

Observations on the history and treatment of dysentery and its combinations ; with an examination of their claims to a contagious character, and an inquiry into the source of contagion in its analogous diseases, angina, erysipelas, hospital ganrene, and puerperal fever / by William Harty.

Contributors

Harty, William, 1781-1854.
Osborn, Samuel, 1848-1936
St. Thomas's Hospital. Medical School. Library
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Publication/Creation

Dublin : Hodges and Smith ; ... London : John Churchill ; Edinburgh : Maclachlan, Stewart, and Co., 1847.

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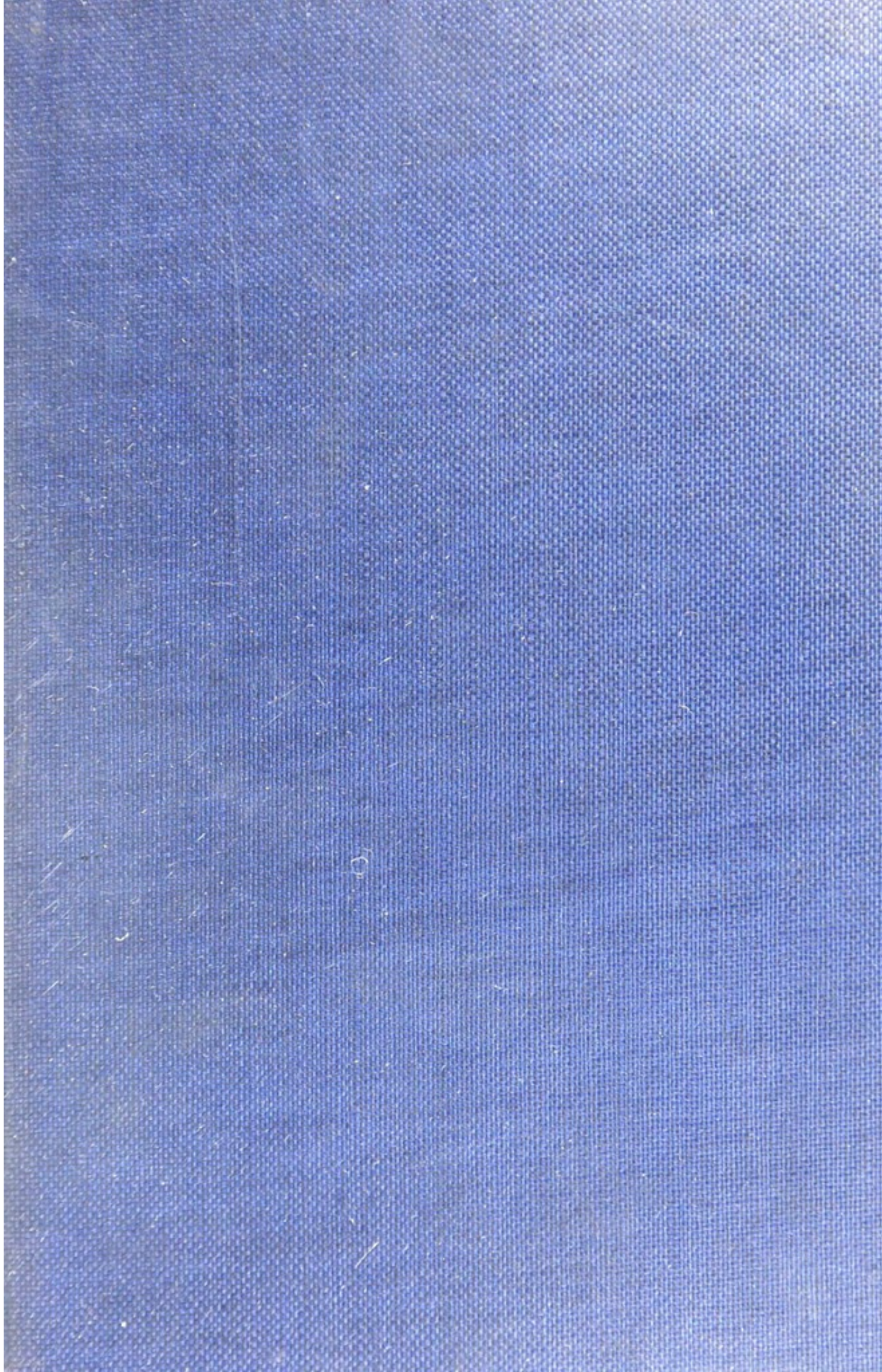
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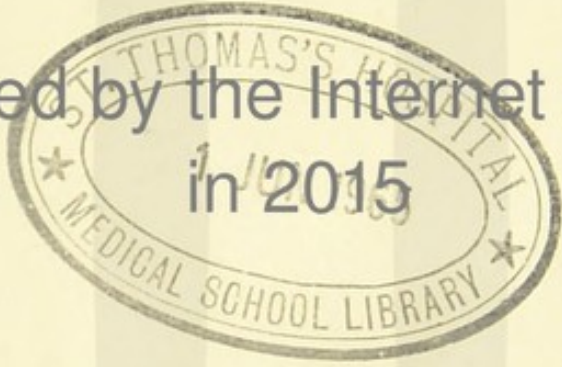
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OBSERVATIONS

DYSENTERY AND ITS COMPLICATIONS

OBSERVATIONS
ON
DYSENTERY AND ITS COMBINATIONS.

Reviews of the First Edition

OF

“DYSENTERY AND ITS COMBINATIONS.”

“This work we find no difficulty in recommending to our readers: it contains, unquestionably, a larger mass of evidence than is any where else to be found of the various species of this formidable disease, and furnishes the most valuable digest of all that has been written on the subject. Throughout, the Author shews much industry in research, and acuteness in judgment.”—*Medical and Physical Journal for December, 1805.*

“In this attempt to establish something like a settled doctrine on the subject, we must allow the Author to have succeeded in no mean degree, and much instruction, we are persuaded, may be derived from a careful perusal of his work.”—*Medical and Chirurgical Review for November, 1805.*

“Among the publications on Dysentery, that of Dr. William Harty is to be particularly distinguished.”—*Critical Review of Medicine, by Dr. Sprengel, Professor at Halle.*

24. C. 27.

OBSERVATIONS
 ON THE
 HISTORY AND TREATMENT
 OF
 DYSENTERY
 AND ITS COMBINATIONS;

WITH AN EXAMINATION
 OF THEIR CLAIMS TO A CONTAGIOUS CHARACTER,
 AND AN INQUIRY INTO THE SOURCE OF CONTAGION IN ITS ANALOGOUS
 DISEASES,

ANGINA, ERYSIPELAS, HOSPITAL GANGRENE,
 AND PUERPERAL FEVER.

BY
 WILLIAM HARTY, M. D.,

PHYSICIAN TO THE KING'S HOSPITAL, AND TO THE PRISONS OF DUBLIN;
 AUTHOR OF THE "HISTORIC SKETCH OF THE CAUSES, EXTENT, AND MORTALITY OF FEVER EPIDEMIC
 IN IRELAND, IN 1741 AND 1817."

Second Edition.

DUBLIN:
 HODGES AND SMITH, GRAFTON-STREET.

LONDON: JOHN CHURCHILL.
 EDINBURGH: MACLACHLAN, STEWART, AND CO.

1847.

Comps



HISTORY AND TREATMENT
OF
DIPHTHERIA

BY
M. H. GILL, M.D.
OF THE UNIVERSITY OF DUBLIN

More than forty years have now elapsed since the
first edition of this work met the public eye. It was
published in 1845, and since that time I had occasion
to refer to it in the course of my professional labours.

DUBLIN:
PRINTED AT THE UNIVERSITY PRESS,
BY M. H. GILL.

mentioned in some of the preceding editions of
this work, and which have since been
revised and enlarged. It is now
the author's wish to present to the public
this work in a form which is more
correct and more complete than
any other that has appeared. It is
of a different nature. A complete
re-arrangement has been made
of the whole, and the subject
is now treated in a more
systematic and extended manner
than in any of the preceding
editions.

PREFACE

TO

THE SECOND EDITION.

MORE than forty years have now elapsed since the first edition of this work met the public eye. It was published in 1805, shortly after I had graduated as M. B. in the University of Dublin, and ere I had yet completed my twenty-fifth year. These facts are mentioned as some apology for the errors and imperfections with which, under such circumstances, that publication necessarily abounded, though they did not prevent it from experiencing, both in Great Britain and on the Continent, a reception far more favourable than the presumption and inexperience of its author merited. A consciousness of these imperfections, a conviction of the importance of the subject, and a feeling of regret that, respecting the disease in question, opinions erroneous, in my judgment, should, by some, be still entertained, have in-

duced me, after this long lapse of time, to revise the work itself, to reconsider the propositions it maintains, and to refer again to the authorities by which these propositions were originally supported, as well as to others not then known to or accessible by the author. This task I have endeavoured to perform with a mind as little influenced by previous impressions as was compatible with human frailty, and have now at least brought to the undertaking judgment more mature, and experience more extensive than the writer of the original work could then pretend to.

The present publication, though deviating but little from the plan of its predecessor, will yet be found very different in its structure and composition, much new matter having been added, and much of the old material either altered or removed. The new matter has very generally been introduced in the form of notes illustrative of the original text ; and that text has itself been so far modified as to constitute a new work, embracing more or less, in conjunction with the notes, every debatable question connected with, or involved in the history of the disease.

To this revised Edition it had been my intention

to have added an Appendix containing some of those singular and interesting cases of disease, which every physician of any standing and experience must from time to time have encountered, and which, but for this opportunity, might, I feared, ere long descend with me to the grave. Impaired health, arising in part from the labour required for the due preparation of this Edition, has, in conjunction with other circumstances, compelled me to abandon this intention, in the hope, however, of carrying it into execution at some future day, should God grant me a longer tenure of an uncertain life. I had also contemplated annexing to this work a statistical report of the mortality in the several prisons of Dublin during the long period of my connexion therewith, and a comparison thereof with the mortality in the other prisons of Ireland; but the difficulty and delay in procuring some returns from the latter have thrown such obstacles in my way as have marred my present design, and compelled me to reserve these my last bequests for another and more favourable opportunity.

Such as it is, therefore, this work is now given to the public during a season remarkable for the unusual prevalence (in point of time and extent) both of dys-

entery and cholera, and at a period, and under circumstances of awful import to the inhabitants of this island, whether rich or poor. The two great plagues of Ireland have ever been fever and dysentery ; sometimes raging apart, but too generally combined in their ravages : in former days they have more than decimated the English armies which, under the rule of Elizabeth, Cromwell, and William, devastated the country; and they never failed to follow any unusual vicissitudes of the seasons, if attended by bad or deficient harvests, more especially if the food thence derived was rendered innutritious or unwholesome. Such was the case, to a great and remarkable extent, in 1741 and 1817; and though no two things could be more different from each other than the seasons of these years, they yet agreed in the ultimate result of deteriorating or diminishing the products of the earth: in the former case, dysentery preceded the fever; in the latter, it accompanied its decline. This subject, from its vast importance, I have already treated at well-merited length in my work on the Contagious Epidemic Fever of Ireland, and have therein drawn a parallel between the two epidemics, which may be studied with some advantage at a moment when we cannot but anticipate a similar visitation, in de-

spite of every human effort which is being made, or may be made, to prevent its occurrence. I have presumed thus to refer to my own work, because judges far more competent and impartial than myself have declared that "it is a work extremely valuable, and of great merit, and one which will, in *future* times, become of great utility," containing, as it does, "a full review of the causes, medical and statistical, productive of epidemic fever in Ireland(*a*).” When giving it to the public, I myself ventured to say, that, "should an epidemic of a similar character unfortunately revisit these islands, I had no doubt that the work, and more especially the documents it contained, would be perused with feelings of deep and general interest, as it would then supply a guide, the want of which was severely felt during the progress of the late calamity."

A similar visitation of famine and of pestilence

(*a*) London Medical and Physical Journal for November, 1820; and also Edinburgh Medical and Surgical Journal for October, 1821. Dr. Thomas Hancock too, in his *Researches into the Laws of Pestilence*, has been pleased to say (p. 342): "I cannot omit this opportunity of bearing testimony to the valuable work of Dr. Harty on Epidemic Fever in Ireland. It contains such a multitude of facts illustrative of the principles I am supporting, that I should scarcely know where to begin with the selection; I must therefore refer to the Historical Sketch itself."

impends over our heads: the latter, indeed, has already begun to shew itself, and that, too, in localities more or less exempt from famine(*a*). What then may we not apprehend throughout the country

(*a*) I here specially allude to the fatal epidemic of contagious dysentery which has prevailed for some weeks past, and still prevails, both among males and females, in the South Dublin Workhouse, containing about 2000 inmates. The following are a few extracts from the official report of its medical officers, made in the last week of November, 1846, viz. : “ The total number of patients under treatment in the sick wards, including the boys’ infirmary, lunatic department, children’s house and nursery, on the 22nd day of November, 1846, is 794. Of these 263 are afflicted with acute dysentery, sixty-one labour under measles, twenty-two suffer from bad forms of influenza, seven are afflicted with hooping-cough. Inflammation of the lungs, of a very fatal character, prevails extensively amongst the children, and many of the aged and infirm are suffering from severe chest complaints. The medical officers have given their most serious attention to the probable causes of the rapid increase of dysentery in the institution, and the means most likely to prevent its further progress ; they cannot attribute the disease to the use of Indian meal, because numerous cases of dysentery appeared in the house several weeks before that article of diet was introduced ; many paupers have also laboured under the disease on admission, and numbers of the inmates have been attacked who never used the stirabout. The provisions supplied to the workhouse are of good quality, and the purity of the water has been ensured by the early cleansing of the reservoir at the outbreak of the disease. The malady commenced in an epidemic form, but it has of late assumed a contagious character, which is much favoured by the crowded state of the *infirm* wards. To prevent the further increase of the complaint, it is strongly recommended to keep one ward con-

after the winter and spring shall have aggravated the existing epidemic influence, notwithstanding the great and peculiar advantages we possess on this occasion as compared with preceding visitations. Nothing, truly, can be more remarkable than the contrast. The sufferers from fever and famine in 1741, amid a population little exceeding two millions, were left to seek and find relief from the private benevolence of the wealthier members of the Irish community *exclusively*, though the Lord Lieutenant of the day brought the subject before

stantly vacant, and to adopt the rotation system of ventilation and cleansing found so effectual at the Lying-in Hospital and elsewhere as a means of checking the spread of contagious disease. Without this arrangement all other measures will prove insufficient. The hospitals are already full, and therefore the only alternative is to suspend for a limited period admissions into the female infirm department. The amount of disease shewn by the above returns renders the duties of the medical officers so onerous that they feel themselves compelled to apply for assistance. They therefore earnestly request the appointment of a temporary medical assistant until the pressure of sickness abates. (Signed) PETER SHANNON, ROBERT MAYNE."

If we may credit the public papers, a similar epidemic prevails in other workhouses, as well as fever in other localities. Such was the commencement of the great epidemic fever of 1741, dysentery being its precursor, which, according to O'Connell (in his *Obs. Med.*), "*sensim per totum autumnum augescens magnam incredibilemque per totum insequentem hyemem stragem edidit.*"

the Irish Parliament in a speech from the throne. The fever and famine of 1800, occurring during the agitating enactment of the Union, were abandoned to the same agency, without any aid from English benevolence or from the State, except by bounties on the importation of corn, by an Act suspending distillation from grain, and by a *sage*, yet well-meant resolution of the Lords and Commons, pledging the members of both Houses to conduct their households after a special economic manner(*a*). The formidable epidemic and famine of 1817 (aggravated as it was by a great increase in the population, and

(*a*) These resolutions are not uninteresting as illustrative of Irish legislative wisdom: they are as follow:

“*Sabbati*, 15^o die Martii, 1800.

“*Resolved, nem. con.*—That in consequence of the high [*price?*] and deficient supply of wheat and other grain, it is expedient to adopt such measures as may be practicable for diminishing the consumption thereof during the continuance of the present pressure, and for introducing the use of such articles as may conveniently be substituted in the place thereof.

“*Resolved, nem. con.*—That the agreement hereunto subjoined be engrossed and laid upon the table of this House, in order to be subscribed by any member who shall think fit to subscribe the same (*viz*):

“ ‘In consequence of the high price of grain, and the evils arising therefrom, we, the undersigned, agree that until the 1st day of September next we will not consume nor permit to be consumed in any week within our respective families more wheaten bread than in the proportion of four pounds six ounces for each

by an unusual deficiency of employment, consequent on the recent peace, and the temporary shock thence sustained by agriculture), depended mainly for mitigation, during the three years of their prevalence, on the same private benevolence, being in no other respect essentially assisted through Parliamentary or Government interference than by inefficient Reports from Select Committees, by the establishment of inoperative Boards of Health, and *specially* by the *gratuitous* support of all the fever hospitals of Dublin, and by small pecuniary grants to those of other localities ;—*whereas*, on the present visitation of famine and a mere prospect of pestilence, with a population exceeding eight millions, private individuals, public communities, and the State, anticipating the natural and probable results, have (whether after a judicious manner or not is another question), conjointly interfered for the prevention or mitigation of

of the individuals of whom our said families may be composed : that we will not use or permit to be used within our families any bread which has not been baked for twenty-four hours previous to the use thereof : and also that during the same period we will discontinue and cause to be discontinued within our said families all pastry made of wheaten flour : and we likewise agree that during the same period we will to the utmost of our power endeavour to economise and retrench the quantity of oats now made use of by our horses.' ” Then follows, on the 26th *die Martii*, “ a message from the Lords agreeing thereto.”

those evils, and by extraordinary efforts are endeavouring to furnish employment and food to idle and starving thousands. These are among the prominent advantages of our present position as compared with those which have preceded it: and yet, during former visitations, under all the aggravation of comparative neglect from the ruling powers, the Irish poor uniformly manifested that calm resignation under suffering, and that gratitude for kindnesses received, by which their natural character had ever been distinguished, and evinced besides respect for property and submission to the law; *whereas*, on the present occasion, notwithstanding every effort made for affording relief, the friends of good order have as yet but little reason to boast of the result, and may have still less ere long, when, despite the patient endurance of the really distressed, the lawless tendencies of the turbulent and disaffected shall have again, as of old, engendered a whole host of "rogues, rapparees, and tories," only to be put down by coercive legislation and a strong arm. Whence this difference, or what the cause of this change in the national character, it is not for me here to inquire. The real cause will but too readily be surmised.

Another and a peculiar advantage, too, we happily now possess beyond our predecessors, that though the one crop, on which the great mass of the population depended for support, has been destroyed and not simply *damaged*, they will, when employment shall have furnished them with the means of purchase, be enabled to procure food of a wholesome and nutritious kind, however deficient it may be in quantity, and will thereby be saved from the great aggravation every epidemic disease derives from food of an opposite quality.—Should famine and pestilence, notwithstanding, prevail amongst us, God grant our rulers wisdom to meet, and our people patience to bear, the double evil, wherewith for our iniquities it may be His good pleasure to afflict us.

Dec. 21, 1846.

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OBSERVATIONS,

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INTRODUCTION.

THE chief object, aimed at in the first edition of this Essay, was to draw the attention of the Profession to a *species of contagion*, which, though incidentally noticed by some writers, had not then engaged that share of general consideration it well merited. This species I denominated Compound Contagion, to distinguish it from that which propagates a single disease ; its most striking and essential property being the power of communicating, to one and the same individual, two distinct, and otherwise independent diseases, which then appear as one disease, but which, *when separate*, are found uniformly to differ in these two respects, viz., that one is a febrile affection of the *whole* system ; the other an affection purely local, and usually of some mucous surface : the former always contagious ; the latter never.

My more immediate object was to demonstrate the influence of this species of contagion in the pro-

duction of a most dangerous form of disease, in the case of dysentery(*a*).

The state of public opinion relative to this disease, whether as regards its history, causes, or treatment, cannot be unknown to any medical reader: amongst the best writers, indeed, we find so little harmony of sentiment on these several points, that we are almost warranted in saying of them, as Pringle did of Sydenham and of Willis, that in their description of dysentery, they scarcely agree in any one article, except the name of the disease they are describing.

(*a*) Roederer, in the introduction to his *Treatise de Morbo Mucoso*, has, with some distinctness, referred to combinations of this character, as applicable to his Epidemic Dysentery. His words are remarkable, and not unworthy of transcription; they are as follows, viz.: “Differt quidem epidemicum a contagioso, duplicemque morborum classem format: altera solius epidemici efficaciam dimanat, citra commercii cum ægrotis aut contagii concursum: altera sine epidemici cujusdam connubio soli contagio debetur nec impetit, nisi quos tetigit miasma. Mediæ indolis sunt morbi, ad quorum generationem, præter epidemicum aeris, requiritur vis contagii specifici: licet et solùm contagium receptum morbo suscitando nonnunquam sufficiat: multis tamen morbis epidemicis commune est, ut nati fere sporadice grassentur: adoliscetes verò cum ægrotantium effluviis verum contagium diffundant. Ex epidemici itaque et contagiosi connubio duplicem naturam induunt, auctâ ferocitate plures simul aggrediuntur atque ingentem stragem interdum edunt.”—“Porro observatur epidemici morbi in alium, prioris naturæ plus minus participem, transitus. Celebris hoc nomine est epidemicorum facile princeps, morbus dysentericus, qui victimarum sub brevi imperio strage non satis exsatiatus, parente vitiosorem prolem progignit.”

Having at the commencement of my medical career been liable to frequent attacks of dysentery, I was thereby induced to devote my earliest attention to its consideration. The immediate result of my labours was rather embarrassing, and I could not but augur inauspiciously of a pursuit, which on one and the same subject presented such a contrariety of opinions amongst writers deservedly esteemed for experience, research, and judgment. Determined either on reconciling such conflicting testimonies with each other, or abandoning a profession which could admit of them, I anxiously prosecuted the inquiry, and, after a minute investigation of the subject, was led to conclude, with Cullen, that there is *one species* only of the disease (very different, however, from that which he supposed it to be(*a*)), and that there are also several important combinations of this species, to all of which (as well as to other affections) the name of dysentery has been indiscriminately applied.

The following positions present a summary view of the conclusions thus deduced ; these are :

(*a*) When commencing this investigation, my mind was strongly impressed by the authority of this celebrated nosologist, who had pronounced of dysentery, "Non nisi unicam speciem novi," and in the same breath had defined that species to be "pyrexia," and that pyrexia contagious; nor was the influence of that authority lessened, till I found it impossible to reconcile the histories of the disease with the first article of that definition, and the opinions of authors with the second.

- 1st. That *the genuine and simple dysentery is unattended by idiopathic fever, and is never of itself contagious.*
- 2nd. That *every other form of the disease, when epidemic, is a combination of the simple dysentery either with intermittent, remittent, or continued fever; and*
- 3rd. That *the combination with continued fever alone is contagious.*

On the first promulgation of these opinions in 1805, I had supposed them entitled to the claim, or liable to the charge of novelty. I must, however, now admit, that, whether well or ill-founded, they are at least *not new*. The subjoined passage from Hoffman (which did not meet my eye for some years after the first edition was published), must satisfy every one who reads it, that this able and intelligent physician, though he did not maintain these distinctions in his *General History of Dysentery*, was yet fully cognizant of them. Any deduction this admission may make from the supposed novelty of these opinions is amply compensated by the additional weight which such authority must attach to them(*a*).

(*a*) The passage here referred to will be found in tom. iv. pars iii. cap. vii. p. 484. Hoffman there describes a dysentery epidemic in the year 1726, and concludes his general account with these remarkable words: "Adhæc tres potissimum mali classes vel gradus adnotare licebat: *Prior* admodum benigna nec nisi vagis horripilationibus et caloribus stipata erat, ubi ægrotantes non

The late much respected Dr. James Johnson, in the third edition of his valuable work on "Tropical Influence" (p. 213), would appear to have greatly misconceived the nature of the opinions I maintain. When reviewing Bamfield's work on dysentery, he states that this author had never seen anything that could lead him to suspect dysentery to be contagious. "This," says Dr. Johnson, "entirely coincides with my own observations, and not with mine merely, but with those of every modern practitioner with whom I am acquainted. The opinion of Cullen, Pringle, Hunter, *Harty*, and others upon this point, must, *therefore, be set aside*. Either the dysenteries of *their* day was a different disease from what it now is, or these eminent individuals were betrayed by their preconceived ideas

semper in lectis decumbere debuerunt: *Alter*a, minus maligna, comitem habebat febrem, vel quartanam eamque continuam, vel semitertianam: *Tertia* verò maligna cum acutâ et veluti inflammatoriâ febre conjuncta, efferatâ symptomatum ferocitate affligebat, magnumque in alios propagabat *contagium*, et tantùm *non semper* brevi lethaliq̄ue solvebatur eventu." Again (sec. 34) he observes, "Quum verò illa plerumque sociam habeat febrem acutam malignam, *ipsâ Dysenteriâ periculosiorem*, hæc quidem tractationem sollicitam ex indole suâ postulat, ne ægrotum jugulet; indoles autem frequentissimè est, *maligna petechizans*," &c., &c. This division of dysentery into three "classes" is given by Hoffman as the result, not of theoretic speculation, but of personal and practical observations made during the prevalence of a particular epidemic.

into a mistake. It is surely of little present importance, which of these alternatives may be the truth; for opinions must now-a-days be decided, not by authority, but by the touchstone of facts carefully observed and faithfully recorded."—That dysentery *of itself*, and in some of its combinations is *not* contagious, has ever been my doctrine: this surely is an opinion that Dr. Johnson would not have wished to "set aside;"—but that, in combinations with continued fever, it *becomes* contagious, was also my opinion(*a*). Many modern practi-

(*a*) The following extract, however, from the October Number for 1838 of the Medico-Chirurgical Review, then edited by the same author (now, alas! no more), will shew that the doctrine I maintain is becoming more popular, and that it had been embraced by the editor himself, who, in p. 542, observes thus: "We doubt much whether dysentery is ever contagious *per se*, but only when it is an attendant on a low or typhoid fever, when the latter may prove infectious, and carry its intestinal character from person to person." Thus also my much lamented friend, the late Dr. Bateman, in an able report on the state of health and disease in London, in the Annual Medical Register for the year 1808, asks (p. 350): "To what does dysentery owe the malignancy and contagion connected with it in modern camps, and in the old city of London?" To its combinations, he answers, with the remittent and continued fevers which have been observed to concur with it. "We know," he says, "that a contagious fever, with which a local disease in some particular organ is accidentally conjoined, will often transfer that local disease, with its other symptoms, as it passes from one individual to another; in this way pulmonary inflammation sometimes spreads by infection, when it has been called pneumonia

tioners never, perhaps, witnessed the disease in that combination, because now comparatively rare, though formerly, and especially during warfare, very common in Europe. Ireland felt its influence during the prevalence of the late epidemic fever in 1817, as did the Continent during the disastrous campaigns of the late war in Spain and Germany.

Dysentery is a disease, an intimate knowledge of which, in its various grades and characters, can now seldom be acquired except by personal service in our fleets and armies, or by long residence in hot climates; in the former I never served, and the latter I never visited. But as there is yet another source of information, and of *experience*, too, which, however inadequate it may be deemed as a substitute for such extensive personal observation, is undoubtedly its best auxiliary, I have endeavoured to establish some claim to an *acquaintance* with the *experience* of others, by a neglect or contempt of which guidance we are often led astray, and have brought discredit on the Profession. Such conduct is particularly reprehensible as respects diseases, which prevail epidemically and are limited to no clime; which, appearing under every possible variety of circumstances, consequently present ever-varying phenomena. Of these diseases we can never acquire an

typhodes; and thus also erysipelas has been communicated in the wards of an ill-regulated hospital." That which I have denominated "Compound Contagion," has latterly and by others been called "Contingent Contagion."

accurate or extensive knowledge by personal observation *alone*: we must, as regards them, be assisted by the experience of others, if the attainment of truth be our object(*a*).

(*a*) The judicious and practical Lind, in his paper on Fevers and Infection, affirms this our view, when he says that "We must add to our own experience that of men who lived before us, or who practise in different places. By the help of this acquired knowledge, the mind becomes stored with experimental truths and observations; and a physician thus qualified, will be enabled to take a comprehensive view of nature and of diseases under their various shapes, and as influenced by climate, seasons, constitution, and medicines," &c.

The same sentiments are reiterated in forcible terms by the indefatigable Dr. Copland, in an able article on the various combinations and complications of dysentery, in his valuable Dictionary of Practical Medicine, a work of Herculean labour and judicious research, which *all* concur in hoping he may be enabled to finish without injury to his mental or physical powers. In a note to p. 701, of unusual length, he says: "It will be instructive to review the characters of those epidemics of which we have authentic accounts, as valuable illustrations of the nature of this destructive malady. It will appear that our knowledge of the disease is but little in advance of what it was two centuries ago, and that even the most recent writers are distinguished rather by confined or exclusive ideas as to its nature and treatment than by comprehensive views of its forms and manifestations in connexion with the various combinations of causes producing it, and the diversified circumstances in which it prevails." Dr. Copland, who had ample experience of this disease, acquired under every variety of clime, considers dysentery neither so simple in its nature, nor so unvarying in its seat and forms, as some recent writers in this country have stated. "That writer," he adds, "but imperfectly performs his duty, who in

Of this class dysentery furnishes a special example, which, though it has engaged the attention of physicians in every age, continues still to demand their serious consideration. The many points of essential difference perceptible in our best histories of the disease (arising as well from the several varieties of which it is susceptible, as from confounding it with other diseases to which it bears some resemblance), the disputes still prevalent relative to its contagious character, and the great diversity of treatment recommended for its removal, are more than sufficient to warrant the assertion that its true nature is not, as yet, generally understood, and to justify a further discussion of the subject.

Though it forms no essential part of my plan to attempt any regular history of dysentery (my great object being to establish the positions already laid down, and thereby to distinguish the different combinations of which the disease is susceptible), yet the reader who will condescend to peruse the details necessary for these purposes, cannot fail thence to derive, and from the most authentic sources, correct and accurate notions of the peculiar characteristics of each combination, and of its appropriate treatment.

giving a history of a most dangerous malady, confines himself to the particular form it has assumed during a few seasons, within the single locality of which he is the centre, and argues that it is always as he has observed it: thereby affirming as true of the genus, what may be hardly true of the species."

Should I succeed in these objects, no man conversant with the history of naval and military enterprises will dispute their importance; but, independently of this consideration, they will be attended with another and a great advantage: they will enable us to reconcile many otherwise discordant testimonies, and that "opposition of opinions so disreputable to the science and profession of medicine," and thereby satisfy us that we may place a reasonable, though qualified, confidence even in the clashing statements of our medical brethren.

CHAPTER I.

SIMPLE DYSENTERY.

SYDENHAM, though he had for three years only seen dysentery epidemic, yet perceived such variety in the appearances it presented, as to lead him to the conjecture, that there might be as many different species of that disease as of the small-pox(*a*). The

(*a*) Sydenham's words are: "Fieri quidem potest ut variæ enascantur dysenteriarum species ut sunt variolarum et epidemicorum aliorum, diversis constitutionibus propriæ et quæ proinde medendi methodum in aliquibus diversam sibi vindicent." He makes this remark in reference to the then notorious "endemic dysentery of the Irish," of which he professes his ignorance, and with respect to which I must confess the regret I feel that we possess such scanty records. My regret in this respect is the greater, because it would appear from Dr. Copland's valuable bibliographical references, that a work in quarto on the diseases of the Irish, et "speciatim de Dysenteriâ Hibernicâ," was published by a Dr. Jones in 1698, of which I had never previously heard, and of which, strange to say, no copy is to be found in any public library of Dublin. I have in vain sought for one, as Jones's work would probably furnish some account of the fatal fever and dysentery which had thinned the ranks of the besiegers and besieged at Derry, and had nearly destroyed Schomberg's army near Dundalk, a few years previously to the publication of his work. Dr. O'Brien (a learned physician of our city) makes no reference to the existence of such a work in his Observations on the Dysentery of Ireland; neither was it known

general truth of this conjecture I shall endeavour to establish, without meaning, however, that the several forms of dysentery are to be considered as so many distinct species, but that as varieties they are as essentially and often more widely different than the two very dissimilar species of small-pox. On the contrary, Pringle, Clarke, and Moseley agree in declaring the disease to be everywhere essentially the same, though in their histories and general opinions respecting it, they differ very widely indeed from each other. "The multiplied distinctions made by authors," says Moseley, "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 is united with it.*" Admitting this to be true, are all distinctions therefore unnecessary? When we consider what those cases are, "where some other disorder or epidemic is united with the disease," how different in their history, and how dissimilar in their treatment, we must, surely, admit the importance of establishing some distinction between the simple disease and its combinations. If, indeed, there be but one species to which the name of dysentery properly belongs, and to which alone it should be given, it must be incorrect so to denominate any of its varieties, or to Mr. Wilde, when preparing his elaborate Medical Report on the late Census of that country. Indeed, all tell me they had never even heard of the work.

to apply the name indiscriminately to those cases where "some other disorder or epidemic was united with it." The name has been ever thus misapplied, and this the reader will find to be a great source of confusion; and as a ready illustration it is not unworthy of remark, that these very authors themselves, Pringle, Clarke, and Moseley, appear alike ignorant, that the disease they each described under the common name of dysentery, was not only not a simple disease, but that it was in each instance differently compounded.

Before I proceed further, it will be requisite to point out that *unica species*, to which alone the name of dysentery ought to be assigned, and which I have stated to be unattended either by idiopathic fever or contagion. Cullen's definition (*a*), on the contrary, commences by affirming the disease to be attended with pyrexia, and that pyrexia to be contagious. Now, to such a statement, as part of the definition, I must object *in toto*; the disease being frequently unattended by pyrexia, and in many cases where it is so attended, that pyrexia not being contagious. The words conveying such an inference should therefore be excluded from the definition,

(*a*) It runs thus: "*Pyrexia contagiosa: dejectiones frequentes, mucosæ, vel sanguinolentæ, retentis plerumque fœcibus alvinis: tormina, tenesmus.*" Cullen is more guarded in defining catarrh, which he says is a "*pyrexia sæpe contagiosa.*" Had he employed similar language in speaking of dysentery, his definition, though not correct in either instance, would still have been much nearer the truth.

which, in other respects, with, perhaps, one exception(*a*), well characterizes a disease known by severe gripings of a peculiar sensation, and by the

(*a*) This exception refers to the supposed retention of the natural alvine discharges, which, in the form of scybala, are said to be *occasionally* evacuated under the operation of medicine. Dr. Cheyne, in his Report on the Irish Epidemic Dysentery of 1818, says that, "with respect to the discharges from the bowels, the only remark I have to make worthy of attention is, that notwithstanding very diligent inquiry, we could not discover that scybala were passed by any one of the patients." He adds, that had there been scybala, "it seems probable they would have been detected in the bowels during dissection, which was not the case in a single instance."

Frank, however, who had ample experience in the disease, says, that "inter liquidas dejectiones interdum hic fæces figuratæ, *globosæ*, cum remissione symptomatum eximiâ, deponuntur." Bamfield admits that he has seldom or never found scybalæ in the dysenteric stools, and in this Dr. James Johnson agrees with him; yet that they are occasionally present cannot well be disputed, though under what special circumstances, except of neglected bowels, it may be difficult to decide. "It *often* happened," says Monro, "that after the dysentery had continued for some time, the sick complained for a day or two of severe gripes, and then discharged along with the stools little pieces of hardened excrement." Heberden says, that the dysenteric stools are "inodori;" whereas Frank states, that "præter materiam mucoso-puriformem, perpaucam, specifico fere odore fœtentem, perparum exprimatur." Heberden is correct as to the early stage of dysentery; but the fœtor varies with the progress and the form of the disease. In my own case, every paroxysm of tenesmus was uniformly terminated by a flow of urine, more or less, indicative of a simultaneous relaxation of the spasm of both sphincters. The posterior part of the bladder is sometimes distinctly affected by the extension of inflammatory action from the rectum.

great frequency of the stools, which, when the disease is fully formed, never exhibit the natural contents of the intestines, but consist in a scanty discharge of mucus and serum, generally mixed with blood, and are always accompanied by a most distressing tenesmus. "Cunctas dejectiones," says Hoffman, "quæ sunt numerosissimæ, et antecedunt, et comitantur, et sequuntur, crudelissima tormenta, non in una loco fixa, sed per totum intestinorum canalem vago itinere progredientia, et sub quâvis sede sentitur molestissimus omnium quasi viscerum descensus." These symptoms sufficiently distinguish the disease from diarrhœa and cholera, with both of which it has not unfrequently been confounded, and on occasions, too, when their histories plainly indicate very distinct diseases. Willis has furnished a remarkable instance of this nature in his *Dysenteria incruenta*; as does Degner also in his epidemic, the descriptions given by both being accurate delineations of cholera, a disease too strongly characterized by copious stools, continued vomiting, and rapid sinking of the powers of life, to admit of mistake: "Illa, enim," says Hoffman, "ad acutissimos referri meretur morbos, et intra paucos dies, ad summum septimo finiri solet: cum dysenteria, nisi fuerit summè maligna, diutius durare deprehendatur. Neque subinde dysenterico fluori semper junguntur vomitiones, nisi forsan in principio, aut vigente mali impetu, si accesserit ventriculi 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(*a*).”

Dysentery may prevail sporadically or epidemically; and in the latter case, more especially, it generally assumes every variety of form of which the disease is susceptible(*b*). Sporadic cases may occur in all seasons of the year, because arising from causes acting partially, or on particular individuals; whereas the disease, when epidemic, is usually limited in the period of its prevalence, because connected with a certain constitution of the air, dependent on or influenced by particular seasons. The sporadic cases

(*a*) Dr. Copland correctly observes, that “it should not be overlooked in this, more than in other diseases, which possess very specific and distinct features in a majority of cases, that it will frequently assume forms intermediate between cholera on the one hand and diarrhœa on the other; between fever with enteric characters and colic or simple tenesmus; that it may be but very slightly different from some one of these complaints, and that in one or other of its forms, it may be consecutive of, or lapse into, any of the maladies named.”

(*b*) Of this feature in every remarkable epidemic I have already given a memorable instance in the case referred to by Hoffman, and quoted in the note to p. 4. Another special instance may be adduced in the epidemic described by Roederer, as prevalent in 1760: “Febris vel tota,” he says, “memorabilis saltem, desiderabatur, vel non nisi post aliqua preliminariorum chronica accendebatur: pauci mox cum febre ægrotabant. Ex febris autem connubio *triplex morbi indoles fuit notata: etenim vel tantummodo erratica quædam levior, vel vera acuta, vel quod frequentissimum, malignitate plus minus stipata febris jungebatur.*”

generally answer the description given of simple dysentery, being usually devoid of fever or of contagion ; whereas the epidemic cases usually exhibit the symptoms of the simple disease, united with fever of some given type, sometimes contagious, sometimes not. In every epidemic, however, many instances of the simple disease occur, neither preceded nor accompanied by febrile symptoms(*a*).

Sydenham, whose accuracy of observation few will call in question, states that in the majority of cases the gripes attack first, and stools succeed, without being preceded by any fever. Pringle, however, suspects that, though the patients may not have complained, they must have had more or less

(*a*) Vignes (p. 125), when describing the dysentery prevalent in the French army encamped around Vienna in 1809, states that in the spring, "Chez plusieurs on observait un peu de fièvre, tandis que chez la plupart on n'en trouvait pas. Ces derniers malades offraient toujours des symptômes plus modérés que les premiers." As the season advanced and the numbers increased, "les dysenteries commençaient à se compliquer avec le typhus, l'adynamie, l'ataxie ou fièvre nerveuse, qui regnerent épidémiquement dans toute l'armée et elles se répandirent aussi parmi les habitans des pays où l'armée séjourna.—Mais une chose digne de remarque, c'est que chez les habitans du pays, on n'observa la maladie que sous les caractères de la fièvre, car la dysenterie ne montrât pas chez eux." From thence he concludes, that dysenteries are generally owing to vicissitudes of the weather : "et enfin à un genre particulier de fatigues, au défaut de bons vêtemens et à l'irregularité de la nourriture, auxquels les citoyens ne sont que peu exposés, tandis que les troupes sont presque continuellement en butte avec toutes ces causes."

of a febrile affection to precede the disease ; yet he afterwards himself confesses that at times the men were suddenly seized with the flux, upon fatigue and exposure to cold during the dysenteric season. It should be remarked, however, that Pringle and Sydenham generally met the disease in different forms, except in those cases of sudden seizure, to which the former alludes, and respecting which they fully coincide.

These authors, though they usually found fever attendant on the disease, yet testify that it is neither necessarily preceded nor accompanied by it ; they therefore warrant us in rejecting pyrexia from the definition, a decision justified by the direct testimony of other writers. Akenside and Stoll met the disease in its simpler forms, and judged of it accordingly. The opinions of the former are perhaps too decided, for his conclusions are drawn from those epidemics alone, which he himself had an opportunity of observing: to the simple dysentery they fairly apply, but not to any other form of the disease. He asserts that 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, 1751, and 1752, 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 attended by fever ; that he did not observe one in ten, in whom a true fever could be said either to precede 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, et hoc etiam multò magis quam ipse rheumatismus.*”

In quoting these passages I do not mean to assert that symptoms of fever may not occasionally precede or accompany the simple dysentery, but merely that these symptoms never indicate the presence of idiopathic or even symptomatic fever, except when inflammation may run high, and then a state of disease different from pure dysentery is induced. It may, perhaps, be said at times of simple dysentery, as Stoll said of the *Dysenteria rheumatico-biliosa*, “*Febre quidem evidenti carebat, non omni tamen motu febriculoso.*”

When dysentery is stated to be a “*pyrexia contagiosa,*” it is, I presume, meant that the disease is attended by fever, not symptomatic, but peculiar to that affection, and that its contagion also is of as distinct a character. Now, that it is not attended by idiopathic fever, the very nature of that fever, when present, and its total absence on other occasions, plainly shew. Indeed, when we hear authors at one time calling this fever inflammatory, and at other times intermittent, remittent, or putrid, surely

we may more than suspect that the disease cannot have an idiopathic fever, peculiar to itself, as such fevers do not admit of all this variety. That the disease is not necessarily of itself contagious shall hereafter appear; and also that, when it becomes so, the contagion is not peculiar to the disease. Are we not then justified in concluding that the genuine dysentery is unattended by any but symptomatic fever, proportioned to the violence and severity of the disease itself, though at the same time it be capable of combining, under appropriate circumstances, with intermittent, remittent, and continued fevers(*a*).

