *degree* of contagion which originates this form is difficult to be marked; the appearance, however, usually shews itself in an early stage of contagion,—in a contagion generated among a set of men, rather than imported from a concentrated source.” Dr. Jackson’s ideas on this subject do not, however, appear to me quite precise or correct, nor do I think on consulting his context, that his meaning is at all equivalent to mine.—Let the reader compare, and judge for himself.

the



the subject. They may, I think, satisfy us that the disease has never appeared, except under circumstances such as would at the same time be perfectly equal to the production of Typhus, or Malignant Contagious Fever, and many of the facts shew that they did exist together; when I say, however, that it is the presence of Typhus which gives malignity and contagion to ulcers, let me not be understood to mean, that an ulcer must necessarily be rendered malignant because the patient who has one is attacked by that fever; experience proves the contrary, and the facts which have been adduced by no means warrant such a conclusion: for they uniformly mark the co-operation of another cause, which I look upon as essentially necessary, and that is the breathing of an atmosphere, rendered highly noxious by confinement, by multitudes crowded together, by want of ventilation, by a general neglect of cleanliness, or by other means, which may tend to the impurity of that element. The truth of this we find evinced in the efficacy of a cool atmosphere, and in the total disappearance of the disease, when due attention is paid to ventilation and cleanliness.—The co-operation of a similar



cause is equally necessary in the production of the disease we have next to speak of.

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## SECTION VI.

### PUERPERAL FEVER.

ON this disease I shall be very concise, and will content myself with stating a few of those general facts that are well ascertained respecting it, because I can with much pleasure refer the reader to a little essay, which was published on this subject in the year 1787, and which has anticipated almost every thing I had to offer. This essay is entitled Practical Observations on the Puerperal Fever, by P. P. Walsh, M. D. In this treatise the whole theory of the disease is, in my mind, truly stated, and fully supported. He maintains, “that the disorder is  
“ not one *sui generis*, confined to in-lying  
“ women, but merely an unusual form of a  
“ very common disease, and in reality no  
“ other than the *common infectious fever*,  
“ complicated with a more or less extensive  
“ inflammation of the peritoneum.” In assenting to the truth of this proposition, I  
would



would wish to have it understood, that by *Puerperal Fever* I merely mean *that fever* which has been occasionally observed to prevail in lying-in wards, or in circumstances similar to those which have taken place in such wards, *a fever* which is spread by contagion, and which is exceedingly mortal. That this disease occurring in hospitals is contagious, I shall here take for granted, as not only this point, but the proposition already advanced, seem almost universally admitted. The disease is termed by some Peritonitis Typhodes, and Dr. Willan, whose authority I am always happy in adducing, expressly makes a distinction between this form and the *simple* Puerperal Fever, which latter is little more than simple peritoneal inflammation, and which may generally be relieved by the same means; “But, says he, when a child-bed fever is epidemic and contagious, the symptoms (those of the simple Puerperal Fever) are connected with the Scarlatina, or Malignant Fever. Particular *situations* and particular *seasons* favour such a complication, which is usually fatal.” *vid.* p. 321 of the Reports, &c. Here the seasons probably refer to Scarlatina, and the situations to Malignant Fever. All the disputes



which have arisen relative to the nature of Puerperal Fever (and the character of no disease has been more warmly contested) have undoubtedly depended on want of attention to this distinction, and also in part upon laying too much stress upon the symptoms and appearances of inflammation in the *compound* Puerperal Fever, for these would seem most decidedly to authorise us in ranking the disease among the *pure* inflammatory affections: and accordingly we find Dr. Hulme and others attempting to explain every phenomenon it exhibits, on the *sole* ground of an inflammation of peritoneum and omentum, the existence of which no one doubts in the least, and the tendency towards which he attributes to the constant pressure these parts are subject to during the latter months of pregnancy. But why, on these suppositions, are so few attacked?—Whence the little benefit, or rather the fatality, experienced from venæsection, and that in proportion to its repetition? Whence the symptoms of great debility, and that tendency to putrefaction, which is seen to prevail? Why should not the disease appear before, rather than after delivery? Why should it chiefly occur in hospitals, and then under particular



particular circumstances only? Why should it then be epidemic and contagious?—These facts are by no means explicable on the *sole* ground of inflammation, but are all readily accounted for, on the supposition of that inflammation being conjoined with Malignant Contagious Fever.

What are the circumstances under which the most authentic histories shew this disease to have prevailed? Is it not in hospitals crowded, ill-ventilated, and uncleanly? Has not the Hotel Dieu been constantly notorious for its production? And have not our own lying-in-houses been frequently molested by it, so long as strict attention was not paid to ventilation and cleanliness? The disease in its contagious form rarely appears in private practice, and when it does, it is in a far milder manner. Indeed it closely resembles Hospital Gangrene in every circumstance necessary for its production, *with this difference*, that in one, the existence of a wound, in the other, the predisposition of the puerperal state is requisite, and it is not a little singular, that at the time Hospital Gangrene prevailed in the surgical wards of the Royal Infirmary at Edinburgh, the Puerperal Fever raged in the lying-in-ward immediately



diately adjoining to them, thereby clearly shewing the analogy between the diseases, and their dependance on the same cause.

I shall not dwell longer on this article than while I again refer the reader to the *Treatise on Puerperal Fever*, by Dr. Walsh,\* and in particular to p. 14 and 15, where various proofs are stated which I have not thought it necessary to adduce. The reader will also find some interesting information respecting this disease, particularly as it appeared in the Lying-in Hospital at Dublin, in a paper by Dr. Clarke, in the *Medical Commentaries* for 1790. Various other authors may be consulted on the same subject with much satisfaction, if we carry with us the true nature of the disease. In concluding these remarks I must recal to the reader's recollection the last objection which has been stated against the assigned cause of Conta-

\* There is, however, one opinion in which I must dissent from Dr. Walsh, when he asserts that the disease is not wholly confined to in-lying women, and urges, in proof thereof, a fact which is true, but is not applicable, "that a Peritonitis frequently supervenes to a previous Typhus, even in the male sex." To this I would reply, merely by asking, Could such a male patient communicate to another man or woman not puerperal, this compound disease? I conceive he could not.



gion in Hospital Gangrene, because the same objection may, and has been, urged in the present case; in p. 21, it is fully considered by Dr. Walsh, but not so satisfactorily refuted: the same answer will, however, suffice in this, as in the former instance, and even allowing the objection every credit, it cannot weigh against the multitude of facts which are balanced against it.

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### CONCLUSION.

HAVING now considered at due length all those diseases, which, in my opinion, are indebted to Typhus for their contagious character, it may not be amiss, before we conclude, to make a few remarks on the general nature of that contagion, as it exists in its simple and compound state, and also to take some notice of certain diseases, which have been supposed, by a very respectable writer, to derive their contagious property from the same source, but which, in my estimation, (with a single exception) have not the least connection with it.

The contagion of Typhus, so far as we have ascertained its nature and origin, ap-



appears to be very different from that of any other disease, possessing this property *per se*. Independent of that compound kind, which I have taken so much pains to illustrate, there seem to be three very distinct species of contagion, differing in some essential respects from each other. Typhus is the only example of one, the Exanthemata constitute a second, and local contagions a third; the general features of the species have been in some degree already detailed; I shall here therefore only mention, and partly in the words of Dr. Blane, some of those particulars, in which the infection of fever appears to differ from the specific morbid poisons; in the first place, “it does not depend  
“ *in all instances* on the disease itself, the  
“ common source of it being the stagnated  
“ effluvia of the human body, from the  
“ want of a change of linen while there is  
“ at the same time an exclusion of fresh air.  
“ These are the circumstances, which con-  
“ cur to produce febrile infection in jails,  
“ ill-regulated hospitals, and ill-disciplined  
“ ships.” The truth of this proposition is established on the strongest evidence: the disease has been repeatedly known to spring up under the conditions specified, where no suspicion



suspicion whatever could be entertained of imported contagion, and we have seen abundant testimony to the same effect, in treating of its combination with Dysentery. Typhus is known, and has been familiar in every quarter of the habitable world, because its causes may exist every where, and can never be destroyed but by constantly obviating these causes: as for its total extirpation, that is, and must be ever out of the question, because, though existing in no one place to-day, its contagion may readily be generated to-morrow. The Exanthemata have characters the reverse of this: they cannot be generated by any combination of circumstances we are acquainted with: they are not known in every quarter of the globe, and only prevail there, where they are once imported: hence arises the possibility of extirpating one \* of them, and of checking the prevalence of all by preventing the introduction of their contagions; the labours of Dr. Haygarth prove that this possibility does not merely rest on theoretic speculation, but that it is supported by actual experience. This

\* It would be a proposal equally ridiculous as vain to think of extirpating Small-pox, by the substitution of Cow-pox inoculation, if like Typhus, it could originate without the previous action of contagion.



gentleman is inclined to think Typhus not contagious before the fourth day: the limits of the Exanthemata are not equally ascertained, and no one, I believe, has undertaken to point out the time when any of them cease to possess that property.

Secondly, says Dr. Blane, "this infection may exist about the persons of men without producing the disease; and this happens to those about whose persons it was generated." Of the truth of this fact, Dr. Blane had frequent proofs in the consequences which too often followed the reception of impressed and other men, apparently healthy. The Oxford assize fully exemplifies it. The exhalations from the bodies of such persons convey the infection.