If, then, the symptoms already enumerated can, independently of "pyrexia contagiosa," constitute a distinct and well-defined disease; and if we are war-

(*a*) Frank, in his Epitome, says of dysentery: "Sæpe cum horrore, interdum cum frigore sat notabili et perhoras adèo extenso incipit." "Sat multos interim apud ægros *modo longè diverso* hæc se habent; *nullus sæpe horror aut calor* morbosus hos a morbi principio divexat; ciborum adeo non in omnibus deletur desiderium; pulsuum *vix manifesta apud alios frequentia* occurrit: quo factum esse censemus quòd plures scriptores febrem inter characteres dysenterix necessarios admittere recusaverint." Frank, however, did not coincide with those writers, as the very first article in his definition of the disease is, that it is "corporis totius febrilis affectio, in intestina, maximè crassa, furiens:"—but I prefer an author's facts to his opinions; and that Frank had extensive opportunities of observing the disease cannot be denied, since he says of himself, "multis sane partimque gravissimis dysenterix epidemiis plures apud populos, octo fere per lustra interfuimus." During these epidemics he met the disease in all its forms, as we shall see hereafter.

ranted in this admission by the experience and testimony of credible witnesses, shall we refuse it the name to which it appears entitled, merely because it does not chance to agree with a scholastic definition? Yet that some have been, perhaps still are, governed by that definition in judging of the disease, an incident which occurred, whilst I was a student, may serve to shew: A patient was admitted into the clinical wards of the infirmary, labouring under every mark of simple dysentery: she had no symptom of fever, and it was not possible to trace the disease to contagion. The clinical professor examined this patient carefully, and in his questions was very particular relative to the "pyrexia contagiosa." Finding, however, that no fever was present, and that there was no ground for believing in the agency of contagion, he felt himself so much embarrassed between the disease and the definition, that at a subsequent lecture he pronounced the disease to be diarrhœa, though he said he would treat it as dysentery.

Before I proceed to consider those other more important forms of dysentery, which arise from a combination of the simple disease with fever of different types, it may not be amiss first to take cognizance of several authentic facts on record, which seem to evince a singular and intimate relationship between dysentery and rheumatism.

CHAPTER II.

AFFINITY BETWEEN DYSENTERY AND RHEUMATISM.

THE opinion that there exists an intimate connexion between these diseases, first suggested by Alexander of Tralles (who called dysentery a rheumatism of the intestines), has in modern times been adopted by Akenside, Richter, Stoll, and others. Independently of the authority of these writers in favour of this doctrine, we find in other authors a few scattered facts, which must carry the greater weight, as they are recorded by individuals unbiassed by any theory on the subject. It shall be my business to furnish the proofs adduced in support of the doctrine, and to offer such explanations thereof as facts may warrant.

Akenside states, that he has frequently observed persons, after they had been freed from dysentery, seized with pains like those of chronic rheumatism, without any preceding rigor, or other signs of fever: and that of such facts he is in possession of more instances than he would wish to adduce; that sometimes he has met men labouring under dysentery and rheumatism, whom he delivered from both by the same means; and that sometimes the gripings of dysentery are conjoined with grievous pains of the whole

body, and particularly of such parts as are usually affected by rheumatism. And not only has rheumatism supervened on dysentery, but the reverse has also occurred. He has 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 to induce tenesmus(*a*); a result uniformly the same, however the purgative be varied. He recites three cases illustrative of this singular conversion of disease: the first, that of a woman, who, after labouring under severe general rheumatism, subdued by bleeding, blisters, and guaiacum, was attacked after an interval of twenty-four hours by genuine dysentery; this yielded to remedies, whereupon the rheumatism again returned to the joints, though not with the same violence as before. The second case was attended by similar circumstances, except in the final result, for the patient, an old woman, sunk under 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 commenting on these cases, he concludes thus: "Denique hanc morborum similitudinem toties observavi et perspexi, ut dysenteriam jam pro

(*a*) This remark I have myself seen verified during a dysenteric season.

rheumatismo intestinorum habeam, et similem utriusque morbi causam et materiem esse contendimus.”

Stoll thus plainly expresses his opinion of the nature of the disease: “Hancce dysenteriam intestinorum rheumatismum fuisse, ab eâdem genetrice materiâ natam, et a rheumate artuum tantum formâ, et sede diversam;” and in page 273, par. 3, he assigns these reasons for entertaining it: “1°. Quod rheumatismos artuum videre contigit, repentè sublatis, dysenteriâ oborta(a). 2°. Nonnunquam eundem hominem, et rheumatismus vexabat, et dysenteria. 3°. Dysenteria quoque subito cessabat, quò primum carpi, aut genua intumuêre, dolumque ad eum modum, quo a febre rheumaticâ solent, in artus incurrente.”

Richter, too, thus commences his account of the dysentery: “What I long considered as highly probable, from the reasoning and experiments of Aken-side, Stoll, and Vogler, I have for some years believed to be quite certain.” He declares himself satisfied that it is a rheumatic or catarrhus affection of the intestines, particularly of the great guts; and in pages 94 and 95, relates several instances of the mutual conversion of the two diseases.

(a) A lady who had had several attacks of acute rheumatism, and was afterwards seldom free from severe chronic pains, was suddenly assailed by dysentery, and, while under treatment for the latter, asked me, with some curiosity, what had become of her rheumatism, which had entirely left her, and did not reappear till long after, under exposure to its legitimate causes.

Zimmermann also furnishes us with cases, not a few, of the conversion of dysentery into rheumatism, for which see his chapter on astringents, aromatics, brandy, and wine. It would appear 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, usually followed the exhibition of aromatics and stimulants. In confirmation of this point, a single fact, recorded by Tissot, and detailed by him without reference to theory, must carry great weight: amongst a variety of bad effects, consequent on the use of opiates and astringents, he 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 plantain, on the second day of a dysentery." Zimmermann (p. 97) quotes this passage, and adds several instances of similar results from similar treatment.—See pp. 64, 98, 101, and 125.

Baker, p. 32, says: "Neque unico exemplo comprobatum fuit, dysenteriam hanc tam imperité tractatam (i. e. astringentibus et opio) in epilepsiam, *rheumatismum*, pleuritidem abiisse(a)."

(a) Other authorities might be adduced in further support of the relationship between these two diseases. Frank, for example, says, "apud alios dolor artuum quasi rheumaticus, intestina liberasse videtur." In another place he adds, "nec sub fluxu

After a perusal of these statements, it will scarcely be questioned, that there exists a close connexion between the two diseases. But if such a connexion really exist, it may be asked, why more frequent instances of their mutual alternation are not observed? It may be answered, that such instances usually arose from the improper treatment of dysentery by opiates and astringents, till lately very common remedies in that disease:—this circumstance made such cases more numerous formerly than at present, and may explain why dysentery is

dysenterico, *nec ab eo jam sidato*, rheumatismi desunt ubique symptomata.” Vignes also (p. 129) mentions, that among the numerous cases occurring in the French army, “On voyait quelquefois disparaître subitement les dejections et les autres symptomes dominans de la dysenterie lorsqu’il survenait des douleurs *rheumatismales* aux genoux, aux coudes, aux poignets ou aux épaules.” He repeats the same remark on other occasions: thus (p. 214), he says: “D’autres fois la dysenterie co-existe, est précédée ou suivie, et *même* remplacée par des douleurs déchirantes des diverses articulations, lesquelles deviennent quelquefois tuméfies. Ces douleurs sont plus communes dans la variété muqueuse que dans aucune autre.” “Elle est souvent accompagnée de douleurs contusives des articulations des membres qui se gonflent: ces douleurs font quelquefois disparaître entièrement les symptomes dysenteriques: elles sont *alors* de longue durée, a moins que les selles et les tranchées ne reparaisent pour les calmer beaucoup, ou les faire disparaître a leur tour.” For a case of rheumatism succeeding dysentery see Greg. Horstii Op. Med. lib. iv. obs. 31, p. 130; also lib. viii. p. 411, obs. 11, for another case entitled, “Arthritis vaga mirabilis Dysenteriam subsequens.” The latter instance that author mistook for a case of gout.

not now so often succeeded by rheumatism: at any period of time it was not to be expected that rheumatism should so readily change into dysentery as the reverse, because the rheumatic action, not liable to the same mismanagement, can quickly shift itself from one part, or joint, to another, without necessarily fixing itself on the intestines, whereas the whole of that canal may be commanded by medicines, internally and externally administered(*a*).

To attempt any rational explanation of the affinity which thus appears to subsist between rheumatism and dysentery, it would be necessary first to institute an inquiry into the nature and seat of those diseases, which the more immediate object of this work will not permit me to undertake at any satisfactory length. Though it might suffice here to say, "causa lateat, res est notissima," yet a few observations on the subject cannot be amiss, if they can in any wise contribute to illustrate the nature of that inflammatory tendency which gives its peculiar character to dysentery.

The seat of a disease may be considered in

(*a*) In Hoffman's *Suppl. Sec.*, tom. ii. p. 269, we find the following remark: "Qui inter affectos versantur et initia mali illico supprimunt continuo astringentium et anodynorum usu, sæpe in febrem malignam petechizantem, funesti fere eventus semet differunt. *Si qui* verò jam exortam dysenteriam nimis citò expugnant copiosè ingestis adstringentibus, aliisque specificis creditis, abscessus juxta costas, aut in visceribus fiunt, aut *Ischiadici dolores graves* exoriuntur itemque *articulares*."

two points of view: first, with reference to the parts principally affected; and next, to the vessels, whose particular action constitutes the disease in those parts:—in both respects the seat of rheumatic inflammation has been a subject of dispute. According to Dr. C. Smyth, its seat is chiefly in the muscular fibre; from the structure and functions of which he deduces the peculiar characters of rheumatic inflammation. The arguments in support of this opinion rest on three grounds: first, on the parts affected; secondly, on the pains excited by motion; and thirdly, that the pains, when shifting their place, follow the course of the muscular fibres. These arguments, however, lose much of their weight when we reflect that the parts most subject to rheumatic inflammation have no muscular fibres, as the knees and other joints; and also that there are other parts, largely possessed of muscular fibre, which this species of inflammation seldom molests, such as the substance of the heart, though its membranous portion, and the membrane surrounding it, are peculiarly liable. If we recollect that there is besides no part subject to rheumatic pains, in which a serous membrane or membranous expansion cannot be demonstrated to exist, is it not on these grounds more probable that its seat is in membranes, ligaments, sheaths of tendons, &c., rather than in the muscular fibre? 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, thorax, in the diaphragm, among the abdominal muscles, and on the inferior and superior extremities, membranes and membranous expansions are to be met with in abundance. Burdin, in his *Medical Studies* (vol. ii. p. 176), 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 aponeuroses, the tendons, the tendinous vaginae, the periosteum, &c. There is great abundance of these parts around the articulations ; 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 the second argument, which proves nothing, for the pains excited on motion might equally arise from an affection of the membranes, as of the muscles themselves. Neither will the third argument, taken from the direction of the pains, prove much ; because in the transition of pains from one part to another, there is no ground for supposing any muscular apparatus necessary, as the sensation of transition arises merely from different and distant parts assuming, in rapid succession, that inflammatory action they could not all labour under together ; the pains, therefore, appear to shift, or take the shortest direction, viz., that of the muscles.

From these considerations we may conclude, that membranous parts are, at least, as liable to rheuma-

tic inflammation as muscular, an opinion strongly confirmed by the fact, that dissections have detected a viscid and gelatinous matter in the joints, and sheaths of tendons, in cases of chronic rheumatism, but have never exhibited any other change in the muscles except loss of substance, arising from want of use by exercise.

As we observe diseases of various characters oft produced by the operation of the same cause, under different states of the body, so in each we find a different set of vessels concerned, or a different action of the same vessels. Thus, in the full and vigorous frame, the whole arterial system is excited by cold into violent action, speedily destructive of life, if proper remedies be applied either too slowly or timorously, This constitutes the phlegmonous inflammation; whereas the rheumatic, generally occurring in persons of a lower standard of strength, is seated not so much in the arteries as in their exhalant extremities, which, when inflamed, pour out a greater quantity of their contents than the absorbents can take up within the same time, thereby inducing both pain and swelling. From this peculiarity in the seat of rheumatism, and the character of the temperaments most subject to the disease, it happens that neither venesection nor other evacuations, that by the skin alone excepted, are equally advantageous or necessary in this as in phlegmonous inflammation. 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 observed on the parts affected by it(*a*). In dropsy, consequent on exposure to cold, the exhalation from the extreme vessels, on account of the weaker condition of the whole frame, is increased without much inflammatory action in the system. If this distinction between rheumatism and dropsy be well founded, it explains 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 chiefly, if not exclusively, seated in the greater intestines, and that their inner or villous coat is the part primarily affected : but for any further information of a satisfactory nature, can we rely on dissections? What can they exhibit to our view, but the state of diseased parts after the diseased action has been able, by progressive changes of structure, to induce death ; thereby depriving us of any distinct conception of that primary condition of the intestines, which constituted the original disease(*b*). As “ pyrexia conta-

(*a*) The cases of abscess after rheumatism, which have been adduced in proof thereof, are of a very dubious character, as in those of Schoenlein, detailed in the *Medico-Chirurgical Review* for January, 1846.

(*b*) “ I hold it,” says Zimmermann, “ as a very important maxim in the exercise of our art, that we should be very careful not to confound the divers periods of a disorder one with another, nor to take from what happens towards the end of the sickness, and still more especially *from the dead body*, the indication of what is to be done at the beginning.” For the best summary of the morbid

giosa" is not at least the usual agent in inducing it, we must look to some local affection of the intestines themselves for an explanation of its essential symptoms. Inflammatory action of one kind or other seems to be more or less essentially concerned in the production of the disease. Dissection shews it to have been present, often in so high a degree, as to induce death

appearances furnished by dissection, I would refer to that given by Vignes, who had such abundant opportunities for observation in the campaigns of Spain and Germany. It is remarkable, however, that, notwithstanding the opinion he thence formed of the nature of the disease, he scarcely ever bled the sick except by leeches. "Les autopsies cadaveriques," says Vignes, "me démontraient que la cause ou la nature de la dysenterie était constamment une phlegmasie plus ou moins considérable des gros intestins; mais quelle présentait des différences très-remarquables et indispensables à connaître: ces différences sont relatives à l'état de bénigne ou de mauvais caractère." He explains this last phrase in a note, by stating that he employs it and "malignité" indifferently, to designate "les états typhoïde, adynamique, ataxique, la fièvre jaune, la peste et les dysenteries combinées avec ces maladies." At p. 38 he states: "On trouvait dans les gros intestins de ceux qui succombaient à la violence de la dysenterie (bénigne) une phlogose plus ou moins considérable avec plus ou moins d'épaisseur de leurs parois:—mais quand on lavait ces parties, on voyait que les matières n'étaient pas du véritable pus, et on ne trouvait d'ailleurs jamais d'ulcérations dans la muqueuse, quelque grave ou violente que fut l'inflammation *bénigne*. Tout le corps du gros intestin était aussi plus ou moins endurci et criant quelquefois sous le scalpel." When the disease, however, partook of the "mauvais caractère," it was otherwise as to ulcerations, which sometimes extended to the smaller intestines. "La profondeur des ulcères," he says, "était en raison de l'atteinte profonde de malignité: car quand il n'y avait eu que tendance à cet état, les ulcérations se

by alteration of structure, by suppuration or gangrene, and the symptoms of severe tormina, with pain on pressure, give a similar indication. Of what kind is this inflammation? Can it be of the phlegmonous character, as dissection proves its occurrence to be the frequent cause of death? This kind of inflammation, however, usually excites high fever, and is

bornaient souvent a la membrane muqueuse : quand au contraire elle avait été complètement de mauvais caractere toutes les autres membranes etaient corrodées par une gangrene atonique, excepté la tunique peritoneale, que je n'ai jamais trouvée perforée quoiqu'il y en ait quelques exemples." In opposition, however, to this last statement, or rather in proof of the examples to the contrary, Dr. Cheyne observes (p. 26), that "death sometimes occurred unexpectedly from the escape of the contents of the intestines into the cavity of the peritonæum, in consequence of a portion of that coat being destroyed by ulceration." Now, Dr. Cheyne's epidemic was of the typhoid character, and in other respects his morbid dissections coincide well with those of Vignes, both authors exhibiting the various lesions of structure induced by the double influence of fever and of dysentery.

"The deaths," says Cheyne, "which took place when dysentery was in its first stage, were owing to fever, or to some other fatal disease; and when owing to fever, a peculiar state of the mucous membrane of the stomach and intestines was noted; it was of a more or less red or purple colour; it was rather thickened, soft, and pulpy, the least violence destroying the continuity of the surface," &c. "In casu lethali," says Frank, "frequentissimè gangrena succedit; in aliis intestina crassa solito densiora, rigida, coriacea et contracta reperta fuerunt." "These appearances," says Dr. James Johnson, "are very striking, yet I hold them to be equivocal. We are deceived by confounding the ultimate changes with the primary diseased movements. I am in every

besides attended with constipation: "Quapropter," says Akenside, "inflammatio contraria videtur dysenteriae indoli, cujus magna pars in dejectionibus nimio plus frequentibus posita est." We may judge of the fallacy of this conclusion by subjoining to his logical enthymem the only minor præmiss which could warrant him in making it. It would run thus: "Sed dysenteria comitatur nec febre inflamma-

case inclined to regard inflammation rather as a sequence than a cause of dysentery, as a contingent effect, and not as a uniform result." "Inflammation and ulceration are secondary or ternary links in the morbid chain; and many a case of real dysentery is checked and cured before either of these takes place; that is, when there is merely an increased afflux of blood to the mesenteric and portal vessels, a super-irritation in the mucous membrane of the bowels, and an increased discharge of acrid secretions from the intestinal glands."—Cleghorn says, "that wherever the ileum, colon, or rectum are inflamed, hardened excrements are for the most part either the cause or consequence of the disease; nor can we hope for a complete recovery while such offensive matter remains in the bowels: six or seven grains of calomel, with one of opium, at night, and a purging apozem of manna, senna, and sal catharticum, have brought off a prodigious quantity of round, hard, foetid lumps, to the great relief of the patient; nor is it easy to conceive how so much had been collected."—The only inference I can draw from such a statement, in opposition to that previously given (note (a), p. 14), is this, that epidemics of this disease vary much in some of their results, as when worms to an enormous amount are found in some, and also when the spleen, the pancreas, the liver, its gall-bladder, and the bile, are found in very different conditions in different epidemics.

toriâ, nec constipatione intestinorum." Now such a præmiss 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, marked by the disappearance of the natural contents of the intestines, and the substitution of frequent scanty stools of mucus, blood, and serum. As simple phlegmonous inflammation will scarcely account for these peculiarities in dysentery, we cannot consider it as exerting any essential influence in the primary production of the disease(*a*).

(*a*) Dr. Abercrombie, one of our most sound practical writers (who considers dysentery, "in all its forms, and all its degrees, as an inflammatory affection of the intestinal membrane," sometimes admitting of spontaneous cure, and sometimes speedily fatal, if not treated with the utmost activity), takes occasion, when speaking of the inflammation of the mucous membrane of the intestines, to observe (p. 247), that "it will now, perhaps, be generally admitted that in talking of dysentery we have been too much influenced by the distinctions of systematic writers, in applying this term to an affection which is characterized by tenesmus and scanty discharges of bloody mucus." "Such a modification of the disease," he says, "does exist, but practical writers of the first authority describe another form of the affection, in which the evacuations are copious, and vary exceedingly in appearance *at different periods of the disease*," as also according to its extent and special seat, whether affecting a part or the whole of the large intestines, or as involving a portion of the ileum also. Under this extended view of the case, Dr. A. defines diarrhœa and dysentery, as merely differing in the nature of the disease, and "not to be learned from the character of the evacuations;"—"the nature of the disease" in the former being "an increased

Many persons, conscious from the symptoms, treatment, and frequent termination of the disease in a chronic state, that phlegmonous inflammation could not produce dysentery, have denominated that species of inflammation which takes place on

action of the canal, produced by various irritating causes applied to the mucous membrane in a healthy state" (the highest degree of which is the cholera of this country), whereas the dysentery is a similar state of increased action, arising from inflammation of the mucous membrane (the highest state of which appears to be the cholera of India)." Without entering on any discussion of these two disputable parenthetical propositions, I shall content myself with observing, that any error into which Dr. A. may have fallen, relative to dysentery and its characteristic evacuations, would appear to have originated in his assuming, as cases of pure dysentery, all those in which the mucous membrane of the smaller intestines was involved, as well as of the greater, *cases* which fell sporadically in his way, and were not illustrative of any epidemic prevalence of the disease. The true dysentery originates in, and is confined to the rectum and colon (as appears by the numerous dissections of original observers), though the inflammatory affection may sometimes extend to a portion of the ileum, and may besides involve other organs, according to the neglect or mismanagement of the case, or the nature of the fever with which it may have been associated, as in the instance of Roederer's morbus mucosus, hereafter described. Hence it is, that the character of the alvine evacuations may vary from that cause as well as from the stage and progress of the complaint; but whether a state of diarrhœa precede the dysentery or not, it is, in its earlier stages, characterized by tormina, by tenesmus, and by scanty mucous stools, which cannot be expected to continue unchanged, whilst the parts discharging them are in progress of structural change, more or less rapid.

the villous coat of the intestines, erysipelalous, without meaning thence to deduce any explanation of the symptoms; but merely wishing to express something essentially differing from phlegmonous inflammation. Little need be said against this designation, the same objection applying to it as to the former, namely, that it offers no rational explanation of the characteristic symptoms of the disease. Spasmodic constriction, which Cullen ingeniously, though not (*a*) originally, supports, as the proximate cause of this disease, I consider as merely an effect of that cause, whatever it be; for if we suppose spasmodic constriction existing to any extent in the intestinal canal, will that alone explain the phenomena of dysentery? Why should we have stools at all, or whence arises that inflammatory state which so peculiarly affects the inner coat of the large intestines? Besides, on those occasions when we know spasm to exist, do we ever find symptoms resembling those of dysentery? We must, therefore, look for some other cause more adequate to the production of the several phenomena of the disease. That cause I would refer to an inflammatory action in the innumerable exhalant vessels on the mucous coat of the intestines, similar to that which takes place in serous

(*a*) Haller, in Hist. Morb. Uratislau. A. D. 1669, p. 84, makes this remark on the condition of the intestines in dysentery: "In dysenteriâ totum canalem, qui sub nomine primarum viarum communiter venit, *spasmodicè* conculit."

membranes, when affected by rheumatism. Under this state 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. I have the more readily embraced this opinion, from the insufficiency of other modes of explanation, and from the facility with which this accounts for the various phenomena of the disease; indeed the symptoms which it presents, the treatment it admits, and, in particular, its close affinity to, and frequent alternation with rheumatism, all countenance the doctrine. It explains why the dysenteric pains, which, in the language of Hoffman, "*per totum intestinorum canalem vago itinere progrediuntur,*" are, like the rheumatic, constantly shifting their ground, and why the dysenteric action, when disturbed in the intestinal canal by opiates and astringents, re-appears in the joints, under the form of rheumatism. It also explains why, in severe cases of the disease, whether arising from neglect or mismanagement, active inflammation is so much to be apprehended, and why, on the contrary, a chronic state so often supervenes. But whether we admit this reasoning or not, it cannot be denied that such an affinity does really exist

between rheumatism and dysentery, as at least warranted Stoll in entitling them *παθηματα αδελφεια* (*a*).

(*a*) Frank draws an ingenious analogy between dysentery and cynanche: "Non alius est qui guttur seu intestinorum per *fauces* introitum, quam qui anum seu intestinorum exitum occupet morbus; ac summa plures cynanchis species inter atque dysenteriam affinitas intercedit. In angina fortiori sensus ardoris, rubor, inflatio, tensio, puriformis, interdum subcruenti, humoris secretio, screatio continua, deglutiendi nisus perpetuus, dolore plenus, ac tenesmo qui ad anum est dysentericis, in omnibus persimilis occurrunt."—For further proofs of still stronger analogy consult the section "De Anguina," as also the continuation of the above passage, in which he shews a striking similitude between the malignant forms of cynanche and dysentery.

Mr. Bruant, in his official report to Desgenettes on the dysentery prevalent in the French army in Egypt, takes notice of a curious sympathy between the pains of chronic dysentery and those of ophthalmia: "L'ophthalmie," he observes, "apportoit toujours un soulagement marqué, lorsqu'elle survenoit dans les dysenteries de long cours: les douleurs des yeux et celles du bas-ventre se remplaçoient mutuellement: mais les dernières reparoissoient pour l'ordinaire apres la cessation des premieres."

CHAPTER III.

COMBINATIONS OF DYSENTERY : AND FIRST OF THE
INTERMITTENT AND REMITTENT FORMS.

HAVING pointed out the characteristic symptoms of dysentery in its simple and genuine form, I now proceed to consider those important combinations of which the disease is susceptible. There are two ways in which diseases may be said to unite : *the one* constituting such a perfect combination of the characteristic symptoms of each, that they seem to possess one common nature, and to exercise one dominion ; *the other* being merely an accidental union of two or more diseases in the same individual, which are naturally, and continue to be, mutually independent of each other. These latter are in general local diseases, or one or more of them may be local, whilst a third is a general affection of the system. But, in this latter case the local complaints arise from specific contagion, as when typhus, scarlet, or remittent fever is combined with itch, syphilis, &c. (*a*) Such combinations

(*a*) We shall hereafter find that these local diseases are viewed by Vignes as originating in contagion which he calls "essential," while, on the other hand, he deems the contagion of typhus as merely "relative," and that of small pox as the result of a "levain inné."

as these, however, exert no special influence over the diseases of which they severally consist, whereby their characters or treatment are seriously affected: not so the former or more perfect combination, in which we always find a general febrile affection, that may or may not be contagious, and one or *more* local diseases, not originating in contagion. This species of combination might be illustrated in almost every epidemic of dysentery, of catarrh, of angina(*a*). But that we may better understand these various combinations, it will not be amiss to advert to those "epidemic constitutions," which, according to Sydenham, so materially influence the diseases prevailing under them.

Sydenham was of opinion that epidemic diseases changed their nature twice a year, to wit, about the vernal and autumnal equinox. Subsequent expe-

(*a*) This form of combination, however, is very different in character from that which Dr. Copland calls the "complications" of dysentery, as "with diseases of the liver, spleen, and some other abdominal viscera; with jaundice, with scurvy, with worms in the primæ viæ, with hæmorrhoids, and with rheumatism." Such complications, though they require some modification of the general treatment, may concur with any disease without seriously altering its character, as remittent fever does that of dysentery. How few cases of simple disease do we ever meet; every case of disease is more or less a "complication." The instance of hospital gangrene originating among the wounded on the supervision of the morbus mucosus (recorded by Roederer), furnishes a special exemplification of two local diseases, not contagious, being spread by contagion, when combined with malignant fever. But of this more hereafter.

rience has confirmed the propriety of his advice carefully to attend to the disease which rages most about the vernal and autumnal equinox, as that disease imparts, if not its name, at least its character, to the constitution of the whole year. But besides the prevalent epidemic there occur other diseases, not dependent on the reigning constitution, though existing under it: these Sydenham called *intercurrents*, and in numerous instances has shewn the importance of accurately marking the distinction. Thus, for example, pleurisies, anginas, dysenteries, may arise from their peculiar causes, and in their appropriate seasons, without any connexion with the reigning epidemic; and are then to be treated, each according to its peculiar nature, as essential diseases, without reference to the epidemic constitution under which they occur: but these self-same diseases, which thus appear as intercurrents, may also exist only as manifest symptoms of the reigning fever, and are then to be treated, not as essential diseases, but according to the method that fever requires. In this sense it is that dysentery may prevail either as an essential or symptomatic disease, i. e. either in its simple form or in some of its combinations, all of which, when epidemic, may be reduced under the three heads of the intermittent, remittent, and typhoid or contagious dysentery, to some one of which, or to the simple dysentery, we may easily refer every instance of the disease on record.

I shall now proceed to illustrate its intermittent

and remittent forms, and, from the great analogy between the two combinations, I need not apologize for considering them together.

Clarke commences his history of dysentery with an observation which strongly confirms the characteristic tendency of the disease to combine with fever: "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(*a*), from the distinct remissions and

(*a*) There can be little difficulty in determining the nature of the epidemic dysentery, recorded by Morton in his Appendix, and in which he found bark and opium so efficacious. His account is very concise, yet clear in its general statements, and is well worthy of attentive perusal, as indicative of the then state of England in regard to the public health, both before and after the great plague of 1666. His details closely resemble those of Sydenham and Willis (hereafter given), and exhibit, as prevalent throughout the whole island, a violent remittent fever, easily convertible into typhus, of which Cromwell and his own father died, and by which he himself and his whole family, servants, maids, and nurses ("quotquot erant intra eosdem nobiscum parietes") were afflicted. With some it terminated in a tertian or quotidian intermittent; with others (as with Cromwell and his father) "*Συνεχῆ* in *Συνεχῶ* demum mutatâ, quam comitabantur deliria, spasmi cæteraque funesta malignitatis indicia, fati cedebant." "Multos passim videre fuit, post sextam vel septimam exacerbationem febre malignâ continuâ, quam diri spasmi et deliria comitabantur 17^o vel 21^o morbi die e vivis sublato." The extent of the disease may be estimated by his assertion that "hoc tempore, si unquam aliàs, ferè tota hæc insula, Nosocomii publici speciem præ se ferebat, et in nonnullis locis sani vix supererant, qui ad ministran-

exacerbations observable in his epidemic every or every other day, was led to embrace the same opinion; and so decided was his impression on the subject that he says (p. 426 of Appendix), his anxious desire was to shew “*Dysenteriam ac diarrhœam*

dum valetudenariis sufficerent.” Such this fever continued till the year (1664) before the plague, when “*fere omnes acuti morbi derepente exulabant et salubris admodum erat istius tempestatis constitutio.*” But in the spring following, “*post brumale gelu prætermo- dum atrox, hæc Συνεχης in pestem funestissimam inopinato mutavit,*” and in the space of one year carried off more than 40,000 persons. Yet, as he subjoins: “*Sæviente hâc ipsâ peste, non penitùs defecerunt vestigia nonnulla febris Συνεχης: quotquot enim ex peste evaserunt, singulis vel alternis diebus in seipsis percipiebant symptomatum exacerbationes et remissiones, stasis horis alternatim sese excipientes.*” Finally, on the disappearance of the plague, the same fever again revived, with its dysenteric associate, “*quæ indies per omnes menses autumnales magis magisque sæviebat,*” and on this occasion it is that he says (as quoted hereafter), “*nec mihi perpercit contagio, &c. et ex hoc morbo ægrè admodum evasi.*” Indeed he states that in London 300, 400, or 500 died each week, “*Fluxu et torminibus cæterisque dysenteriaë exeruciantis diris symptomatis confecti.*” “*Vires tamen spirituum non adeo ubique prosternere videbatur, ut non natura, saltem sub initium morbi, post stas vel incertas periodos, vires suas plus minùs colligeret atque inde victoriam sæpius reportaret: unde remissio atque exacerbatio torminum cæterorumque truculentorum symptomatum, singulis vel alternis diebus recurrentes, evidenter sese in conspectum dabant: durante enim remissione, symptomata cuncta exeruciantia nonnunquam *ultra* minuebantur: recrudescente autem paroxysmo opiatorum ipsorum vires spernebant. Unde,*” he concludes, “*clarè deprehendi dysenteriam symptomatis duntaxat rationem habuisse, febrem autem eamque verè Συνεχη, primarium fuisse morbum.*”

torminosam, quæ fere per septennium vigeabant ac unoquoque autumno ab anno 1666 ad an. 1672, solenniter hic Londini revirescentes, ubique epidemicè grassabantur, subortas fuisse a febre vere *Συνεχει*; atque ut a verâ ideâ hujus morbi formatâ, curativæ indicationes in posterum certiores desumantur, mihi in votis est, ut communi medicorum consensu, diarrhœa et dysenteria castrensis nomine proprio *Συνεχειος*, spuria ac colliquativa, posthac designetur."

Sydenham, in denominating dysentery a "febris introversa," obviously refers to the same cause, for which Morton and Clarke consider the disease a symptom of the fever. That this opinion of Sydenham's originated in the frequent combination of dysentery with intermittent and remittent fever (in his day very prevalent throughout England), will appear probable from his account of an epidemic remittent, which he calls the "febris dysenterica," and which he supposed to be the "febris introversa," productive of dysentery, in consequence of the great influence it exerted over the symptoms of that disease(*a*). If this be not Sydenham's meaning, and this the origin

(*a*) 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 evacuation by stool in the dysentery,

of his opinion, I confess I cannot understand him: for if we take his phrase of "febris introversa" in its literal and obvious sense, the opinion he adopted must appear unfounded, there being no evidence of a "febris introversa," because in such a case the fever should disappear, the dysentery being substituted in its place: on the contrary, the fever does still exist in combination with dysentery, and without any necessary connexion, as to cause, with that disease. Sydenham's opinion has been differently received: "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." Moseley, too, observes, "I have invariably found the truth of Sydenham's opinion, and have remarked, that as the flux conforms by the number of stools, and by its rapidity, to the degree, so it does to the state of the fever of the season, when it prevails; the stools being more frequent, and all the symptoms more aggravated at those hours when the current fevers are in their exacerbation, and the reverse when these fevers are in their remission." Pringle, however, when shewing an analogy between remitting fevers and dysentery, though he and the symptoms thereon necessarily depending, this fever should seem to be wholly of the same nature with that disease." Sect. iv. chap. 4. This is obviously the description of a fever, which sometimes attacked alone, and sometimes in combination with dysentery.

would seem to incline to Sydenham's opinion, yet says that, "upon the whole, the nature of the two distempers appears so much alike, that at first sight Sydenham seems to have expressed himself justly when he called this flux "the fever of the season turned upon the bowels, but 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." This mode of reasoning, however, was inadmissible with Moseley, who pronounces the following censure thereon: "By this," says Moseley, "he must suppose, what I fancy no other person does, that dysenteries are always infectious, or that fevers never are; or that epidemic dysenteries are infectious, when epidemic fevers are not." Now this clearly was not Pringle's meaning, nor will his reasoning, on a little reflection, appear unfair: for as that form of dysentery, with which he was most conversant, was undoubtedly contagious, and very different from that which Moseley was in the habit of seeing, how was it possible for Pringle to infer that a disease of a contagious nature could originate from the introversion of fevers which were not: not meaning thereby that fevers are never contagious, but that the intermitting and remitting forms were not.

A few passages from other writers will satisfactorily demonstrate the reality of the intermittent and remittent forms of dysentery. Clarke, Cleghorn, Hunter, Roederer, Willis, and Rollo furnish abun-

dant proofs to that effect. I shall commence with the latter, who gives a concise and correct account of these two forms of the disease, as occurring in the West Indies; and so constantly did he find dysentery in combination with these fevers, that, although he assigns satisfactory reasons for considering them independent diseases, which, from the joint action of their distinct causes, took possession of the same patient, he yet fell into the error of thinking fever an essential part of dysentery, and this fever, he says, assumes the form of an intermittent or remittent. When it assumes the intermittent form, the symptoms peculiar to dysentery appear in the paroxysm, and 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. He further observes, that when dysentery prevails, intermitting and remitting fevers frequently appear, and it then generally assumes one or other of these forms, more commonly the latter, their symptoms being similar, except in such as are peculiarly dysenteric. In this latter remark he would appear to contradict a former position, that fever constitutes an essential part of the dysentery.

Cleghorn observed such a similitude between tertian fevers (the epidemic of Minorca) and dysentery, as to have been induced to make trial of the bark in

the latter: "When the fever and gripes," he says, "were regularly exasperated every day, or every other day, at stated periods, the bark has often effectually 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.*" It is impossible more clearly to demonstrate the existence of dysentery in combination with intermittent fever, as also its own independent character, evidenced by its continuance after the cessation of the fever (*a*). Roederer, when 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 febre intermittente, et dysenteriâ simul laborarint, sive vera intermittente dysentericâ. In vico autem vicino epidemica grassata est febris intermittens sola, ut quinque et ultra in eâdem domo laborarent; in alio vico paullò remotiori, montibus vicinis cincto, eodem tempore sola dysenteria sæviit, multosque jugulavit; in alio denique vico inter priores medio, uterque morbus rarissimus fuit."

Clarke, when describing the Bengal dysentery,

(*a*) "Vidimus et nos," says Frank, "prævis: huic febre periodicæ, dysenteriam, ut morbum alienum, successisse eamque suspendisse: cùm vero hæc ipsa sanata jam esset, febris iterùm quartanæ typum, ut antehac, rediisse. Quam febris intermittens, ut symptoma, producit ægritudinem, hanc recta illius pertractatio facile satis dissipat."



says(a): "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. In a day or two, sometimes later, the dysenteric symptoms made their appearance, and were attended with the *greatest prostration of health 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 aggravated; tongue became very black, and the teeth were covered with a black, tenacious 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 tendinum*, tremors, and delirium were added: at this stage several vomited a viscid fluid, which tinged the linen and bed-clothes black: some had pustules on various parts filled with ichorous matter, which degenerated into black, putrid

(a) In the first edition of this work I have quoted Clarke's History as illustrative of the contagious or typhoid form of dysentery. On a more attentive perusal of his treatise, I am conscious I was in error, and that his epidemic properly belongs to this head. I shall explain myself more fully when discussing Clarke's opinions relative to the contagion of dysentery, opinions altogether influenced by those he entertained on the contagion of fevers.

sores. The duration of the disease was uncertain: at Bengal it frequently carried off the patient in a few days: at China, if neglected, it proved fatal in seven or eight days, and in most places it was seldom protracted beyond the sixteenth day, except it assumed a chronic form, and then it sometimes proved fatal after six or seven weeks. In some the dysenteric fever at Bengal, through the whole course of the disease, had regular remissions. In others it was accompanied with a pain in the region of the liver, a tickling cough, and a vomiting of viscid slime. The delirium was never constant, the senses and judgment remaining at intervals entire." This description needs little comment; it is so unlike the usual course of dysentery, and so characteristic of the disease in combination with remittent fever, that Clarke remarks that the dysentery seemed rather a symptom of the fever, than an original disease.

Hunter says that between dysentery and the remittent fever in Jamaica there subsists an intimate connexion, the one frequently changing into the other, and both often complicated with various degrees of violence. Nicholl states (Edin. Med. Jour. for July, 1815), that at Seringapatam the disease is frequently combined with remittent and intermittent fevers, and Trotter says, that on the coast of Africa and the West Indies the same combination takes place. Even in our own climate Willan mentions that in the worst cases of the epidemic of 1800 (a period remarkable for cold, wet, and famine), a considerable degree of

fever prevailed from eight to ten days: the pulse 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.

Hoffman describes two dysenteric epidemics as occurring in 1719 and 1726, which differed singularly in some respects from each other. Both arose in very dry and hot summers, the former chiefly in August and September, raging along with intermittent fevers of the semitertian type, "in illis regionibus ac urbibus, quæ multa stagna paludesque ac flumina ipsas alluentia foveant, maximèque in illis domibus, quæ proxime ad flumen sitæ erant, majorem ac crudeliorem exercebant tyrannidem. Contrà verò urbes ac regiones in locis altioribus sitæ vel prorsus immunes ab hoc diro malo fuerunt vel illud mitissimum sunt expertæ." Hence he concludes that, as this epidemic ceased in the beginning of October, when there was every profusion of fruits, its cause was to be sought "in miasmate quodam maligno ex peculiaribus terrarum effluviis generato." Hence, too, he says we are instructed, "nimium æstum et fervorem solis mortalium corporibus longe majores inferre noxas, quàm quidem frigus utut sit intenssissimum." The epidemic of 1726 was differently characterised, for it began in June: "et deinceps, contra consuetum ordinem, in montosis et celsioribus locis majorem et graviorem, quàm quidem in planis et campestribus, exercuit tyrannidem." At

its commencement, indeed, it seemed to partake of a severe choleric character: it then assumed the tri-fold form so accurately described by him in the introductory observations. In this latter epidemic there was not only a combination of the disease with intermittent and remittent fever, but with the contagious typhus:—hence the difference of the localities observable in these two epidemics(*a*). I might

(*a*) It would appear from the subjoined statements that in the connexion existing between dysentery and intermittent and remittent fevers, there are some singular anomalies; thus Dr. Buel describes a bilious fever and dysentery as prevalent in Sheffield (State of Massachusetts), in 1796, arising from the obvious influence of marsh miasmata, and states that in some “the dysentery came on while the patient was affected with the fever, in which case the type of the fever soon became obliterated, and the accompanying febrile symptoms were similar to those in original dysentery.” “Sometimes the fever came on upon the dysentery: the type of the fever was not in this case easily ascertained, until an abatement of the dysentery took place, when, as the dysenteric symptoms subsided, the fever would appear in its proper form. The two disorders appeared to be complicated; that is, they both seemed to exist at the same time, rather than to act in alternation.” “In this sickness,” he adds, “there is every reason to ascribe identity of cause to the two disorders: they were circumscribed in a very striking manner by precisely the same limits, and they both began and ceased to prevail at the same time.” Dr. B. was convinced that “neither of these diseases was propagated by specific contagion.” Dr. Ffirth also, in his Dissertation on Malignant Fever, says that the crew of the ship in which he sailed to Batavia (seventy-six in number), had, with the exception of eight, either marsh fever or dysentery; that the fever appeared to alternate with dysentery; when the weather was bad the latter prevailed; when good, the former.” From this latter

adduce a variety of examples to the same effect, but may for the present be content with the proofs already furnished of the intermittent and remittent forms of dysentery, because, besides the additional demonstration supplied in the note, I shall, when treating of the exhibition of bark in this disease, have occasion to mention other instances in which the efficacy of that medicine was conspicuous.

The connexion between dysentery and intermittent and remittent fevers being thus clearly established, it remains to explain (what was such a problem to Pringle), how exposure to the same cause, as he thought, indifferently produces intermittent or remittent fever, or dysentery, or either of the two former in combination with the latter. Exposure to cold, we know, will, under certain circumstances, induce dysentery, and exposure to marsh miasmata both intermittent and remittent fevers. Soldiers are

statement it is not to be inferred that the two diseases "alternated" in the same individual, in the same sense that Dr. Buel speaks of their alternation: in this case when one disease was prevalent, the other was not. Hargrave, too, in his *History of the Walcheren Fever*, states that "in proportion as the autumn grew cool, these fevers (bilious remittent), abated of their ardour, and formed more easily into intermittents, though still irregular and of a bad kind. The dysentery was never general, though not uncommon, and it was observable that those who were seized with it usually escaped the fever, or if any had both, it was alternately; so that when the flux appeared the fever ceased, and when the first was stopped the other returned: whence it appeared that, though the two distempers were of a different form, they proceeded from a like cause."

frequently exposed, after fatigue and sweating, or without the shelter of warm clothing, both to the cold of night in a warm season, and to the miasmata of marshes at the same time; can it, under such circumstances, be surprising, that one man shall be attacked with dysentery, another with fever, a third with both, whilst a fourth shall be laid up by rheumatism? We cannot be surprised at the occurrence of dysentery or fever as a consequence of such exposure; and why are we surprised that the causes of both diseases should operate on the same individual, producing a dysentery with distinct intermissions or remissions, when we see that such a combination is frequent in those countries where the diseases themselves often exist independently of each other? This subject is explained after a similar manner by Rollo, who 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 those affected with dysentery have been always previously exposed(*a*).

We may here refer to the opinion of the learned

(*a*) Dr. Copland, in his survey of the various forms under which dysentery presents itself, states (p. 694), that as to its type, it is "remittent and continued," though in his explications of the phenomena of the disease (p. 704), he is forced to admit that though the "inflammatory, typhoid, and more malignant forms are generally continued or obscurely remittent, yet that the other forms may assume an obviously remittent or even an *intermittent* type," owing, as he says, rather to the concurrence of those causes to which periodicity in fever is owing, with those on which the

yet sceptical Bancroft on this subject. He thinks dysentery specially producible by marsh miasmata, and though he does not altogether deny the operation of wet and cold in its production, yet he would seem to insinuate that this latter cause alone does not induce *genuine dysentery*, as he "suspects (p. 541) that in such cases the disease is not exactly like that which principally results from marsh effluvia, but that it has a greater similitude to diarrhœa, and if accompanied with fever, that this is nearly related to catarrh;" yet in p. 538 he states that wet and cold are "often productive of dysentery, either alone or in conjunction with miasmata and other causes;" and after citing instances of both cold and wet, and sharp, acid fruits, *eaten to excess*, having caused the disease, he yet thinks that "in these, as well as other cases, marsh effluvia must have been the principal cause," and in proof thereof adduces the remissions and exacerbations frequently observable in such cases. But he goes even further, and says, that "another supposed cause of dysentery has been alleged by so many respectable authors, that it

dysenteric phenomena are more immediately dependent, than to the production of two distinct kinds of diseases. "When both kinds of causes concur, as they frequently do, in unhealthy situations and seasons, a form of disease is directly produced in which many of the characters of both disorders are blended." What is this really but saying the same thing in other words: if not, his position is scarcely consistent with the experience of Cleghorn, Hunter, and others already detailed. Dr. Copland makes a similar objection to the supposed combination of dysentery with typhus, of which more hereafter.

would be improper in him to reject it, though he had never seen any decisive evidence of its operation, that is typhus fever or its contagion ;” and yet, after giving some testimony in proof of its influence, he suggests if “it may not become a matter of doubt whether the dysentery in these cases was the consequence of a typhus fever thrown upon the intestines, or whether the patients had been exposed both to marsh effluvia and febrile contagion at different times, and that each having produced its effects separately, the fever and flux were thus accidentally combined.”—I do not say that marsh effluvia alone are not competent to induce dysentery under the circumstances in which that disease usually occurs, more especially in hot climates, but this I maintain, that the influence of wet and cold in its production is more clearly, satisfactorily, and universally demonstrable than that of marsh effluvia, and that it is consequently more legitimate to conclude that, as dysentery can occur in every possible relation to remittent fever, either as absent or present with it, as preceding it or consequent upon it, or as remaining after the fever has ceased, or *vice versâ*, so it cannot be likely that both are always produced by one and the same cause, more especially as those exposed to marsh effluvia are at the same time generally exposed to wet and cold.

Before I proceed to consider the last and most important combination of dysentery, this is perhaps the most appropriate place for reviewing Degner's "dysenteria bilioso-contagiosa," as also the dysentery described by Willis under the names of "cruenta and incruenta," respecting which some strange misconceptions have been entertained, rendered remarkable by the fact that Willis's dysentery was the same epidemic with that observed by Sydenham.

Willis states that two species of dysentery appeared in the years 1670 and 71. "In unâ sedes aquosæ, et quasi limpidæ cum subitâ virium prostratione ; in alterâ cruentæ, attamen tolerabiles existunt. Anno 1670, circiter æquinoctium autumnale, quamplurimi dysenteriâ *incruentâ*, verùm atroci admodùm et valdè periculosâ, laborabant. Affectus, et subitò, et frequentèr absque manifestâ occasione invadens, laborantes cum *vomitu immani*, et *sedibus crebris et aquosis*, cito in maximam debilitatem, inque spirituum horrenda deliquia, et virium omnium prostrationes redigebat. Novi, inquit, plures pridè satis sanos, et valde robustos, intra duodecim horas, morbi hujus tyrannide adeó 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." Some of these symptoms might appear to justify Willis in calling this disease a dysentery, and

Zimmermann in designating it malignant: but if so, how did it happen that Willis and Sydenham, two eminent contemporary physicians, resident in the same city, should so widely disagree respecting 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. Morgagni appears at first scarcely less astonished than Pringle, but is not so slow in devising an explanation. 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, 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 abound with a more fluid, and those of another quarter, with a more lentescent and mucous serum(*a*). This hypothesis, however, being utterly inadmissible, we must look for some other mode of explaining so singular a diversity.—Is this dysenteria incruenta, I would ask, entitled to the name of dysentery?—would not that of cholera more exactly suit it? Should such be the case, we see at once how it is that Willis and Sydenham appear so inconsistent.

(*a*) See pp. 70 and 73, vol. ii. of Alexander's translation.

Which of the characteristic symptoms of dysentery is present? Scarcely one; the disease came on suddenly, 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; while the rapidity of the disease and its treatment put the point beyond question; besides there is no tenesmus; indeed, the only point of resemblance is the frequency of stools, and yet these differ from the dysenteric both in quantity and quality. On the other hand, every circumstance warrants us in asserting the disease to have been cholera; the season of the year, the mode of attack, the conjunction of vomitings and copious stools, the debility and other effects immediately consequent, the rapid fatality of the disease, and the mode of treatment, all unite in pronouncing the disease to be cholera, which we know to be devoid of contagion, as Willis declares to have been the case with the dysenteria incruenta, for he says: "Porro hic loci quamvis plurimi ægrotabant, morbus haud per contagium propagari, sed tantum prædispositos afficere videbatur, nam in eâdem familiâ cum affectis conversati, haud magis quam eorum contubernia vitantes, corripiebantur." Willis himself mentions that there was nothing peculiar in the epidemics of 1670 and 1671, for he says that he has often observed, that there are two different species of flux, which almost every year appear in London about autumn; in one

the stools are watery, with sudden prostration of strength: in the other they are bloody, but the strength remains tolerable. These autumnal fluxes are obviously cholera and dysentery; and though Willis has given them the same generic name, he so evidently referred to these two different diseases, that he apologises for employing the same title for both: "Quamvis dysenteriae vocabulum, communi acceptione alvi fluxum cruentum, sicut diarrhoea humoralem denotet; attamen, *salvâ etymologiâ*, libet nomen istud morbo huic Londinensi, etiam quando minime sanguinolentus est, applicare. Etenim sæpe et jamjudum observavi fluxus istius (qui fere quotannis hic circa autumnum increbescere solitus, vulgo ventris tormina appellatur), duas et *longè diversas*, esse species." Hoffman, too, whose accurate diagnosis between cholera and dysentery I have already given, understood full well the nature of Willis's dysentery incruenta, as appears by the very first passage in the chapter whence the diagnosis has been taken: "Cœlius Aurelianus, inquit, choleram per fellifluam passionem interpretatur, et Willisius *dysenteriam incruentam* adpellat."

That other intelligent writers have fallen into the same error of confounding cholera with dysentery, will appear by a short extract from Zimmermann, as quoted by Dr. A. Wilson, who thence infers that he (Zimmermann) would reduce cholera to the class of dysentery, if I am not misled, says Wilson, by what he states in p. 164: "The stools are sometimes

inconceivably copious, and this is so very dangerous a circumstance, that the patient will appear in a dying condition in the space of twelve hours, and often really dies in that time." Wilson properly remarks, that if this be cholera, 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, however, Zimmermann furnishes proof of having confounded these diseases in name only, and not in treatment, for 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(*a*). Willis mentions that spirits of wine burned on sugar was a popular and as it were epidemical remedy in the dysentery incruentia: "atque in tali dysenteriâ," he adds, "ferè semper proficuum, etsi in alterâ cruentâ indifferenter usurpatum, sæpe noxium deprehenderetur."

If we now review Willis's second species, the dy-

(*a*) His Treatise on Dysentery contains many excellent facts, though at times erroneous views, as when he maintains that Willis's dysentery incruentia, as also Degner's, was a genuine dysentery belonging to the malignant species, and that those who doubted it "seemed to him to deserve compassion." He follows up this misconception, however, by a very correct declaration (p. 156), that, in his opinion, "the various species of dysentery are not to be distinguished by the difference of the evacuated matter, but of the fever by which they are accompanied."

senteria *cruenta*, we shall see that it is identical with Sydenham's epidemic; for when Pringle states that they seemed to agree only in the name of the disease they are describing, he must have alluded to the first species, as Willis's dysenteria *cruenta* differs in no essential respect from that described by Sydenham and Morton.

Willis relates that a very cold winter and hot summer succeeded the autumn of 1670: "Autumnus anni 1670, dysenteriâ *incruentâ* insignem, hyems impensè frigida, qualem vix quisquam tunc vivens cognoverat, insecuta est: nec minus alterius extremi particeps erat æstas succedens, calidissima nempè et siccissima; ingruente posthæc æquinotio autumnali febris epidemica exorta, per totam fere Angliam grassabatur; intermittentis typum servans, paroxysmos modò quotidianos, modo tertianos habuit." "Dum hæc febris ruri per vicos et oppida ubique fere palabatur, Londini dysenteria *verè* *cruenta*, complures in sepulcrum subito præcipitabat. Et cùm dysenteria *cruenta* Londinenses afflixit, febris intermittens tertianæ hemitritææ similis reliquam Angliam infestabat:" to this epidemic fever he attributes many of the characteristic features of the dysenteria *cruenta*, just as Sydenham did to the febris *dysenterica*; so that these fevers would appear to be identically the same(*a*). With respect to the dysenteria *cruenta*

(*a*) "Febrem huic similem in Puretologia nostra," says Willis, "olim descripsimus, quam et similis anni constitutio præcessit." On referring to his description of this fever, we find in it many

itself, Willis says: "A prima hujus morbi invasione, plerumque cum ventris dolore, et torminibus, cruor copiosè et crebrò dejiciebatur; vigiliæ pertinaces cum febre et siti ingenti urgere solebant, usque ta-

circumstances detailed which shew that, though it bore the remittent character generally, it frequently lapsed into a continued fever, and was contagious; a fact which explains Willis's statement respecting the occasional agency of contagion in the production of his dysenteria cruenta. Of this fever he says, especially when mismanaged, "post unum paroxysmum vix finitum statim alter successit, indeque morbus periodos vagas et incertas sine *απυρεξίας* intercessu habere et *postea in febrem veluti continuam* transire solebat, cum malâ cerebri et nervosi generis affectione, ut non rarò lethargus aut delirium, sæpe etiam spasmi aut motus convulsivi excitarentur." "Insuper malignitatis cujusdam subibat notam et certa satis contagii atque perniciæ suæ documenta dederat." Though he would maintain that this fever "est proprie in intermittentium censum referenda," yet he relates several circumstances in which it differed from that class: amongst these are the absence of the cold stage, the want of *απυρεξία*, and also "quod facile per contagium in alios *subinde* propagatur." He also states that it sometimes happened "dysentericos affectus hunc morbum comitari;" et "dein morbum non sine miasmatis cujusdam communicatione *subinde* ad alios translatum suspicari est." "Propter hujusmodi apparatus," he adds, "dysenteria Londinensis ordinaria et non admodum maligna exoriri solet, quæ licet præ sedibus cruentis, horrenda statim et plerumque diuturna est, haud tamen valdè contagiosa, aut sæpius lethalis existit: verùm insuper, hic morbus interdum virulentus, et quasi pestilentialis, plures interficit, et miasma suum per contagium latè explicat." These details satisfactorily explain why Willis's dysenteria cruenta, like Morton's, was in general not contagious, and why it sometimes became so, by a change in the character of the fever with which it was conjoined.

men vires mediocritè constabant, ita ut affecti postquam circiter hebdomadam ægrotantes, fere vigesies quotidie dejecerint, lecto exurgere potuerint."

Sydenham's description of the same epidemic is, that it often began with strongly marked febrile symptoms: "Sæpe vero nulla antecedit febris præsensio, agmen autem ducunt tormina, dejectiones subsequuntur: dejectiones mucosæ sunt omnes, non stercorosæ, nisi quod nonnunquam stercorosa interponitur, idque sine dolore insigni. Interea temporis æger, si vel ætate floreat, vel cardiacorum ope incalescat, febricitat, linguâ subalbidâ quâdam mucilagine densé obsitâ, et si vehementius fuerit excalectus, nigrâ etiam atque siccâ. Prosternuntur admodum vires, dissipantur spiritus, nullum non adest febris *malè moratæ* indicium." Sydenham, however, in developing the singular tact with which he traced the course of epidemic constitutions, makes some comments which serve to shew that he, too, gave the name of dysentery to some affections not entitled to it. He compares the severity with which the dysentery set in, to that exhibited by the plague which had so recently raged, and which, at its access, at once cut off persons, "dum in triviis versarentur, nihil prorsus mali præsentientes;" such he states to have been the case with the dysentery, whose relative mortality was greatest at the commencement, but then he adds: "quo diutius perseverabat morbus, eo magis humoralis videbatur, primo enim quo invasit autumno, quamplurimi *nullis*

omnino dejectionibus molestabantur (!!); torminum verò quod spectat atrocitatem, febris intensionem, subitam virium prostrationem aliaque symptomata insequentium annorum dysenterias longo intervallo post se reliquit." He states elsewhere that these "tormina sine dejectionibus neque naturâ suâ neque illâ quâ promptissimè sopiebantur methodo, a dysenteriis multum dissideant."

Having thus disposed of Willis's two species of dysentery, and proved, upon his own shewing, that his *first* (the *incruenta*) was simple cholera, unattended by contagion, and that the second species (the *cruenta*) was the ordinary dysentery of London, sometimes contagious and sometimes not, according to the fever with which it was conjoined, I shall now endeavour to shew, and upon similar grounds, that the epidemic described by Degner under the imposing title of the "*dysenteria bilioso-contagiosa*," was not entitled to that designation, diseases, dissimilar in their nature, being often thus confounded in name.

I must admit that, when preparing the first edition of this work, and occupied in multifarious references, I was, to a great degree, imposed on, as others before me have been, by this assuming title, and that I was inadvertently led, in that edition, to give to Degner's epidemic a place to which I do not now consider it entitled. Indeed, after a careful

perusal of his statements, and a close scrutiny of the few cases he has furnished as illustrative of his epidemic, I can no longer view it as genuine dysentery, but must class it, along with Willis's dysenteria *incruenta*, under the head of cholera. Zimmermann, when discussing Degner's merits, says, that he "has not well distinguished the bilious or putrid dysentery from the malignant, in the history of his epidemic, which was *compounded of both species*." In that opinion, founded as it is on the assumption of the disease being really dysenteric, I cannot concur, though I do in his protest against Degner's statement, that "contagion is the chief occasional cause of this disorder with every body." Degner, indeed, goes so far as to say that a true and exquisite dysentery is "*per se et suâ naturâ contagiosa, adeo ut contagium inter signa pathognomonica veræ dysenteriae reputetur*." He, however, candidly admits that he saw whole families "*in quibus ne unicus ab hoc malo immunis manebat: vidit alias in quibus pauci adfligebantur et etiam alias, in quibus unicus tantum ab hoc morbo infestabatur(a)*." Notwith-

(a) Among his various arguments for establishing the contagious character of his epidemic, he mentions that up to a certain period "*Neminem extra pomœria nostra hæc lues adflixit, ut sola nostra civitas ab eâ tacta fuit*." Now Willis, in his account of the dysenteria *incruenta* (which, being cholera, was, as he states, not propagated by contagion), mentions a similar fact respecting London: "*Dumque intra hanc urbem dysenteria ista popularis adeo immaniter grassata est, ruri aut saltem ultra tria milliaria fere nullus decubuit*."

standing this equivocal evidence, he at great length insists that his dysentery was the product of a "fermentum specificum, subtilissimum, peregrinum, venenatum, maximè corrosivum, non solum humores nostros in dissolutionem putredinosam convertens, sed et exhalationis specie alios contactu mediato vel immediato inficiens et ad æqualem corruptionem disponens." He further infers, that as the poison of arsenic induces the same precise symptoms as occurred in his dysenteric cases, therefore the dysenteric poison partook of an arsenical character(*a*). Now, these symptoms, according to his summary of them, were "cardialgia subita et summa, constans conatus vomendi, vomitus actualis, *profluvium alvi continuum*, biliosum, cruentum, purulentum,

(*a*) In pp. 104-5, he details, in illustration of this position, a curious case (similar to one which occurred in our city during the prevalence of epidemic cholera in 1832), of a man who died on the third day after suffering all the symptoms of his epidemic dysentery. No physician was called to his aid, and, under the pretext that he died of the dysentery "sine ulteriore inquisitione sepultus est." But as there had been perpetual broils between husband and wife, and as she had not only utterly neglected him, and exhibited no signs of grief at his death, "verum, uti ante ejus obitum, sic etiam post, cum aliis amatoribus versata fuerit," she was strongly suspected of having poisoned him: "dico," says Degner, "suspicio nata fuit, rem affirmare nolumus, cum ambo venena, dysentericum nempe et arsenicale similem inter se edant effectum." Now Hoffman, on the contrary, when speaking of dysentery as producible by poisons (Part 3. tom. 4, c. 8, p. 493), says, that "quæ ab auctoribus de dysenteria a sumptis venenis excitatâ referuntur observationes, eas *ad choleram omnes* adplicandas esse puto."

dolores intestinorum intensissimi, eorum corrosio sine notabili febre, huic insequens inflammatio, &c., quibus tandem accedebant singultus, convulsiones," &c. The production of these symptoms he refers to the immediate agency of acrid and putrid bile, which he considered as the seat of the dysenteric poison, and that it is capable of producing such powerful effects he instances in the case of cholera, which (in p. 111) he states to be a disease, "dysenteriae biliosae aliquo modo affinis (*præterquam quòd contagiosus non sit*) et in quo saburra biliosa adeo caustica et corrosiva evadit, ut veneni septici instar, non tantummodo vias per quas transit arrodât et exoriet, eoque ipso magnos cruciatus, dolores, cardialgiam summam, vomitum, alvi fluxum, aliaque lethalia et dysentericis maxime familiaria symptomata excitet," &c.(a)

(a) Sydenham's description of the cholera of 1669 is worthy of transcription, as a parallel to that of Degner's dysentery: "Malum ipsum," says Sydenham, "facile cognoscitur: adsunt enim vomitus enormes ac pravorum humorum cum maximâ difficultate et angustia per alvum dejectio; ventris ac intestinorum dolor vehemens, inflatio ac distensio: cardialgia, sitis, pulsus celer ac frequens, non raro etiam parvus et inequalis: insuper et nausea molestissima, crurum et brachiorum contractura, animi deliquium, partium extremarum frigiditas, cum aliis consimilis notæ symptomatis, quæ astantes magnopere perterrefaciant, atque etiam angusto viginti quatuor horarum spatio ægrum interimant." In further illustration of the disease he refers to the intense severity of the cholera epidemic in 1676, when, from the unusual heat of the season, the disease exhibited a greater violence of convulsion than he had ever previously witnessed: "Neque enim solum ab-

Degner has thus furnished us with such a detail of the symptoms occurring in his epidemic, and in cholera, as plainly manifests very little essential difference (if any) between them; some suspicion of which would, at times, seem to have crossed his mind, the great objection to designating his epidemic a cholera being, that the latter is not contagious, or, to his admitting it to be (which many of his cotemporaries evidently maintained) a "fluxus hepaticus," that this last is neither contagious nor epidemic. But, independently of this general identity of the two diseases, if we examine his *four* cases, and his observations on particular symptoms, we can scarcely hesitate in pronouncing his "dysenteria bilioso-contagiosa" to have been an epidemic cholera, which, either from its intense severity, its neglect or mismanagement, frequently terminated in the "symptomata dysentericis maxime familiaria," though, in the first instance, these latter symptoms did not manifest themselves(*a*). Let the reader

domen, uti aliàs in hoc malo, sed universi jam corporis muscoli (brachiorum crurumque præ reliquis), spasmis tentabantur dirissimis, ita ut æger e lecto subinde exiliret." He details a case of this character, in which ("cum pulsu vix micante") he succeeded, by repeated doses of laudanum, in rescuing the patient from the grave.

(*a*) Dr. Copland well distinguishes the two diseases, and in p. 712 makes an observation specially applicable to Degner's epidemic, when he says, "it should not be overlooked that cholera sometimes runs into dysentery, evidently owing to the irritation excited in the large bowels by the morbid secretions poured into

compare Willis's description of his dysenteria incruenta (i. e. cholera), with that given by Degner, and but little difference will be found between them, except in the greater intensity of abdominal pain sometimes experienced in the latter: "Accedebant statim," says Degner, "sub initium mali, fastidium ciborum, nausea, conatus vomendi, imo vomitus actualis, unà cum magnâ virium prostratione: in aliis statim a principio morbi lypothymiaë erant frequentes, maxime vero ægrotos angebant cardialgia summa, dolores, torsiones et cruciatus intestinorum gravissimi, utpote molestissima morbi symptomata." In the milder cases, he says, "excrementorum fœtor non adeo intolerabilis: imo in quibusdam fœces fere sine omni odore erant: tormina et cruciatus intestinorum non adeo erant sensibilia, imo nonnulli plane iis carebant." Then of the tenesmus, an essential symptom of dysentery, he merely says (p. 18), "tenesmus vir tolerabilis, *complures* excruciat;" and in p. 184 he employs these remarkable words (shewing the frequent absence of this symptom),

the digestive canal." Degner himself mentions a remarkable fact confirmatory of this view, and opposed to his own opinion of contagion in the disease, which, he says, attacked neither the French nor Jewish residents in the town. In p. 30 he writes thus: "Deinde et hoc singulare videtur, morbum in principio grassationis suæ jam satis acutum et lethalem, in sui decremento maligniorem fere evasisse, violentioribusque stipatum fuisse symptomatibus: id quod sane contra vulgarem contagiosorum morborum indolem evenisse videtur."

“ubi vero tenesmus malum comitabatur,” &c. As to the amount and character of the alvine discharges, these he states to have been at first of the colour of beer: “flavæ aut potius subrufæ, sed sensim aut fiebant porracæ, æruginosæ aut subcruentæ,” and he adds that the matter ejected by vomiting differed but little from the alvine; then in p. 20, endeavouring to give some idea of the quantity he says: “Mente vir concipi et credi potest quanta liquidorum humorum copia in hoc morbo per intestina deposita sit. Totum fere corpus in liquidum solvebatur et per intestina expurgabatur, ægrique tanta macie conficiebantur, ut nil nisi sceleta sine ullo succo adparent.” “Nonnullos intra tres vel quatuor dies ita mortuos vidi.” “Febris in aliis satis notabilis, in aliis verò plane nulla;” “certam periodum febris non servabat; quandoque schema tertianæ anomalæ ludere videbatur;” “et hæc mihi ratio videtur (p. 33) cur in quibusdam dysenteriâ ægrotantibus se species febris tertianæ manifestarit, dum ægroti tertio quoque die gravius laborarunt(*a*).” Should any doubt of the real character of this epidemic exist after a perusal of the foregoing passages, it must be re-

(*a*) On another occasion (p. 23), speaking of the fever which at times attended the disease, he says: “Non omnem morbi adsultum comitabatur febris;” and to this he adds the remarkable fact, so characteristic of severe cholera, but not of dysentery, viz.: “Quo gravius enim malo laborabant ægri et quo citius inde jugulabantur, hoc mitior sæpius febris ex pulsu dijudicari poterat, et defectu potius quam excessu ut plurimum errabat pulsus.”

moved by a reference to the four cases recorded by him (p. 37) as illustrative of the disease. In none of them is tenesmus mentioned, and all had most copious liquid stools, with other symptoms indicative only of cholera. The first patient being attacked with the usual symptoms of continual vomiting and diarrhœa (the discharges both ways being "magna quoque quantitate"), was, on the second day, synoptic; on the third, had an intermittent pulse, "qui die quarto plane deficiebat: facies mortem propinquam significabat: *algebat per totum corpus* et sub continuis his symptomatibus die octavo exspiravit." The second patient, in like manner, after the usual symptoms, had intermittent pulse on the third day, which altogether ceased to beat on the fourth, attended with coldness, not only of the extremities, but of the whole body; and though the heat and pulse gradually returned on the fifth and sixth days, and with hopes of recovery, yet on the seventh and eighth days a hiccough was occasionally heard, "et nono die placide exspiravit." The third patient was attacked at night, when "*dysenteria cum vomitu adeo vehementer eum incessit ut viribus maxime prostratis, altero mane crederes eundem jam per mensem decubuisse.*" The disease, however, ceased within two days: "evacuatâ multâ bile flavâ et viridi(a)." The fourth patient (a female, "octogenaria

(a) Degner indicates elsewhere that this free discharge of bile was a good sign in his epidemic, though he attributes much of

sana sibi visa") was suddenly attacked in the morning, after returning from church, "vomitus ac dejectionibus alvinis, adeo frequentibus, conjunctis modo lypothymiis, modo convulsionibus intercurrentibus [were not these the cramps of cholera?] ut circa horam 11^m P. M. jam per semihoram pro verè mortuâ haberetur: rediit tamen sensim ad se et intra biduum faucibus mortis erepta est." Who can read these characteristic descriptions, and not at once recall to memory the scenes incessantly presented to our eyes during the prevalence of epidemic cholera in 1832? The frequent and copious discharges upwards and downwards; the rapid sinking and altered appearance; the quick failure of the pulse, and its occasional revival; the coldness, not merely of the extremities, but of the whole body; the "cardialgia summa et subita;" the rapid fatality, together with other symptoms which he elsewhere enumerates; the "oculi insigniter cavi," and the "voce privantur," evince the close consanguinity, if not identity, of Degner's epidemic with the cholera of 1832, though this latter, as did Degner's occasion-

the disease to the acridity and not to the absence of that secretion. "Bonum fuit indicium qui bilem flavam et *adhuc magis*, qui bilem viridem, quocunque morbi tempore, vel sponte vel medicamento exhibito, vomitu vel dejectionibus alvinis, excernebant: hoc ipso sæpe omnis mali fomes quasi una vice auferebantur." Such a discharge is good in dysentery, but still more so in cholera, as being more decisive in the latter of the removal of the great cause of the disease.

ally, attacked its victims under two different forms, characterized chiefly by the absence or presence of the acute epigastric pain. There can be little wonder that Degner should deem his epidemic contagious (though disputed by his contemporaries), when we know what contrariety of opinion (even after all our experience of the disease) prevailed, and still prevails in the Profession, respecting the contagious character of the Indo-European cholera of 1832. I had some experience of it under very peculiar circumstances, whence I concluded it to be diffused, like influenza, by atmospheric influence, and not by contagion, except, perhaps, in those cases where the patient rallied from the first attack, and died of the consecutive fever(*a*). Degner, however, will admit

(*a*) I have stated those circumstances, and the grounds of that opinion, in a paper published in the third volume of the Dublin Medical Journal for 1833. The late Dr. James Johnson, in his Review for January, 1832, p. 272, anticipated the same view of the subject: "Is it not more rational," he asks, "to attribute this [i. e. cholera], as well as other epidemics, to some general cause, of which we are ignorant, while the emanations from the sick in crowded, filthy, and unventilated apartments endow the epidemic with a malignity and *even an infectious character*, not attendant on its origin or essential to its nature? Can we much wonder that in such places, where whole families were huddled together in one room, with little food and less clothing, a malignancy and infection were superadded to the original epidemic? The same takes place in fever, in dysentery, in erysipelas, and in many other diseases." And elsewhere (p. 208), he says it is clear that the proportion of cases in which fever follows the choleric symptoms is much greater in Europe than in India, and this may

no cause but contagion, and thus very summarily rejects atmospheric influence. "*Creduli,*" he says, "infestum aeris inquinamentum cœlitus demissum, aeremque ipsum corruptum esse venditant."—But Roederer, a more philosophical physician, does assent to such contamination, and in admitting the great power of the seasons and their changes "ad fovendum vel destruendum seminium quoddam morbosum epidemicum," adds, "quænam autem aeris dispositio atque mutatio ipsum apparatus producant, et quænam pro singulis epidemiarum varietatibus causarum concurrentium diversitas requiratur, *ignoramus*. Non semper eadem aeris cœlique temperies eademque vicissitudines, certi cujusdem generis morbi primordia evolvunt eandemque ægritudinis speciem producunt. Singulare atque incognitum *θεῖον* quoddam pondus, in aere diffusum, epidemicam morbis notam imprimat, necesse est." The same author, when reviewing the general character of epidemic constitutions, accurately describes the apparently anomalous course of Asiatic cholera, when visiting Europe. "Adeo lento tractu morbi

well account for the greater cause which we have for suspecting a contagious character in the European disease. Fevers of all kinds are now generally acknowledged to be capable of taking on a contagious character under circumstances of accumulated population, filth, and deficient ventilation. The fever of cholera may, therefore, like that of dysentery, pneumonia, and other diseases, assume the fearful and dangerous attribute which the above circumstances confer."

epidemici integras interdum regiones pervagantur, ut presso quasi pede eorum vestigia legere et consecrari nobis liceat: neque verò ventorum iter aut lineam quandam rectam, sed alios tramites, nondum satis extricabiles, lineamque obliquam sequuntur, *factis interdum in progressu saltibus.*" This last peculiarity constituted a remarkable feature in the progress of Asiatic cholera on its visit to these countries, and was also noticed in Degner's epidemic, which, so far as I am competent to form a judgment upon the evidence he has supplied, began as cholera, and terminated occasionally in dysentery of bad type, and, in such cases, probably contagious. I have dwelt on the subject at greater length than it may appear to merit; but I have done so because every writer (myself among them) would appear to have taken for granted that his was a genuine history "de dysenteriâ bilioso-contagiosâ."

Having thus disposed of this and other collateral questions, I now proceed to consider the last and most important combination of dysentery, that with contagious fever.

CHAPTER IV.

COMBINATION OF DYSENTERY WITH CONTAGIOUS
FEVER.PROOFS FROM SYMPTOMS, AND FROM THE GENERAL HISTORY
OF THE DISEASE.

THIS combination constitutes the true "pyrexia contagiosa" of Cullen, to which alone his definition of the disease is applicable; to which alone the property of contagion belongs; and to which may be attributed all the horrors usually annexed to the very name of dysentery. The strong features of this combination are very clearly marked in the most celebrated epidemics of the disease; and between this and all other forms will be found the most decided difference in access, progress, treatment, and attendant danger. In conducting this portion of our inquiry, we have to establish two important *propositions*, viz.: 1st, That such a combination of dysentery does really exist; and 2ndly, That this combination alone is contagious. Each proposition shall be considered separately; though in discussing the first, it will be impossible to avoid occasional anticipations of the second. The proofs

necessary for establishing the first proposition are arranged under two distinct *heads*. Under *the first head* are considered such proofs as may be deduced from symptoms marking the presence of continued fever, and from those circumstances in the general history of each epidemic, whence may be inferred the presence of that disease. Under the *second head* are comprised such proofs as may be drawn from the mode of treatment suited to the different forms of dysentery. As this species of proof, however, comes under consideration more appropriately when discussing the general treatment of the disease, I shall defer it till we enter on the latter subject. The proofs of the second *proposition* will be found in the subsequent chapter on the contagion of dysentery.

As an appropriate introduction to such an investigation, I shall here propound a few ingenious queries proposed by the intelligent Rollo, who, though led, and seemingly warranted by his own experience in the intermittent and remittent forms of dysentery, to deny contagion in the disease, was yet neither so vain of that experience, nor so presumptuous in its infallibility, as to disbelieve, on negative evidence, the positive testimony of others, that the disease is occasionally propagated by such agency. When admitting that fact, however, he proposes the following pertinent 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, as arising from the marsh effluvium, become, in any situation, contagious?" And again, he asks another question which, in a great measure, embraces the preceding: "May 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?"(a) These queries there will, I would hope, be little difficulty in answering, after a perusal of this and the subsequent chapter. For further information I refer with pleasure to Rollo's work (p. 32), and to his extracts from Lind's paper on Fever and Infection, with the observations and queries suggested thereby: their perusal gave me peculiar satisfaction, as the same suggestions, and a similar train of reasoning, led me to the present investigation long before I had met with Rollo's treatise.—But to proceed with our proofs.

We have seen that a few well-marked phenomena constitute dysentery; and that fevers, attended with intermissions and remissions in conjunction with these phenomena, constitute the intermittent and remittent

"It is not improbable," says Mr. Annesley (in his work on the Diseases of India, p. 207), "that some diseases, which at first originate from an epidemic influence solely, may become contagious, or rather infectious, from the circumstances in which those affected by them may be placed; as from imperfect ventilation, want of cleanliness, crowding of the sick," &c. "We have," he adds, "proofs of such occurrences in fevers and dysentery," &c., &c.

forms of that disease ; can it then be unfair to conclude that, if the symptoms of continued fever be found to exist in combination with those of dysentery, they must together constitute that form of disease we are about to discuss. That such a combination does take place, few, I trust, will call in question, after a perusal of this and the subsequent chapter.

No author can more distinctly declare the real nature of contagious dysentery than Etmuller. After discussing the general productive cause of dysentery, which, in common with the more ancient writers, he attributes to some acrid substance irritating the intestines, he observes (tom. ii. p. 149): “ Porro ratione hujus causæ materialis, 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, interdum pestilentiali, simulque epidemicè grassatur, ac sæpe integram regionem devastat, et potenti contagio se multiplicat.” He further adds (p. 142): “ In malignâ sæpius est conjuncta febris plus minus intensa, eaque interdum petechialis. Lingua subalbida, mucilagine densè obsita, et si vehementius æstuet corpus, nigra etiam et sicca observatur, unâ cum *summa viriam prostratione*. Grassatur communiter in incolas instar pestis, estque contagiosa, seque tunc temporis sub formâ *febris malignæ*

multiplicat." That Etmuller and other authors, in using the term malignant fever, thereby in general understood a contagious fever, such as our typhus, will appear by the following passage from the same author. Speaking of malignant fevers he says (p. 376): "Porro febres malignæ sunt vel contagiosæ, 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(a)."

Rogers, in his Essay on the Epidemic Diseases of Cork, furnishes satisfactory evidence of the real nature of the dysentery now under discussion: he has clearly marked the constant connexion existing between the endemial epidemic fever or malignant typhus of Cork, and the malignant dysentery: he

(a) Zimmermann (p. 162), states that "the circumstance which impresses on a dysenteric complaint the peculiar mark of malignity" is this, that, "with the causes common to that disorder at all times, others are joined which corrupt the humours very quickly. The malignant dysentery, therefore, is that in which, either from external causes, or from a putrid fomes within the body, a malignant fever supervenes; the pathognomonic signs of this species being formed by the symptoms of a malignant fever mixed with the usual symptoms of a dysentery." He further explains his meaning (p. 158), by stating that dysentery is sometimes a symptom of intermittent fever, and also of petechial fevers at the beginning: "It also very often terminates, or is a concomitant symptom of, putrid and malignant fevers; but when a malignant fever supervenes on a dysentery before subsisting in the body, this is quite a different case, and constitutes a peculiar species of dysentery."

states the years in which this destructive fever prevailed, when also dysenteries of a very malignant sort were common, the winters of 1728, 1729, and 1730 (during which this fever was again renewed), being also notoriously infamous for bloody fluxes of the worst kind. He remarks that these epidemics of fever and dysentery did not admit of the treatment recommended by Sydenham, but, on the contrary, only yielded to the highest alexipharmac course of medicine, and the most generous rule of diet; and 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. He adds 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.

Sir J. Macgregor, whose valuable testimony applies to the three combinations of this disease, in the sixth volume of the Medico-Chirurgical Transactions, states (p. 421), that "intermittents of long standing, as well as dysenteries, appear to have some connexion with the liver or the biliary secretion; and that

these diseases were found to be generally connected in the Peninsula, and intermittents were found to terminate frequently in dysentery. Sometimes dysentery from its commencement appeared to be unattended with fever, and would, in a very short time, run into the chronic stage. In such cases, however, he is inclined to believe the disease to be symptomatic either of disease of the biliary system, or of the mesenteric glands. In most cases he thinks dysentery was accompanied by fever of the inflammatory type: "The type of fever, however, accompanying dysentery was," he says, "very much modified by that of the prevailing epidemic. In the hospitals in the Alentijo and Estremadura in 1812, intermittent fever prevailed or accompanied dysentery, and remittent fever, when the army advanced so rapidly and remained some time stationary in the two Castiles, in July, August, and September. Every case of dysentery which appeared in the battalions of the Guards in 1812 and 1813 was accompanied by the typhus gravior, and very generally had a fatal termination, as did many at Ciudad Rodrigo, &c., where the same form of fever was prevalent." At that place, the situation of which is unhealthy, aggravated by the interment of 20,000 bodies in the course of a few months, the mortality was great. "The changes from intermittent to dysentery were very common, and seemed to suspend the intermittent for a time, but on the removal of the dysenteric affection the intermittent returned; in some instances both dis-

eases attacked the same patient at the same time, and then the dysenteric symptoms were aggravated."

I now proceed to consider the opinions of an author, who had witnessed the disease in its worst forms, and whose observations may be relied on as the result of extensive experience and sound judgment. Pringle's general account of the dysentery is, "that early in the season some cases of the disease 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 disease, in the more early cases, was simple dysentery, of a mild character, and not very frequent, which in autumn, from the peculiarities of that season, became epidemic, and *from another cause*, contagious; as it is obvious from the context 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 their being confined in *fixed camps*, the great source of contagion and malignity in diseases of the army(*a*).

(*a*) "The village of Feckenheim," adds Pringle, "was taken for an hospital, into which about 1500 sick were sent from the camp, and of that number the greatest part ill of dysentery, by which means the air became so vitiated, that not only the rest of the patients, but the apothecaries, nurses, and other attendants, with most of the inhabitants of the village, were also infected. To this acceded a still more formidable disease, the hospital or gaol fever,

I have already mentioned the doubts entertained by Pringle of the truth of Sydenham's assertion—that the disease, even in the majority of cases, began without any feverish sensations. We should, no doubt, be slow to suspect the accuracy of Sydenham; but we should equally respect the motives, and well weigh the grounds which might lead others to entertain such a suspicion. Pringle had reasonable grounds for his doubts, as he had such frequent occasion to observe the disease, after its genuine character had been materially altered by *fixed camps* and military hospitals, where it seldom attacked without well-marked fever. The following passages well warrant this assertion. After describing a low and dangerous kind of fever which sometimes

an inseparable attendant of foul air from crowds and animal corruption. These two combined caused a great mortality; while on the other hand such as were seized with the dysentery *and not removed from the camp*, though wanting many conveniences, kept free of this malignant fever, and commonly did well." "At Feckenheim few escaped; for how mild or bad soever the flux was, this fever (the malignant) almost surely supervened: the petechial spots, blotches, parotids, frequent mortifications, contagiousness, and the great mortality, set forth its pestilential nature." Out of fourteen mates employed about the sick, five died; and all the rest, with one or two exceptions, had been ill and in danger. The hospital lost nearly half the patients, but the inhabitants of the village were almost entirely destroyed by the dysentery and fever together. "From all which," says Zimmermann, I conclude that not only a fever of different kinds for the most part accompanies dysentery, but that this fever is, in certain circumstances, of the most alarming nature."

accompanies the 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 *gaol distemper*, which at all times infects foul and crowded wards, but never so much as when they contain men labouring under a putrid disease." In another place he takes notice of Degner's epidemic, and remarks: "As to the violence of the symptoms mentioned by that author, I own it exceeds anything I have observed upon the first seizure, but when a number of men, *even with the most favourable cases*, have been crowded into the hospitals of the army, the dysentery has *then* appeared with all the virulence that it did at Nimeguen(*a*). Pringle further 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, as he says elsewhere, "It is to be apprehended that when a single person is taken ill of any putrid disease (such as dysentery), &c., and lies in a small and close apartment, he may fall into this malignant fever."*(b)*

The next author I shall quote furnishes not less

(*a*) From the desultory character of Degner's work few can or do read it with the attention requisite to detect the misnomer he has given his epidemic; and hence the admissions made by Pringle and other writers.