Q. When the infection is generated by the disease itself, in what secretions is it contained, or how conveyed to another body? Is it generated in the lungs, by the skin, or in the intestines? Or is it conveyed in the excretions from these different organs? It is, I imagine, generally supposed to be contained in them all, but I am disposed to think, without much reason as to the two former, for I look upon the mouth and intestines, as the great sources whence the contagion of Typhus issues. When it is asserted,



ed, that the offensive odour of the breath is presumptive evidence in favour of the lungs, and that the undenied fact of the majority of persons being infected in consequence of inhaling the breath of fever patients, is proof the most decisive, I would reply that analogy (though it be deemed remote) makes me doubt the certainty of a conclusion founded on premises that may be fallible: I do not deny that the breath is not offensive, or that it does not convey contagion, but I hope it may be allowed to doubt, that such is the nature of the breath, as it issues *immediately* from the lungs, and that none of these properties are superadded to it in its passage through the mouth. The analogy to which I have alluded is that of Mercury, which is supposed to taint the breath, and to be carried off by the skin. Now, neither of these is fact, and both points I have ascertained by experiment. Let a mercurial patient, the fœtor of whose breath is intolerable, expire through the nostrils, and the air will be found to pass out with as little odour as it entered. And again, let the Mercury be administered *internally*, and not by frictions, let all its usual effects be induced on the system, and for any duration, let the result be

be



be attentively observed, and we shall no more hear of watches or money being affected by the Mercury in its passage through the skin: such an effect is always produced by the evaporation of the Mercury from the thighs, or its actual contact with these substances; as then in the former case, it is the affection of the mouth which gives the fœtor to the breath, why may we not suppose the same thing in fever, more especially as we constantly observe such vitiated secretions encrusting the mouths of patients labouring under the latter? It might easily be put to the proof, though perhaps not so safely, or with so little concern, as in the experiment on the mercurial breath.—As to the skin, I know of no proofs which *unequivocally* show, that contagion is generated and conveyed in its perspiration, as persons apparently infected by it, were at the same time liable to receive the breath of the patients, or exposed to the smell of their excrements: and the communication of the disease by infected clothes, does not necessarily infer that such infection was produced by the skin.—The intestinal evacuations have often been fatally found to convey the contagion of Typhus, and hence it probably arises that  
the



the feces in Dysentery are so powerful in spreading that disease. From all I have been able to observe, the mouth and intestines seem to me the great agents in generating and dispersing the Contagion of Typhus. Q. It is not primarily received into the mouth, fauces and stomach? And does not the latter organ, as being the more sensible, always appear primarily affected? But this speculation has engaged the pens of abler writers: I shall not further touch upon it.

Thirdly, says Dr. Blane, there is another difference between this disease and the Exanthemata, “It may be caught more than “once in life.” I believe he might have gone further, and said, that the disease creates in some degree a predisposition to its own reproduction; this we see exemplified in the constant tendency to relapse, never observed among the Exanthemata, and in the fact that thousands have repeatedly suffered from its attacks. Yet, says Dr. Trotter, p. 213, vol. III. “During our extensive and long experience of the origin, “progress and extinction of contagion, in “ships and every where else, I have entertained a strong *suspicion*, that Typhus infection *very seldom* affects a person more “than



“ than *once* in a life-time. There must in-  
“ deed be some truth in the observation :  
“ after *perfect* recovery the body seems no  
“ longer susceptible of the disease, and  
“ breathes a tainted air with impunity, as  
“ daily happens in infected ships. When  
“ contagion is introduced to a ship a second  
“ time, after some interval, it attacks a *new*  
“ set of men ; the former fever patients are  
“ exempted.” There is indeed so far some  
truth in the observation, that persons, who  
are long and habitually accustomed to breathe  
the infectious air of jails, of hospitals, and  
of ships, are much less apt to be affected by  
it, than any new set of men, but should  
such persons be for a time removed to a pure  
atmosphere, and again return to breathe in-  
fectious effluvia, they will find their suscep-  
tibility of the disease, little less than it had  
been originally.

With respect to the contagion of Typhus  
in its compound state, or where, (in the  
language of Willan, it is propagated by in-  
fection under a double form, there can be  
little necessity for dwelling on it here, as it  
is governed by the same general laws, which  
influence contagion in its simple state, and  
as I have already anticipated, in treating of  
Dysentery



Dysentery and the other diseases, almost every thing I had to offer on the subject. One point however, yet remains to be spoken of; It has been stated with respect to those diseases under the influence of compound contagion, that, in the first instance, they require certain manifest causes for their production, and as the principle of contagion may be afterwards superadded, they were then propagated by that agent; but, it may be asked, is the co-operation of these manifest causes still required? Thus, Dysentery is produced by exposure to cold in certain seasons: can it be propagated by contagion without the assistance of cold, and in a different season? Van Swieten answers this question: in p. 60, vol. 16, he undertakes to refute that objection to the contagion of Plague and other Epidemics, taken from their origin: viz. as the first person seized with the disease, must have it without contagion, why may not all others. To this he replies, that on another occasion (sect. 1582) he has proved, that diseases may be produced in the human body by manifest causes, of which causes, these diseases are the effects, and through them, those diseases induce such a change in the body of the patient,



tient, as to enable him to infect other persons with the same distempers, though never exposed to those manifest causes, from which the sick person himself took the disease. He illustrates this position in the case of Camp Dysentery, and quotes Degner's observation on the contagious nature of that disease: "Miasma illud contagiosum *semel* natum, "potentiam consequi se diffundendi, ac per "contactum sese aliis communicandi." Indeed, says Van Swieten, "when this con- "tagion is once produced, it is not neces- "sary that the same constitution of the air "continue, from whence the disorder pro- "ceeded, but the disease goes on to exer- "cise its power, though *another* constitution "of the air may prevail," In the same manner I should suppose, that the Hospital Gangrene and Puerperal Fever require a certain vitiation of the air for the production of their contagion, yet when once generated may go on to act without the further co-operation of that cause.