(*b*) This last assertion of Pringle's would appear to be sustained by a singular case recorded in page 163 of my work on the Epidemic Fever of Ireland, &c.

convincing evidence. In the Appendix to the third volume of the *Acta Nova N. C.* we have from Grimm three essays on three different diseases, which appeared in the same territory in three successive years, and which he introduces to our notice in these remarkable words: “Grassati sunt nunc delineandi epidemii et *tandem* contagiosi morbi ab anno 1759, ad annum usque medium 1762, nec raro quibusdam proprietatibus inter se copulati.” And yet he states that, as to the general health of the locality, “adeo salubris urbs illa et omnis fere regio (Isanacensis) ut nisi tota Germania patiatur [as it then did from the ravages of war(*a*)] vix unquam epidemiiæ ægritudinis et contagiosæ cujusdam incommoda patiatur.” The first essay contains the history of a malignant fever, “quæ in urbe et agro Isanacensi, epidemicè grassata est;” the second that of a “dysentericiæ malignæ, quæ autumnò an. 1760 et 61, in iisdem locis grassata fuit;” whilst the third describes the malignant epidemic measles of the year 1762. The fever, which began in the December of 1758, raged throughout the spring of 1759, when it remitted somewhat of its violence, though for the remainder of that year “semper aliqua morborum occurrentium in malignitatem pravitas remanserit.” “Febris

(*a*) Besides the ordinary occasional causes for the production of disease, &c., originating in the state of the seasons, he says: “Ad has autem potissimum publicas a bello productas calamitates, anxias incolarum curas et timorem futuri mali referendas esse arbitror.”

erat maligna, epidemia et contagiosa” “ ut quartus quisque ferè homo ab ista febre correptus, trucidaretur.” It revived in the winter and spring months of 1760, and in the August following gave way to another and more destructive plague, the dysentery, which continued its ravages to the end of November, when it again passed into the malignant fever ; and so striking was the connexion between both, that he remarks: “ Eadem maligna febris, pro diversâ anni tempestatis ratione, modo exanthematica, modo dysenterica appellari mereatur.” “Vidi,” he says, “ dysenteriâ laborantibus (eâdem calidis violenter sedatâ), petechias et miliaria erupisse, malignas autem febres, dysentericæ similes diarrhœas fœtidissimas secum duxisse:” et “ si qua forsân fieri potuisset subitanea veris in autumnum, autumnî in ver mutatio, tunc subitò ægroti dysenteriâ malignâ in petechialem febrem, petechialis verò febris in dysenteriam transiisset.” The fever, from his description, would seem to have been of the worst possible character(*a*), and

(*a*) Judging from Grimm’s details, few typhoid epidemics have exhibited more formidable features, whether we look to the head, chest, or belly: “Quanta autem humorum corruptela, cadaver abunde demonstravit: nullum enim cadaver et sub frigidiorè aura riguit: molle potius omne et flexile permansit: citissimè incredibilem odorem sparsit. Aliquando venter et collum ad rupturam usque intumuit.” In one respect it differed from our epidemic fever of 1817, which quickly and more fatally assailed those of the better classes: “Memorable esse videtur,” says Grimm, “hunc morbum *citius graviusque* illos comprehendisse, qui miseram vitam degebant, *tardius* autem et verè jam contagiosum, *mitiusque* illis contigisse, qui meliori fruebantur sorte.”

that the dysentery in question was a combination therewith, will further appear from the subsequent passages. (P. 173) he thus begins his account of the disease: "Alter morbus, qui tantâ atrocità plurimos est adortus et multos sustulit, dysenteria *cum malignâ suâ febre* copulata, fuit." "Hæc alvi dejectio cruenta febrem malignam periculo equavit, dolorum immanitate superavit." Like Morton and Clarke he viewed the disease as a symptom of the malignant fever, as they did of the remittent, for he distinctly says, "Febrem verò morbum primarium valdè malignum, ex peculiari humorum corruptelâ obortam, intestina præ reliquis visceribus in abdomine sitis, malè afficientem." Indeed the circumstances under which it arose and spread strongly confirm our impression of its special character: "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." He further adds: "Memorable præ reliquis puto, morbum eo tempore imprimis familiarem vitæque infensum fuisse spurcitiei deditis, pedibus nudis incedentibus, in conclavibus angustis, obscuris, madidis, mox frigidulis, mox vehementer calefactis viventibus, tenui victu et duro utentibus, in ratione spurcitiei, angustiaeque hypocaustorum, vitæque miserâ." "Neque prius disparere iterum hæc lues (dysenterica) potuit donec mutatâ cœli naturâ, occasionales caussas imminuit, et contagium, *morbi effectum et caussam*, iterum suppressit." Among

a variety of other circumstances he remarks: "Comprehendit mox morbi initio ægros *debilitas tanta*, ut nec pedibus initi, nec caput tenere erectum potuerint: aliis paullò tardius accessit insignis illa virium imminutio:" and in speaking of the manner in which death took place he says: "Ratio vitam ponendi omnibus ferè eadem, ac in febribus malignis fuit, nono die ad duodecimum." Indeed the whole history of the disease, its causes, its termination, and its propagation, so strikingly resemble typhus, as to leave little doubt of the specific character of this combination, *a character*, marked by some singular traits in the epidemic we shall next epitomize, as described by Roederer.

That epidemic, to which he gave the name of "morbus mucosus," is well worthy of attentive study, from the peculiar circumstances under which it occurred, and from the changes in character which it consequently exhibited. In July, 1760, he states intermittent fever (often of a malignant type), to be prevalent, and from August to November dysentery: "quæ brumali tempore, pauciores aggressa est, tandemque indolem genuinam exuit." "Sensim evanescit dysenteria vel potius transitu facto, degenerat in epidemiam mucosam et jam multum verminant ægri." "Febris mucosa acuta interdum cum typo hemitritæo decurrit: haud rarò in speciem malignam biliosam vel putridam adscendit, præcipuè in nosocomie castrensi." In two months after (March, 1761), he states that this "morbus mucosus in petechizan-

tem evehitur, junctis deliriis furiosis et soporibus," constituting his "Febris mucosa acuta maligna." He then points out the close similitude of the morbus mucosus with the dysentery: "Eadem utriusque est origo ex intermittentium epidemia, eo saltem discrimine, quòd dysenteria genetricem proximè excipiat: morbus mucosus ex dysenteria nascatur. Ut plurimum incipiunt ex prægressa diarrhœa: urgent in utroque morbo nausea, vomituritio, sitis, borborigmi, frequens ad desidendum stimulus, tormina ventris. Dysenteriam æmulatur febris mucosa dejectionibus mucosis, biliosis, putridis, quin interdum inter tenesmos cruentis," &c., &c. "Cadaverum saltem analoga est ratio quoad permulta phenomena: inflammata villosa, escharæ etiam gangrenosæ in superficie internâ crassorum, hepatis livores, pancreas durum, et infarctum pulmonum parenchyma, utrobique occurrunt." As he thus gives us the analogical morbid appearances, it will not be out of place also to point out in what respects they chiefly differed, as in connexion with the special causes productive of each, we may thence be enabled to explain the difference between the two diseases. Now the morbid appearances in dysentery he describes as being nearly the same with those observed by Bonetus and Pringle, viz., "Intestina multum inflammata, passim gangrenosa: et quo a ventriculo erant *remotiora*, eo majori in gradu depravata. Tunica villosa *tenuium*, veluti arte anatomicâ, vasculis pictis injecta, copiosissimis punctis parvisque areolis nigris

conspersa: superficies interna *crassorum* lacera, inæqualis, igne quasi combusta, obscurè rubra, quin nigricans." "Raro in *crassis* lumbricus unus vel alter hospitatur: *tenuia* nil vermium recondunt atque tunica interna, licet inflammata, tamen continua est: hepar striis lividis variegatum: substantia interna vitii conspicui expers est. Lien et reliqua viscera labe notabili carent. Pancreas multum induratum." Such are the chief morbid appearances in the dysentery, whereas at the *commencement* of the morbus mucosus he says: "Folliculi mucosi ventriculi et intestinorum eleganter conspicui sunt in cadaveribus: hepar acinosum: superficies intestinorum crassorum escharis tegitur, ut in dysentericis. Ipsa intestinorum substantia crassa deprehenditur et propter inflammationem plane singularem, villosæ imprimis, per reliquas tunicas transparentem, colorem cærulescentem intestina referunt." "Uberior in cunctis folliculis mucæ secretio: et hinc non solum ventriculi et intestinorum, *tenuium imprimis*, faciem internam obvestit magna vis mucæ viscidæ, tenacis, ægre abstergendi, sed subtus etiam conspiciuntur folliculi, mucæ stagnante pleni: sæpius in conspectum veniunt istæ mucæ lacunæ in ventriculo et duodeno: rarius in reliquo tenuium tractu, et vix in corpore sano isti folliculi sine arte deteguntur." From this accumulation of mucus the fitting nidus was prepared for worms, of which great quantities, and in great varieties, were engendered, much aggravating the intestinal irritation, and, together with a conges-

tion of acrid bile, changing the character of the disease, "ut in primis viis veram putredinem concipiat, atque ex putrida et mucosa indole morbus componatur." "Quum autem per epidemiæ naturam multum adhuc fureret indoles putrida, morbus inflammatorius fiebat, sed maligno modo. Tunc præcipue siccitas pelvis, cum pertinaci interdum alvi obstructione simulque nisus in caput atque deliriorum soporumque satellitium notata sunt. Prona etiam fuit morbi indoles ad expellendas *petechias*." "Sensim frequens inflammatorii maligni connubium cum siccitate pelvis, nisu sanguinis in caput atque exanthemate petechiali fuit."

Now the simple inference I draw from the rather complicated details of Roederer's work is this: that (under circumstances to be detailed), epidemic dysentery set in at the usual season, appearing in all its varieties(*a*); that in the course of the winter season this disease lapsed into the morbus mucosus, attended

(*a*) In bad cases, Roederer says, "Statim vires franguntur, urget sitis, frequenti ad desidendum stimulo lacessitur æger, junguntur tormina et dolores atroces in imo ventre cum tenesmo, &c. Lingua rubra, sicca, aspera, in sulcos finditur veluti exulceratos." "Ingravescente morbo, interdum die 5^{to} inflammantur genitalia cum regione ani: sensim inter atrocissimos sub dejectionibus alvinis et urinæ missione cruciatus, excoriantur, exulcerantur et tandem fiunt gangrenosa." After enumerating other bad symptoms, familiar only to the worst cases of typhus, he thus concludes the description of the final scene: "Subsiliunt tendines, convelluntur artus, imprimis superiores et tandem ineluctabilis mors scenam claudit."

with every variety of fever, remittent, inflammatory, and malignant: that the dysentery, as appeared from dissections, affected chiefly the large intestines, while the other was an affection of the mucous follicles of the stomach and small intestines chiefly; and that both were highly contagious when in combination with typhus or malignant fever; and that the aggravated state of both diseases was owing to the peculiar circumstances under which the garrison and inhabitants of a closely-besieged and straitened city were placed. These circumstances are painfully described by the author ("horresco referens," he says), emanating from all those causes which could add to the misery of the wretched inhabitants, crowded together in winter by the accumulation of a large and hostile garrison, itself severely harassed by the enemy(*a*). A cold, damp winter, sadly aggravated by want of fuel; unsound and scanty provisions; bad wines, no beer, filthy water, excrements from men and horses (there being a large cavalry force), and the dead bodies of the latter, all heaped together in every nook and corner: "ut commune sterquilinum referrent" et "multis adeo aer, multâ jam humiditate vapidus, variique generis exhalationibus immundis inquinatus, in pestiferum contagiorum vehiculum degeneravit." Want of fuel led to every

(*a*) "Calamitate enim bellica pressi incolæ, ærumnis obruti, copiis et lixis Gallicis adeo coarctabantur, ut miseram inopemque vitam colentes, sæpius omnem sui curam negligenter." The French garrison of 8000 men exceeded the inhabitants in number.

contrivance for excluding the external air, and preventing all ventilation(*a*). Hence we cannot wonder that all the horrors of a contagious influence were speedily exhibited: “In nosocomio castrensi ægrotantium *omnes ac singuli*, ex quocunque morbo etiam laboraverint, morbi epidemici miasmate polluebantur, et per medentium et visentium cum aliis commercium latius indies morbus per urbem diffundebatur.” Hence, from the general as well as domestic impurity of the air, it arose that “non solum in nosocomiis, sed in honoratioribus quoque, omni licet curâ adhibitâ, durante hâc epidemiâ mucosâ vix ulla operatio chirurgica prospere successit: eadem et aliorum vulnerum ratio est, licet levissima fuerint.” On another occasion he says: “Vulnerati quovis modo omnes fere, liceat antea fuerint sanissimi, *ex mucosi epidemici connubio*, citiùs lentiùsve succubuerunt. Pro gravitate vulneris accenditur febris acuta, *ex inflammatione* paratur pus mali moris, fœtens, ichorosum, sequitur *gangrena*, *juncto simul* symptomatum febris mucosæ, acutæ malignæ, plus minus biliosæ, inflammatoriæ, putridæ *satellitio*.” But this supervention of hospital gangrene was not limited to the wounded soldiers in hospital, for “Eadem fere sors

(*a*) “Locuples morborum semini promptuarium erat nosocomium castrense, magna semper ægrotantium frequentia stipatum neque ventilatore munitum. Sedulò potius opera navabatur, quo minus cum aere infecto simul per rimam quandam elaberetur calor: nec per urbem defuit ægrotantium celebritas, et multæ domus totidem fere nosodochiorum ærunnas complectebantur.”

fuit præfectorum in urbe ex vulneribus 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 fuscus, laudabili indole gelatinosâ, glutine et consistentiâ destitutus, diffluens, justò dilutior: ægrè coit in placentam, multo sero circumfusam, tenui crustâ inflammatoriâ tectam. Ipsa vulnuscula phlebotomo inflictâ ægrè consolidantur, et post plures dies, madida labia vulnusculi adhuc dehiscunt."

I have been thus diffuse in my details of Roederer's epidemic because produced by peculiar causes, and attended by some singular effects; and amongst them by the unusual prevalence of worms not of the ordinary kind, as also by jaundice, which, from its great prevalence, was seemingly epidemic. Many of these his statements are confirmed by Riepenhausen in his "Morbi Epidemii" of the same season and place. The besieged town was Goettingen, a fact (strange to say), not to be collected from the whole of Roederer's history.

I shall now conclude this chapter with a detail of some interesting facts in the statistics of dysentery in London, with which Heberden, Jun., has furnished us in his work on the Increase and Decrease of different Diseases. He states that in the seventeenth century the 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 twenty-five years together, from 1667 to 1692, they every year amounted to above 2000: but from the beginning of the eighteenth century things were materially changed. After the year 1733, the article of griping of the guts was joined to that of the colic; if then we take the three diseases of bloody flux, colic, and gripes, their decrease is nearly as follows:

The annual average of deaths

From the year 1700 to 1710	was	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 extraordinary disproportion of deaths in the beginning, middle, and end of the eighteenth century cannot be referred to any improper treatment of the disease, though that may formerly have added con-

siderably to its mortality; such a circumstance cannot account for the vast disproportion of deaths in the different periods of this century, which was as follows :

At the 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.

These facts relative to the gradual decline of dysentery, or rather of its mortality, in London, are not to be paralleled in the history of any other disease; the plague itself furnishes no parallel case, because that disease either raged violently or disappeared altogether: not so the dysentery, which has declined, not so much, perhaps, in the numbers attacked, as in its positive mortality. 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 in London and all the great towns, and in the manner of living throughout the whole kingdom, particularly with respect to cleanliness and ventilation. The great influence of the assigned causes I by no means dispute; on the contrary, I am perfectly satisfied of their efficacy in the production of these happy results: in admitting this, however, I consider them to have been not the *immediate* but merely the *remote* agents in effecting such a change in the mortality of dysentery. The

influence of these causes was exerted through the medium of another disease, which, if the preceding views be correct, is the great source of *danger* as well as *contagion* in dysentery: their operation was primarily exerted in mitigating and preventing contagious fever; by that means acting secondarily on dysentery, which, when separated from this its dangerous associate, is no longer an object of terror to the patient or his attendants. This opinion is supported by various facts relative to the plague, to be found in the same author. It would appear that the disease, so called, was seldom absent from London previously to the great fire of 1666, after which event it never more visited the metropolis; it appeared also that dysentery, antecedently to that period, was in general both malignant and destructive, and that shortly after there was a wonderful decrease in its frequency and severity; and though dysentery still prevailed from that time to 1692, a space of twenty-five years, it had much decreased in mortality. Morton states it to have been epidemic from 1666 to 1672, and at first exceedingly fatal (300 or 400 dying every week during its *acme*), though less so towards the end of that period; from Sydenham's description, dysentery appears, except occasionally, to have been devoid of contagion; and Willis's History of the Dysenteria Cruenta of the year 1670 coincides generally with this statement.

Now I consider it almost demonstrated by Herberden, that the plague of London was nothing

more than the malignant contagious fever, exalted by various auxiliary aids to such a pitch of destructive violence as well to merit that name; and that the identity of the two diseases is satisfactorily supported by authority and facts. The singularly rapid decline in the mortality of dysentery, when the plague ceased to visit London, or rather when fevers lost much of their malignity, cannot but lead us to conclude that the immediate cause of this change was owing to the diminished influence and dominion of malignant fever in London. We may indeed take it as an admitted fact, that the malignity of dysentery has ceased with the disappearance of the plague. What so common among medical writers, down to the eighteenth century, as accounts of malignant and contagious dysentery? Do not their writings abound with concurrent histories of the plague or of very malignant fevers. Diemerbroeck, for example, mentions, amongst the precursors of the plague which ravaged Nimeguen in 1636: "*Morbi epidemii mali moris, ut erant variolæ, morbilli, dysentericæ valde malignæ et contagiosæ, imprimis febres putridæ, malignissimæ et purpuratæ, plurimisque cathales(a).*"

(a) Diemerbroeck, in his *Treatise de Peste*, states that after a summer, "*Vehementer calida et sicca,*" "*in nos (Noviomagi) et totam Geldriam aliasque plures regiones febris quædam pestilens epidemia diris furiis debacchata fuit, magnamque hominum stragem edidit. Circa autumnum, adhuc permanente, imo magis adaucto aeris fervore, summis siccitatibus conjuncto, undique adhuc majora putredinis indicia et undique plures maligni morbi apparuerunt, variolæ, morbilli, diarrhœæ et dysentericæ pes-*

Having, in the preceding pages, demonstrated the existence of dysentery in combination with intermittent, remittent, and typhus fever, I shall at once proceed to consider its claims to a contagious character, and to inquire under what circumstances it becomes possessed of that property.

simi moris ('valde malignæ et contagiosæ,' as he says elsewhere), *passim grassabantur, sed omnium maximè prædicta febris pestilens, quæ indies majora incrementa sumens, magis magisque in pejus mutabatur et purpurata evadebat, donec tandem in apertissimam pestem transiret.*" The intimate connexion of the three diseases, fever, dysentery, and plague, almost simultaneously raging, is here well and clearly marked.

It is to be noted of this author, that, as regards dysentery, he is no *exclusive* contagionist, for though he unequivocally maintains that the dysentery, which prevailed at the same time with the fever above referred to, was both malignant and contagious (v. Obs. xxviii.), he yet states the reverse to have been the case with another epidemic dysentery, "*quæ Montforti per totum oppidum inter vulgus passim grassabatur, multosque necabat: integræ quoque familiæ hoc morbo corripiebantur, quem propterea multi malignum et contagiosum esse judicaverunt; sed malè, quia non contagii, sed temporis anni et dietæ ratione communis erat.*" He then describes the effects of a hot autumn, of an intensely cold winter, and a warm spring, by which the poor were compelled to live on the very worst diet: "*Hinc tot passim dysenteriae excitatæ sunt: non tamen alibi quam apud vulgares, divites enim qui liberalius vixerant, ab hoc malo immunes erant. Hinc etiam evenit, quod tres quatuorve in iisdem ædibus sæpe hoc morbo inficerentur, non autem ratione contagii, quòd æque alios, qui ipsis adibant, quam hos invasisset.*"

CHAPTER V.

CONTAGION IN DYSENTERY.

ANY person at all conversant with the writers on dysentery, must have read with surprise the most contradictory assertions respecting its supposed contagious property ; some (more especially among modern authors), deny it *in toto* ; others as positively declare it to form one of the strongest features of the disease ; whilst a few, more undecided in their opinions, waver between these extremes, and though they admit or reject contagion in some particular epidemic, do not therefore pronounce it absent or present in all others. If we look to mere authority as the basis of our opinions (and to what besides this can we resort in the decision of such a question ?), we can form no satisfactory judgment, inasmuch as the opposing authorities are nearly balanced. Thus, if we suppose that the authors who have written on dysentery described under that name the same unvarying and identical disease, it is clear that on the ground of authority we can come to no settled conclusion. But if it be true that there are forms of this disease very different in their nature, and that these authors have, under one and the same name, described these its different forms, then may contra-

dictory assertions be reconciled without injury to the credit of any party; each describing his own epidemic with the accuracy and fidelity of an historian, and therefore necessarily differing from each other, both in their description of the disease, and in their opinions respecting it.

The extremes of opinion entertained on this subject 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. Between these extremes the truth will, most probably, be found, and accordingly the following propositions, already more than once alluded to, are advanced as containing that truth. The propositions are as follow: 1st, That THE SIMPLE DYSENTERY IS OF ITSELF NEVER CONTAGIOUS, NOR THE INTERMITTENT AND REMITTENT FORMS OF THE DISEASE; 2nd, That THE COMBINATION WITH TYPHUS IS ALONE POSSESSED OF THAT PROPERTY, THAT PROPERTY ORIGINATING, NOT IN ANY VIRUS SPECIFIC TO DYSENTERY, BUT IN THE CONTAGION OF FEVER.

The validity of these propositions may be established in manner following. Various authors, who describe the disease under its different forms, have been already enumerated; on a survey of these and of some others not yet referred to, and on contrasting their sentiments on the subject of contagion, we shall find that, with few exceptions, easily explained, such of them as describe the disease either in ITS SIMPLE FORM, OR IN COMBINATION WITH

INTERMITTENT AND REMITTENT FEVER, uniformly pronounce it NOT CONTAGIOUS ; whilst those who met it in COMBINATION WITH TYPHUS FEVER, as regularly and decisively declare it TO BE SO. The obvious inference from such premises must be, that the foregoing propositions are true.

I shall commence this survey by reviewing the sentiments of Cullen, whose definition of the disease has exercised such weight in the question. Should I succeed in shewing how he was led, naturally, yet erroneously, to embrace the doctrine promulgated in his definition, his authority will then no longer stand in the way of truth, or exert undue influence over others. This is my sole motive for commenting on the opinions of Cullen, as otherwise it had been my intention to confine the present review to those original observers of the disease, who described it as they saw it, and not as they found it described ; amongst these Cullen does not pretend to rank, for he spoke not from his own, but from the experience of others. His authority, therefore, is on a level with that of the writers he consulted, and whose opinions he adopted. It was his uniform practice, after giving the definition of a disease, to enumerate the authors he had referred to, by which practice he has furnished us with the means of estimating the weight that should attach to his own opinions. His opinions relative to dysentery are plainly specified in the first sentence of his definition, which states it to be a "Pyrexia contagiosa:" he even thinks it doubt-

ful if the application of cold does ever produce the disease, unless the *specific contagion* has been previously received into the body. The reason why Cullen adopted these decisive opinions will at once be obvious, from the very names of the authors to whom he has referred: I need but mention Pringle, Degner, Roederer, Zimmermann, Grimm, Helwitch, Bontius, Huxham, Cleghorn, &c. &c., all of whom, with one or two exceptions, had seen the disease in combination with typhus; and this they all (with the same exception), pronounce to be contagious. Had these been the only authors whose opinions could be relied on, Cullen would have been fully justified in his definition, but as the case stands we must perceive that he has, in place of defining the disease itself, merely given us a definition of its combination with typhus(*a*).

(*a*) Home (another systematic writer of the same school), in his *Principia Medicinæ*, distinguishes dysentery into different species, and, with respect to its cause, he says: "Contagio propagatur dysenteria quia excitatur ab odore fæcum, ab alvi in eadem sellâ exoneratione et a lacte nutricum hoc morbo laborantium. Hinc sævit inter nutrices quæ ægris in nosocomiis, præcipue castrensibus, ancillantur." Another systematic writer, the discerning and elaborate Sauvage, amongst his twenty species of dysentery, could not fail to enumerate the several varieties of the disease, upon which I have insisted, though he has no where pointed out the characters of the simple dysentery. Amongst his species we find the dysenteria epidemica, the dysenteria castrensis, and the dysenteria intermittens. Of the second he says: "Illa epidemicè grassatur: suspicio est, ne contagiosa sit per halitus putres ore et

In prosecuting this inquiry I shall follow the order already adopted in discussing the different species of the disease, and shall accordingly commence with those authors who met it either in its simple state, or in combination with intermitting and remitting fever.

SECTION I.

PROOFS THAT THE SIMPLE DYSENTERY AND ITS INTERMITTENT AND REMITTENT FORMS ARE NOT CONTAGIOUS.

MOSELEY, whose extensive experience of this disease in its simple and remittent forms might entitle his opinions to some respect, entertains sentiments relative to its supposed contagious property very different indeed from those of Cullen ; that he is not far from the opposite extreme may appear by what follows: "As to *contagion* from *infection* in dysentery, I must confess," says Moseley, "I never saw an instance of it, neither do I believe there is any such thing, either in this disease or in the plague, or in any other pestilential fever. I cannot even venture to conjecture what that agent is, which determines the species, and spreads epidemic disease." "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

podice exceptos." This is the only allusion he makes to the contagious property of the disease.

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 passage plainly points to the agency of a cause which Moseley has affected to deny; as dysentery, arising from contagion begins, like other contagious diseases, with a few, and gradually extends its ravages(*a*). The epidemics described by Grimm and Roederer furnish perfect examples of dysentery from contagion, and the case of Cromwell's army before Dunbar of that from cold and wet, as the disease suddenly raged throughout his tents, in consequence of exposure to a few days' rain. The fate of the English army after the battle of Dettingen fur-

(*a*) Dr. Luscombe, in his "Practical Observations," states that he entertains great doubts of dysentery being propagated by contagion, and thinks (*and justly, too*), that the first appearance of the disease was very different from that of a contagious disorder, which usually commences in a few persons only; whereas, on the contrary, he always had seen dysentery attack great numbers on its very first appearance, which he conceives must be attributed to other causes than contagion, to wit, exposure to wet and cold after great previous excitement of the system by solar heat during the Peninsular campaigns. It is much to the credit of our medical staff that so little of the contagious combination of this disease appeared in our army, though 23,000 cases of dysentery and diarrhoea had occurred within thirty-one months of the Peninsular campaigns, while it appears from Vigne's account, that the case was much the reverse in the French armies, both in Spain and Germany.

nishes an instance of the operation of both causes. The night after this battle the weary troops were exposed to a heavy fall of rain, and on the next were encamped on wet grounds: before the battle there had been very hot and dry weather; in less than six weeks half the army was afflicted with dysentery. In this case the operation of cold gave origin to the disease, whilst contagion, arising from its usual source, the accumulation of the sick, contributed to keep up the distemper(*a*). I have taken notice of these circumstances to shew, in the first place, that dysentery, as observed by Moseley, was not, in his estimation, contagious; and in the second place, that he himself unconsciously furnishes evidence sufficient to overturn his own position 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 prevailed in London in the years 1779 and 1780;” he does not, however, say that dysentery is never infectious, but thinks there is some

(*a*) From the imperfect details supplied by Bontius relative to the epidemic dysentery at Batavia in 1628, I was at first disposed to view it as an instance of the combination with typhoid fever, and therefore contagious, as he considered it. From such details as he has given I now would set it down as a combination with the ardent and pernicious fever of that locality, more especially as it occurred during the “hottest and most rainy season of the year, and seized many *at the same time and quickly*.” This, which was his chief reason for considering the disease contagious, is mine for viewing it in a different light.

difficulty in determining a question of this kind, "for 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 doctrine of contagion, speaks in decided terms of the epidemics under his observation: "The disease," says he (meaning the epidemic of 1800), "almost always arose after some imprudent exposure to cold;" and "neither in the present nor any preceding period, has the dysentery in London appeared to me contagious."

Akenside, Cleghorn, and Sydenham, in enumerating the different causes of the disease, take no notice of contagion; from Sydenham's silence, however, we can draw no special inference, as he scarcely makes mention of contagion in diseases unequivocally possessed of that property (*a*). Huxham stands

(*a*) Some cases of Sydenham's epidemic appear to have assumed a very bad form: "Æger, inquit, febricitat, lingua subalbida, quâdam mucilage densè obsita, et si vehementius fuerit excalefactus, 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

in the same predicament with the last-named authors(*a*).

Richter (p. 115), 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

contagious quality might escape their notice." Pringle, however, on this occasion has made two mistakes, for their epidemics were far from being of the mild character he supposed, and Willis, though he expressly denies contagion in the dysentery incruenta (i. e. in cholera), yet as expressly states, that it occasionally operated in the production of the dysentery cruenta.

(*a*) Huxham's epidemic in 1743 was evidently of the remittent character. His account of it is as follows: "Maxime sæviit hæc dysentery in oppido Plimton ejusque viciniâ, solo scilicet pingui et *uliginoso* morbisque plus satis obnoxio, febribus lentis præsertim et intermittentibus: duravit autem ab initio Aprilis ad Maii finem. Huic plerumque accessit febris, rectius fortasse dicerem hæc fuit ipsius febris *symptoma*; nam ab ipso sæpe principio atque ante ulla tormina haud levis urgebat febriculosus ardor cum pulsu in citatiore ac linguâ scabrâ; quid quod et sæpissimè cum manifesto satis horrore ingruebat morbus, et non rarò utique more Hæmitritææ cursum tenebat. Haud certè scio an fuerit febris epidemica, mox describenda, ad intestina translata; hoc sanè mihi compertum est, dysenteriam epidemicam non esse frequentem Veris sobolem."—*Vol. alt.* p. 98. This being evidently either the intermittent or remittent form of dysentery, we cannot be surprised at Huxham's silence on the subject of contagion; but, being struck by the unusual and concurrent prevalence of fatal apoplexy, he asks whether they did not both arise "ab enormi ac intempestivo æstu sanguinem subitò nimis expandente?"

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 bad forms of the dysentery(*a*), which might be deemed contagious, yet decides very positively against contagion. Page 327, Part III., he says: "Contagium dysentericum *pauci* in dubium vocarunt, idque effluvium ex uno homine expirans, communicari adstantibus 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 novi, inquit, dysentericorum dejectionibus aërem, ex quo omnes tanquam ex communi fonte potant, fædo putore corrumpi, morbosque putridos, per eminentiam *nosocomiales* appellatos, invitare; verùm quòd dysentericorum exhalationes *eundem* in aliis morbum producant, id quidem arbitror observationibus adversari, et magni

(*a*) 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 nempè biliosâ, fieri supra narrabam."

interesse puto non ignorare, dysenteriam contagio carere. Quo enim animo medicus dysentericorum domos, pauperum præprimis tabernas, adibit, de morbi contagio persuasus?"—Language somewhat similar is employed by Dr. A. Wilson, when 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!!!

Sir G. Baker, whose epidemic coincides in most respects with that of Huxham, though he denies contagion in the greater number of cases, yet, when discussing the necessity of a strict observance of cleanliness, specifies other cases wherein contagion did operate, under circumstances, however, such as must instantly suggest a suspicion of the presence of typhus: he observes that cleanliness, whilst it is advantageous to the patient, is absolutely necessary to insure the safety of the attendants: "Quamquam enim in ædibus civium locupletiorum rarò propagari visa sit hæc nostra dysenteria, longè alia tamen et miserior sors erat immundæ paupertati, *neque omnino dubitandum*, quin in *infimis* de plebe familiis, disseminaretur contagio, et pestilentiae ritu ab alio corpore ad aliud transiret." Here we have it distinctly stated that in the same epidemic there were some cases contagious, others not: what could have created the difference except the presence of some other disease, capable, at least, of propagating itself; and

that this disease was typhus, the circumstances mentioned render more than probable.

I have already anticipated Willis's sentiments on this subject ; suffice it to say that he denies (as he well might) any contagious property in the dysentery *incruenta*, that is, in cholera: but that when he states the dysentery *cruenta* to be in general devoid of contagion, he adds, "aliquando tamen hic morbus virulentus, et quasi pestilentia, plures interficit, et miasma suum per contagium latè explicat." This he states, not so much of the dysentery *cruenta* of that year as of the disease generally ; and we can well believe his statement from the concurring testimony of contemporary writers, for Morton, who describes the disease as epidemic from 1666 to 1672, states it to have been highly contagious and fatal at its commencement, though much less so towards its close(*a*).

Hillary's sentiments relative to the causes which produce this disease deserve to be recorded, as he kept accurate journals of the state of the weather for a series of years at Barbadoes, where dysentery is almost endemic. (P. 203) he says: "I have always found, from the best observations I could make, that if the months of May, June, July, and August were very hot and dry, and the following months of September, October, and November were accompanied

(*a*) Degner's statement (in note to p. 71) relative to his assumed dysentery, furnishes a remarkable contrast with this course in Morton's epidemic.

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 of shorter or of longer continuance (*a*). As, then, we have dysenteries constantly returning every year in the West India islands, with those 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 sufficient to produce it?" Immediately after he adds: "It is also probable that it may be sometimes produced by *infectious miasmata*, exhaled from diseased bodies, and floating in the air, which are received into the mouth 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 symptoms, when they meet with a constitution fitted by the

(*a*) The description of the dysenteric constitution by Grimm very closely coincides with Hillary's. In p. 147 Grimm says: "Transiit autem æstas sexagesimi anni in autumnum perpetuis pluviis, imbris, nebulisque obrutum, valdè obscurum, atque ingratum, æstuante tamen Augusti parte, et mense Septembre. Sub maximâ humiditate, diebus vix triginta frigidioribus interpositis, transiit autumnus, quin eadem humiditas duravit per omnes hyemales, et vernaes insequentis anni 1761, menses ut hyems nulla, continuus autumnus adfuisse visus sit."

above-mentioned causes to receive those infectious effluvia, and to produce the disease." "And thus 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*(*a*)." This last passage would seem distinctly to refer to the combination of dysentery with typhus, which alone I admit to be contagious.

I cannot conclude this section without some reference to the remarkable contrast between the more ancient and modern writers in their opinions respecting the contagious character of dysentery; the former seldom or never doubting its possession of that property, whereas such scepticism has of late prevailed among the latter, that some boldly declare

(*a*) Dr. Copland, who had some experience among the "dark races," states that "the negroes particularly are more liable to dysentery than to any other disease; that it assumes an extremely low or putrid form among them, when confined in ill-ventilated situations; and that when a number are shut up in such places, the cutaneous secretions, in them so abundant and offensive, accumulate in and vitiate the surrounding air (unless frequently renewed); the systems of those thus circumstanced are thereby infected, and instead of an infectious typhus, which would be the result in the European constitution, a putrid dysentery, spreading rapidly through all breathing the impure air, is developed." He further states, that the disease is considered by the Africans as infectious as small-pox, and that during the slave-trade one-half of the unhappy victims of that traffic have on some occasions died of this pestilence in the passage across the Atlantic.

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 is employed by Wade and others, in speaking of the contagion of fever and dysentery(*a*). This is language that must be condemned, because employed by persons who are plainly drawing a conclusion which their premises do not warrant, for though the disease they met might not have been contagious, yet it did not therefore follow that the same must necessarily have been the case with others. The following section demonstrates the absolute fallacy of such conclusions.

(*a*) Mr. Robertson, for example, in his Medical Notes on Syria, says of dysentery: "My experience inclines me to view the doctrine of its contagious nature with great doubt, if not entire disbelief; and after a careful perusal of all that has been written for and against the contagious nature of dysentery, I am, *from experience*, inclined to deny it such a power *in toto*."—*Edin. Med. and Surg. Journal* for July, 1843.

SECTION II.

PROOFS THAT THE COMBINATION OF SIMPLE DYSENTERY WITH
TYPHUS FEVER IS CONTAGIOUS.

I SHALL now refer to the direct evidence of those writers who appear to have met the disease in combination with typhus, and who with one voice bear witness to the truth of the foregoing proposition. The presumptive evidence in its favour is strong: we have seen that the disease is held by the best authorities to be, if not always, at least very often, contagious: and as we have shewn that it is devoid of this property both in its simple state and in combination with intermittent and remittent fever, so we must necessarily infer that the combination with typhus fever alone *can* constitute the contagious form of the disease. This species of evidence, though strong in its kind, must still rank far beneath the direct testimony which I shall now submit to the judgment of the reader.

Before I produce this direct testimony, it is necessary to take some previous notice of the opinion advanced by Clarke respecting the Bengal dysentery, an account of which has been given in the chapter on the intermittent and remittent forms of the disease, and which Clarke has unhesitatingly pronounced to be contagious. We shall find that his authority, however valid upon other points, may, without any difficulty or delicacy, be set aside on this. His state-

ment is, that 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 effects of land winds." By the expression "always contagious," does he mean that the disease is *in every instance* contagious, or merely that when epidemic in unhealthy situations it is then always contagious? However equivocal the phraseology may appear, I should suppose that he considers dysentery "always contagious" in the latter case only; why he thought so will be evident on reference to his opinions respecting the contagious property of *all* fevers. He maintains, concurrently with Dr. R. Fordyce, "that continued fever does not differ more from the remittent than the remittent from the intermittent type; and that their frequent changes into each other, and perhaps again to their original form, prove them to be the same genus." He maintains also that they are alike in the causes which produce them, and also that "they are all apt to become contagious." "This contagious power, inherent in fevers," he says, "they possess in very different degrees, according to the different modifications of their remote causes. Thus regular intermittents, which derive their origin from the purer marshy exhalations, are only *slightly* con-

tagious; whereas remittents, originating from corrupted exhalations after hot summers or in warm climates, are *very* contagious, and from this cause often assume a continued form; and when this happens, these remittents differ in no respect from that variety of continued fever, which is propagated in camps, gaols, &c." "With regard to continued fevers, whether they at first appear in their own proper form, or degenerate from the remittent type, I am convinced that when they become prevalent in any town, or even in a single family, they are always contagious." He afterwards says, that "although the remittent fever at first seems only to be produced by moist air or exhalations from marshy grounds, yet there is no doubt that the disease is afterwards *often* propagated by contagion." In another place he distinguishes that variety of fever peculiarly denominated "contagious" from all others, as one "which originates from virulent contagion," that is, the contagion of which merely differs in degree from the cause which may produce other fevers.—The person who entertained such opinions could have no hesitation in pronouncing the remittent form of dysentery to be "always contagious." Having thus disposed of his testimony on the subject, we proceed to others of less equivocal character.

I need not here repeat the sentiments of Etmuller, of Roederer, or of Grimm, on this subject, as these have been distinctly pointed out in passages quoted from their works in the last chapter.

Morton, in his own person, had experience of the contagious nature of the epidemic he describes : “Nec mihi, inquit, pepercit contagio, dum mense Augusto sedes dysentericorum minus incautè inspicerem, iterumque sequente autumnno ex eâdem occasione infectus sum(*a*).”

Pringle’s sentiments have been already detailed: 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; and 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 nature is this infection? how does it so regularly affect the intestinal canal? So conscious was Pringle of the insufficiency of the exhalations from the sick, received in the general mode of contagion, to explain this circumstance, that he seems inclined to adopt the agency of animalculæ 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

(*a*) This is the only specific mention made by Morton of the contagion of dysentery; indeed his whole account is so perfectly consistent with that already given by Willis, that we may fairly conclude that its contagious property arose under similar circumstances; that is, by the frequent conversion of bad remittents into continued fevers of a contagious type, a species of conversion, as we have just seen, strongly insisted on by Clarke.

vitiated part of it, by a certain law in the animal economy, must be thrown upon the intestines for excretion(*a*).”

Tissot's opinion is very decided: “If the corruption of humours, which creates *malignant fevers*, be united with the causes which produce dysentery, 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,” he says, “within these few months, a terrible example of its infection.” The case was that of a young man attacked by a severe dysentery, so truly malignant, that in a few hours it *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, except contagion.

Geach, in his Observations on the Dysentery Epidemic at Plymouth in 1781, states the following facts demonstrative of the contagious nature of the disease, and of the circumstances under which it

(*a*) Though Pringle would seem so decidedly of opinion that dysentery was always and of itself *contagious*, yet he makes an observation which *indirectly* would infer the contrary. He says: “It is true, indeed, that this disease is not so catching as most others of the contagious kind; but whenever it has been epidemic, I always found it *in some degree* infectious, and *especially* in military hospitals and in the houses of the poor, who want the means of cleanliness.”

became so. He says: "The disorder till this last summer was chiefly among the soldiers confined to barracks. And 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, ran through the stages of the disorder, till at length one of them died, and the other with difficulty recovered." One thing is observable on this as on other occasions when the disease is said to be contagious, that there are symptoms present which do not attend the more common course of dysentery, and which very generally belong to contagious fever.

Sennertus de Dysenteria, p. 127, tom. iii. (Paris Ed.), 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 dysenteria, quæ anno 1624 hic grassabatur, compertum sit, unum ab altero infici, et hinc totas familias dysenteriâ 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, et phthisi pars una reliquis salvis afficitur, sed sæpe tota massa sanguinea inquinata est (unde etiam

febres dysentericæ sæpe conjunguntur) (*a*) nec conversationem cum dysentericis satis tutam puto, et dysenteriam ex lecto dysenterico communi, vel communibus poculis, aut patinis, aut aere inspirato attrahi posse existimo, cum experientia doceat, non paucos ex conversatione cum dysentericis in dysenteriam incidisse, qui nunquam in locum communem alvum exonerant." I should have doubted the agency of typhus 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, from that source *only* could it be capable of spreading in the ways and to the extent it too often does. Hildanus, speaking of dysentery, confirms the foregoing statement of Sennertus, for he says: "Familiaritas quoque et conversatio cum ægrotis, et præcipuè latrinarum usus cum iis communis periculosus est, et facillimè inficit."

Darwin asserts that "the contagious matter of this disease consists in the mucous or purulent discharge from the membrane lining the intestines, and not in the febrile perspiration or breath of the patients: for the fever is only the consequence and not the cause of contagion." Though it be admitted that the mucous or other discharge from the intestines may convey contagious matter, it is yet clear how

(*a*) That these his dysenteric fevers were usually of very bad type may be concluded from his observation (p. 130): "Si dysenteria sit symptome febris malignæ, pestilentis et contagiosæ, periculosa ea est et plurimi eâ intereunt."

ill-founded the reasoning of this ingenious author is, if the testimony adduced be sufficient to prove that the accompanying *fever* not only frequently precedes the disease, but is also essentially necessary to the production of its contagion.

Grainger, when giving an account of a contagious dysentery that broke out among the soldiers, and raged more especially among the fresh conscripts, describes the symptoms as very severe; and as those of typhus were conjoined, he makes the following remark: "Symptomata quædam sanè haud vulgò descripta aderant." Among other reasons for considering the disease contagious, he says: "Duos colonos, in quorum stabulo dysenterici cubabant, invasit; immò et aliis morbis afflictos, quamvis ab illis separatos, corripuit."

Roupe, in his excellent Treatise, makes mention of a violent fever which raged at Weissemburgh with different degrees of violence, being attended in many with colliquative sweats and petechiæ, but in the *lowest parts* of the town with *colliquative sweats, very fœtid; petechiæ, broad livid spots, frequent hæmorrhages, and offensive dysenteries*, generally fatal in four or five days. It would seem to have been a bilious remittent(*a*), afterwards converted into a

(*a*) That these fevers may suffer such a conversion will appear by the following passage from Pringle: "In the autumn of 1757 several soldiers were brought into the hospital at Portsmouth with a disorder complicated of the *autumnal* and *gaol* fever; for when these men, upon being seized with the common fever of the

contagious malignant fever, under circumstances plainly specified by Rouppe himself: for he speaks of contagion originating from 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 time the air between decks be not well purified, the sick and healthy kept separate, cleanliness preserved as much as possible,—if, 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 dysentery: "But these disorders, as they arise from a general cause, usually rage epidemically in ships, and from being epidemical, unless great care is taken, soon become *contagious*."

Hoffman, too, whose remarkable precision in marking the three great combinations of dysentery I have shewn in the introductory observations, though he does not state these distinctions in his general

season, were confined to the holds of the crowded transports, their distemper assumed that form." The epidemics of Morton, Willis, and others, furnish further proofs of such conversions, and though Dr. Bancroft is correct when he states that "Sydenham no where intimates that his epidemic dysentery was contagious," yet he is clearly incorrect in saying that "Willis distinctly asserts it was not," as we may see by reference to the statements already quoted from that author.

history of the disease, yet respecting its contagious character says: "Notari adhuc circa dysenteriaë historiam meretur, quòd sæpenumerò contagiosa existat." He does not, however, on this occasion specify the circumstances under which it became so, as he did in the former extract from his work.

To this long list of authors I shall subjoin only two or three more, each for a particular purpose: the *first* not merely for the strength of the evidence he furnishes, but because I felt much surprise on the perusal of his work, that it had not long since led to the distinctions I have endeavoured to establish; the others I shall bring forward for the purpose of exhibiting the sentiments entertained by one of our latest systematic writers, and of shewing what little use had been made of the experience of others in regulating our opinions respecting this disease.

Zimmermann describes dysentery as epidemic in 1765, and states that about the same time the putrid fever, and the putrid pleurisy, began their ravages, and were extended far and wide. He details the course of the disease, and then draws such an analogy between the putrid fever and the dysentery, as must convince us of the truth of Sydenham's observations on the close connexion between the epidemic disorders of the same year: for, after "an astonishing number of putrid fevers," there followed dysentery, attended likewise with a putrid fever. The analogy between them was to be seen in the resemblance of the symptoms, of the method of cure,

and even of the effects that followed the errors therein committed. "It appeared to me," says Zimmermann (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:" and (p. 47) he says: "Experience has shewn that the smell of the patient was least dangerous, that their breath was worse, and their stools worst of all(*a*)."

(*a*) Dewar, in his Observations on Diarrhœa and Dysentery, comes to a conclusion, on *theoretic* grounds, nearly similar, 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 appear incongruous, and others unfounded. In the first place it is stated, and with truth, that the disease is local; but how is it 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. As 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, 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

(P. 133) he observes that a distinction is usually made between a dysentery with fever and one with-

fever, is *never contagious*, and therefore that when contagious, its contagion, like that of fever, may exist in the matter of perspiration, or may be absorbed by the skin. 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 organs 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 œsophagus in eructations, or even what is secreted in the internal fauces, and carried off in vapour with the breath, contains the contagious effluvia of the disease." Is it not somewhat inconsistent to state that the contagion of dysentery may be contained and conveyed in those secretions by which the febrile contagion is communicated, after denying 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 only. The impression left on my mind, from reference to the best authorities on the subject is, that dysentery, when contagious, may be communicated by all the modes in which febrile contagion can be conveyed, viz., *directly* by the breath, by the perspiration, and by the morbid secretions of the excreta, and *indirectly* by fomites. Pringle (p. 122) distinctly states that the "straw became infectious." Hufeland, in describing a dysentery epidemic at Jena, in 1795, states it to be a topical disease, but that it can have many complications, degrees of violence, and effects. "It generally excites fever, which may be inflammatory or typhoid, and must be treated with blood-letting or bark accordingly." His opinion of its contagious nature is, that among the poorer class, where four or five patients lay in one narrow room, where, from want of help, the stinking stools were not removed, and the air became absolutely pestilential, the disease took on a putrid character, and in this state he is convinced that a contagion may be produced and communicated by the matter evacuated, so as to create a contagious dysentery ;

out fever; a contagious and non-contagious dysentery: "this distinction," he adds, "between a dysentery with fever, and one without fever, appears to me to have a dangerous tendency, and in my opinion should be banished from the schools of medicine, as it rather determines the limits between a dysentery and a diarrhœa(*a*)."

Now, in my judgment, little more the disease was, he says, conveyed in several instances by a clyster-pipe used with healthy people, after having been employed with dysenteric patients.—See *Annals of Medicine*, vol. i.

(*a*) Vignes (a great admirer of Zimmermann) imitates him in his division of dysentery into "les deux especes, savoir, en *benigne* et en *maligne* ou de mauvais caractere." Each of these he subdivides into several varieties: "La premiere se compose de l'état essentiel ou purement inflammatoire des intestins, et des complications muqueuse, bilieuse, et bilieuse-putride, et qui constituent les variétés de cette espece. Cette espece est caracterisée par une tendance vers la resolution, quel que soit son degré d'intensité, *pourvu* que des causes accidentelles majeures ne contrarient pas sa marche; car *alors* elle peut souvent s'accompagner (surtout quand elle est compliquée), de phenomenes generaux de mauvais caractere." Whoever studies his descriptions of these varieties will perceive that they generally constitute the combinations of dysentery with remittent fevers, easily convertible, under military crowding, into the *malign species*, "qui a pour caracteres des symptomes de malignité, qui sont une faiblesse generale: une inflammation ou sub-inflammation des intestins, de couleur plus ou moins plombée, avec une tendance vers la dissolution de solides. Cette espece se compose des variétés typhoide, adynamique, ataxique et celles combinées avec la fièvre jaune et la peste." This is the combination with continued or contagious fever, as we shall see hereafter. For proofs of the remittent character of the former varieties, see p. 223, &c., of his "*Traité Complet*."

was wanting to make this distinction perfectly accurate than a further distinction of the febrile dysentery into varieties according to the nature of the accompanying fever, and indeed this would seem to have been Zimmermann's own opinion, for in p. 156 he says: "All this taken together, I give it as my opinion, that the various species of dysentery are to be distinguished, not by the difference of evacuated matter, but of the fever by which they are accompanied." He is incorrect, too, in stating the ground of distinction between diarrhœa and dysentery to consist in the fever, as that alone, without other symptoms peculiar to each, would be insufficient for the purpose; but Zimmermann has occasionally fallen into an error, no way uncommon, of confounding dysentery with other diseases to which it bears an affinity.

(P. 136) he says that Sydenham's expression of a "febris introversa" does not perfectly please him yet 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 compounded of both, sometimes as a malignant fever, and sometimes as a bilious one, accompanied with manifest tokens of malignity(*a*).

(*a*) Zimmermann here seems to have correctly understood the *application* of Sydenham's idea; not so Blane, who (p. 449), says: "These two diseases (meaning dysentery and fever) may therefore be considered as *vicarious*, the one substituting itself for the other

He then details the circumstances under which dysentery becomes contagious, namely, by a conjunction with malignant, gaol, or hospital fever: the effects of this 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 conclusive on the source of contagion in dysentery. I shall merely select a few passages to illustrate 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 he adds, that he does not see "that camp dysenteries are in themselves more malignant than those that happen in cities, although in the army and military hospitals they become excessively malignant and contagious from several circumstances: the same, however, takes place in cities, when a great quantity of people, attacked with this disease, are crowded together in a small space, or where the other different causes subsist of a peculiar or general malignity." And in p. 151 he says: "*When hospitals are filled with dysenteric people some of the assistants are attacked only with the dysentery, and others with the gaol or hospital*

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*."

fever, that ends in bloody and gangrenous stools(a)."
 "From all these observations, made partly by me,

(a) Vignes gives a melancholy description of the general state of the hospitals of the French army after having advanced on Vienna. Part of it (p. 139) may be quoted as furnishing a specimen of the circumstances under which the contagion of typhus must necessarily be generated and communicated to every patient sick of any other malady within the same wards: "Cet hopital (une eglise), manquait d'infirmiers, d'alimens legers, et de presque toutes les fournitures necessaires pour coucher les malades: mais ce qui etait plus digne de remarque, c'etait l'eglise, encombré egalement de malades couchés pêle-mêle sur la paille, que nous ne pumes obtenir de faire changer qu'un mois apres. Cependant elle etait infecté par les excremens que les y rendaient, faute d'une assez grande quantité de vases." "Aussi l'atmosphere etait elle continuellement chargé des miasmes délétères," &c. "Tel etait le triste etat de cet etablissement pendant plusieurs semaines: il en mourait un grand nombre: et la plupart des medecins, chirurgiens, &c. contracterent les maladies regnantes de mauvais caractere et plusieurs en furent victimes." "Aucun medecin," he adds, "ni officier de santé, ayant deja eu la maladie (le typhus), n'en fut atteint une seconde fois a notre connaissance. Il y a meme des medecins de l'armée, qui observent depuis plus de vingt ans dans les hopitaux militaires, qui pretendent qu'il ne se reproduit pas deux fois chez le meme individu, de meme que la peste: nous ne pourons pas etre de leur avis; car au fait ce n'est qu'une maladie dependante des fatigues, de l'irregularité de la nourriture, des intemperies de l'atmosphere, et surtout des exhalaisons qu'on respire et qu'on absorbe dans les hôpitaux encombrés des malades. *Il serait absurde de croire que le typhus et d'autres maladies de mauvais caractere se propagent par contagion essentielle; c'est tout au plus par contagion relative!*" "Un grand nombre furent victimes de la complication de la dysenterie avec ces fievres: il perit environ le tiers," and in other instances more than one-half the sick died.

and partly by other physicians, I conclude," says Zimmermann, "that the dysentery is very often only *accidentally* contagious," but that it also frequently becomes essentially so just before the death of the patient ;" and of this in p. 150 he mentions some instances.

After a perusal of these passages can we hesitate in adopting the opinion I have been endeavouring to establish? Yet Zimmermann's work has been translated into our tongue above thirty years, has been in general circulation during that time, and recommended from high authority, without having to my knowledge produced such an effect ;—of the truth of this assertion a few extracts from the next author will be sufficient to convince us.

The fourth volume of Wilson's Treatise on Febrile Diseases, published in 1804, may serve as a fair specimen of the doubts and uncertainties of more modern opinions respecting this disease. This author 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. He says (p. 582): "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." "I may observe, by the bye," he adds, "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 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 attending it, may excite a real typhus *independently* 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 (*a*) does not appear 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.*" "The fever in dysentery," he says, "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."

(*a*) I should like to know on what facts he rests this assertion; as he appears intimate with the treatise of Zimmermann, perhaps he built it on the observation already referred to, viz., "that when hospitals are filled with dysenteric people, some of the assistants are attacked only with the dysentery, and others with the gaol or hospital fever."

Notwithstanding this enumeration of the different combinations of dysentery, he does not appear to have suspected the real source of its contagion, for 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 shew what those *many cases* are in which the disease is contagious, for this is an admission that there are cases of the genuine disease not possessed of that property. He then states the sentiments of Zimmermann on the same subject, but without any reference to that combination of dysentery and typhus which he had himself previously mentioned, nor does the smallest ray of light appear to strike on his mind, notwithstanding the decisive language of that author. He even says (p. 616): "It is very doubtful whether (as Zimmermann 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." From this he would seem to be of opinion that dysentery, *qua* dysentery, is contagious, and of course that the *many cases* of the disease which are possessed of this property cannot owe it to typhus or any other disease: indeed he would appear to have had no knowledge of that species of contagion which can propagate two diseases,

one of which shall be contagious, and the other, in itself, utterly devoid of that property.

Dr. Copland, in Part III. of his Dictionary, published in 1835, furnishes a better specimen of the far greater accuracy of medical judgment now existing on the same point. P. 699 he says: "The contagion of dysentery has been much disputed, chiefly owing to the different forms of the disease not having been distinguished with any degree of precision, and to the loose notions attached to the words contagion and infection," &c. "It may be stated that the sthenic forms are seldom or never infectious," as they occur under circumstances unfavourable to the generation and accumulation of infectious emanations, because the vital energies are not depressed nor perverted to such a degree as to give rise to the depravation of the circulating and secreted fluids requisite to the production of infectious emanations, as is the case when the causes of the asthenic states come into operation ;—whence "it must be inferred that these forms are infectious on occasions favourable to the action of the emanations which proceed from them." Dr. Copland, however, proceeds further, and (in p. 705) endeavours indirectly to negative the propositions I have been contending for, that the combination of dysentery with typhus is the special cause productive of contagion in the former disease:

"Many writers," he says, "conceive that the asthenic varieties are complications of simple dysentery with different kinds of fever, and that when they are

infectious it is not the dysentery but the fever which possesses this property. *Some* authors suppose that the typhoid variety especially is a complication of this description. But if such," he adds, "be the case, wherefore should the disorder which is communicated be always dysentery and not fever? Moreover, this form of dysentery is often present where a case of typhus cannot be found." He then summarily disposes of the whole question by asserting "the fact as incontrovertible, that the asthenic forms are direct and necessary, and uniform results of certain diversified but concurrent causes, and not contingent associations of two diseases capable of separate existences." The fact thus laid down as "incontrovertible" being not merely a *petitio principii*, but the very assertion of the questions at issue, I shall pass it by, and proceed to examine his two arguments; and *first* as to the inference he would have drawn from the questions above quoted: "Wherefore should the disorder which is communicated be *always dysentery* and *not* fever?" The question is doubly erroneous, inasmuch as the disease sometimes communicated is the fever and not the dysentery; as, for example, when, according to Zimmermann, hospitals are filled with dysenteric people, some of the assistants were attacked only with the dysentery, and others with the gaol or hospital fever; and that "this fever may attack people in health without being attended with the dysentery, though it arise from the putrid and confined vapours of that dis-

temper(a).” Again, the dysentery that is communicated is not dysentery merely, but dysentery in combination with the fever. *This* assertion of an “incontrovertible fact” is amply borne out by the multifarious authorities already quoted, such as Pringle, Hoffman, Zimmermann, Vignes, Grimm, Frank, &c. His *second* argument consists in the statement that “this [the typhoid] form of dysentery is often present where a case of typhus cannot be found.” Now for this statement no authority is given, such as might be investigated; but is not Dr. C. well aware that, under favourable circumstances (such as those recapitulated by Frank and others), there is no disease more readily superinduced or spontaneously generated than typhus, a position fully supported by the several authorities already quoted in illustration of this very typhoid variety, and which

(a) This statement is borne out by a singular analogy with puerperal contagious fever, illustrated by the following facts from p. 490 of Barker’s and Cheyne’s *Account of the Epidemic Fever in Ireland*: “About the termination of the epidemic, puerperal fever arose in the Lying-in Hospital of Dublin, and was fatal to many of the patients. A circumstance related to us by Mr. Creighton, Surgeon to the Foundling Hospital, would seem to prove that the connexion between these fevers, which has often been suspected, actually exists. Two infants, whose mothers had died of puerperal fever in the Lying-in Hospital, were sent from that establishment to the Foundling Hospital, where, after being washed, and their clothes cleansed, they were given to two healthy nurses; these nurses both took typhus within a fortnight, and were sent to the Fever Hospital in Cork-street.”

I have myself further illustrated in my work on the Epidemic Contagious Fever of Ireland (see p. 161, *et seq.*) All these authorities distinctly shew that the circumstances under which contagious dysentery rages, are the same with those which give origin or aggravation to the prevalence of typhus. The same was amply evidenced in the concurrence of contagious dysentery with the same epidemic fever which raged in Ireland during the three famine years of 1816, 1817, and 1818(*a*).

Having thus taken an extensive review of the chief historians of this disease, and having given a candid statement of their opinions respecting it, I shall now recapitulate their names, arranging them after the

(*a*) Dr. O'Brien, of Dublin, seems to entertain doubts respecting the real nature of that combination which produces contagious dysentery, as in p. 34 he would from analogy argue that the combination of typhus and dysentery ought to produce in another typhus fever only; and he infers this because the same fever, when in combination with other internal diseases—as hepatitis—only produces its kind. To this my answer is, *first*, that analogy, however strong, is no answer to facts; and *secondly*, that the analogy itself is not good, because pleuritis and hepatitis, which he instances, are inflammations of serous membranes, and dysentery of a mucous membrane, a membrane the secretions of which have an exit upwards and downwards. “It may seem strange,” says Dr. O'Brien, “that I should admit a contagious type of this disease, which I have never verified by observation. I own I do so solely in deference to the opinion of many able physicians who have supported this side of the question, the respectability of whose authority is such that it were childish pride and self-sufficiency in any one individual to reject it on the *ground of his own experience.*”

manner in which they delivered their evidence for or against the agency of contagion in its production.

NON-CONTAGIONISTS :

Moseley, Hunter, Willan, Aken-side, Cleghorn, Sydenham, Huxham, Richter, Stoll, Rollo, Willis? Baker, Hillary, Milne, Somers, Fergusson(a),	}	<p>These authors describe the simple disease, or its intermittent and remittent forms : they all with decision, greater or less, give their voice against contagion in any of these forms. Some of them deny contagion <i>in toto</i>, others are silent on the head, and some again, whilst they admit that the disease, as they saw it, was not contagious, do not deny but that under particular circumstances it may become so. This species of disagreement must the more firmly convince us of the truth of that proposition in which they all coincide, namely, <i>That the</i></p>
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(a) "Dysentery," observes Mr. Fergusson (2nd vol. *Medico-Chirur. Transactions*), "from my experience in Holland, the West Indies, and south of Europe, I believe I can declare to be in no case contagious. I never saw anything like it except in Holland, after the weather became cold, when it was seen as a symptom, or irregular local form of typhus fever: but more could not then be said than that it was about to be swallowed up in the prevailing epidemic, and *formed a combination* rather than a distinct type of disease." Mr. F. was Inspector-General of Military Hospitals to the Army in Portugal, and consequently possessed an ample field of experience, of which we shall hereafter see 'the fruits in his successful treatment of the disease.

dysentery, in its simple, its intermitten, and remittent forms, is not contagious.

On an opposing list are names not less respectable :

CONTAGIONISTS :

Clarke,
Hoffman,
Etmuller,
Roederer,
Grimm,
Morton,
Pringle,
Tissot,
Geach,
Sennertus,
Hildanus,
Grainger,
Roupe,
Zimmermann,
Rogers(*a*),
Frank(*b*),
Vignes,
Dewar,

These authors generally met the disease in combination with typhus; they all, with one voice and without hesitation, pronounce it contagious. Some assert contagion to be the sole, others the principal agent 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, in connexion with the preceding proposition, we must conclude, that the dysentery is contagious *only* in case of its combination with contagious fever.

(*a*) This author I have set down amongst those who vote for the contagion of dysentery; for though he does not plainly say that it is contagious, his whole history of the disease speaks it for him. The same may be said for Monro, who describes "the bilious and malignant fevers as often terminating in the dysentery, or were accompanied with it, when it might be looked upon as a symptom of these fevers."

(*b*) "Contagii hoc in morbo rationes," says Frank, "nunc satis conspicuæ, nunc nullæ esse videntur. In castris, exercitibus, in

Dysentery, therefore, can no longer be deemed a contagious disease in itself, though capable of acquiring that property under peculiar circumstances. Thus we see that a contagious disease can so intimately associate itself with another, not possessed of that power, as to communicate both diseases, and not that merely which was in itself contagious. As I shall hereafter endeavour to illustrate this singular property of the contagion of typhus in other instances of disease, I shall now conclude the subject with a concise enumeration of the different signs by which each form of dysentery may be recognised.

locis obsidione cinctis, in carceribus, ergastulis, nosocomiis, apud plebem, domibus angustis, impuris compressam, et ingressu prior et frequentior dysenteria esse consuevit. Ast verò nec hæc quidem perpetua sunt. Interdum pauci ejusdem habitaculi incolæ, interdum plures hoc a morbo plectuntur. Solitaria exempla dysenteria et quæ in alios, quamvis sibi diu expositos, nequaquam transiverit, haud raro occurrunt. Per decem annos, quibus Vindebonensi præfixi fuimus magno nosodochio, plures quidem in illud dysenterici suscepti fuerunt: sed ex his in alios morbus ita non transiit, ut manifesta esset hoc in loco contagii illati propagatio." In this statement we have evidence for and against contagion—evidence easily reconciled when we consider the different circumstances under which the disease occurred. On another occasion he is very precise in his statement, when he says: "Quodsi febris quæ morbum talem (i. e. anginam ulcerosam) vel quæ dysenteriam malignam pro symptomate agnoscit, contagio suam debuerit originem: difficultas quoque ipsa intestinorum *particeps erit contagii*, nec tamen erit illis in casibus, in quibus morbi contagiosi effectus non fuerit."

The simple dysentery may be readily known by the presence of all those symptoms detailed in Cullen's definition, excepting the "pyrexia contagiosa(a)"; it is neither preceded nor attended by fever of *idiopathic* character, any feverish symptom present arising from the severity of the disease, and being entirely symptomatic thereof. It is a disease by no means dangerous unless grossly mismanaged or neglected, and when art does not interfere effectually to its cure, it usually terminates in a chronic state; in all these respects, and in its mode of treatment, closely resembling rheumatism, to which it is nearly allied in its general nature and properties.

It may occur at any season of the year under the prevailing influence of its occasional or exciting causes, though most prevalent in the autumnal months, and all sporadic cases are usually of this description; instances of it are also to be met with in every epidemic, whether of the remittent or continued character, but more especially of the former, as may be seen on reference to the authors who have described that form of the disease.

(a) 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 dysenteriâ laborare omnes consentiunt medici."

The intermittent and remittent dysentery is known by all the marks which characterize 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 throughout its course, and may, whether by the efforts of nature or of art, be removed, leaving dysentery behind. When present they modify that disease so far, that it partakes of the remissions and exacerbations of the fever, and this may generally be known by the same occurring in the current fevers of the day. The intermittent form of the disease is far from being dangerous, unless 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, the dysentery still continuing of as long and uncertain duration as in the simple disease.

The remittent form, however, is usually far more violent, dangerous, and rapid in its course, according to the season of the year, the unhealthiness of the climate, the severity and bad character of the fever, and the circumstances of the parties attacked. The autumn is the usual season for its epidemic prevalence, and the extent and ravages of its career are mainly dependent on the more or less favourable character of the season for its production, on the type of the fever, and on the state of the parties exposed to both, as in fleets and armies. Tropical climates, no doubt, furnish, and must continue to furnish, the

most formidable epidemics of the remittent character, though our own temperate clime has, in days of yore, before the introduction of sanatory regulations or extensive agricultural improvements, been liable to frequent visitations of such epidemics as have been described by Sydenham, Morton, and Willis, and which were attended by such fearful mortality(*a*). “La cause de la mort,” says Vignes, “est

(*a*) Whilst this sheet was in the compositor's hands I had occasion to visit Southampton, and having heard that a work which I had long anxiously sought, and sought in vain, was to be found in the library of the London College of Surgeons, I hastened to that metropolis, and was at once kindly admitted to a perusal of Durondeau's *Traité de la Dysenterie*, justly designated by Dr. Copland as a very “learned work,” (of which more hereafter). I was also most kindly received by the Librarian to the College of Physicians, who took the trouble of consulting not only his own catalogue, but that of other public institutions, for Jones' “*De Morbis Hibernorum*,” &c.; and after some research informed me that it was to be found in the British Museum, whither of course (being limited in time) I at once proceeded; and, without any demur as to forms, was forthwith gratified by its production. This is the work to which I have made reference in the note to p. 11, and which, when produced, explained at once the cause of its absence from our Irish libraries. It is in fact only an “*Exercitatio pro Gradu Doctoris*” delivered in our university, and which he enlarged and afterwards published in London. It is elegantly and classically written, and, for the time, a very creditable composition on the subject of dysentery, but without any pretension as a treatise “*de morbis Hibernorum*.” I was disappointed, however, at finding no other reference to the recent sufferings of the English army by dysentery in Ireland than what follows: “*Huc etiam spectat illud remedium a Rayo in synopsi sua memoratum, neque a me propter egregiam, quam non ita pridem exercitui*

souvent due, dans les dysenteries de mauvais caractere, à la qualité delétère de l'infection qui a préparé et souvent aussui déterminé la maladiè."

The combination with typhus, though now happily less frequent than formerly, is by far the most important form of the disease ; it agrees in every respect with Cullen's definition, and is to be distinguished by the marks characteristic of typhus fever. For, as is well remarked by Frank, "*Qui fidam typhi seu febris nervosæ gravioris imaginem a nobis depictam coram oculis posuerit, eique dysenteriaë phenomena, ut symptomata, superaddiderit, is longiore hujus*

Anglicano in Hibernia gravissimâ dysenteriâ laboranti, tulit opem, sine piaculo hic loci reticendum: fungosa, scilicet, substantia nuclei Juglandis lobos intercedens, vinoque modicâ quantitate exhibita." Now in Ireland at that time (1698) there existed no sanatory regulations, and so little of agricultural improvement that the Irish Government, about thirty years after that date, gratified its wisdom by obtaining an Act of Parliament to compel, under a penalty of forty shillings, every holder of 100 acres to keep five of them at least under tillage ! Hence, together with the influence of extensive tracts of wood, both standing and felled, the general prevalence of agues, remittent fever, and epidemic dysentery, both remittent and contagious. Dr. Jones would appear to have been some time practising as an M. B. before he wrote his *Exercitatio*, as by and bye we shall find him referring to his experience in a very fatal epidemic of the disease which prevailed during these years in South Wales: "*Ubi adeo immaniter desdeviit tantasque strages edidit, ut in ædibus non paucis vix unus aut alter superesset, qui sepulturæ traderet defunctos.*"

dirissimi morbi (i. e. dysenteriae asthenicae malignae) descriptione haud facile indigebit." He then enumerates some of the symptoms, such as "subita et magna vel summa mox virium vitalium prostratio, cum pulsu exili, artuum tremore, subsultu tendinum, spasmis, somnolentia, deliriis, &c., non raro petechiae, maculae lividae, nigrae, miliaria," &c. "In castris, in urbibus obsidione circumductis, et in classibus maritimis, nec minus in carceribus, ergastulis, strages saepe haec dysenteriae species immanes, a plebe ad cives reliquos transire solita, edidisse est visa"—"Quod si," he adds, "febris quae dysenteriam malignam pro symptomate agnoscit, contagio suam originem debuerit; difficultas quoque ipsa intestinorum particeps erit contagii, nec tamen erit illis in casibus, in quibus morbi contagiosi non fuerit effectus."

This form is always preceded by symptoms of fever, and its access is generally attended by rigors, nausea, vomiting, and *great prostration of strength*(a).

(a) "Satis apparet," says Van Swieten (Tom. 3, p. 193), "quam multiplex dysenteria sit quoad causas, unde producitur. Verum et mira saepe talis causae invenitur subtilitas, contagio communicato inficiens sanos." "Observaverunt tamen medici castrenses, quandoque dysenteriam grassari, licet causae procatarticae non adfuerint manifestae, sed lateat infesti stimuli natura: tunc saepe in ipso morbi initio ilico prosternuntur vires, non a copia excretae materiae exhausto corpore, sed venenata quasi vi suscepti miasmatis morborum, deleta omni robore in sanissimis etiam paullo ante hominibus." Though Van Swieten was obviously ignorant of the circumstances under which dysentery became contagious, he yet very accurately marks the mode in which the disease attacks when it is.

Its progress is accompanied by the more advanced symptoms of the same fever, and its termination, too often unfavourable, is marked by all those which are usually present in typhus, the fatal termination and crisis falling on the same days as in that fever ; and in every respect, says Grimm, "Ratio vitam ponendi omnibus ferè eadem, ac in febribus malignis fuit." This form is exceedingly dangerous, and its more immediate danger generally proportioned to and dependent on the severity and bad type of the fever: it is propagated solely by contagion ("licet causæ procatarticiæ," as Van Sweiten states, "non adfuerint manifestæ"), and in its mode of propagation resembles fever. Even when terminating more favourably, by the subsidence of the fever, the dysenteric affection may and does generally continue more or less, to the no small danger of the patient: the contagious property of the disease, however, I should think, ceased with the influence of the fever(*a*), as upon that it originally depended.

(*a*) The following statement, extracted from Lind's first paper on Fever and Infection (p. 37), may be objected to this position: "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 patient's stools, which were slimy and very offensive: upon debarring him

Of the more immediate and remote danger attending its visitation, ample testimony is furnished by Willis, Vignes, Cheyne, and sundry others. "Dysenteria," says Willis (cap. x. de Febris, p. 121), "est effectus in febris continuis adeo frequens ut quibusdam annis epidemius evadat, et peste non mitior, plurimos interimat." Vignes, who encountered the disease in all its forms during the French campaigns in Spain and Germany, gives many interesting and minute details on the subject generally, and on the mortality specially. When the disease was severe and complicated, and the condition of the soldiers unfavourable, there often perished from one-third to one-half of the sick. On one occasion,

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 appear, however, from this relation, that the rheumatic patients or nurses were infected with dysentery, for he 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, or 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 among the patients and nurses was dysentery; it was more probably a diarrhœa, arising from some cause common to them all: for infection from a dysenteric patient must produce a disease like to itself in every respect. "Il y a beaucoup d'autres causes determinantes (says Vignes, p. 205) des dysenteries, mais nous ne nous arrêterons que sur celles des emanations des excremens des dysenteriques, parceque beaucoup

speaking of the employment of warm baths in malignant dysentery, he cautions against the free use of them for fear of fainting, and adds, "les trois variétés de la dysenterie maligne sont en general si graves, qu'il y a tres peu d'espoir de sauver les malades."

The details furnished by Cheyne of the dysentery which prevailed in Ireland, concurrently with the contagious epidemic fever of 1817 and 1818, are highly instructive on many points connected with this combination of disease, and exhibit specimens of the various phases it may assume according to the character of the fever. It would appear that the disease manifested itself in different places at different periods and at different seasons of the year, being very prevalent and fatal, and in general deemed conta-

de gens de l'art sont encore dans l'opinion qu'elles propagent la dysenterie par contagion. Mais ils ne s'expliquent pas par quelle espèce de contagion: car on peut en reconnaître deux espèces, la contagion *essentielle*, qui se propage partout, et la contagion *relative*, que ne se communique que sur les lieux, par une plus ou moins longue exposition aux miasmes infectés des corps. C'est par cette dernière que la dysenterie se communique, assez souvent dans les lieux où il y a plusieurs dysentériques: nous avons vu beaucoup de malades la contracter en allant aux lieux communs où les dysentériques avaient rendu des selles. Nous en avons vu plusieurs autres dans les salles des hôpitaux, être atteints aussi de dysenterie après avoir séjourné pendant quelque temps à côté des dysentériques et sans que nous ayons pu voir que la condition d'une cause brusque déterminante, comme un froid subit, fut nécessaire. Ainsi comme on le voit, ce n'était qu'après une exposition réitérée aux miasmes putrides des excréments des dysentériques qu'on gagnait leur maladie."

gious. At Cork one in three of those attacked was the estimated mortality. From all the statements he had received, and they were numerous and authentic, Cheyne was disposed to conclude that "the inhabitants of Cork were liable to dysentery in each of its principal varieties, both as connected with continued fever and with intermittents: the former varieties contagious; the latter not at all so." And further, that the dysentery of 1818 "rather belonged to that great variety of dysentery which originates in causes that are also productive of continued fever(*a*)."

Cheyne's accurate delineation of the epidemic, as it appeared in Dublin, is, that it "very often arose *during convalescence* from fever, and in many instances he ascertained that the preceding fever was not attended by any unusual gastric irritation. Sometimes it commenced at the termination of fever, and frequently it arose in the course of fever," but, from the peculiar nature of the fever of the season, it was sometimes difficult to mark the precise time when the new disease commenced. The fever having, during the prevalence of dysentery, in place of extending to the end of the second and third week, assumed a duration of five, seven, or nine days, and then very liable to one, two, or more relapses, it was

(*a*) Dr. Cheyne, whose professional attainments and private worth all who had the pleasure of knowing him so highly respected, here refers in flattering terms to the first edition of this work, imperfect as it was.

observed that dysentery frequently occurred at that period of recovery from fever when there seemed the greatest liability to relapse; and that, whether in connexion with fever or not, it often commenced with a rigor, and terminated in free perspiration; that it was sometimes converted into a fever, while on the other hand fever was frequently converted into dysentery: in short, these forms of disease were convertible the one into the other, so that the opinion of Sydenham, that dysentery is a *febris introversa*, received support from our observations." He further remarked that "the most severe cases in the *first* stage, and the most unmanageable throughout, arose in the course of fever; the mildest cases were those which seemed to have the *least* connexion with that disease." At one period a majority of the patients died who had been ill more than six or seven days, and of those who were emaciated scarcely one recovered. "In some cases the disease hurried to a fatal termination with a speed which nothing could retard; and sometimes a case, at first marked with no symptoms of unusual danger, suddenly changed its character to that of uncontrollable severity(*a*)."

The great prevalence of the contagious form of

(*a*) Frank noted similar results: "Dysenteria, febris asthenicæ, contagiosæ, seu typhi gravioris effectus, quamplurimum ominosa est. Sæpe febris dysenteriam sub initio satis mitem offert, eundo vero larvam, mox abjicit, ac improvisa nunc struit pericula." "Paucarum dierum spatio dysenterias malignas subinde mortem induxisse jam diximus."

dysentery is not limited, like the last, to the autumnal season, though the combination may generally originate at that period of the year: it extends its ravages to the winter months, being dependent not so much on the seasons as on the febrile contagion, and continues so long as the causes of that fever continue to operate. The last quotations from Frank and Van Swieten, and Cheyne's statement, strongly illustrate these positions, diversified though these may be by the peculiarities of the original fever. That fever may, as we have repeatedly proved, be either originally a malignant typhus, or it may have been a remittent of bad character, converted by the peculiar circumstances of the sick into one of a contagious continued type. Such a change, though doubted by Bancroft, is too well authenticated to be called seriously into question (*a*). "Nous ferons remarquer," says Vignes (p. 365), que l'infection d'une maladie ne produit pas toujours la meme espece d'affection: l'infection de la dysenterie peut, par exemple, occasioner une indisposition bilieuse ou une fièvre adynamique," &c., "de meme qu'on peut être affecté de celle-ci (la dysenterie) par toute autre exhalaison délétère que celles des dysenteriques."

If there be any truth in the statements now submitted to the reader, from authorities ancient and modern, and from observers in every clime, the im-

(*a*) Blackbourne's chapter on Infection in his Treatise on Scarlet Fever and on the Origin of acute Contagions, may advantageously be consulted on this point.

portance of the distinctions I have proposed for his consideration must be obvious. In *theory* they are so far satisfactory, as they furnish a ready solution of the many difficulties and contradictions we meet with in the best histories of the disease. But it is in a *practical* point of view that they possess the strongest claims on our attention. By them we are admitted to a clear and, I would hope, a correct view of all its varieties ; and, no longer embarrassed by uncertain opinions and indecisive practice, we are led in one case not to entertain unnecessary fears of contagion, or in another to deride them as unfounded ; but are thereby taught when to consider precautions requisite, and when to reject them as needless. These distinctions, too, will be found to constitute our best guide to a safe prognosis and a sound practice. The truth of this latter position will be amply exemplified in the subsequent chapter, which may also satisfy us of the correctness of Rush's remark that "as the discovery of truth in religion reconciles the principles of opposite sects, so the discovery of truth in medicine reconciles the most opposite modes of practice."

CHAPTER VI.

TREATMENT OF DYSENTERY.

PROOFS THENCE DEDUCED OF THE TRUTH OF FOREGOING
CONCLUSIONS.

IN conducting this inquiry it has been my object not to give a regular or systematic account of the disease, but to establish certain propositions respecting it. In like manner it is not my intention on the present occasion to enter into any minute history of its treatment, but merely so to review the principal remedial plans proposed or adopted by various authors, as may enable us to judge whether their practice was influenced by any difference in the combinations of the disease, and whether the truth of the propositions themselves is affected thereby.

If I am justified in the distinctions that have been made, their truth must be perceptible in the different plans of treatment adopted in the disease: and if any difference be obvious in the general treatment, it must be found principally in the class of evacuants, and more especially in the use of a remedy that usually takes precedence of all others, whether employed with propriety or not.

And first, then, of VENESECTIO. Respecting the necessity or advantage of this remedy, we find considerable variance of opinion amongst practitioners. Some consider it highly necessary; others deem it useless; and others, again, look on it as injurious. We shall be at no loss to discover that such as met the disease in its simple state, or in combination with intermittent and remittent fever, form the two first classes, whilst such as 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; yet it must 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 inflammations;” and with respect to extreme cases, Hunter observes, “that in slighter cases, or when the disease is treated early, other remedies are so effectual 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 is decidedly in favour of this operation: “Sanguinis detractionem, inquit, supervacuum fore, medicis insedit diu, in vereravitque malè fundata, malè ominata opinio. Id verò auxilium inter initia, *maximè si signa inflammationis aliqua adessent, nunquam alienum* deprehendi.” He even

speaks of its repetition under particular circumstances, and concludes with this observation: "Neque prudentis est ac circumspecti medici timere, ne imbecillum reddat ægrotantem, cùm capitis res agatur."

Hoffman uses language as strong: "Multiplici ductus experientiâ statuo, quòd in subjectis *valdè plethoricis*, diætæ vinosæ adsuetis, si dysenteriâ *cum febre continuâ* corripiantur, omnino perquam sit necessarium venam ab initio morbi secare." And again he says: "Ego certe sanguinis missionem cum judicio et discrimine in hoc morbo et præcavendo et curando institutam, tanquam remedium et necessarium et saluberrimum certâ experientiâ et ratione sufficienti fultus censeo."

Huxham, the remittent character of whose epidemic has been already described, deems venesection very necessary, "ob magnum sanè et insolitum tempestatis calorem;" to which he adds: "Vix unquam datur dysenteria epidemica, ubi intestina non sunt, aliquo saltem gradu, inflammata." We may observe that neither of these authors positively recommend venesection, 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 more or less independent of that disease arose; and the latter found it necessary in plethoric subjects, "si dysenteria cum febre continuâ corripiantur."

Rollo thinks venesection seldom requisite in dysentery, as it is not at first necessarily connected with

inflammation, which when it occurs is, he says, of the erythematic kind, is superficial, and produced by no inflammatory diathesis, but by causes immediately acting on the interior surface of the intestines.

Moseley recommends venesection under two different circumstances: "There are but few instances," he says, "where it may not safely 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." He afterwards adds (p. 282), that "when the patient, after taking his diaphoretic medicine, instead of sweating, becomes restless and hot, there venesection or an emetic is necessary, which never fails to dispose the body to a sweat"

Tissot says that dysentery sometimes 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 it is united with a putrid fever he does not mention the remedy, and when joined with a malignant fever he cautions against its use(*a*).

(*a*) "Quodsi dysenteria," says Frank, "subjecta robusta, juvenilia, vel ad morbos energicos disposita corripiat, ac febris hypersthenicæ symptoma constituat; si dolor intestinorum atrox, ventris major ad attactum sensibilitas: si pulsus satis constans, magnus ac fibrans aut præ doloris vehementiâ contractus, parvus sed durus compareat: tunc facta atque repetita adeo primis morbi temporibus venæsectio, ac methodus debilitans præstantis-

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, indicate, and the circumstances of the patient will permit. Sydenham generally bled, and Stoll used venesection occasionally. Richter makes no mention of it(*a*).

“In the inflammatory dysentery,” says Zimmermann (p. 179), “venesection in the beginning is a

simum et unicum subsidium.” But to this he elsewhere adds: “Ad venæsectionem quod pertinet, ea in verâ febre asthenicâ indicari certè haud potest: suspicamur interim, in tropicis morbos non paucos acutos, sub suâ invasione, sthenicæ potius indolis esse, ac vi stimuli majoris citius quam alibi, in asthenicos transire: igitur sub primo tam præcipitis morbi insultu, sub ventris dolore atroci ac pulsu contracto et duro sanguinis mittendi necessitas omnino subintret.” These are suggestions well worthy of attention.