I shall now, in the last place, proceed to notice an opinion advanced by Dr. Blackbourne, relative to the nature and origin of some acute contagions. The great object proposed by this gentleman is, to demon-  
strate



trate the possibility, if not of extinguishing, at least of checking the progress, and limiting the ravages of all contagious diseases. For this purpose he takes into consideration the different modes by which infection may be communicated: he denies that simple contact is ever sufficient, and very satisfactorily proves it of the Plague, which he deems analogous to Typhus and the Exanthemata: many facts too would seem to shew that the latter diseases are never given in that way, for it has been ascertained that uninfected children may lye in the same bed with a small-pox patient without receiving that disease, provided they do not expose themselves to the inhalation of his breath; and Dr. B. observes, that “infected clothes or fomites  
“ do not infect by simple contact, but can  
“ only impart infection under that degree  
“ of temperature, in which the miasms they  
“ have imbibed, assume the form of va-  
“ pour.” Consequently, as the introduction of infectious particles by simple contact is impossible, and as Inoculation is entirely a voluntary act, and highly to be reprobated from its pernicious tendency, the inhalation of infectious vapours by the mouth and nostrils is the only remaining source of



contagion which is to be guarded against. In the chapter on Infection, Dr. B. takes a very extensive and satisfactory view of the origin of Epidemic Fevers, and of the manner in which they became contagious; in the distinctions which he establishes on this occasion, I entirely coincide with him, and am happy to find, that many of those facts, which have been adduced relative to Dysentery, have a strong tendency to confirm them: for we have perceived, that in many cases, where that disease was accompanied by simple fever only, it became contagious under those circumstances well known to be capable of inducing Typhus. Having established in a very clear manner the conversion of Epidemic into Contagious Fevers, this author goes further, and endeavours to maintain (more, however, as he says, for the sake of illustration, and of exciting discussion, than from any ability to form *positive* conclusions,) the identity of the contagious source productive of *Plague* or *Bubonary Fever*, of *Variolous*, *Morbillous*, *Scarlet*, and *Yellow Fevers*: and that it is the *contagion of Fever*, joined to certain incidental circumstances, which probably gives rise to all this variety of acute contagions. It is on  
this



this part of Dr. Blackburne's essay that I mean to offer some observations, to assign my reasons for doubting the theory he has suggested, and for concluding that there is very little ground for supposing any analogy whatever between Dysentery, Small-pox, and the other Exanthemata, in the source of their contagion.

With respect to the Plague, this forms the exception I have already alluded to, for I conceive *so much* analogy to exist between it and Typhus, that I look upon their contagions as perfectly identical, and differing only in degree. Dr. B. however, is more inclined to class it with the Exanthemata, and in p. 148, makes mention of those incidental circumstances, which cause the Plague to assume the *bubonary* form: these are the great heat and hot parching winds of Egypt, inducing a peculiarly dry, irritable state of skin, and impeded perspiration, whence follows increased absorption, and a distended and irritable state of the lymphatic glands. “ An Egyptian, or any individual, says Dr. B. being exposed to  
“ paludal gas, and attacked with fever, in  
“ this irritable state of skin and lymphatic  
“ glands, will be said to suffer the Plague in



“ his own person, and if subjected to the  
“ circumstances which generally render fe-  
“ vers contagious, will communicate a dis-  
“ ease of a specific form to a second indivi-  
“ dual receiving his effluvia.”—And “ thus  
“ it is transmitted, he afterwards adds, not  
“ only on the spot, where a conjunction of  
“ circumstances gave it birth, but to other  
“ climes and regions, where such united in-  
“ fluence does not exist.” Such is Dr. B.’s  
theory of the origin of the Plague, the ge-  
nuine character of which is, he observes, and  
with truth, undoubtedly *typhal*, and its bu-  
bonary form merely adventitious. To all  
this I would reply, that it follows of course,  
that the Plague must always be *imported* to  
those temperate climates, where such an irri-  
table state of the skin and lymphatic glands  
could never of itself occur, and therefore  
that the Plague never was generated in these  
northern regions. This necessary conse-  
quence of the doctrine, in my mind, fully  
refutes it, for almost every history of the  
Plagues of London is directly against it.  
These were seldom long absent from that me-  
tropolis previous to the great fire in 1666, and  
since that period never appeared as before :  
surely no one will say, that it was because the  
Plague