(*a*) Bianchi, after discussing the propriety of venesection in what he calls “bilious dysenteries,” but which with more propriety might be designated bilious colics, or bilious fevers with dysenteric pains, tension, or tenesmus, says, p. 556: “Epidemica maligna dysenteria, quæ ab improba aeris constitutione progenita, inflammatorias sanguinis in visceribus stases secum plerumque conducit, neque quidem ex unâ bile, sed ex omnibus corporis impensè alteratis humoribus ortum obtinet, sæpe phlebotomiam inter initia depostulat. Ita habet Duretus in epidemica illa malignâ dysenteria quæ in hominum vitam invasit Lutiæ Parisiorum ann. 1578, detractionem sanguinis ex majoribus venis multis ingenti auxilio fuisse. Imo tametsi dysenteria maligna non sit, sed duntaxat epidemica, videtur principio phlebotomiam exigere. Ita Sydenhamius, in epidemicâ quatuor annorum dysenteria quò primùm arcessebatur, tendendam cubiti venam suadebat.”

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 astonishingly quick and good effect." "This species of dysentery," he says (p. 160), "shewed itself 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 tenesmus and unquenchable thirst, and such an inflammation from the œsophagus downwards 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. Some who were walking about the streets at five o'clock in the afternoon, were seized with it, and died at ten o'clock at night."—I have given at full length the description of that disease, which Zimmermann has called the inflammatory dysentery, because I think it carries with it sufficient evidence to prove that it was not any form of dysentery whatever. If it be dysentery, it certainly differs essentially from any epidemic of that disease on record, in its mode of attack, in its symptoms, and rapid fatality. Besides the great and striking advantages of venesection in this disease, Zimmermann adds (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 a further argument against calling the disease a dysentery(*a*).

(*a*) Vignes (p. 82) gives an instructive case, which he designates "une dysenterie inflammatoire porté au plus haut degré de violence et compliquée d'embarras bilioso-muqueux," and in which the inertness of French practice is fully exemplified. Dissection (on the twelfth day of the disease) shewed extensive inflammatory redness in the whole of the large intestines, and in the inferior portion of the jejunum, with thickening, &c., more especially in the rectum and anus, together with effusion (4 oz.) of a turbid liquid in the peritonæal cavity. In vain were employed for relief "un diete absolue, des boissons delayantes, des lavemens emolliens, des bains tiedes, d'ipécacuanha, de laudanum," &c. The patient, as might be expected, died, "au milieu du torment des douleurs." We would unhesitatingly bleed, both generally and locally, and freely administer mercurials; whereas the remarks of Vignes (a most observant Hypocritic physician) on the case are as follows: "Si j'avais maintenant une semblable dysenterie a traiter, je ferais de saignées locales a perinée ou a la partie superieure des cuisses, ou enfin sur l'abdomen. J'en fus detourné par les symptomes bilieux, qui se manifesterent des l'invasion de la maladie, mais considerant que le malade n'était entré que le deuxieme jour a l'hospital, la phlegmasie avait fait trop de progrès pour qu'on put remedier aux desordres qu'elle occasionne: ne connaissant pas d'ailleurs d'exemple de guerison de dysenterie aussi intense que celle-si, *je doute* donc qu'on puisse jamais guerir des inflammations intestinales aussi violentes, ni peut-etre en arreter les progres en leur opposant les saignées, soit generales, soit locales," &c. Yet in his preface he says that "la saignée," though injurious in several varieties of dysentery, "n'est pas moins un moyen *heroique* dans les dysenteries tres inflammatoires!" He notwithstanding ventures (p. 270) to recommend (besides other his usual means) "les sangsues au perinée, a la partie superieure et interne de cuisses, ou sur l'abdomen et mieux a la region lombaire, et la saignée du

Except in this species of dysentery, Zimmermann makes no mention of venesection unless to condemn it, more especially in the malignant form of the disease. "I, for my part," says he, "entirely reject venesection in the *malignant* dysentery, especially as I have already found it unnecessary in the bilious dysentery. In another place 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 venesection, though dysentery *of itself* may not require that evacuation, but, from the accompanying inflammatory symptoms, it is frequently indispensable, 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 dysenteries, as being of a more inflammatory nature; in these he found venesection and rhubarb sufficient

bras dans quelques cas, doivent etre hardiment employés, si l'inflammation est violente et surtout si les sujets sont robustes." In the mucous variety, even though it increased in intensity, he says (p. 279): "on doit ordinairement suspendre les diaphoretiques et les aromatiques: mais malgré cette augmentation la saignée generale n'est presque jamais necessaire: la saignée locale au perinée, par les sangsues, peut seule trouver quelquefois son emploi." He says the same of the bilious variety; but in both these varieties it is that he specially recommends evacuants (emetics and purgatives); and in the former he even ventures to recommend calomel in small doses—from two to four grains; and he would go even further, "si les malades ne s'en trouvent pas trop incommodés;" but in the bilious variety (in which we would specially employ it) he does not once allude to that remedy.

without vomits, for then the stomach is less disordered. Pringle, we may remember, refers the treatment of the *malignant* dysentery to that of the hospital or gaol fever: in which venesection must be generally inadmissible.

When detailing the circumstances incidental to the campaign against the rebels in Scotland, he says that "the flux, with every other disease of this camp, being attended with sizy blood and other marks of great inflammation, made large and repeated bleedings more necessary here than in a warmer climate." "But when the malignant fever was joined to any common inflammatory disease, a mixture of the two arose, which made the most perplexing case, as the indications of cure were so contradictory."

"When the disease," says Clarke, "is attended 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 he afterwards adds: "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."

"Most of the recent fluxes which we saw," says Monro, "were at first attended with a good deal of

fever and pain in the bowels, and required more or less blood to be taken away. When the patients were strong, and complained of sharp pain in the bowels, attended with a fever, we used the lancet freely ; nor were we discouraged from bleeding in *the beginning* by the low, quick pulse which often attended the disorder ; and we frequently found the pulse rise as the blood flowed from the vein." In a note he adds that, "in recent cases the operation was extremely necessary, and contributed greatly to the relief as well as to the cure of the patient: but when the sick were low and weak, without much pain or fever, and the pulse soft, we were more sparing of the vital fluid."

Even local blood-letting would sometimes appear to have been of little service in *our* dysentery, since Willan declares that the repeated application of leeches to the abdomen did no good. I have, however, found it far otherwise, having seen great benefit experienced in cases where the local pain was greater than usual, and in some such instances venesection was also required.

I cannot conclude this article without exhibiting an incautious extract from Wilson's Treatise on Febrile Diseases on the employment of this remedy in dysentery. He says (p. 631): "the reader will find many speaking 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 essential change in the state

of the local affection, and therefore insist upon its employment wherever the general state of the system admits of it; and if an inflammation of the bowels always attended the 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 (*a*). Instead,

(*a*) Dr. Cheyne's experience in the Irish dysentery, epidemically concurrent with the fever of 1818, is an adequate answer to these queries, shewing that they should be answered in the affirmative, and not in the negative, as the propounder of them supposed. "Finally," says Cheyne, "we are often cautioned against venesection, which was certainly the remedy the *least* equivocal in its effects, the most uniformly useful of any which we employed. When, with pyrexia, the stools chiefly consisted of bloody mucus, more especially when the abdomen was tender, venesection was never omitted; the blood drawn was in many cases cupped and buffed: the tenderness of abdomen was relieved: the character of the stools was sometimes immediately altered, and a large, loose,

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."

Though not indisposed to subscribe to some of the foregoing observations, as applicable to the dysentery of these temperate climates, yet even there a little mismanagement or delay in the employment of appropriate remedies may render the operation of blood-letting indispensable, whilst at the same time I cannot hesitate to admit that, in countries

and feculent stool, without straining, was not unfrequently passed after the loss of sixteen ounces of blood by patients who for several days before had passed nothing but mucus mixed with blood." He adds that those who object to venesection have surely never witnessed the great relief which profuse hæmorrhage from the bowels sometimes affords to the dysenteric patient. Frank confirms this statement when he says, "*profluvium cruoris ex ano, etiam sat multi, sub hujus morbi exordio proficuum esse solet;*" and it should be remembered also that the fever concurrent with Cheyne's epidemic dysentery bore venesection well in its early stage. "It is worthy of remark," say Drs. Barker and Cheyne, in their history of that fever, "that almost all who had given a full trial to moderate venesection in the early stage of the fever, were liberal in its praise." Though Cheyne's testimony to the special benefits of venesection, even in cases approaching to the chronic state, may be deemed sufficient, yet we are not without additional evidence from other quarters of the advantages derived from venesection in cases of long standing. Sydenham, for example, mentions the case of a woman (a neighbour of his), who for three years was molested by a dysenteric state of the bowels after an acute attack of the disease: "*Cum remedia quàmplurima esset experta antequam ad me accederet, venæsectionem tantùm,*

under a warmer sun, where disease runs a more rapid and fatal course, it is highly expedient to meet the danger, which notoriously attends the tropical dysentery, by an early use of the lancet and the free exhibition of mercurials. Holding, therefore, in view the variable mortality of the disease according to the climate, we should be cautious in adopting too generally Wilson's axiom, rather to "avoid" than employ the operation of blood-letting. Dr. J. Johnson very justly remarks: "we should, however, never forget that a disease, though not primarily inflammatory, may often have a strong tendency to

missis cæteris præsiidiis, celebrandam censui: quam ut sæpius repeterem, longioribus tamen intervallis, tum sanguinis color, pleuriticorum sanguinis æmulus, tum insigne illud quòd post singulas vices, magis ac magis auctum sentiebat levamen, mihi addebant animos: cujus ope tandem pristinam sanitatem consequuta est." We possess further evidence on this subject in that highly interesting tract of Botallus, "De Curatione per Sanguinis Missionem," first published at Antwerp so far back as 1582, in which he boldly and successfully contends with all the prejudices of the day against venesection, fostered as these were by Galenic and Hippocratic aphorisms, a dangerous mode of conveying knowledge, though useful and unavoidable, perhaps, in the early periods of the art. In chapter iv. of that tract will be found some valuable facts, and, *for the day*, much sound reasoning on the necessity and advantages of venesection, both in the acute and chronic stages of dysentery, unless a state of ulceration shall have been already established: "quæ exesio et corruptio," he says, "necessario omnes graves dysenterias sequi solent, quas qui non videt fieri ab inflammationibus et eas pro curatione non minus desiderare sanguinis missionem quam pleuritidem (sed certè non tam largè et confertim) sciat se artis medicæ parùm cruditum fore."

run into that state, and that we should commonly use the lancet as boldly as we do in severe cases of spasmodic colic, and with the same views, namely, to remove pain and to prevent inflammation. Whenever the pulse and heat are high, and the abdomen painful on pressure, i. e., *permanently* painful, and confined to any given point, there is reason to fear local inflammation, which it behoves us to subdue by vigorous depletion." Mr. Bamfield's Treatise on Tropical Dysentery (1818), may be read with advantage on this head, though perhaps he goes too far when he states that "the symptoms and events of cases soon convinced him that the dangerous symptoms and fatal terminations of idiopathic dysentery were those of inflammation, which could not be subdued and averted without the aid of bleeding;" which indeed he liberally, and, as would appear, successfully, employed amongst his sailors, whenever there was "constant, fixed, acute pain in some part of the abdomen, with unequivocal inflammatory fever(a)."

(a) Mr. Bamfield, in a note (p. 55), says that "when European dysentery is said to be contagious, it is now generally believed that the contagion is propagated by the fever of the typhoid form that accompanies it." He subsequently states that he has never seen dysentery communicated by contagion, and then suggests that "it would, perhaps, be more proper to consider the typhus fever as contagious, and the dysenteric symptoms as peculiar to a variety of typhus, which might be denominated dysenteric typhus instead of contagious dysentery." This is a suggestion which I had long since anticipated and obviated. Before I quit this very practical and energetic practitioner, I may be permitted to com-

Dr. Somers (who seems to think that bleeding was not a very usual practice in dysentery) states that "in every recent case recourse was had to immediate venesection, repeated at certain intervals so long as blood was voided by the stools, and that success almost universally was the grateful result of the *adventurous* practice(*a*).” He adds that “where, in recent cases, from local influence, intermittent was superinduced, he disregarded the presence of the latter, and, aiming only at the dysentery, he has taken away blood ;” the result frequently being that the intermittent was softened down into a mild continued fever, and was no further an obstacle to the urinary indications. He further states that in the Peninsula he never met with a case of recent dysentery complicated with low typhoid fever, and that therefore “the venesectionary plan was resorted to with a full and sure anticipation of a successful issue.” He denies a specific contagion to dysentery, and on the evidence of his own “not inconsiderable experience,” states that he “never witnessed the production of

plain of a very exaggerated statement made by him of my opinion (p. 128) regarding diaphoretics, though accompanied by a complimentary note. He states that I “had drawn a conclusion that this disease is cured by perspiration in every instance.” I never laid down such a doctrine, nor entertained it, though favourably impressed with the beneficial agency of such remedies in cooperation with venesection, emetics, and purgatives, &c.

(*a*) It may not be amiss here to direct the reader’s attention to Dr. O’Beirne’s remarks on this “adventurous practice :” they will be found under the heading of “Opiates,” article Tobacco.

contagion by pure unmixed dysentery." He had, however, met with some cases "in which low fever and chronic dysentery were coexistent: in those unquestionably there was *contagious disease*; but this evidently arose from the superinduction of typhus upon dysentery." Dr. Somers is not explicit in stating here whether this "*contagious disease*" was simply typhus, or the disease compounded of both; I presume he merely means the former.

Dr. Luscombe, in his "Practical Observations," states that in thirty-one months of the Peninsular campaign, 23,000 cases of dysentery and diarrhœa had occurred, and that, taught by experience after some deaths by dysentery, he had changed his treatment to the antiphlogistic plan, "with the most decidedly beneficial result." In his "note, however, on the treatment of dysentery," he mentions that in a fatal case, "admitted with symptoms of diarrhœa, which became aggravated, with tenderness of the abdomen on pressure," death occurred on the tenth day, though the man was "twice bled with temporary relief." On dissection there was found a considerable quantity of coagulable lymph effused on the surface of the small intestines, together with many recent adhesions between different convolutions of the upper portion of the intestinal canal," which were, as he states, "precisely the appearances found in the former cases, and left no doubt of the inflammatory nature of the disease." Now the case here given, as a specimen, was a case of peritonæal and not of dysenteric inflammation.

His rule was (and it was a correct one), always to bleed in recent cases of dysentery, when "tenderness was experienced in the abdomen on moderate pressure by the hand." Sir James Macgregor states that dysentery was the disease which produced the greatest mortality in the army, attributable, no doubt, to the very unfavourable circumstances in which they were placed. Sir James approved of Dr. Somers' plan of treatment by free venesection, and says when there was much pain on pressing the abdomen, he has seen the lancet give immediate relief(*a*).

I have thus brought together the principal authorities respecting the employment of this important remedy, and may safely leave the reader to form his own judgment thereon. I shall hereafter have a more favourable opportunity of stating my own views, when specifying the treatment appropriate to each form of dysentery.

(*a*) It may not be amiss here to subjoin some judicious remarks of Vignes (p. 224) respecting the inferences to be deduced from pressure on the abdomen: "Quelques considerables, (dans la variété bilieuse), que soient les tranchées et le tenesme, *qui sont constans*, la pression sur l'abdomen ne les augmente pas ordinairement. Cette circonstance qui semble s'expliquer par la presence de la bile, qui exciterait les douleurs, a fait dire qu'il n'y avait pas de phlegmasie dans la dysenterie bilieuse; mais nous savons tres-bien que la pression sur le ventre ne dècèle pas toujours la phlegmasie des intestins. Dans d'autres cas, au contraire, il y a de tumefaction de l'abdomen et alors la pression augmente toujours les douleurs: dans ce cas, il est a craindre qu'il n'existe une forte phlogose, non seulement dans tout le corps de l'intestin, mais aussi dans la peritoine."

EMETICS.—Few articles of this class have been overlooked in the treatment of dysentery : indeed there is scarcely any one of them which has not been at one time or other recommended with extravagant praises ; and though this circumstance alone is sufficient evidence of the general efficacy of emetics, as anti-dysenteric remedies, we should cautiously guard against an indiscriminate use of them in every state or stage of so diversified a malady. The articles of this class, according to their nature or mode of administration, may 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 to the surface. 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. Some articles in this class possess superior efficacy in producing one or more of the effects mentioned, and upon their possession of this property, and not of any specific power, must their reputation in dysentery rest. On reviewing the treatment of this disease we shall find that emetics have been employed by some with the sole intention of unloading the stomach, by others with the view of 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 former indications : they are most beneficial in the intermittent and

remittent forms : 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. "As the disorder," says Munro, "was for the most part attended with sickness in the beginning, we gave a vomit after bleeding ; which not only discharged the contents of the stomach and a quantity of bile, but relieved the sickness, and frequently threw the patient *into a breathing sweat* ; and made the purgatives, which were given next day, operate more freely, and with more evident good effects, than where no vomit had been administered."

Moseley, in his West Indian practice, after bleeding, began with a vomit of ipecacuanha, to relieve the stomach from a load of porraceous and bilious impurities ; but after this effect the great expectation he entertained from vomiting was the determination thereby made to the surface. In London it was his practice to order the patient to bed on taking the ipecacuanha, and direct that the operation of sweating, rather than vomiting, should be promoted.

Zimmermann gave emetics even when the stools were very bloody, as less blood came away afterwards ; "nature herself pointing 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." He always omitted the emetic when there was the least suspicion of inflammation. He says that Eller found no evacuation so conducive towards the cure of an epidemic dysentery as vomiting, excited at repeated times,

thoroughly to expel the irritating bilious matter. He further observes (p. 236), that "in the malignant dysentery, evacuations must sometimes be entirely omitted; emetics being sometimes noxious at the beginning, though very often it is necessary to give a vomit first, and afterwards purges."

In general emetics are most effectual and have the best effects when they operate likewise by stool. Pringle, in imitation of Eller's practice, so managed the ipecacuanha as to effect both purposes, for he gave it in small doses several times repeated, till a vomiting or purging ensued; fifteen grains administered in this manner procuring a larger evacuation than thirty grains given at once. This practice he laid aside, from the great sickness which generally accompanied its operation, though he declares 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 may be very requisite; we must then employ the emetic tartar, alone or in combination with the ipecacuanha, as more powerful in clearing the stomach and intestines of their contents(*a*). Akenside's mode of giv-

(*a*) Vignes, who employed emetics very generally in small and frequently repeated doses till they acted for relief of "l'embarras gastrique," prefers in all the varieties of the disease (except the typhoid), the tartar emetic, when the patient's strength is good: "Le tartrate," he says, "convient de preference a l'ipecacuanha, en ce qu'il procure ordinairement une transpiration ou une sueur, en même temps qu'il établit une irritation momentanée sur l'esto-

ing the ipecacuanha was still more in the extreme, for he gave but one grain every six hours, and on it alone (with the exception of venesection and an emetic) he relied for the cure of dysentery.

The efficacy of ipecacuanha has by some been attributed to an anti-spasmodic, and by others to a purgative power : by Friend more justly, perhaps, to the diaphoresis consequent on its use : “ Radix ipecacuanha,” he says, “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 vindicat.” Piso, who bestows extravagant praises on this remedy, attributes part of its good effects to an astringent quality : “ Quippe præterquam,” he says, “ quod tutè et efficaciter tenacissimos quosque humores per ipsam alvum, sæpissimè autem per vomitum ejiciat, et a parte affectâ derivet, vim quoque astrictivam post se relinquit(*b*).” Sir G. Baker prefers

mac.” Even in the typhoid variety he does not hesitate to administer emetics, but in that case prefers the ipecacuanha. “ L’expérience ayant prouvé que malgré la nature de cette dysenterie, les vomitifs sont indiqués et tres souvent utiles *dès le commencement*, il faut profiter de ce temps pour les administrer. Le tartrate antimonié de potasse ne convient pas aussi bien que l’ipecacuanha, parce qu’il porte, comme tous les antimoniaux, une atteinte considérable dans les forces vitales, qu’il est toujours tres important de ménager.”

(*a*) Blane bears testimony to the good effects of ipecacuanha, without specifying the mode in which it acted. “ The best me-

the emetic tartar, as superior in emetic and diaphoretic effects: and as possessing this great advantage, that, after clearing the stomach, it procures also a full evacuation of the intestinal canal. From its power of producing these two important effects, the *vitrum antimonii ceratum* obtained its great celebrity in dysentery, in which it would still maintain a name, but that the occasional violence and uncertainty of its operation rendered its exhibition at times either dangerous or nugatory.

All are thus more or less agreed on the beneficial action of emetics, whatever may be their mode of operation: all, too, seem equally agreed on the expediency of their *early* exhibition in the disease. M. Bruant is the only practitioner who familiarly employed them in the more advanced stages: "le jour de son arrivée a l'hopital, le malade prenoit un vomitif (d'ipecacoanha). Je ne m'inquiétois pas du temps qui s'étoit écoulé depuis l'invasion de la ma-

dicine," 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 more delicate constitutions in private practice. Twining, in the *Calcutta Transactions*, recommends repeated doses of from three to six grains of the ipecacuanha, combined with an equal quantity of extract of gentian, as (without disturbing the stomach) acting very favourably on the bowels, with the occasional aid of the blue pill after venesection. He thinks hepatic derangement a frequent *consequence* of dysenteric affections. Others have beneficially administered the same remedy in much larger doses in combination with full doses of laudanum, blue pill, &c.

ladie, et j'ai donné des vomitifs avec avantage au dix-huitième ou vingtième jour après l'invasion : il suffisoit que la faiblesse ne fut pas considérable, et ce remède loin d'abattre les forces, les relevoit au contraire."—Vignes would seem to countenance him in this practice, for he says (p. 271), that the ipecacuanha agrees better (than the tartar emetic) "quand les sujets sont délicats ou affaiblis par la maladie, *surtout* si elle est déjà éloignée de son origine."

Thirdly, PURGATIVES.—These remedial means, though less obviously indicated in dysentery than emetics, are yet essential, more or less, to a successful management of the disease. When practitioners permitted their judgment to be guided by appearances, the frequency of stools led them to entertain erroneous notions of the disease, and to adopt a dangerous practice:—hence the early use of astringents and opiates in dysentery, and hence the similarity of treatment in that disease and diarrhœa(*a*). We are now, however, better acquainted with the disease, and in proportion as our knowledge has increased, purgatives have advanced in estimation :

(*a*) We may observe the influence of this doctrine in all the old, and even many modern works on materia medica : in these we seldom find an article praised for its efficacy in diarrhœa, that is not said to be equally good against dysentery.

they are now as generally employed as before they were uniformly neglected ;—the question no longer being, shall purgatives be given, but what purgative, and in what manner.

Hunter always began by giving a brisk purgative, either of the Epsom or Glauber salt: its operation was assisted by drinking plentifully of diluting liquors, till a full and copious evacuation was produced. Sir G. Baker speaks highly of the former: “*Nil fere quidpiam,*” he says, “*aut certius aut citius alvo moranti calcar addit.*” As to purgatives in general, Hunter observes, that though various articles of this class are recommended, 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. After a favourable operation of the purgative he gave an opiate at bed-time: the former 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 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 gentle purges ; and he observed that the patient, instead of growing weak, appeared to acquire a greater degree of strength and cheerfulness from the relief that followed the operation of each purge ; and indeed, says Zimmermann, “ the truth of this great medical maxim appears evident with respect 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, to wit, when the disease has been violent, the purgatives frequently repeated, and the symptoms still recur, while the strength is greatly impaired. In this state of things he gave a strong decoction 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 Zimmermann, “ 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 indications of an acrid putrid matter in the bowels, without inflammation or suppuration.” He always chose them of the gentle and acid kind, as strong purgatives always occa-

sion an intolerable colic, and greatly weaken the patient.

Pringle declares the necessity for continuing the purges is rather to be determined by the pertinacity of the gripings and bearing down, than by the blood that appears in the stools ; and he thinks it impossible to effect a cure without copious evacuations of this kind : he therefore advises us not so much to attend to the dose as to 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*, and calomel ; and these it was necessary to give in succession, or in different forms. Strong doses were 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 repeat the opiate draught 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 be borne oftener than every second or third day (*a*). Zimmermann (p. 242), though he admits the great benefit derived from emetics and purgatives in the malignant dysentery, yet cautions against their indiscriminate or repeated use in that form of the disease ; and as to choice, he decides in favour of the sulphate of magnesia, 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 efficacious. Sydenham, whose favourite purge consisted of an infusion of tamarinds with senna, rhubarb, and manna, says of rhubarb: "licet enim cholerae atque acrioribus humoribus evacuandis sit dicatum, attamen nisi mannae aliquid aut syrupi rosacei eâ quantitate admisceatur ut ad pleniorum catharsin assurgat, in dysenteriiis curandis non ad-

(*a*) "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 costiveness, 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 excited, provided the faeces do not accumulate in such quantity as to produce obstruction or annoyance."

modum conducit(a)." Zimmermann, who, like Degner, would seem to have occasionally confounded choleric with dysenteric affections, made use of it when other purgatives would not remain on the 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. With M. Bruant, in Egypt, rhubarb and cream of tartar was his favourite "*minoratif*." "Others," says 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;" but that cases may occur where the other would be more advisable, as when there is a sense of weight about

(a) "La rhubarbe," says Vignes (p. 274), "a été prônée et l'est même encore, mais elle est trop active: elle exaspère les douleurs des intestins: on doit la bannir, *en general*, du traitement de la dysenterie simple. Tous les purgatifs drastiques doivent être soigneusement écartés aussi: le séné et ses follicules sont également trop actifs." From the scrutiny we have already made of Degner's severest cases, characterized as they are by the most marked symptoms of bilious cholera ("nam in nostra lue," as he says, "*magna copia corruptæ bilis aderat*") we cannot be surprised at his praise of rhubarb, as compared with the effects of more active purgatives, in such a disease. He employed it generally in the state of watery tincture, and, if we are to judge from his language when he calls it, "*divinum potius quam humanum remedium*," we must suppose with considerable advantage.

the stomach, arising from the biliary organs being clogged with bile, and which emetics have failed to remove, then, he says, the calomel has the best effects(*a*).

“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 other in relieving that peculiar state of the bowels which constitutes the disease. Clysters, administered as laxatives, are out of the question for obvious reasons; even those of a mucilaginous and anodyne quality, so much praised by Vignes and continental writers, are unfavourably reported on by Frank; for he says that, as injections into the fauces in cynanche sthenica irritate and inflame

(*a*) On the contrary, says Vignes (p. 273), “On a vanté le calomelas comme une des meilleurs et des plus doux purgatifs: mais nous ne partageons pas cette opinion; car l'expérience apprend qu'il est infidèle comme purgatif, et qu'il détermine presque toujours des coliques: les uns sont purgés avec deux ou trois grains, tandis que d'autres ne le sont qu'avec dix grains, et il n'est pas prudent de porter la dose aussi loin.”

them the more, "ita et intestinorum hâc sub difficultate, nisi feces illis duræ inhæreant, clysteres, quamvis ex rebus mucilaginosi constantes et copia non magna injecti, sæpe magis irritant, quam causam mali auferunt."

I shall conclude these remarks by observing that the testimony of the most judicious practitioners is decidedly in favour of such purgatives as possess an emetic property, and more especially of such as have a tendency to act on the skin: such purgatives, with the aid of opiates, may often be relied on for the cure of simple dysentery, independently of other aid; whereas such as operate solely on the intestines, though effectual in relieving the disease, fall far short of the former as anti-dysenteric medicines(*a*). We shall feel more strongly impressed with the correctness of this opinion after we shall have considered the various testimonies in favour of the next class of remedial means, deemed by many very efficient in the subjugation of the disease.

(*a*) I know not in what light the Profession may view Dr. Mac Gregor's practice of administering in dysentery, as well as in other diseases of India, large doses of croton oil (gtt. v.), with large doses of opium (gr. iii.) or hyoscyamus: if beneficial, it is but illustrative of the efficacy of large doses of calomel in similar combination, and of the truth of the old maxim in dysentery, "*singulari, non satis laudanda, efficacîâ pollet laxantium cum opiatis connubium. Virtute juncta seseque vicissim corrigente, sopiunt spasmos, mucum sine stimuli noxa ejiciunt, atque adeo ad faciliorem versus cutem progressum disponunt.*"

DIAPHORETICS.—The foregoing remedies, venesection, emetics, and purgatives, though well calculated in their *direct* operation to afford considerable relief in dysentery, yet often fall far short of effecting a perfect cure of the disease. This office would seem to be well performed by the class of medicines now under discussion, with due aid from the evacuants already mentioned. Of the truth of this position I hope to produce satisfactory testimony. Indeed we have presumptive evidence in its favour, derived from the high character of ipecacuanha, tartar emetic, and the vitrum antimonii ceratum, which by their diaphoretic action have long laid claim to the title of specific remedies in dysentery. I shall now adduce more positive testimony.

“ This early and reasonable measure,” says Blane (meaning a brisk purgative), “ will, in many cases, put a stop to the disease, *especially* if the patient is thrown into a sweat immediately after the bowels have been thus thoroughly evacuated. It is of great service to promote free perspiration, and even a plentiful sweat, which may be effected with great advantage by giving, at bed-time, a medicine composed of opium, ipecacuanha, and a little neutral salt, accompanying it with plentiful warm dilution. Nothing tends more to relieve griping and tenesmus, than a general warm moisture on the skin. The ipecacuanha, which is an ingredient in this medicine, is one of the *best* anti-dysenteric remedies we know ; the opium procures rest ; and this, joined to the su-

dorific effect of the whole, not only gives a temporary relief, but tends to carry off the disease."—(p. 456).

Vignes very frequently marks the value he attaches to a moist state of the skin in the disease, which, when approaching to convalescence, almost uniformly exhibits this state: "*une legere moiteur s'establissait generalement et quelquefois des sueurs plus ou moins copieuses: c'etait ce qui pouvait arriver de mieux aux malades.*" In another place (p. 63) he notes, "*ce que nous observames de plus remarquable c'est que la maladie paraissait se terminer d'une maniere plus particuliere par des sueurs abondantes. Ces sueurs arrivaient chez les uns le quinzieme jour et chez les autres le vingtieme.*"

"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 repeating 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 surprisingly 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 giving a few doses of a restringent diaphoretic opiate, and was thus, as it were, snatched from the jaws of death."—(p. 214).

Moseley, one of the most strenuous advocates for the sudorific plan of treatment, to the neglect, perhaps, of other efficacious means of relief, would appear, however, from extensive experience, to have been justified in his partiality. It was his practice, after an emetic and opiate, to empty the bowels in such a manner as not to increase the determination of blood in that quarter, and thereby 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 as a diaphoretic and purgative Moseley chiefly depended on. James's Powder he found well calculated to answer this intention, as in his estimation it possessed the great advantage of effectually cleansing the primæ viæ, and, when properly given, of exciting a plentiful sweat.

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, Moseley states that 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.

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 through the medium of an unirritating drink. Hoffmann strongly commends whey, and says, "Rustici lacte cum aqua fontanâ mixto, vel sero lactis per totum morbi decursum non sine singulari fructu utebantur."

LaMoniere, Fabricius Hildanus, Baglivi and others, bear witness to the good effects of sudorifics : 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, but shall content myself with the mention of a few remedies, found to be possessed of much efficacy in this disease, the great tendency of each of which must have been that of inducing considerable relaxation of the skin, and thereby disposing it to a free discharge. I shall begin with the

Warm Bath.—This is a means of relief that had been much overlooked in dysentery, and yet it is one that, from its general operation, I should consider as likely to rank among its most efficient remedies. 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 long since have led to its extensive employment: and the positive benefit derived from its use would have been the best argument in favour of its continuance. The analogy of other diseases should lead us to hope that the remedy would prove beneficial: experience strongly confirms these hopes. Baker, whose name I have mentioned more than once with the respect 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 universam hebdomadam malè habuerat dysenteria, in aquam demisso, dolor omnis ilico conquievit, *alvus largè resoluta est* : facilis inde ad bonam valetudinem recursus.” Besides the immediate ease from pain experienced in this case, another important advantage was gained, the “ *alvus largè resoluta* :” in this point of view the warm bath is an effectual and valuable remedy ; it acts immediately on the skin, and remotely on the intestines, relieves all their uneasy sensations, and removes that condition which counteracts the free expulsion of their contents. I have understood from undoubted authority that the warm bath was extensively employed by one of our surgeons in Holland, and that his success in the disease was very considerable from that circumstance alone : the effects derived from its use were immediate relief from the tormina and tenemus, and a great facility of cleansing

the bowels by the mildest laxatives, the difficulty and pain of accomplishing which first led him to the trial. It both curtailed the duration of the disease, and procured a great amelioration of all the symptoms(*a*).

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 states to be 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

(*a*) Vignes made extensive use of the warm bath among the French soldiers in all the varieties of the disease, but cautions us against any frequent repetition or long stay therein under the typhoid variety; he sanctions them *once* or *twice* for a *few* minutes, "avec la precaution d'en retirer le malade aussitot qu'il paraissait être menacé d'une defaillance. Les Bains ont plusieurs avantages: d'abord celui d'apaiser les symptomes de réaction de la phlegmasie intestinale, ensuite de nettoyer la peau et de faciliter la transpiration on une sueur, *ce qui peut arriver de mieux aux malades.*" *Magnos,*" says Frank, "certis hujus febris (i. e. dysenteriaë asthenicæ) in casibus tepido a balneo effectus obtinui-mus; in dysenteriâ verò gravi, sub tanta ægrorum debilitate, ac sub tam continuo ad alvum deponendum nisu, rara satis, rarior apud pauperes, illud administrandi concedetur occasio."

sudden an effect in allaying the agonies of tenesmus, that any patient, who has once experienced its good effects, will not fail to call for its repetition upon a recurrence of the same sufferings(*a*). "Tepid baths could not well be employed 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(*b*)."

Rollo observes, that the most important benefits have been derived from warm bathing and fomentations. Rush mentions that the warm bath was found in

(*a*) I have imparted considerable relief in tenesmus by applying to the anus lint dipped in equal parts of olive oil and laudanum, or by suppositories of solid opium: hot fomentations to the anus give greatest relief and benefit when applied immediately *before* and *after* a motion. A large sponge is best for such a purpose. "Des bains de vapeur d'eau chaude," says Vignes, "reçus a l'anus plusieurs fois par jour, soulagent considerablement les malades."

(*b*) Let the variety of the disease or its mode of treatment be what they may, there is no one matter to which greater attention should be paid than that of protecting the person, and more especially the feet, of the patient from cold under the very frequent calls to stool. Indeed, when the patient's system is under the influence of diaphoretic medicines, he should not be permitted to leave the bed, but should be encouraged as much as possible to resist the impulse excited by tenesmus; in which purpose he will be much aided by hot wet sponges, with or without laudanum, applied to the anus.

many cases to be an effectual remedy for an obstinate diarrhœa, consequent on dysentery. Rollo says the temperature should not be much *above*(*a*) blood heat.

With such testimonies in favour of this remedy, we cannot hesitate in giving it our approbation. After venesection or an emetic it is, perhaps, one of the earliest remedies to be adopted in dysentery, and also one upon which, under circumstances favourable to its use, much reliance may be placed during the course of the disease. It constitutes a powerful auxiliary to the sudorific plan, and renders essential assistance to the operation of purgatives ; for these, when exhibited alone, often create a temporary aggravation of the symptoms. It is a remedy of which Cheyne might have found the value in "those many instances he records wherein purgative medicines seemed greatly to increase all the sufferings of the patient ; and wherein castor oil, to which a preference is usually given, frequently roused the dormant griping pain."

I shall now speak of a diaphoretic practice, the great advantages of which are strongly insisted on by Mr. Dewar : I mean swathing the abdomen with flannel ; its beneficial effects would appear, from that gentleman's account, to be far greater than one would at first expect. He describes (p. 112) the particu-

(*a*) The degree of heat best adapted to dysentery is not expressly mentioned by any other author, who has spoken of the remedy. It is a point of some moment, and therefore I take notice of Rollo's injunction.

lar manner in which he employed it, and says, that "when begun early, and well attended to, not neglecting the usual collateral means, it seldom fails to effect a cure: it acts partly on the same principle as the tepid bath, one of the best remedies for inflammatory diseases attended with slight, but constant, internal pain, and proceeding from cold: the flannel bandage 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, invariable in its heat, one which will, on no occasion, weaken him by profuse perspiration, and which, being never laid aside, till it is no longer necessary, cannot expose him to the effects of cold by a change of temperature." The effects of this swathing he states to be, 1st., that it removes that local torpor of the abdomen, under which a dysenteric patient often labours. 2nd, it obviates rawness and griping; as the flannel bandage prevents the impressions of cold, which form the chief cause of these uneasy sensations. 3rd, 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 healing process in the intestines, proceeding from an improved state of sensation in those organs. He does not, however, attribute all the good effects of this practice merely to its uniform temperature, but says that,

when the bandage is first applied, much of its efficacy arises from its pressure : as pressure, even with the palm of the hand, often gives temporary relief to the bowels(*a*) ; the warmth, however, produced by the bandage is of more lasting benefit, and should be kept up without intermission till the dysentery is cured. Purgatives in the beginning of the disease are greatly aided in their operation when accompanied by this species of swathing ; the body being thus bound up, and the cold air entirely excluded, the bowels then are solely operated on by the force of the medicine: the excitement, which they consequently receive, being regular and uniform, evacuates them more completely, whilst at the same time it has the best tendency to restore their healthy functions. I consider this practice deserving of the recommendation it has received ; but previously to its application, I would, when possible, 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, being satisfied that its effects are very beneficial, especially when preceded or accompanied by the warm bath.

(*a*) We cannot be surprised, that in dysentery much benefit should be experienced from the bandage on the principle of its pressure : as we know, both from the sensations felt by the patient and from observations made on the disease, that irregularly spasmodic motions of the intestines give origin to the most distressing symptoms ; these are best obviated by such means as will restrain these motions, though other assistance be necessary entirely to remove them.

Besides the warm bath and swathing, I had formerly ranked amongst the best of our diaphoretic remedies in dysentery, an article which, whatever its *modus operandi*, has been highly and deservedly eulogised. I allude to Mercury, which by many is said to possess a specific power over dysentery, though its beneficial influence may fairly be attributed to its efficiency in removing obstructions, in equalizing the circulation, relieving inflammatory action, and modifying morbid secretions.

Clarke was among the first to propose mercury as a specific in dysentery, not only in its acute, but chronic state(*a*); he declares himself thoroughly

(*a*) Dr. Houlston, in his Observations on Poisons, and on the Use of Mercury, has clearly preceded Clarke in the practice of administering this remedy in obstinate dysentery and agues, as did also Riepenhausen in the cure of dropsies. The celebrated Boyle, however, took precedence of all in his commendations of calomel for the cure of obstinate dysenteries, though the remedy had been previously extolled as an anti-dysenteric on the supposed connexion of the disease with worms. Boyle states, that he was confirmed in his opinion of the virtues of the mercurius dulcis by the authority of an eminent army surgeon, who assured him that he had cured hundreds of soldiers by means of it, as a great secret, and that he gave from eight to fifteen grains, with a little rhubarb, for a dose, so that the patient might be moved thereby two or three times. Notwithstanding this statement, we may feel some surprise at finding the intelligent Hoffman thus expressing himself against the use of calomel in dysentery: "Unde non mirari satis possum quosdam, et præsertim Boyleum, mercurium dulcem in dysenteriâ commendare: quippe qui ejus est indolis, ut ab admixtione salium acriorum, quæ in nostro morbo

persuaded that it is possessed of powers to remove inflammation and ulceration of the intestines, 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 was doubtful whether to attribute its effects to its purgative or mercurial quality, but was soon convinced its virtues were owing to its mercurial powers, on finding in two patients, to whom he administered the 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 says, the disease will seldom abate much of its violence, till some degree of tenderness is perceived in the mouth: care, however, being 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

largâ præsto sunt quantitate, causticus reddatur." The remedy neither is nor was a favourite on the Continent, for we find that Vignes very seldom mentions the employment of mercury, and then not with much commendation; while Frank candidly states of himself, "*nec usum mercurii (ex dictis rationibus) in hoc morbo tentavimus.*"—How slow has been our progress in the exhibition of this valuable remedy since Boyle's day!

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.

I shall now, as concisely as possible, review the mercurial treatment of this disease, as adopted by gentlemen, who in our fleets and armies had the widest field for observation, and who were in every respect highly qualified to profit thereby. I shall commence with

Sir James Macgregor, who states that the form, in which he met dysentery most frequently, was in combination with other diseases, or when there was diseased action or disorganization of the abdominal viscera. The viscus most frequently affected was the spleen : next to that the liver, in nearly an equal proportion of cases. The mesenteric glands were not unfrequently affected, and sometimes the pancreas was one mass of disease. " It was when there was disease of the liver, or diseased action of the biliary system, that mercury was found highly useful." In such cases (marked by the usual symptoms) mercury never fails to cure, or at least relieve(*a*). " There was likewise a chronic state, where

(*a*) Profuse discharges of acrid bile are reckoned among the causes productive of dysentery : " Nous disons," says Vignes, l'acreté de la bile, quoique nous soyons persuadé que tous les medecins ne se rendront pas a cette verité, tres connue des anciens. Mais peut on la revoquer en doute, lorsqu'on voit des matieres

not even the most obscure hepatic symptoms could be detected; where, without fever, and with but few stools daily, diseased action was kept up, mercury introduced gradually, and gently affecting the system, effected a cure. But in the early stage of the acute and unmixed disease, and before repeated venesection has been performed, it will aggravate the symptoms." I have already mentioned that Sir James is an advocate for Dr. Somers' "saving lancet;" the omission of which this latter gentleman would seem to think the "fatal negative cause" of the great mortality of the disease.