Plague was regularly imported antecedent to that event, but never afterwards; or that the heat of the climate has been materially affected. The fact is, that that event has conspired with other causes to diminish the frequency, and lessen the virulence of all malignant diseases in London by its influence over Malignant Fever, and by this means we are enabled satisfactorily to explain the whole chain of effects subsequent to that period. Then, does not the frequent existence of the Plague in the most temperate climate, with its disappearance from many of them, for so long a period, and for such obvious reasons, fully refute Dr. B.'s notions on the origin of Bubony Fever? *vid.* Heberden on the increase and decrease of different diseases, and from p. 90 to 96 of this treatise.—In speaking of Inoculation, p. 67, Dr. Blackburne admits, on the evidence of a single *equivocal* fact, the possibility of communicating the Plague in that manner, and thus making out a strong analogy between it and the Exanthemata, which alone seemed communicable that way, and even among them we are only certain of it in Measles and Variola. He appears satisfied on the report of Sir Robert Wilson, that the case of Dr. White was a



case of true Inoculation, and therefore that the Plague in this respect differed from Typhus, and resembled the Exanthemata.— There are, however, circumstances in this case, which induce me to suspect, either that it was not one of true inoculation, or that the disease of which he died was not the Plague; and on all occasions we should be slow in deciding a question on a solitary fact, however authentic, but particularly so where any uncertainty can exist of its real nature. Now it does not *satisfactorily* appear from Sir Robert Wilson's report, or any other *extant*, that he died of the Plague from *inoculation*, for he instituted these experiments during the time the Plague *raged*, when he might have been conversant with the sick, and nothing is said to the contrary: and after having failed twice, the third attempt is said to have proved fatal, for “in three days after the symptoms appeared he died, falling a lamented victim to a disinterested zeal.” Here neither the interval between inoculation and the appearance of symptoms, nor the symptoms themselves are mentioned, and we are left to form our own conjectures.— Q. Might not his death have been caused by the absorption of putrid matter, or the inflammation



flammation of an absorbent vessel, to which unfortunate accident many eminent anatomists have fallen victims? I do not say that it was owing to this cause, but merely suggest these objections to shew, that it has not yet been satisfactorily proved, that the virus of a pestilential bubo can communicate the disease by inoculation.

With respect to the origin and source of contagion in Variola, Measles, Scarlatina, &c. Dr. B. has grounds much weaker than in the case just discussed: there the fever is undoubtedly *typhal*, but surely it must occur to every one that the fevers of Variola and Measles are so far from being of the typhoid character, that they not unfrequently require repeated venæsection; and we have already seen, in the instances of Malignant Dysentery, Catarrh, Puerperal Fever, &c. that V.S. was in general utterly inadmissible. But without entering into any detailed refutation of each or any of these diseases owing their origin to a combination of incidental circumstances with infectious fever, the statement of a few objections which apply equally to them all will answer our purpose.—It has been proved that Dysentery, and some other diseases were contagious, in consequence of such a combination: if it



were true of the Exanthemata, is it not likely that some little analogy should exist between them? if so, how does it come to pass that the former diseases may be taken any number of times, while the latter never affect the same individual more than once? If this objection be not explained, and Dr. B. has not attempted it, it lays to the ground at one blow, the whole of this hypothesis as it relates to the Exanthemata, and *Pertussis*. With respect to this latter disease, it may be asked, why this author excludes it, or rather does not enumerate it in the catalogue of acute contagions: Surely, it must rank among them, or be excluded from contagion entirely, as no one will enlist it among those of a chronic character. There was nothing in the history of this disease, which could countenance the supposition, that it derived its contagion from Typhus. Indeed it must be confessed, that, though there is a greater uniformity in the source, whence various diseases derive their contagion than has been imagined, there is not such a general identity in it as Dr. B. would wish us to believe; the Exanthemata in particular possess characters so peculiar, and so obviously different from those diseases, which have been demonstrated

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to derive their contagion from Typhus, that it is impossible to admit for a moment such a supposition, however ingeniously supported.\*

Were further arguments needed to overturn this hypothesis, it might be urged, granting for a moment the truth of Dr. B's theory, that no reason has or can be assigned, why variolous or morbillous patients, though they cannot infect a person who has had those diseases with either a second time, should not still be able to propagate the *fever*, which forms so essential a part of the contagious compound; yet we never hear of nurses, or anxious mothers, so constantly watching over their sick charge, and for a length of time exposed to fatigue and distress of mind, causes strongly predisposing to Typhus, having taken that disease in consequence of exposure to the contagion of Variola, Measles, or Scarlatina. Now that a contagious disease, compounded of Typhus, and some other, might give origin to the former alone, where the patient was not susceptible of both, would appear from some

\* *Vid.* chap. on infection from p. 144 to 155. There are some observations relative to Influenza, in p. 154, which had not occurred to me in writing that article, or they should have had a place there.



instances on record, and which would render it highly probable, that the Exanthemata, if they were similar diseases, ought to do the same. It is well known, that, though the eruptive fever of Small-pox be not a Typhus or of typhoid character, its secondary fever especially in the confluent species often strongly partakes of it. Q. Is this secondary fever ever capable of giving Typhus to a person insusceptible of the Small-pox? A curious fact recorded by Willan, p. 12, Diseases of London, may perhaps solve this question; in speaking of the contagious Malignant Fever of a particular month, he says, “ that seven out of eleven cases of this  
“ disease were in one family. The infection  
“ seemed to arise from an infant who died  
“ about the sixth day of the confluent  
“ Small-pox, attended with an eruption of  
“ purple spots, and a hæmorrhagy. Every  
“ individual of the family was taken ill of  
“ the fever on the seventh night after the  
“ death of the child. In the children and  
“ servants the fever was brought to a crisis  
“ on the fourth day. The father and mo-  
“ ther, who had aphthous ulcerations in the  
“ fauces, began to recover on the fourteenth  
“ day