Mr. Ferguson, in a valuable paper (vol. ii. of the *Medico-Chirurgical Transactions*), states that the "universal epidemic prevalent among the military in Spain and Portugal was dysentery: that when mild it admitted of easy cure by acting steadily on the bowels with mild purgatives, or that it was cured with nearly the same facility by acting on the skin without purgatives, through the influence of active diaphoretics." He states, however, that it sometimes began with such urgent symptoms as to

bilieuses rendues par le vomissement, irriter et inflammer meme la gorge. N'at on pas vu aussi cette meme liqueur occasioner des rougeurs erisipelateuses le long des cuisses, que des dejections de cette matiere avaient touchées. N'irrite-t elle pas tres souvent l'anus aussi dans les maladies dites bilieuses? Peut on douter d'apres ces faits de sa qualité tres-acre.?"

The dysenteric influence of mercurial action on the system, is it not chiefly owing to the discharge of acrid bile excited by the remedy?

require a power beyond either of these two, to save the patient. In the aggravated form, when the urine was high-coloured (even green), scanty, and pungent, without any other discoverable sign of hepatic affection, this was his signal for pushing the use of mercurial remedies," of which he had experienced the decided benefit in his own case. He gave *half-grain* doses of calomel with one of ipecacuanha every hour till the gums were affected (generally in forty-eight hours), when a solution of the disease (in curable cases) might be looked for with confidence, "the most certain precursor of which was the urine resuming its natural condition." He states elsewhere, that he considers the mercurial treatment "by *inunction* to be the best practice." He thought that relief from pain was much more effectually obtained by mild purgatives than by opium, which he considered hurtful, and even dangerous." "The *apparent* seat of the disease was in the intestines, but we are," he says, "to look to the liver principally for its source," and that, before a cure could be expected, "this grand organ of the bile must be specifically stimulated, and its ducts steadily emulged and duly opened, by the operation of mercury." Dr. Gray, however (referred to in the same paper in terms of praise), is of opinion that in European campaigns the liver is seldom to be looked on as the source of the disease.

Dr. J. Johnson, who, with great apparent truth, insists on the necessity of restoring "healthy per-

spiration and biliary secretion," as the *sine qua non* of curing this disease in hot climates, was in the habit of administering scruple doses of calomel two or three times in the twenty-four hours, combined with opium, Dover's Powders, antimony, &c., and, as he states, with great success in his own practice and that of others. In his own instance, it obviously saved his life in a formidable attack of the disease, for on pushing the remedy to salivation, he fell into a profound sleep, and awoke free from disease, with his skin "covered with a warm moisture." He also used venesection occasionally, and laxatives (castor oil); the scruple doses always relieved tormina and tenesmus. "The whole amount of my experience," says Mr. Cunningham (quoted by Dr. Johnson), "is this, that calomel administered in scruple doses twice or thrice a day, is an almost certain remedy for dysentery, in hot climates at least." "From much experience in this disease," continues Dr. Johnson, "I may with confidence assert, that I scarcely remember to have lost a patient in primary attacks, or where the constitution was not cut down by climate and repeated attacks, *when* calomel was given freely, so as to open the bowels and bring on a ptyalism."

Dr. Archibald Robertson, in his Medical Topography of New Orleans, states that, in the severer forms of the disease, after premising a brisk saline or lubricating cathartic and venesection (which moderated local pain, and diminished the severity of

the griping), he immediately commenced with the calomel, generally in scruple doses night and morning, seldom less than ten grains thrice a day. "It certainly seldom in any case increased tormina and tenesmus, but generally lessened both very materially, and produced five or six large motions, with less straining and blood. I have in this way," as he says, "given from sixteen to thirty-two scruples in the course of half as many days before the mouth was affected. When the gums were fairly sore, with some ptyalism, the calomel was omitted; the tormina, tenesmus, and general fever disappeared *as a matter of course*, and the bowels gradually returned to their natural state, the stools often changing in one night's time from a dark brown or spinage colour to a bright, healthy yellow, with the odour of natural fæces." He afterwards remarks that dysentery, in that aggravated form in which it has so often scourged our fleets and armies, is a very baffling, intractable disease; "and in those who had previously served long in warm climates, and whose livers were thereby affected, almost uniformly fatal. Dissection always brought to light extensive visceral obstructions, particularly chronic inflammation, or abscess of the liver, with or without enlargement of that viscus." "In whatever garb it made its appearance, disease of the liver, and consequently a vitiated state of its secretions, were undoubtedly the primary cause of the mischief. Dissection shewed structural derangement,—a soft, friable condition,

and generally suppuration of that gland." "How such extensive disorganization and formation of matter could take place without any preceding palpable indication of local mischief, is to me still a mystery." "But I by no means go the length of saying, that dysentery in our own climate always requires the excitement of ptyalism for its cure, being almost always a slight disease, and, compared with the fell and fatal form of tropical flux, might be termed the spurious dysentery, and differing widely both in cause and character from the *true* dysentery of warmer, but less salubrious, regions."

Milne, who explains all the phenomena of the disease upon the supposition of stricture and consequent over-determination of blood to the contracted and excited parts, thinks calomel and mercurial frictions well 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. He gave the calomel purgatives in quantities of from eight to twelve grains, to be sure of its effects. Not one dose, however, perhaps not three, will be found sufficient; so that it must be given without interruption, that is, night and morning, accompanied by mercurial friction of the abdomen until the mouth becomes sore, when the disease very often yields, so that, as he infers, mercury must be possessed of a powerfully antispasmodic or relaxing quality. Occasional purgatives should be employed till the bowels are per-

fectly 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; and 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 he adopted; but whether the remedy acted as a diaphoretic he does not state, neither does he furnish us with any detail of cases, as Clarke does, whence such an effect might be inferred; the following paragraph, however, would seem to shew that it must 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." May it not be fairly inferred from this passage that, with him, the *mercurial action* pretty uniformly operated in "*determining powerfully to the surface*."

But, besides the casual evidence furnished by Dr.

J. Johnson in the details of his own case, when he states that, on salivation setting in, he fell into a profound sleep, and awoke free from disease, "with his skin covered with a warm moisture," we have in Clarke's cases more unequivocal testimony to that effect. This intelligent writer has given us eleven cases of the acute disease, and one of the chronic, treated on his mercurial plan : in the first three there is no allusion to diaphoresis as consequent on the use of mercury, or as simultaneous with a favourable change in the disease ; but in the fourth case 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. Her disease then yielded; her stools became natural, and she was in *constant, uniform perspiration.*" In case sixth, 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 eighth was exceedingly severe, and somewhat advanced before the first visit : on giving the calomel and opium as usual, the patient is described, previously 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 to have been somewhat easier after having taken thirty-nine grains of calomel, the mouth being made a little tender. On the following day 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.*

That such diaphoretic influence has been or will be the general result on the occurrence of ptyalism in dysentery I cannot undertake to assert, but this I can say, that I have myself frequently witnessed its occurrence in those cases wherein I found myself forced (as I have often been) to resort to mercury for subduing severe or obstinate cases of the disease, such as I have occasionally encountered in and around the Irish metropolis. In general, I have been able to subdue the complaint without exciting mercurial influence, though calomel or blue pill were administered ; but whenever I found the disease at all obstinate, I at once had recourse to the active administration of mercurials until the gums were distinctly affected, when the complaint either

altogether disappeared, or greatly abated of its violence. This necessity for exciting the mercurial influence I observed specially in those cases wherein I had any reason to suspect hepatic derangement, and I have, upon such grounds, anticipated the probable necessity. Indeed, let the *modus operandi* of mercury in this disease be what it may, I most cheerfully subscribe to its efficacy in subduing it, and that, in doing so, it acts much more efficiently and certainly than in the special disease wherein a specific virtue is attributed to it. Neither is its efficacy confined to acute cases only, for it is clear, from numerous instances on record, that it has exercised a beneficial influence in many chronic cases also, though it be not very clear what, in those instances, may have been the actual condition of the intestinal canal, or whether its continued affection was not mainly dependent on diseased action in some one or more of the abdominal viscera. In acute cases of the intermittent and remittent forms, more especially when the liver has been primarily or secondarily engaged, either in producing or aggravating the dysenteric affection, the mercurial influence operates most beneficially, as also in chronic cases of the same character. Houlston's cases were all of a chronic type, and of this description.

Cheyne, however, when managing the dysentery, concurrently epidemic with the contagious fever of Ireland in 1817, states that the treatment by means of mercury did not succeed so generally as he expected;

and that administered in the form of calomel and blue pill, in large and small doses, and in combination with ipecacuanha, Dover's Powder, and opium. And yet he says: "I have no doubt that the mercurial treatment is entitled to confidence, although not, probably, to the same degree of confidence that it is in other climates." "It results from a consideration of my cases," he adds, "that venesection, calomel, and opium (in large doses), followed by the capivi mixture, with farinaceous diet, proved more successful than any other method in the severest cases; yet it often lamentably failed. It ought not to be forgotten, that when the skin was restored to a natural state by the mercury, the case generally proceeded favourably." In cases unattended with much fever or pain, and in the early stage of the disease, he states, that a purgative in the morning, with Dover's Powder in the afternoon, and repeated at night, restored many to health; "but it must be observed," he adds, "that in several of these cases, the disease spontaneously ended, *as the concurrent fever often did*, in free perspiration." Cream of tartar (a favourite remedy with Vignes), was tried in half ounce doses (very finely levigated), repeated every fourth or sixth hour. "We soon discovered, however," he says, "that this medicine failed in many instances; but it must be allowed that some patients who took it were restored to health, who, I think, would have sunk under any of the methods of treatment then in use. The first dose sometimes much aggravated the

patient's distress, but, when persevered in, it often brought down bile, and then, by giving it only once or twice in the day, and alternating it with Dover's Powder, the capivi mixture, and using baths, the cure was completed." Cheyne thought it excelled most purgative medicines in the property of bringing down bile, and that in this way its efficacy in dysentery may be explained, "for that the suspension of the biliary discharge is a very important part of this disease, few will deny who have seen the great relief which sometimes follows a discharge of bile(*a*)."

TONICS.—Having entered on the treatment of dysentery, principally with the view of establishing certain positions respecting the nature of that disease, and having already shewn how far the employment of evacuants warranted those positions, I shall now consider various testimonies in favour of remedies possessing a different character, all of which

(*a*) In a case, recently under my care, of simple dysentery, with some severity of symptoms, in a man of bilious temperament, the disease went on for some days with little mitigation, under the use of blue pill, with Dover's powder, and an occasional dose of castor oil with laudanum, when suddenly (and probably under the commencing influence of the mercury) a violent attack of cholera set in, with severe pain, and great prostration. Active doses of calomel and opium, with a large blister to the whole belly, speedily set all to rights; and the symptoms of cholera and dysentery gave way on a state of mild salivation supervening.

I shall rank together under the general title of tonics. I begin with bark and wine, as they constitute the principal articles of this class. And

1st, *Of the Bark.*—This remedy has been employed in general with one of two views : either to put an end to such diseases as are possessed of the periodic character, or to obviate debility. It has been exhibited in dysentery in its various forms and stages, with different views, and with variable success. On an examination of evidence, 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 employed to obviate the debility so constantly present, and that it has also been employed to remove that state of chronic weakness usually consequent on the disease in all its forms.

The person who seems first to have used the bark in dysentery was Morton. He employed it in conjunction with opium, not only in 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 *Συμμεμενων*, ex quantulacunque alternante symptomatum exacerbatione et remissione, et non *malignas* esse proderent, pharmaci hujus polychresti vires experirer, neque eventus, mehercule, spem meam unquam fefellit.” No language can be more precise respecting the circumstances under which he admi-

nistered the bark : the exception he makes may at first sight appear singular, as that would seem to be a case wherein the aid of bark might appear most requisite ; the exception is conveyed in the words “ et non *malignas* esse proderent :” in which case he would not try the powers of this valuable remedy. What could be the reason, or what his meaning in the passage ? I should suppose that the “ *malignas*” referred to the combination of dysentery with typhus, over which the bark possesses no such powers as over the intermittent and remittent forms : the exhibition of that article having reference to the fever and not to the dysentery, though, from the strong connexion between these diseases, by destroying one we often put an end to both(*a*).

Morton’s practice has been imitated by several, with a result agreeable to his promises. Dr. J. Hume observing, that towards morning the sick were always remarkably easy, and free from griping or

(*a*) “ Primum vires hujus Febrifugi Antidysentericas experiendo comprobavi in Ægro Dysenteriâ Tertianâ laborante: hujusmodi enim aliquæ mihi in praxi occurrebant, ubi tormina et fluxus alvi mucosus et sanguineus, tertio quoque die horâ statâ, non obstante diligentissimo opiatorum usu recrudescabant, cùm in intervallis truculenta hæc symptomata vel Landani dosi pusillâ vel sua sponte sæpius mitigarentur.” He accordingly gave ʒi. of bark with a grain of opium every fourth hour for six doses. “ Quid fit ?” he asks,—“ a quinto haustu exhibito fluxum ac tormina unâ cum febre reliquisque symptomatis comitantibus penitè exulasse, neque unquam in posterum periodis solitis recruduisse.”

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 I have already mentioned the use which Cleghorn made of the bark in the intermittent dysentery.

Reide, who thought dysentery so intimately connected with remittent fever that he considered it the same disease, and the one no more contagious than the other, 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 bowels : 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 three hundred 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."

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 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 irritable, that, notwithstanding the use of opiates, the medicine was speedily carried 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 Malacca, in China, and in England, without producing any good effects: it seemed, 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 wanting to complete the cure, except bracing the relaxed intestines." Now in the Bengal dysentery the disease partook of considerable remissions, and hence the good effects

of the bark ; but when he tried it where the disease was probably not attended by remissions, it was of no advantage whatever, 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 without reverting to Sydenham's observations respecting epidemic diseases, but in particular to his statement that "these self-same diseases (pleurisies, anginas, dysenteries, &c.), 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." I need scarcely remark how fully this observation is justified by every authority quoted in this section.

"This disease," says Tissot, "is sometimes combined with an intermitting fever, in which case the dysentery must be removed first, and the intermit-

tent afterwards. 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, "the tendency of this disease 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 useful in dysentery of long standing, and which has occasioned much debility, although it be not complicated either with typhus or intermitting fever, appears, he says, from many observations. In such cases Akenside gave it combined with a cathartic.

That bark is also beneficial in the combination with typhus, would appear from Zimmermann, who states (p. 250) : 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 say that it has been given with complete success, yet he states 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(*a*).

These statements respecting the use of bark will be found consistent with the observations made at the commencement of this section ; the result will be little less satisfactory, after we shall have considered the testimonies in favour of

Wine and other Cordials.—If our view of this disease be just, it must be obvious, that wine is inadmissible in the dysentery itself, and that some essential change must have taken place in the circumstances of this disease to warrant the use of that remedy. We have seen that venesection, though not absolutely necessary in very many cases, may be employed not only without injury, but frequently with decided benefit : we have besides seen that other evacuations were indispensable to its cure ; in such cases as these wine cannot fail of being detrimental when administered during the height of the disorder ; accordingly we find various authors, and Zimmermann in particular, exclaiming against its use, and detail-

(*a*) Grimm, in the management of his malignant dysentery, after the preliminary administration of hippo and rhubarb, says: “Tunc febris imprimis rationem habui et illa ulterius exhibui quæ avertere potuerant ab intestinis irruentium corruptorum humorum vim. Ut debilitato atque arroso intestinorum canali consulerem, ut putredini, gangrenæ matri, obessem, summo et incredibili cum fructu Corticis pulverem et decoctum copiosè usurpavi.” He regrets, however, as well he might, the difficulty of giving this medicine in sufficient quantity.

ing numberless instances of its mischievous effects : “Aromatics and wine in general,” says the latter (p. 112), “excite in dysenteric persons a dangerous irritation of 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 dangerous sensation of anxiety in the pit of the stomach, that very often accompanies 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 dysenteries. Brandy is here an absolute poison, and all these remedies occasion, even in convalescents, a most dangerous relapse.” The pernicious tendency of such practice demands little proof : let us endeavour, then, to ascertain the particular cases wherein it may be required, and the circumstances which render it necessary.

“In the advanced stages,” says Wilson, “when the debility is considerable, or earlier if typhus attends, wine has been found eminently useful. Dr. Brocklesby sometimes allowed his patients a pint and a half of port wine, or even more, every twenty-four hours. But some object to the early use of port wine, on account of its astringency ; and Zimmermann and others to the use of all kinds of wine and distilled spirits at *every* period of the disease. It is probable,” he adds, “upon comparing what

Zimmermann says of these remedies, with what is said of them by others, that he did not distinguish with sufficient care the cases in which they should be employed(*a*).” The latter part of this passage, reflecting thus severely on a practitioner eminent for his integrity and discernment, is not altogether correct. Wilson could not have read all Zimmermann has said on the treatment of *malignant* dysentery, as in page 245 he expressly states that

(*a*) Vignes, who had experience in every variety of the disease, concisely points out (p. 18) the circumstances in which wine was or was not admissible : “ Le vin pure,” he says, “ etait un des meilleurs moyens qu’on pouvait mettre en usage, non seulement dans les cas de grande faiblesse, mais encore dans toutes les circonstances ou les dysenteries epuisaient les malades : mais quand ces maladies reconnaissaient pour cause une phlogose benigne *encore* aiguë, ce moyen loin d’être utile etait tres nuisible.” “ Quelques cuillerées de vin pur de temps en temps convenaient également tres bien dans les cas de faiblesse atonique et *mieux encore*, si elle etait de mauvais caractère. In p. 311 Vignes mentions a curious case, taken from the *Dict. des Sciences Med.*, of a young man cured of a severe chronic dysentery (of four months’ standing), by “ une bouteille de fort bon vin de Bourgogne, dans lequel il mettait beaucoup de sucre, de canelle, de noix muscade et des biscuits : le toute chauffé.” The results from drinking the whole are said to be “ quil s’endormit profondement pendant seize heures. A son reveil, plus de coliques, plus d’envie d’aller a la selle ; des lors la dysenterie ne reparut plus.” I have myself, *after* a severe attack of dysentery, experienced benefit from the soporific and stimulating influence of a bottle of good Madeira; and Sims states that he has “ known a debauch with punch or wine, when followed by a sweat, of eminent service in this distemper.”

“ 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 like a malignant fever, in which nothing could exceed the effects of wine with such patients as were weak and deprived of all strength ; he therefore advises the use of that remedy in this dysentery when the patient’s strength is decreasing, and his voice low and weak ; but says at the same time that we can never be sure of the effects of wine till we have tried it. I have already (p. 83) indicated the species of treatment which Rogers found indispensable in the malignant dysentery of Cork, which, as he says, “ yielded to nothing excepting the highest alexipharmac course of medicine, and the most generous rule of diet(*a*).”

(*a*) Jones, already referred to as the author of the *Exercitatio de Morbis Hibernorum*, would appear, from his varied *modes* of treating dysentery, to have met the disease in its various forms : “ Phlebotomia et catharsis (he says) quibus *vulgaris* dysenteria medendæ rationem maximè pendere diximus, in epidemicâ curandâ usque adeo non prosunt, ut vel plurimum noceant certoque pene sint exitio. Quisquis epidemicæ mederi vult, necesse habet ut Alexipharmacis et diaphoreticis *omnino* incumbat, quibus solis tota ejus medela absolvitur ;” and of this he states he had ample experience in the fatal epidemic which raged during three years on the coasts of South Wales, and which proved so destructive, “ ut in ædibus non paucis vix unus aut alter superesset, qui sepulturæ traderet defunctos.” In this latter respect his words are almost identical with those employed by Morton when describing a most destructive epidemic in England ; and his mode

With respect to other tonics and stimulants, Zimmermann says, that camphor deserves a place next to bark for raising the patient's forces in the malignant dysentery : and that it may be conveniently joined with the extract of bark, and even with ipecacuanha, but must not be given in large doses. Dewar states, that camphor forms one of the most useful articles in the class of cordials. He observes, that "if repeated with proper frequency, it has a more lasting effect than wine, in relieving the patient from sinking of the heart and prostration of strength, and produces an agreeable sensation of warmth in the belly."

Where the bark and wine fail in supporting the patient, Zimmermann recommends musk on the authority of Monro, 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(*a*).

of treatment is likewise precisely similar in language and meaning to that of Rogers in his management of the typhoid dysentery of Cork. I am not aware that any other record exists than that of Jones respecting this remarkable Welch epidemic.

(*a*) Grimm, in treating the very malignant form of dysentery he had to encounter, adopted nearly the same remedies recommended by Zimmermann, viz., moderate venesection in some few with stout pulse and embarrassed respiration ; ipecacuanha ten grains every half hour, from the difficulty of exciting vomiting ; mild purgatives, acid drinks, light beer, "vini

“ Si vires,” says Frank, “ magis magisque fractæ id exigant, opium cum camphorâ vel cum moscho et gummi Arabico jungi, *ac demum* singula quæ in febre nervosâ graviore consulimus, imprimis vinum rubrum, morbo discrimine pleno opponi oportet.”

Under the head of SEDATIVES, I shall now speak of opium and tobacco, and shall conclude with a few observations on the beneficial influence of blisters in this disease.

Opium.—The obvious effects of this remedy, when employed to an adequate extent in dysentery, are the temporary alleviation or cessation of its most distressing symptoms, and the production of costiveness : could the former object be attained without the latter, no medicine could be more valuable ; but as that cannot be, few articles in the materia medica have done more mischief than the intemperate or untimely use of opium in dysentery. All authors now caution us against its indiscriminate use, and yet though they in general agree that it must be avoided in the beginning, or before evacuations have been employed, they are not equally unanimous as to the extent, or the object for which it should be

parum ;” camphor with nitre and sal ammoniac, which he denominates “nobilissimum remedium ;” also blisters, sinapisms, and extract of saffron, with bark after the fever had subsided. “ Robustissimos,” he says of the disease, “ secundum externam faciem invasit, et si qua forsan viscerum infimi ventris labes latuerit *olim familiaris*, illos oppressit et è medio sustulit.” In such cases it is that the mild action of mercury might have been invaluable.

administered. I need not, therefore, adduce any authorities against the free or exclusive use of opium in the early stage of dysentery, as it stands nearly on the same footing with astringents, which, when employed to stop the flux, cannot be too strongly condemned at that period. Zimmermann (p. 205) has, in proof of this point, brought together various testimonies, and appears to reflect on Sydenham for having sanctioned by his authority the improper use of opium, from which, he says, Sydenham does not seem to apprehend much danger. Zimmerman would not have passed this censure had he attended to the different occasions on which Sydenham administered that remedy, and to the different conditions of the epidemic he described. In the *fully-formed* dysentery he bled, and then purged repeatedly for several days, with an opiate after every purge ; but if the disease did not yield to this treatment, he gave the opiate every morning and evening, and sometimes in larger doses every eight hours, until it yielded : 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 from them, and 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, the former of which was of a more subtle

nature than in subsequent years, he found purgatives less effectual, but that the frequent repetition of *warm* clysters, and large dilution with *cold* whey, were attended with complete success in both diseases, for the patient, on being put to bed, fell into a spontaneous sweat, which was kept up for four-and-twenty hours, but not provoked by medicine. These two last observations tend to shew the very different states of the dysentery in this epidemic, or perhaps the too great latitude 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, evacuations were *absolutely* necessary before having recourse to laudanum; yet in any consituation, which has a *less tendency* to this disease, they may be safely omitted, and the cure completed by the shorter method, namely, by the exhibition of laudanum alone(*a*)."

(*a*) Van Swieten does ample justice to Sydenham, when commenting on his employment of opium, as being given "ut symptomatum ferociam debellaret atque inducias impetraret, dum cum humore peccante exterminando ipsi res esset." On the same occasion he alludes to Bontius' practice, in Indian dysentery, of administering the extract of saffron; which extract, he says, contains a large quantity of the best opium. "Horrendam dysenteriam" says Van Swieten, "quæ a levissimis laxativis irritabatur cum incredibili ægrorum cruciatu et certa sæpe pernicie, curare nunquam potuit tutius aut citius quam si extractum croci miseris ægris daret." Cheyne, whose sound judgment none that had the pleasure of knowing him will dispute, states, that when

Richter, next to Sydenham, appears to have made the freest use of opium. In his epidemic there was a great overflow of bile and considerable irritability of the stomach, so that other medicines could not perform their office ; emetics and purgatives, in particular, 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 symptoms 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 stercoraceous. I have cured many also, he says, by emetic and purgative medicines, *without*

dealing with the severer cases of the disease, "the patient has, in the course of a single hour, been largely blooded, has taken two grains of opium, and a scruple of calomel, has had the warm bath, and been swathed in flannel, by which means we have obtained breathing time to pursue our plans more deliberately. Were the same cases (he adds) again to be placed under my care, I would not hesitate to prescribe opium in doses of four or five grains, as it was the opium chiefly which seemed to me to arrest the progress of inflammation : and whatever in such a case procured rest for the patient from agony, sometimes proved of permanent benefit." Many similar testimonies might be adduced from various quarters, of the beneficial cooperation of opium in the cure of dysentery. And. Lowe, in his *History of the Epidemics of Hungary* (A. N. C., vol. i. p. 45), says that in a bad epidemic "Opiata fere omnem absolverunt paginam, imo fateri debeo me fere nullam dysenteriam sine frequentiori exhibitione laudani de tribus, commodè restituere potuisse."

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 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 their use. The former, he says, evacuate the bile, an accessory irritation, diminish the spasm in the intestines, and promote a gentle perspiration. He preferred ipecacuanha, which seemed 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 stimulants in the intestines: "I affirm," he says, "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. or the extract. opii cum ipecac. 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 practice would appear adverse to the

experience of other practitioners ; but they may easily be reconciled if we attend to the effects this remedy produced according to the mode of its administration. With practitioners who speak unfavourably of opium, it had been given early, before proper evacuations had been employed, and then, after a treacherous calm, it 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 amendment(*a*). " In thus defending Richter I do not desire to hold up his practice for imitation, but would adduce it as a

(*a*) That other practitioners have also experienced this same effect from opium, will appear by the following passage from an author who, though he condemns the premature use of astringents, opiates, and sudorifics, yet says (p. 362) : "Opiata, non tantum astringenti, et soporiferâ quâ pollent virtute, sed etiam plenior per cutis poros diaphoresin invitando, et humores ad corporis peripheriam ab intestinis invitando, ad diarrhœæ et dysentericæ curationem conferunt." *Vide* "O'Connell de Morb. Acut. et Chronic." Indeed abundant proofs are not wanting of the diaphoretic quality of opium : we know how powerfully sudorific it is in combination with ipecacuanha.

further proof of the efficacy of sudorifics in the disease ; and Moseley says that opium may be given in small doses with safety, and without its usual inconvenience, while the patient is sweating.

“ The real use of opium,” he adds, “ is to arrest the hurry of the disease ; to procure time to put some rational 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 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 harasses 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 themselves against disease(*a*).”

(*a*) Zimmermann, from witnessing the great abuse of this remedy, was prejudiced against its use : “ The pains,” he says, “ could not be more effectually relieved than by taking away the sharp corrosive matter, which sometimes was so abundant that, in spite of all evacuations, enough remained to keep up the disorder and excite the most intolerable pains. In *these* cases I very seldom made use of narcotic anodynes, and never without the greatest caution. But I was always, and in all circumstances, extremely attentive to assist the bowels with a proper quantity of soft and mucilaginous liquor.” Continental physicians are much more attentive to this latter important point of regimen than we are, and of its value Vignes furnishes some striking proofs. He would

Blane observes that opiates are least admissible in the beginning, but that as the disease advances they become more and more allowable and useful. The cautions necessary in their administration he states to be : 1st, to premise sufficient evacuation, blood-letting when required, but more especially purging ; 2nd, to obviate the effects which an anodyne has of causing a retention of the contents of the intestines ; and 3rd, to prevent feverish heat and delirium. This he 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 : an effect not so much required in the chronic as in the acute state. He says 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, according to Rollo, is only admissible in cases of extreme debility, and even in these opium

seem to coincide in some degree with Zimmermann as to the employment of opium. "L'usage de l'opium," he says, "devenait également salutaire, tant pour faire disparaître les coliques, que pour moderer les selles : mais il n'opérait un bien réel que quand la maladie était sans complication, surtout *bilieuse*."

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 expedients by which we procure for our patient a temporary suspension of his sufferings, and thus enable him to accumulate a little 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 thus see how cautious practitioners have been of administering this remedy in dysentery, and we may observe some analogy between this disease and gonorrhœa, not only in point of treatment, but also in the pernicious effects experienced in both from the premature use of opiates and astringents. In each disease there is an increased discharge, which a false indication would put a stop to, and accordingly the exhibition of opiates and astringents at an early period is often attended with consequences equally detrimental and irreparable, whereas the object in both cases 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, may be then suppressed with safety and advantage. That this is the case with respect to dysentery, the use which Sydenham made of opium after full evacuation has

sufficiently testified, and Sir George Baker's declaration is not less to the point : " Quamprimum, inquit, alvi excretiones naturali se more quodammodo haberent, jam tandem laudanum illud Sydenhami, non modò *tutum* fuisse confiteor, sed etiam aliquandò ad consummandam curationem magnopere *necessarium* contendo."

I cannot conclude the subject of opium without remarking that there may be no great occasion for this remedy in the acute state of dysentery, provided the warm bath, fomentations, and swathing be employed ; as these conjointly possess almost every advantage that can be derived from the use of opium, without its inconveniences ; if these means, however, cannot be commanded at this period, we need not hesitate in calling opium to our assistance even in the earliest stages, in which case we must be guided by the practical cautions already delivered (*a*).

Tobacco.—The use of this powerful medicament, in the form of fomentation (from two to three ounces on two quarts of boiling water), has been strongly recommended to our notice by Dr. O'Beirne

(*a*) From Grimm's statement it would appear that he found it necessary to be cautious in administering opium in his very malignant dysentery. His words are, " Perpetua fomenta abdomini adplicata et ano florum chamæmeli decocto imbutis, ac infusum solum sedulò haustum, immò opio efficaciora esse animadverti, et generatim opii usum in hoc morbo providum debere, probè perspexi."

of Dublin, in the fourth volume of the Transactions of its associated Physicians. He has given seven cases in detail, illustrative of the practice, and its beneficial influence in allaying (with the preliminary administration of a mild purgative) "the obstinate and distressing tormina and tenesmus; its rapid restoration of the skin, stomach, and kidneys to their healthy functions; its removal of the great *apparent* debility, which marks enteric inflammation, and its power of procuring sound sleep;" together with the facility it imparted to the purgative of "unlocking the bowels." Such advantageous results are, beyond doubt, of no trivial character, and if confirmed by the experience of others, are highly to be prized, more especially in our fleets and armies, in which latter Dr. O'Beirne (who served during the Peninsular war) had extensive opportunities for observing both the disease and the results of the ordinary practice. "I forcibly recall," he says, "the repeated detractions of blood *found necessary during weeks* before this disease could be subdued, the requisite debility obtained, or the bowels freed, the tormina and tenesmus in most cases resisting this as well as every other means. In strong subjects such were the inroads on the constitution from an actively depleting system, that convalescence was rarely attained for a month, and perfect health seldom in less than two or three; while the debilitated and harassed soldier too often sank under the *only* treatment considered likely to save him. Whether the

use of tobacco," he adds, " shall ever supersede that of the lancet in this disease, is a point which must remain for future and more extensive trial to decide: if it should not always prove a substitute for bleeding, it at least cannot be doubted but that it will ever be found a useful and powerful adjuvant."

Dr. O'Beirne states that in acute cases, more especially when affecting robust subjects, it will be necessary gradually to increase the strength of the tobacco infusion, until the required influence be obtained. He also states that after using the same fomentation for three or four hours, it loses very sensibly its powers, and that fresh fomentations should be employed, whenever we wish to increase or reproduce its effects. Dr. O'Beirne had previously tried the effects of tobacco enemata, but he found this mode of administering it injudicious, and then adopted the abdominal fomentations, which were generally continued till a sense of giddiness, nausea, and weakness came on(*a*). Though Dr. O'Beirne's

(*a*) It is not a little curious that Diemerbroeck has, in his *Observationes Medicæ*, left on record a case of dysentery, and has referred to two others (*Obs. xviii.*), in which the disease was rapidly but most dangerously cured by the internal use of tobacco, employed by a patient " qui medicorum consilia petere noluit, sed sibi ipsi medicus esse voluit. Viderat aliquoties me," says Diemerbroeck, " dysentericis in principio curationis medicamentum purgans exhibuisse ; quapropter hoc meum consilium in suo ipsius morbo sequi et ipsemet sibi tale purgans præparavit : sumpsit tabaci minutim incisi semiunciam idque per noctem maceravit in cerevisia tenui, manè parum coxit et colavit cum ex-

communication was published in 1824, I cannot find that the practice has been adopted by others, or, if adopted, with what result. It is unquestionably well deserving of trial, and I regret that I have myself omitted it in cases where I entertained doubts of the expediency of bleeding. In most of Doctor O'Beirne's cases much excrementitious matter in the form of scybala was discharged.—I shall now conclude the subject of general remedies in this disease by a few observations on blisters and other counter-irritants.

pressione, ac totam colaturam unico haustu ebibit. Paulo post summâ anxietate usque ad animi deliquium correptus fuit, ita ut ipsum jam moriturum crederent domestici. Eandem enormis vomitus et ingens per inferiora purgatio supervenit, magnaue variorum humorum, maximè bilis flavæ ac viridis, copia sursum deorsumque ejecta est, atque eo modo causâ morbi violenter et simul evacuatâ, hoc unico haustu à dysenteriâ in totum liberatus fuit.” Though Diemerbroeck vouches for the authenticity of this case, he yet prudently adds, “at proinde ego non omnibus auctor esse velim ut hujus experimenti periculum faciant.”

Having referred to Diemerbroeck for this singular case, I may as well make mention of another (Obs. xxix.) recorded by him at full length, as having been under his own care, and cured by the *intemperate* use of cold well water. The patient, a stout man about forty, “*prædictâ dysenteriâ (i. e. malignâ et contagiosâ), correptus fuit :*” all proper medicines were exhibited from time to time : “*verum hæc omnia nihil profuerunt, ita ut tandem, viribus ægri ad extremum dejectis, ipsum pro desperato haberem, neque per biduum in tali statu vivere posse crederem.*” The patient, however, being very thirsty, and desirous of cold water, “*jussi in desperato casu illam in magna quantitate et frigidam ægro offerri, ut hoc copiosiore aquæ potu causa morbifica (si forte*

Blisters.—These would seem to have been usually employed in dysentery for a symptom not necessarily belonging to the disease, though by mismanagement or neglect 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 internal remedies. This indicates a tendency to a more active inflammatory action in a particular part of the intestinal canal. It

fieri posset), ab intestinis ablueretur ejusque acrimonia vi frigoris retunderetur.” The patient drank the cold well water in great quantities throughout the whole night, which at first rapidly passed through the bowels, “et magna tormina inducens per inferiora defluebat : postea minora tormina faciebat,” and toward morning, “dolores fere nulli erant et dejectiones multò rariores : circa meridiem per aliquot horas quietè dormivit : ante vesperam fluxus omnino substitit, atque ita hic ægor, convenienti postmodum diæta reffectus, convaluit, insperato hoc remedio a morbo desperatissimo liberatus.” That Sydenham was not disinclined to this method of cure will appear from his praises of whey in the cure of dysentery : “ingesto *affatim* liquore, quem *frigide* propinandum curavit.” His feeling on the subject is still more strongly marked by the language he employs in speaking of the successful adoption of this his practice by his friend Dr. Butler at Tangiers, “qui profitebatur optimè istic sibi cecisisse methodum istam, quâ *liquoris diluvio submerguntur* dysenteriaë.” “L’eau pure et tres froide,” says Vignes, “donnée en abondance dans la dysenterie intense tres bilieuse est quelquefois un remede heroique, de meme que pour la fièvre bilieuse.”

is to be obviated by frequent fomentations, and to these, if taken in time, it generally yields. When it does not, it will be often relieved by the application of leeches, and still *more frequently* by a large blister. If it resists these remedies, we must determine on a general blood-letting, provided the patient's strength at all admit of it." When the fixed pains of the abdomen did not yield to fomentations, Pringle applied blisters with the greatest success ; 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. I have found leeches and blisters very effectual under circumstances which would lead Bamfield to employ active venesection. Our field of practice, however, was very different.

"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 the disorder, as well as in the immoderate diarrhœa attendant on putrid fevers, and indeed in general in all obstinate alvine fluxes." He mentions that in his epidemics there were some slight dysenteries among children, very obstinate and tedious, and 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, but transitory success ; with

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 Zimmerman, " in this case, laid them upon all three places at once(*a*)."

(*a*) Dewar relates (p. 149), on the authority of a French physician in Egypt, that " 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." Vignes, too, is equally favourable to the use of this remedy. " Quand enfin, la nature offrait encore des ressources à l'art, des sinapismes sur l'abdomen étaient appliqués avec avantage quelquefois et dans toutes les variétés de la dysenterie, excepté quand elle était inflammatoire, ce qui ne se voyait guère chez des hommes, qui avaient souffert beaucoup des fatigues: ces excitans momentanés ou les vésicatoires eux-mêmes opéraient souvent une heureuse revulsion du siège de la maladie vers la peau, surtout lorsqu'ils étaient secondés par la chaleur douce du lit, laquelle était nécessaire aussi dans toutes les autres circonstances de la maladie pour favoriser la transpiration, qui était *le meilleur* des remèdes, quand elle s'établissait bien." And again he says (p. 312): " Parmi les moyens externes, on doit surtout attendre les plus heureux résultats de l'emploi des vésicatoires sur l'abdomen, comme rubéfians: et s'il s'établissait à la suite de leur application une suppuration abondante, il ne faudrait pas toujours s'en alarmer. La plupart des médecins de nos armées ont, comme moi, observé les plus heureux effets de cet excellent remède: mais c'est seulement lorsqu'il y a de débilité et flaxidité de la fibre, et beaucoup d'humidité dans le ventre, que les vésicatoires et les sinapismes sont bien efficaces. Ces deux moyens ont l'avantage inappréciable de rétablir la transpiration et de faire cesser ainsi l'irritation et par conséquent les autres symptômes."

These observations apply to the use of blisters as a topical remedy in dysentery, whenever symptoms of fixed pain or local inflammation arise ; in such cases I should think their beneficial effects must be proportioned to the size of the blister and its proximity to the parts affected ; indeed, vesicatories to the abdomen seem well adapted to relieve or finally reduce that species of inflammation which belongs to dysentery : and the analogy with other diseases would countenance their use, for we know with what difficulty stools are often procured in some species of cholic, and with what facility they follow the moment a blister applied over the abdomen begins to operate.

But, besides their immediate topical effects, blisters seem to exert a very beneficial influence in dysentery, when in combination with typhus ; and here perhaps they operate as much on the system at large as on the local disease ; being in this respect as much, if not more, indicated in malignant dysentery than in contagious fever, both as a topical and general remedy. "The honour of having first revived the use of vesicatories in malignant dysenteries, belongs," says Zimmermann, "to two physicians, 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 convulsions 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 patient's stools and anxiety diminish, and their strength increase, as fast as these plasters operated; he therefore never neglects this auxiliary in malignant dysenteries, except a great deal of pure and dissolved blood comes away with the stools." "Ces rubefians," says Vignes, "sont dans quelques cas (de mauvais caractere) les seuls moyens sur lesquels on doive fonder des esperances(a)."

But besides blisters, we are furnished with an excellent and active rubefacient in turpentine, more especially when applied after the manner indicated by Dr. Copland. He directs flannel wrung out of hot water, and moistened with spirits of turpentine, to be applied to the whole abdomen, and allowed to remain as long as the patient will endure it. The usual effects of this epithem are a most copious per-

(a) Dr. Wilson, in his Medical Notes on China, lately published, states that the following rubefacient has been employed with good effect in dysentery in place of blisters, whereby the irritation of a large blistered surface was avoided, and the action established was, he says, "not only more steady and constant, but also more effectual on the whole." The rubefacient consisted of an ointment of equal parts of mercurial and iodine ointment, with a small portion of cantharides plaster: to be rubbed carefully over the belly to the extent of a drachm daily. Wilson's general treatment of the disease consisted in a large bleeding (from twenty-five to thirty ounces), followed by hot bath, leeches, large poultice to the belly, and, if necessary, a large blister to the whole abdominal surface, calomel, opium, and hippo (three grains of the first with one and a half of the other two), every third hour, till the gums were touched.

spiration, with burning heat of the skin where it is applied, and, consequently on these, a total remission of the tormina and tenesmus. In advanced stages of the complaint, when the internal congestion is very great, and the skin harsh, dry, and livid, repeated applications of this epithem, as warm as the patient can endure it, are sometimes requisite to its full effect.

Such are the effects attributed to this active rube-facient by Dr. Copland: such too are the analogous effects attributed by Dr. O'Beirne to the tobacco fomentation. Are both remedies equally applicable in the same state and condition of the disease? The tobacco, it is true, may be more applicable in the early stage, and the turpentine better suited to the more advanced periods: yet I can see no reason why either, however opposed they may be in properties, might not be employed with equal benefit at some given periods of irritation approaching to the inflammatory state. There are, we know, cases of disease in which hot or cold applications may be used indifferently and with equal advantage.

I shall now bring my observations on particular remedies to a close. The subject has carried me farther than I had expected, but I trust to be excused for having thus presented to the reader a valuable summary of the practice adopted in the disease by the most respectable physicians; if it has conduced to give stability to the propositions which I have advanced, one of my purposes is answered;

but a more useful end may be obtained for the reader, by its enabling him to judge of the proper application of each remedy to the different forms and stages of the disease. To assist him in that important object, I shall conclude the subject by a concise reference to that plan of treatment which, from the foregoing details, would appear best suited to the disease in its various combinations; and in doing so, I cannot refrain from expressing my *general* concurrence in the bold and decided views inculcated by Dr. J. Armstrong respecting the treatment of dysentery, though he fails to make adequate allowance for the differences such treatment must undergo according to the character of the combination; and as he makes no mention of contagion, he would not seem aware of that form of the disease which such agency produces. Dr. Copland, however, is minutely particular on all the important points of treatment (perhaps too much so for the young practitioner), and amply supplies the deficiency referred to in Dr. Armstrong's instructions.