“ day of the disease.”\* It cannot be an easy matter to procure testimonies to the same effect from those diseases, which undoubtedly derive their contagion from combination with Typhus, such as Dysentery or Puerperal Fever, because the persons in general exposed to either, are usually as susceptible of the compound as of the single disease; yet we may remember that passage I have quoted from Zimmerman, in p. 137 of this work, in which he says, “ *When Hospitals are filled with Dysenteric people, some of the assistants are attacked only with the Dysentery, and others with the jail, or Hospital Fever*”. A similar observation is quoted from Wilson in p. 139.— With respect to Puerperal fever, a combina-

\* To this account however, it is right to subjoin an observation which Willan makes in a note on this history: “ It was probably incorrect, he says, to refer the disease thus produced to the head of contagious Malignant Fever. I have since, he adds, repeatedly seen a febrile complaint attended with aphthæ, originate from intercourse with patients labouring under the purple Small-pox, and from other putrescent effluvia.” Willan does not here say, that such a *febrile complaint* resembled the seven cases thus set down, and the candour of his remark must not induce us to conclude he was mistaken in denominating the disease as he has done.

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tion of Peritonitis and Typhus, and of which Puerperal women alone are susceptible, it would appear from an assertion of Dr. Walsh's in the Practical Observations before referred to, that it communicated Typhus to those, who were insusceptible of the compound disease: in p. 14, he says, " In the Royal  
 " Infirmary of Edinburgh, the similarity of  
 " the cause was shewn by its effects, as two  
 " of the attendants on the Puerperal women  
 " were seized with the common Synochus—  
 " and that this was not a solitary instance,  
 " as in other places the same thing had like-  
 " wise been observed."\* These circum-  
 stances taken together, should at least render it probable, that in case Dr. Blackburne's supposition was true, the Exanthemata ought frequently communicate Typhus to such as were insusceptible of the compound disease, since we see that it may occur in those which really consist of such a combination.

I have now concluded all those observations I had to offer on Dysentery, and its

\* Candour however obliges me to declare, that I have heard Dr. Hamilton, Professor of Midwifery in Edinburgh, assert that such a fact never occurred in the Lying-in-wards of the Infirmary.



analogous diseases.\* I am conscious of having left the subject still imperfect, “nam  
“multum adhuc restat operis;” but I am sure of the indulgence of those, who know how extensive the field is, and am confident of their thanks, if it shall appear that I have cultivated *any* part of it with success. That mankind may reap fruits of it an hundred-fold, is the sincere wish of the

AUTHOR.

\* The same cause, which induced me to omit the first part of this chapter, as promised in the Prospectus, will not now permit me to enter on the general subject of contagion and infection, as was proposed in the same. I must therefore for the present suppress such observations as I had to submit respecting them, and the laws of specific actions.

FINIS.



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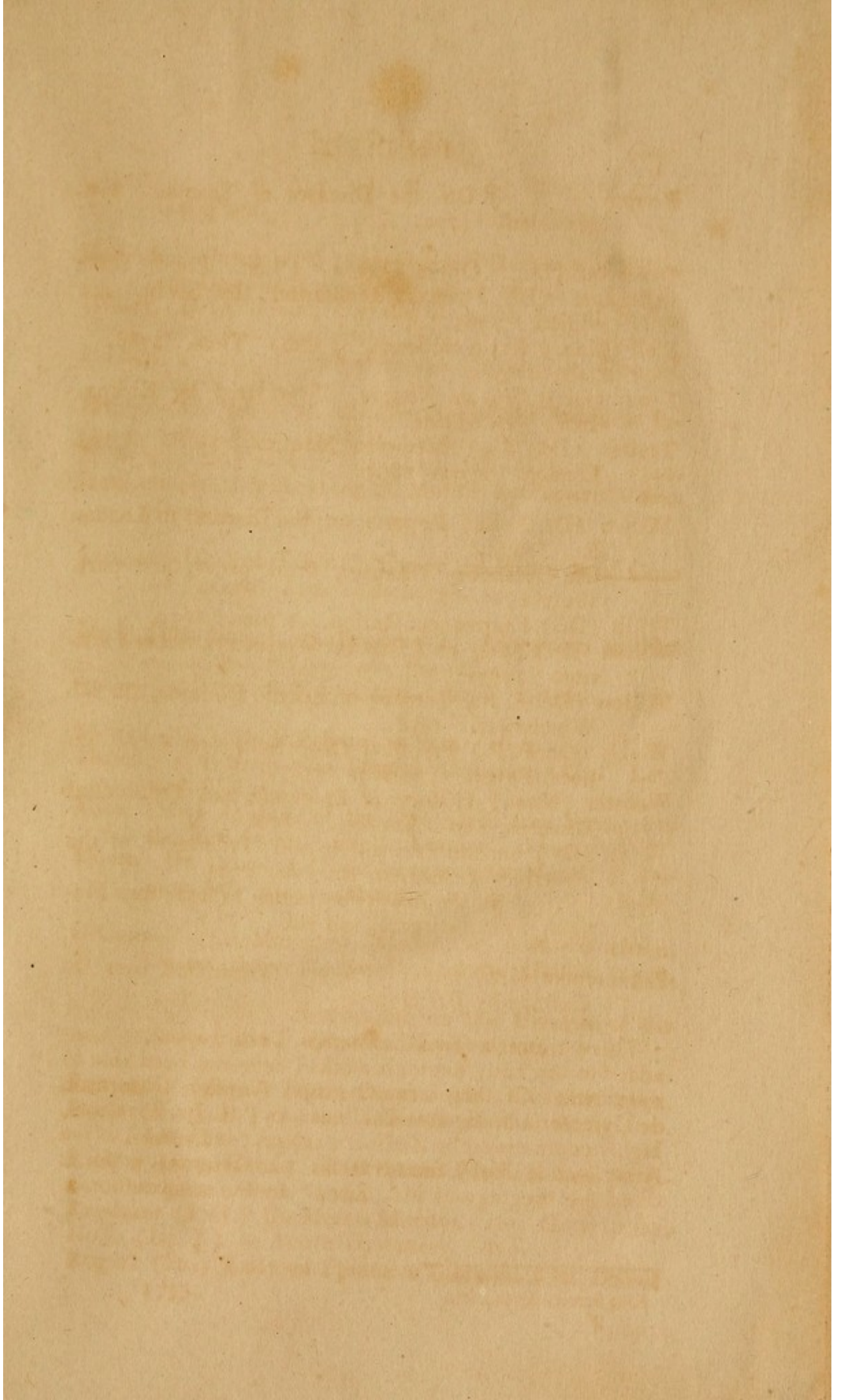


## I N D E X.

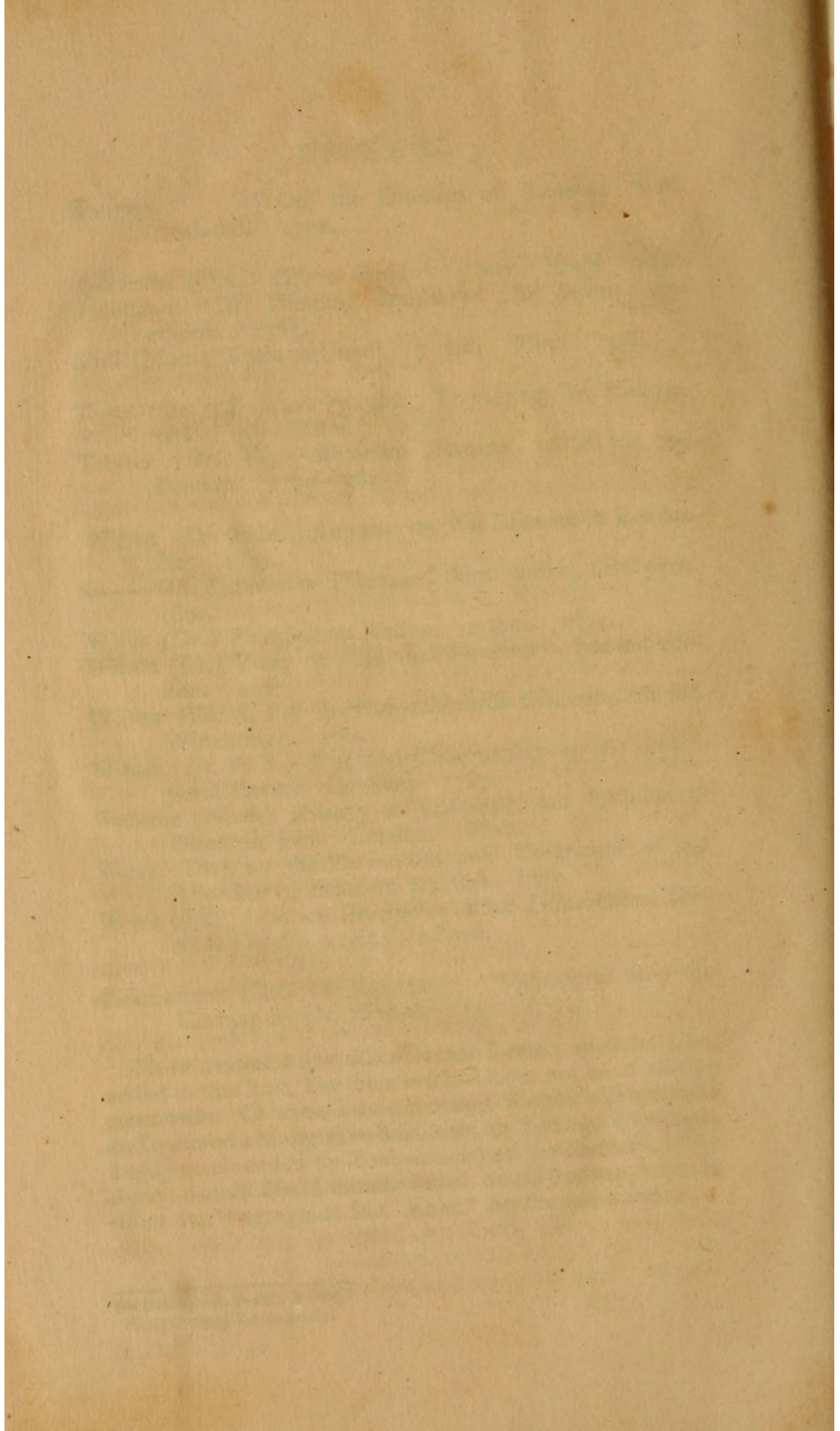
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There are yet a few other names I could wish to have added to this List, but their works I have not been able to meet with. Of these I may mention Wepfer's *Dissertatio de Dysenteriâ Malignâ*.—Baldinger, (a Prussian Physician, highly commended by Zimmerman) on the Diseases of the Army.—And Mr. Conrad Rhan on Dysentery; he is stiled the “egregious Mr. Rhan” by the same author.





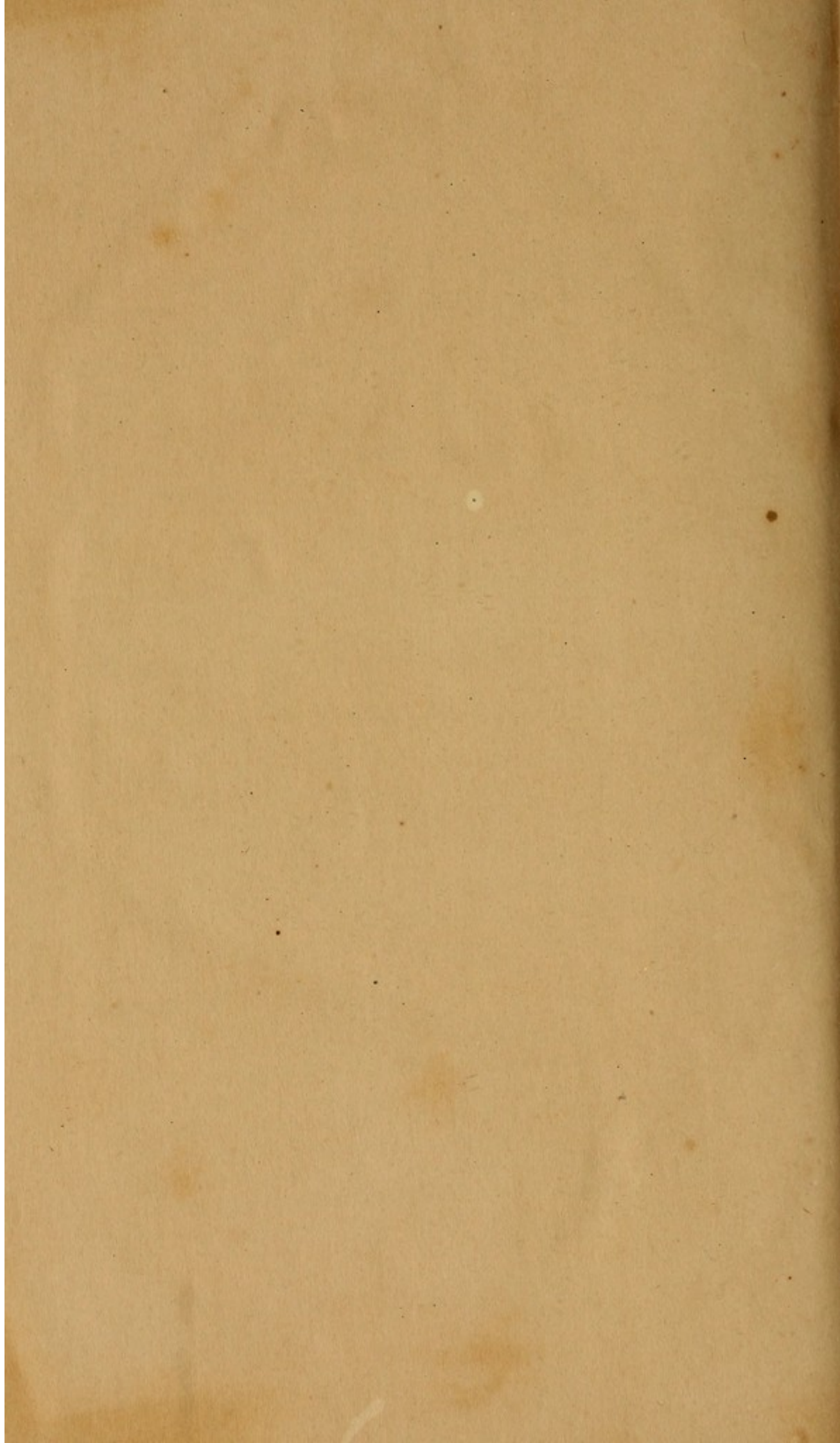














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