CHAPTER VII.

TREATMENT OF DYSENTERY IN ITS SIMPLE AND COMBINED STATE.

AND *first, of the simple dysentery.* The treatment of this form will not present much difficulty if the disease be recent, and the remedies well-chosen, and seasonably applied. Of itself in these climates it seldom requires venesection, though in subjects of strong inflammatory diathesis, that remedy may be employed with safety and advantage, and repeated, if called for by circumstances already detailed.

Emetics are not often demanded on account of any great sickness at stomach, or for the purpose of unloading that viscus. They may, however, be administered in conjunction with other remedies, either with a view of clearing the intestines, or of promoting a diaphoresis(*a*).

Purgatives are more or less essential to a cure in every form of the disease, though in some more

(*a*) The strongest testimonies are 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 combined with advantage.

than in others, as fæcal accumulation is apt to occur repeatedly in particular forms. The object in employing or selecting them should be, to clear the intestines of their contents, and to do so with as little irritation as possible, by the previous or simultaneous use of the warm bath, fomentations, swathing, or opium. After a first liberal evacuation, purgatives are still occasionally necessary, and should be repeated according to the indications for their use(*a*).

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

(*a*) The purgatives, to which experience has given a preference, appear to be the sulphate of magnesia, oleum ricini, and calomel. Of the latter, ten, twelve, or twenty grains may be administered for a first dose, with or without opium, and may be repeated according to the emergency of the case. While small doses irritate, I have in this, as well as in other diseases, seen a large dose exercise an anodyne influence, and compose to sleep. Rhubarb, though inadvisable at the commencement, yet seems to be of service towards the end of the disease.

(*b*) The sudorifics which have been most advantageously employed in dysentery, are the ipecacuanha, pulv. ipecac. comp. tartar emetic, pulv. antimon. opium, &c.; 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

In cases of severe tenesmus, leeches as well as fomentations to the anus have been found highly beneficial, as well as opiate suppositories. I have found equal parts of olive oil and laudanum applied by means of lint to the anus afford great relief. To relieve the tormina and tenesmus, clysters are strongly recommended, as possessed of considerable influence in allaying them; they, no doubt, are useful when composed of proper ingredients, and well administered. This remedy, however, is far from being unobjectionable: it appears at least that the benefits are often more than counterbalanced by the disadvantages attending its 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 attributes in part to the clumsy manner in which they were administered. Blane says, that he was "at first tempted to think that a *very frequent* injection of such clysters would be very useful, by cleansing and soothing the

keeping up that state of the skin which is so requisite to prevent a relapse. There is also another advantage attending its use, that patients are more careful in guarding against cold, whilst under its influence, than they would be under the operation of other medicines. The mercurial action, though seldom required in the simple dysentery, yet in obstinate cases should be excited in the manner already detailed, and in subjects under any suspicion of hepatic congestion it is more or less indispensable.

colon and rectum, so as to prevent further *exulceration*, and dispose the parts to *heal*. But, besides the objection arising from the tenderness 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 exhausted." He adds, however, that, when not abused, they are of the most eminent service in this and other complaints. The testimonies of other authors are not less decisive: "Clysteres," says Baglivi, "*copiosè* præscripti, quandoque exasperant morbum et exulceratis intestinorum fibris majorem orgasmum excitant. Dentur igitur rarius et in parvâ copiâ." And O'Connell declares: "Enematum quorumcunque repetitionem in hoc morbo *frequentiorem*, noxiam semper et infaustam observavi." This author makes a distinction in the mode of administering an emollient and restringent injection, which should be attended to: "Enemata restringentia," he says, "parvâ tantùm singulis vicibus quantitate, injicenda 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." "Enemata autem emollientia, et humorum acrimoniam obtudentia, largiori paulò quantitate injici possint et debeant; quia ob diversas prorsus intentiones, hæc et ista subministrantur," &c(*a*).

(*a*) Vignes, however, dwells strongly and repeatedly on the value of "les demi-lavemens emolliens, dont on donnera plusieurs par jour, des bains de vapeur d'eau chaude, reçus à l'anus plu-

These observations relative to clysters are applicable to all the different forms of dysentery; and 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(*a*). Towards the decline of the disease, however, when the intestines 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

sieurs fois par jour, qui sont en general de la plus grande utilité pendant la force de la phlogose et des tranchées." He also insists on the vast importance of a strict ("severe") regimen, and on the frequent use of emollient and gummy drinks (his "boissons adoucissantes et gommeuses"). This very strict regimen was indispensably necessary to give efficacy to the very mild means employed in French practice, and he cites several interesting cases to prove its value.

(*a*) Whey, more especially what we in Ireland call "two milk whey," has by many been highly eulogised; amongst others by Sydenham, who, after venesection, gave it as freely in dysentery as he did the light chicken broth in cholera: "non verò," he says, "ut illic jusculo e carnibus pullorum, at lactis ipsius sero (ingesto *affatim* liquore) quod *frigide* propinandum, tepidè vero inferius injiciendum curavi. Tormina et dejectiones sanguine permistas, rejecto jam quarto enemate, semper evanescere animadvertēbam." "Lectulo protinus ægrum commisi, ubi brevi sponte sua in madorem solvebatur (a sero lactis sanguini immixto) quem per horas viginti quatuor continuari, *minimè* provocari jubebam: nihil interim concedens præter lac crudum paulo tepefactum."

added ; he observes also, that melted butter is a remedy long in use among the Irish against dysentery, and, therefore, probably not without its advantages.

Evacuants, it is obvious, are more required in this form of the disease, than tonics of any kind ; indeed any of the latter are only admissible during the state of convalescence. With respect to articles of diet, regimen, &c., these points are so fully considered by most authors, that it would be needless to detail them here. Attention to the minutiae of regimen is of great consequence to the recovery of the patient, nothing being so easily produced as a relapse, often less curable than the original disease.

Treatment of Intermittent and Remittent Dysentery.

In the management of these forms we should hold in remembrance the observations of Sydenham relative to certain diseases, occurring sometimes as symptomatic, sometimes as essential diseases of the season. When they appear in the former character they can only be treated with success by that method which the fever they are combined with requires, adapted to each particular case. Sydenham, Morton, Pringle, Cleghorn, Clarke, Zimmermann, and others, bear abundant testimony in support of this proposition. Accordingly the practitioner must carefully study the nature and treatment of the reigning fever of the season, and when he is in possession of this knowledge he must apply it to the particular disease, under such restrictions as its peculiar character may require. That dysentery is powerfully influ-

enced by the fevers of the season has been abundantly demonstrated when describing its intermittent and remittent forms, and I have already anticipated much of the peculiar treatment suited to these forms.

I have detailed the steps adopted by Cleghorn and Rollo in the treatment of intermittent dysentery. Tissot gives nearly the same advice, only in more general terms : "this disease," he says, "is sometimes combined with an intermitting fever ; in which case the dysentery must be removed first, and the intermittent afterwards. Nevertheless, if at each access the fits of the fever have been very strong, bark must be given as directed for that fever." Emetics and purgatives are the more indicated from the presence of fever, and the propriety of venesection must depend mainly on the character of that 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. "*Asthenicæ cura dysentericæ,*" says Frank, "*pro gradu et indole febris, cujus hæc symptoma constituit, diversis, mitiora nunc remediâ, nunc vero efficaciorum sibi methodum requirit.*" "*Dysenteria periodica, intermittens febris soboles, istius in modum cum opio mucilaginis gummi arabici ope aquæ aromaticæ juncto, ac demum Peruviano cum cortice fuganda est.*" A good dose or two of calomel is also in general an indispensable preliminary to the beneficial action of the bark, though mercury, as we have seen, was no favourite with Frank.

In the remittent as well as the intermittent dysentery, the propriety of venesection must depend much on the same general circumstances; though from the greater tendency of the remittent to assume a typhoid form, and to acquire, under favourable circumstances, a contagious character, it is less admissible, except at an early stage, unless the signs (already indicated) of local inflammation in the intestinal canal demand the free use of the lancet(*a*).

Emetics are highly necessary 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 will be attended with beneficial effects in allaying

(*a*) Hoffmann gives the following criterion for judging of the immediate seat of intestinal irritation: "Ipsa quod attinet intestina," he says, "determinari facile potest, quænam eorum potissimum discrucientur. Quod si enim proximè circa umbilicum atrox sentitur dolor, sique hunc tardior sequitur dejectio, certum est tenuia laborare. Quando autem ad epigastricam regionem, ubi colon situm est, aut hypogastrium atque hypochondria torminum vis incumbit, ipsæque feces brevi post rejiciuntur, crassa laborare in aprico est." "Neque enim negaverim quin vicinæ partes maximeque hepar ac ductus biliferi per consensum etiam experiantur mali tyrannidem." Frank, too, very pointedly remarks respecting the *time* and *causes* of supervening inflammation, "Etsi vero dysentericorum cadavera, intestina hinc inde non parum inflammata ostendant plerumque; vix tamen primis morbi, non summi, diebus hæc ita se habebunt; sed istis aut sine curâ debitâ aut perversâ sub methodo transactis, oportet sane ut canales illi membranacei, tot vassi sanguineis instructi, inflammationem tandem localem sæpissime concipiant ac diræ periculo gangrenæ submittantur."

the great irritability of the biliary system, and the irritation caused by its disordered secretions, which the exhibition of an emetic sometimes tends to increase.

Purgatives are indispensably requisite to evacuate the bowels of an inconceivable load of bilious 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 under such circumstances, the patient, instead of being weakened, will daily gain strength.

The employment of the sudorific plan in remittent dysentery, appears to me, if not objectionable, at least not as well warranted as in the simple disease. How far the warm bath may be admissible in this form I cannot positively pronounce; swathing I should deem innocent as regards the fever, and beneficial as regards the dysentery. Indeed every means, consistent with the character of the fever, of keeping the skin moist and perspirable, may be employed with safety and advantage. There is one remedy, however, which possesses powers peculiarly adapted to the circumstances of this combination, whether it be viewed in the light of a purgative, sudorific, or deobstruent; I allude to mercury, which acts against this form 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 seldom fail of exhibiting for some of these purposes, as well in the intermittent as in the remittent form, until by a timely use of the bark, I was enabled 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 others, to convince us: to his former statement on this subject I shall here subjoin another: “Et nullus dubito,” he says, “quin diarrhœa quævis epidemica et quæcumque alia cum febre juncta, imo febres castrenses cum hujusmodi symptomatibus ut plurimum sociatæ, hâc methodo compendiariâ multo certius sanari possint, quam magno illo apparatu rhabarbari, &c.; atque equidem ut a verâ et genuinâ ideâ hujus morbi formatâ, curativæ indicationes in posterum certiores desumantur, mihi in voto est, ut communi medicorum consensu, diarrhœa et dysenteria castrensis, seu quæcumque alia epidemia, nomine proprio *Συνοχῆος* spuria et colliquativa, posthâc designentur.” It is indeed a 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 the bark produces no one salutary effect on dysentery

when alone, but when combined with 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 against the dysentery, we may be successful in removing the fever. Thus Zimmermann, who followed that plan says: "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 then attack the dysentery by the means already directed, and with better prospect of success.

In these forms of dysentery wine and cordials should only be allowed in moderate quantities during the state of convalescence; if the disease have been properly treated, there will in general be little occasion for tonic medicines, unless in cases such as those described by Wilson in his Medical Notes on China, where he states that "the dysentery prevailing on its coasts was unusually intractable, as it was seldom an idiopathic uncomplicated affection, being most frequently associated with intermittent fever, secondary to, and alternating or co-existing with it." There was also an unusual tendency to the generation of intestinal worms, "owing to the enfeebled and unhealthy condition of the alimentary canal," a cause that will be found to produce similar results in all similar cases, as in

that recorded by Roederer, when the inhabitants of the besieged city were reduced to great straits. Wilson found the infusion of buchu, with the tinct. muriat. ferri (as recommended by Roederer), useful in convalescence; and he gave the former, in combination with arsenic, in the sad chronic consequences of the disease.

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 *ripe* fruits in 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." Zimmermann 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 either productive of or noxious in 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, at times, it appears to pass into the contagious variety (*a*), for it may be remembered that a great majority of those authors, who describe the remittent dysentery, make mention of its having become contagious from inattention to these two particulars; but yet we must cautiously guard against too free an exposure of the patient to cold air, which in this complaint may do more harm than fresh air can do good.

Treatment of Dysentery in combination with Typhus or Contagious Fever.—In the management of the intermittent and remittent dysentery, we have seen the necessity of directing a large share of our attention to the fever which attends; such necessity exists in a still greater degree in this form of the disease. I may here introduce an appropriate observation of Tissot's: "the cause of malignant fever is," he says, "not unfrequently combined with other diseases, whose danger it extremely increases. This may be known by the union of those symptoms which carry the marks of malignity, with the symptoms of the other diseases. Such combined cases are extremely dangerous; they demand the utmost attention of the physician; nor is it possible to prescribe

(*a*) "Dysenteriæ obnoxii," says Strack (in his *Præcepta Med. Pract.*) "statim a consortio aliorum sanorum et ægrorum dimovendi sunt: hi enim ut plurimum ærem in cubilibus et nosocomiis ita inficiunt et impurum reddunt, ut cæteri etiam ibidem degentès aut morbo eodem aut febre malignâ putridâ invadantur."

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." The justice of these observations will be the more manifest, when we remember that Pringle almost identifies the treatment of this combination with that of malignant fever, as does Hoffman, when he says: "Curationem quod attinet, ea prout vel benignum vel magis malignum fuit malum, duplici ratione instituta. In malignâ dysenteriâ cum vehementemente febre conjuncta principalissima cura *febri* tributa fuit." And in another place (sec. 34) he observes: "Quum verò illa plerumque sociam habeat febrem acutam malignam ipsâ dysenteriâ periculosiorem, hæc quidem tractationem sollicitam ex indole suâ postulat ne ægrotum jugulet: indoles autem frequentissimè est maligna petechzians," &c.

Zimmermann has given at some length the method of cure appropriate to this combination, and as he has treated it with much discrimination, I shall not hesitate to repeat his observations. In the first place *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 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. Vignes repeatedly and strongly dwells on the vast importance of atten-

tion to all these cautions of Zimmermann, more especially in military hospitals and encampments.

Respecting the employment of evacuants, I have already explained how far each article of that class was admissible in dysentery: though great judgment and skill be requisite in balancing any contraindications, not unfrequently arising in so dangerous a combination, we should always 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 claims of the dysenteric affection. In confirmation of the propriety of this advice, we find venesection almost unanimously rejected, too often from the sad experience of its pernicious effects(*a*). We find too that emetics and purgatives, though they may be used at the commencement with considerable advantage, yet demand more caution in the mode and extent of their administration in this, than any other form of the disease(*b*). As to sudorifics they would appear

(*a*) "Difficillimæ curationis erant hi fluxus," says Diemerbroeck, "in quorum curatione venæsectio parum aut nihil proderat: sæpe quoque nec purgantibus nec astringentibus, nec sudoriferis aliisque remediis, in hujusmodi morbis adhiberi solitis, quicquam proficiebatur." "In brachio instituta venesection," says Grimm, "sub initio utilis fuit illis [very few in number] quibus, post vehementem horrorem, plenus, fortis et undosus restitit pulsus: noxia verò contrarias proprietates gerente pulsu." Roederer (as might be expected) says of his "morbis mucosus," "pessime consulitur huic morbo methodo antiphlogisticâ."

(*b*) "Les purgatifs," says Vignes (p. 292), doivent etre bannis,

not very admissible ; and as it is the fever which constitutes the great source of danger, so the sudorific plan, however advisable against dysentery, must be given up, when contra-indicated by the fever. Should the disease appear to incline to this evacuation we need not hesitate in promoting it moderately. 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 advantage. "It is, without doubt," says Zimmermann, "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(a).

How far the warm bath, swathing, and the *mercurial action*, may be admissible in the malignant dys-

en general du traitement de la dysenterie typhoide comme de toutes les autres varietés de mauvais caractere. On peut se permettre tout au plus une decoction de tamerins *dans quelques cas !*"—When the strength appears to give way, "il faut mettre en usage sans delai les moyens indiqués pour le typhus lui-meme, parceque les symptomes de cette maladie *principale* present plus que ceux de la dysenterie, puisque celle-ci n'en est qu'un epiphenomene."

(a) "Ut in dysenteriâ ita et in hoc malo analogo," says Roederer, "dici vix potest, quantum emolumenti a remediis nauseosis possit expectari. Multum ad bonum morbi eventum confert, si dierum criticorum habitâ ratione molimina naturæ modestè juvamus (pro re natâ et indicatione) vomitoriis, alvum ducentibus, aut diaphoreticis."

entery, I will not undertake to say, nor do I know of any facts which would authorize a positive opinion on the subject; according to Cheyne, the last remedy is, perhaps, less admissible than the two former, though even in the use of the warm bath some caution is necessary, for Vignes says (p. 300) that in the malignant dysentery, "on doit employer quelques bains-tiedes: mais ici, on a á craindre les defaillances: on devra donc les employer sagement." "Les trois varietés de la dysenterie maligne," he adds, "sont en general si graves, qu'il y a tres peu d'espoir de sauver les malades." Blisters he states elsewhere to have constituted his chief resource in desperate cases; they are deserving of every commendation in this as well as in the other forms of the disease. It would appear, from the trials which Currie made of cold and warm affusion in an epidemic dysentery at Liverpool in 1801, that much is not to be expected from either. 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 his description, often to have partaken of the symptoms of the latter disease, being unusually fatal, and its treatment very difficult. The heat, after the appearance of the dysenteric symptoms, rose to 102° and 103°, the tongue became furred, the skin dry, and the pulse rose from 100 to 120 in the minute. "I did not try," says Currie, "the cold affusion, 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 affusion 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."

But the influence which the presence of typhus exerts over dysentery, is in no respect so strongly manifested as in the necessity it produces for the exhibition of cordials and tonics; and in this necessity it differs from all other forms of the disease. Under the section of tonics I have brought together the evidence which testifies this necessity, and the advantages derived from complying with it: to that head, therefore, I shall refer for the mode of administering them, and for the occasions which demand their use: indeed they are the same as may occur in the malignant contagious fever; whatever indicates their use in the latter calling still more loudly for their exhibition in malignant dysentery, in which, after we have obviated the most urgent symptoms of the flux, the debility consequent on such evacuations must be proportionably greater.

More might be added on this head, but, lest I be too tedious, I shall here conclude my observations relative to dysentery, by recurring to a pro-

position which, *in my mind*, has been supported by proof approaching to demonstration. I have maintained 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 been established by reasoning *a posteriori*; the first part of the proposition might, I also think, be inferred *a priori*. It may be argued thus: if dysentery be *in itself* contagious, it must range under some one species of contagion, which we know to be communicable in three ways only, viz., 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 small-pox. The diseases which range under these different species of contagion are possessed of strongly distinctive 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 it be entitled to a place among any of them; and first, of its title to the third species. Under this head few will think of placing it, as the diseases of this 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 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 possessed of contagion, is capable of acting without the necessity of direct contact(*a*), neither can its infection be

(*a*) Whether dysentery in any or what form be communicable by direct contact of intestinal excretions to the anus, I do not take upon me to decide: we know that in general its contagion is propagated and received in all respects like that of fever, as when Van Swieten says he knew an eminent physician “*qui dum dysenterici moribundi feces olidissimas lustrat, tetro halitu afflatus, ilico in eundem morbum delabitur;*” but whether we can rely on the following observation of Hildæus Padoanus, I will leave to the judgment of others: “*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 aliquando remanent.*” This observation is quoted by Moseley, and though he makes no remark thereon, we may easily conjecture his sentiments. Cheyne, p. 10 of his *Observations on Dysentery*, states, on the authority of Dr. Barry of Cork, a fact worth mentioning in connexion with this assertion of Hildeus. He says that in the years 1797, 1798, and 1799, dysentery prevailed in the Caithness Legion to some extent: “The surgeon, anxious to determine the question as to its infectious nature, caused the same clyster-pipe to be used, without cleansing, for those labouring under dysentery and those who were free from that disease: the latter, notwithstanding, were not infected: from whence he concluded that the dysentery of Cork is not infectious.” It does not distinctly appear, however, under what form of dysentery either these or Hildeus’ patients laboured. The analogy of hospital gangrene, in which both a general and local infectious power exists, would make me hesitate in refusing to admit a similar power in dysentery.

exhibited 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 ; but dysentery is at times unaccompanied by any fever, and is a disease of no definite duration, continuing often for weeks or months. Thus at least theory cannot induce us to doubt what facts and the best authorities have established, namely, that dysentery can no longer hold a place among *original* contagions, but must form one of a distinct order of communicable diseases, which become *secondarily* contagious, that is, are propagated by means of a disease possessed of contagion, while they themselves are devoid of that property.

Vignes, whom I have already so often quoted with approbation as an accurate Hippocratic observer, possessed too of extensive experience in the armies of Spain and Germany, has some peculiar opinions on this subject, which I shall endeavour to convey in his own words. Page 362 he says : “ Tous les auteurs, excepté le celebre Stoll, qui ont écrit sur la dysenterie prétendent que cette maladie est contagieuse. (In this assertion he was clearly in error, as we have seen). Si l'on entend qu'elle l'est par *infection seulement*, je suis de leur avis : mais si au contraire ils pensent qu'elle est essentiellement contagieuse, c'est à dire qu'elle se communique d'individu à individu, ou par l'effet d'un *virus sui generis*, je ne partage point leur opinion.” He maintains

that syphilis, itch, hydrophobia, and vaccinia are alone possessed of *essential* contagion, or of a virus *sui generis*, and even denies this property to small-pox (which, according to him, is “l’effet naturel d’un virus ou levain inné dans nous”), much more to typhus, to the yellow fever, and the plague. Having satisfied himself that yellow fever (and here he may be right) “n’est pas non plus essentiellement contagieuse, mais qu’au contraire elle est la suite de causes generales,” he does not despair but that some other physician “demonstrera clairement, plus tard sans doute, que la peste d’Orient n’est également contagieuse que par infection.” In fact he is a non-contagionist where it is at all possible to maintain the doctrine, though he is not very clear in his definition of the terms he employs: “En effet,” he observes, “le typhus, l’adynamie, l’ataxie regnent notamment: le premier dans les lieux encombrés de monde, surtout de malades et ou la plus grande propreté ne saurait être entretenue; et plus les causes sont considerables, plus ces maladies sont nombreuses et dangereuses et plus aussi elles se propagent par infection, avec l’apparence de la contagion essentielle: mais cette contagion ne s’étend pas au delà du rayon influencé par les causes, qui lui donnent primitivement lieu.” Can we from all these details distinctly collect the author’s opinion whether the clothes of a typhus patient can convey the infection and communicate the disease, independently of crowded and filthy habitations? That they do may be main-

tained on unequivocal experience, and that therefore a contagion, or virus *sui generis*, is engendered by typhus fever may as surely be upheld, though the disease itself may have spontaneously originated under the influence of those general causes assigned by Vignes(*a*). But we shall find him floundering still more when we detail his opinions on the contagion of dysentery, and refer to the facts upon which he bases those opinions, more especially as both the facts and the opinions contribute essentially to support the views I have taken on the subject. His facts, which will be found in pp. 368, 369, and 370, are very equivocal, and are distinctly and clearly consistent with the position I have taken of dysentery being contagious, not *per se*, or by a virus *sui generis*, but by a combination with contagious fever. His opinions are not altogether so *decidedly* with me as his facts, those opinions being: "1°. Que la dysenterie benigne, *tres violente*, peut se propager par son infection dans l'etendue du foyer qui est corrompu. 2°. Que les dysenteries de mauvais caractere s'etendent encore plus frequemment. 3°. Qu'il en est de meme de toutes les maladies de mauvais caractere; mais que leur contagion est loin d'être essentielle et par consequent point semblable a celle de la syphilis. 4°. Que les dysenteries *tres-moderées* ne sont presque jamais susceptibles de se

(*a*) See in p. 144, *et seq.*, of my Historic Sketch on Fever in Ireland, "a review of the causes, medical and statistical, productive thereof," and proofs of its spontaneous origin.

gagner ;” and in proof of this latter proposition he states that “ nous n’avons *presque* jamais vu les dysenteries benignes moderées se communiquer, quelques frequentes que fussent les relations des dysenteriques avec les individus qui n’étaient pas atteints : nous avons eu occasion de faire cette remarque une foule de fois.” In proof of his second proposition he states (p. 370), that in the temporary military hospitals, which were often both dirty and crowded, “ j’ai vu mainte fois la dysenterie benigne *intense* se communiquer aux malades le plus rapprochés des lits des dysenteriques : quelquefois elle se communiquait a ceux des seconds lits de chaque coté, mais rarement plus loin, a moins que l’infection ne fut generale. *Mais* c’était surtout les dysenteries *de mauvais caractere*, notamment la *variété typhoide*, qui se propageaient frequemment. J’ai en effet rarement vu les malades des lits voisins des dysenteriques echapper à cette infection ; mais elle ne se manifestait guere avant le septieme ou le huitieme jour du contact.” I am content with these admissions, and shall conclude by repeating his preliminary observation, “ que les exhalaisons des excremens des dysenteriques, leurs sueurs &c. peuvent, quand on en est fortement influencé, donner lieu a cette maladie, surtout si elle est de mauvais caractere.”

Before I quit the direct subject of dysentery I may be permitted a few remarks on Durondeau’s learned treatise, which I had the good fortune recently to meet in the library of the College of Surgeons in

London, after I had vainly sought it elsewhere both at home and abroad. I was there indulged with an opportunity (the only one I could command), of running through two closely-printed octavo volumes during a space of two hours, and in doing so could not but admire the laborious industry of the author, whose work, had it fallen into my hands at an earlier period, would have saved me much research, and have furnished me with some documents of which now I can make no use. He takes an extensive and interesting review of various contemporaneous and consecutive epidemics from a remote to a very recent date (1789), and details the various causes in which they had originated, or, *with one remarkable exception*, were supposed to have originated. That exception, strange to say, related to the agency of contagion in the production of the disease, respecting which he observes, so far as I could collect, a dead silence, though he repeatedly quotes authors, such as Pringle, Degner, Zimmermann, and others, who strenuously insist on the powers of that agent. He dwells strongly on the important value of meteorological observations, and says that those made by Stoll "confirment le sentiment de ceux qui soutiennent que les epidemies dysenteriques marchent presque toujours a la suite des temperatures extraordinairement seches:" "et qu'une cause qui excite le cholera peut bien donner la dysenterie." Considering the very critical eye with which he appears to have examined the various authors he quotes, I was

surprised at his admission of Willis's "Dysenteria Incruenta," and Degner's dysenteria bilioso-contagiosa, into his catalogue of dysenteric epidemics, from which unquestionably they ought to be excluded. He had himself much experience in some recent epidemics of the genuine disease, and did not fail, amongst other things, to remark the mutual interchange of dysentery and rheumatism. "Nous avons vu," he says, "plusieurs dysenteriques soudainement delivrés du flux par un acces de rheumatisme, de goutte, de sciatique." "Ce rapport etoit palpable dans notre epidemie. La facilité avec laquelle le flux se convertissoit en un de ces maladies, et le gain qu'il y avoit a faire au change, me le fit desirer tres souvent, surtout lorsqu'il y avoit sujet de craindre un gastritis ou un enteritis." On the whole his work is well deserving of attentive perusal. The author modestly suppresses his name in the title-page, merely saying "par M. D." Hence, perhaps, arises the difficulty in procuring a copy of the work, when inquired for in the name of its learned author.

CHAPTER VIII.

ARE THERE ANY DISEASES ANALOGOUS TO DYSENTERY
IN THE SOURCE OF THEIR CONTAGION?

AGREEING with Pringle in a conviction, that "the further we penetrate into nature, the greater we shall find the analogy amongst her works;" and with Roederer in a belief "omnia ad certas et perpetuas fieri leges, nihilque in totâ rerum naturâ casu quodam fortuito et extra causas suas contingere," I did, in the first edition of this work (prematurely, perhaps), introduce a discussion under this head, with the view of ascertaining, 1st, *whether* there are not other diseases, besides dysentery, at times contagious, at times not: and 2ndly, *whether*, if there be any such, they derive their contagious property from the same source. I was then of opinion that certain diseases came within this category, though respecting some of them I could offer little more than plausible conjecture, whilst with regard to others I could submit something like satisfactory proof. Amongst the former, I had named catarrh (one species of which being deemed contagious, has been denominated catarrhus contagiosus or influenza), and also the Egyptian ophthalmia. Of the contagious nature of the latter little doubt can be entertained, though

the origin of that contagion may be questioned, notwithstanding the plausible statements of Mr. Power in his treatise on the subject, respecting the influence of the "putrid virus" of Egypt in producing that disease, as well as plague, eruptive fever, and dysentery. Of the contagious nature of influenza I entertain great doubts, viewing it, as I now do, in the same light with Asiatic cholera, merely as an epidemic remittent, convertible in some cases, and under peculiar circumstances, into fevers of typhoid type(*a*).

(*a*) The great universality and rapid diffusion of both cholera and influenza constitute, with me, insuperable objections (independently of other considerations elsewhere stated) to their admission into the class of diseases propagated by contagion. If, however, we are to credit the ancient writers, we can scarcely hesitate in believing that pleurisy and peripneumony have been so propagated, and have been peculiarly destructive, when preceding, or coexistent with, the plague or pestilential fever: indeed, facts are not wanting to shew that, even at the present day, they often exist in fatal combination with typhus fever. Huxham, in vol. ii. *De Aere et Morb. Epidem.*, furnishes some important details respecting such a combination (p. 128): he says, "Gallos inter, et Hispanos *captivos* sævit admodum Febris quædam *catarrhalis maligna* jugulatque perplurimos: 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 et perplurimos captivorum mittit ad infernos; 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

The diseases which I now consider more or less analogous to dysentery, in the source of their contagion, are the angina maligna, the erysipelas typhodes, hospital gangrene, and puerperal fever; and though Dr. Blackburne, in his Treatise on Scarlet Fever, &c., endeavours to extend the principle further, and to maintain that it is the contagion of fever, joined

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.*" P. 144 he discriminates between them; and (p. 150) he says, "*Febris putrida, nervosa maligna usque grassatur, pluresque opprimit. Et peripneumonix quoque multæ mali admodum moris; hæ post tres quatuorve dies, in malignam nervosam, aut petechialem degenerant, viresque summopere deprimunt. Vix sanguinis missionem adeo admittunt, multò minùs repetitam aut largam.*" I have already incidentally referred to Dr. Bateman's authority on the subject, and that of other equally observant authors. Among the more ancient writers we find Heurnius, in his Opera Medica, cap. 21, de Peste, stating (p. 89), that in "*pleuritide pestifera nec venesectio nec purgatio aliave alteratio quicquam solatii adfert, sed diro malo majorem adferunt sevitiam.*" "*Angina quoque pestilens eodem fere modo curatur.*"—And by Etmuller (tom. i. p. 257) it is stated that "*ab Egineta describitur colica pestilens: a Tracassato peripneumonia pestilentialis: a Wiero pestis pleuritica, phrenitides, dysenterix, hepatides, itemque anginx pestilentiales.*" I have already quoted authorities to shew that, by malignant and pestilential diseases, these ancient writers, almost universally meant diseases communicable by contagion. "*Est enim,*" says Roederer, when speaking of the morbus mucosus, "*quædam species ejus accessoria, quæ aliis affectionibus primariis characteres suos impertit;*" and of this faculty he adduces several instances, some of which have been already specified.

to certain incidental circumstances, which probably gives rise to all the variety of acute contagions exhibited in plague, small-pox, measles, and yellow fever, I more than doubt the theory he has suggested, and can see no reasonable ground for concluding that any analogy whatever exists between dysentery and the exanthemata in their contagious origin. In the first edition I gave my reasons at full for dissenting from Dr. Blackburne's positions, which I shall here omit, as those positions have long since been deemed untenable, and are not likely to be again revived. In his chapter on Infection, however, Dr. Blackburne takes an extensive and satisfactory view of the origin of epidemic fevers, and of the manner in which they become contagious. In the distinctions which he there establishes I entirely coincide, and am gratified to find that many of the facts, which I have adduced in the case of dysentery, tend strongly to confirm those distinctions.

Without further interruption, therefore, I shall proceed to detail such circumstances, and name such authorities, as would seem to warrant me in classing angina, erysipelas, hospital gangrene, and puerperal fever, amongst the diseases analogous to dysentery in the source whence they derive their contagious property. And first of

ANGINA MALIGNA.

The real nature of this disease has been a subject of some uncertainty, as evidenced by the controversies which long agitated the medical world relative to the contagion and varied forms of scarlatina ; as it is, however, now well-established, that the scarlatina simplex, anginosa, and maligna, all originate from the same contagion, I shall not enter on that question, but will at once, on the authority of an author of great experience and sound judgment, establish the existence of a species of angina, which owes its contagious character to a source similar to that of dysentery. Willan, in the Preface to his Report 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 angina maligna would have been most properly applied. Medical writers, by not distinguishing it from the scarlatina anginosa, 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*, exhibited deep ulcerations of the tonsils and adjoining parts, covered with ash-coloured sloughs, and surrounded by livid edges. By his de-

scription we may see how this angina maligna differs from the cynache maligna of Cullen, though he erroneously makes the latter a different disease from scarlatina anginosa. Willan, in his Work on cutaneous diseases, when treating of scarlatina, observes (p. 333): "It may, therefore, be concluded that no British author has yet described any epidemical and contagious sore throat, except that which attends the scarlet fever. The title 'angina maligna' would have applied with equal, if not with more propriety, to the sore throat connected with a different species of contagion, viz., that of the typhus, or malignant fever, originating in the habitations of the poor, where no attention is paid to cleanliness and ventilation. The fever and sore throat are sometimes communicated together, and the disease, thus complicated, does not become epidemic like the scarlatina, nor is it attended with any eruption except petechiæ. 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(a)."

(a) Willan, in a note, refers to two authors, as having spoken of this combination, whom I have had no opportunity of consulting: these are Petr. a Castro de Febr. Malig. punctulari (sec. xxii. p. 232), Norimbergæ, 1652; and Ramazzini, Constit. Ann. 1691-4 (sec. xx.). Frank, too, we may remember, draws a double analogy between angina and dysentery; *first*, in the character of their symptoms respectively; and *secondly*, in the nature of the fever with which they are often conjoined, and by which they frequently acquire a contagious property.—See notes to pages 39 and 143 of his work.

ERYSIPELAS(*a*).

With respect to this disease, Willan properly remarks, that, though usually ranked amongst the exanthemata, it has little affinity with the diseases arranged under that order. He states that the disease was divided by the ancients into erysipelas phlegmonodes, and erysipelas œdematodes, for the first of which they direct blood-letting, purgatives, and a cooling diet. The latter, with less redness, exhibits more tumefaction. It is attended with a quick, weak pulse, with delirium or coma, and has a tendency to gangrene, not always to be prevented by peruvian bark, mineral acids, and a cordial regimen. The œdematic or gangrenous form of the erysipelas is, he says, the most usual in London. "The form," he further observes, "entitled erysipelas phlegmonodes does not seem communicable by contagion: however, when the fluid contained in its vesicles is inoculated into the arm, it excites a diffuse inflammation and swelling, with a slight degree of fever. The

(*a*) A broad distinction may be made between the diseases now under discussion and dysentery, as the latter may prevail epidemically, whereas angina, 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 requires for its production certain states of the atmosphere, acting as remote causes: while the latter require *certain* predispositions on the part of the individuals, and certain states of the local atmosphere which they respire; hence the sphere of action in the latter is limited, in the former much less so.

œdematic or gangrenous form of erysipelas may be combined with *malignant fever*, and *thus* communicated 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 Medical and Chirurgical Transactions, gives a few cases illustrative of the contagious nature of erysipelas. The fact of contagion he seems to prove in a manner satisfactory enough, but he does not enable us to judge of the circumstances under which it originated: the cases are not detailed with much minuteness, and it is only in one instance we 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 terminated unfavourably with several of his patients, and the tonic plan was alone successful. He also mentions some instances where the disease seemed to spread by contagion in the hospitals of London and Edinburgh. For further information on this subject I have to refer the reader to the conclusion of the article on Puerperal Fever, where he will find a concise summary of the singularly interesting epidemic, which raged at Aylesbury in 1831, manifesting a curious combination and communion between contagious erysipelas and contagious puerperal fever.

MALIGNANT ULCER, OR HOSPITAL GANGRENE.

Wherever this disease has appeared, its destructive influence has demanded attention, and its singular nature has excited curiosity; yet, notwithstanding the union of motives so powerful, and the frequent occurrence of the disease in large ships, transports, and hospitals, it may still be asserted that, previously to the publication of the first edition of this work in 1805, the source of its contagion had never been distinctly pointed out. Indeed, that it is contagious at all, some gentlemen undertake to doubt, and under that doubt to deny. Trotter, in particular, who has given detailed reports of the disease, under the name of malignant ulcer, seems to me to fly in the face of the evidence laid before him by the surgeons of the fleet, when he doubts, much more when he indirectly denies the conclusion that unavoidably followed from it. "With respect to its contagious nature," he says, "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 inquiry." And again, he says (p. 489, vol. iii.), "Mr. M'Dowal speaks of the infectious nature of this ulcer. This language has been employed by others, and it is an easy way to get rid of a difficult question." Though he admits that some *strong* facts have been brought forward to countenance the contagious character 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 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 *suspicion*, that a long and excessive use of spirituous liquors most frequently precedes 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 constitutions, covers the face, nose, &c., with red and irritable spots and eruptions; it taints the fluids, impregnates them with foetid hydrogeous gas, which exhales from the lungs, and is known by its bad smell; it excites to excess the moving fibres, and disposes them to gangrene with more rapidity than any other ordinary stimuli." He also looks upon a long residence in warm climates as often forming part of this disposition to the malignant ulcer. But Trotter himself 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 ardent spirits was to give it origin, and he himself observes (p. 212, vol. ii.), "that peculiarities of constitution have not been remarked in our patients, and men who had *lately* come to sea were equally sufferers." Had he recollected that this dis-

ease often originates in crowded and ill-ventilated hospitals, and with equal facility 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 practical inference for that of theoretic speculation.

Another gentleman, possessed of equal opportunities for observation, has expressed himself very differently on this subject. Blane says he was led to believe ulcers infectious from observing that some ships were much more subject to them than others, though in the same circumstances in point of climate, victualling, and the duties of the service. "Some facts," he adds, "which 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.*, and similar proofs may also be found in Trotter's *Medicina Nautica*, art. Malignant Ulcer: I will not here detail these proofs, but shall merely mention a few of the phenomena which the disease presents. "The propensity to this complaint was such," says Blane, "that the smallest sore, whether from a hurt or a pimple, fell into the state of an ulcer. Blistered parts were also affected in the same manner. Sores which 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, as also the sentinels and nurses who were about them. The

incisions of those who underwent surgical 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 manner. These ulcers were attended with symptoms of the most virulent and malignant 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." Trotter himself mentions that "contused spots, even where the cuticle was not broken, were not exempted from the general tendency to ulcer. But parts that had been burned or scalded, above all other accidents, most quickly assumed the nature of this horrid sore ; spread and inflamed more rapidly, and in the end put on the most formidable appearance." Again he adds : "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, nor contusion, could be said to have first taken place." He then describes the manner in which the disease proceeded, till it assumed every characteristic symptom with concomitant fever and subsequent ulceration. It would appear that sores, possessing a specific action, as the venereal, scrofulous, and variolous, 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 the dressers, when the skin was broken, fell into the same sort of ulcer. The practice usually adopted and attended with any success was an emetic and purgative during the inflammatory stage, with wine, cordials, and nourishing food, the moment gangrene appeared, after which the patient required the most generous diet. These articles, says Trotter (meaning wine, cider, 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 of cure. I have indulged in this detail that we may have the less difficulty in establishing a point I shall, by and by, insist on. Trotter considers this as a nondescript disease, though he says we cannot assert it to be a new one(*a*). We shall,

(*a*) Mr. Blackadder fully refutes this position; and Trotter, when he made the assertion, could not have read Lind, his much-

however, soon find that it has been described by authors of remote date. The disease often appeared in the Hotel Dieu at Paris, and also in the Royal Infirmary of Edinburgh, and in both places so uniformly arose from filthiness and want of ventilation, that since these causes have been obviated, the disease has not shewn itself. In hospitals its action produced the same phenomena as on board ship, and its contagious nature was but too often satisfactorily manifested. 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. The symptoms were both

valued predecessor, who, in the second edition of his *Essay on the Diseases of Europeans in hot Climates*, published in 1771, describes the malignant and fatal diseases produced by the climate of Batavia (aggravated by the Dutch practice of intersecting the city by canals or ditches, the stench from the mud of which, after evaporation, was intolerable): "Nor was the sickness," he states, "at that time confined to the ships: the whole city afforded a scene of disease and death: streets crowded with funerals, and horses jaded with dragging the dead to their graves." "At that time," he says, "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 adds, "is so extraordinary, that upon a single testimony credit would hardly be given to it, yet on board the *Medway* and *Panther* they had the most fatal experience of it." The fever, whose influence was on this occasion so destructive, he proves in another part of his treatise to have been infectious.

local and general; the constitutional symptoms usually preceded; they were those of *debility*(a).

In opposition, however, to this latter statement of facts, we find Messrs. Blackadder, Hennen, and Copland Hutchinson, maintaining that the local and not the constitutional symptoms usually precede: that "it is a local disease in the first instance, afterwards exciting reaction in the system; that it is decidedly a *contagious* disease, and capable of existing in combination with other diseases of a highly *infectious* character; that it is the production of a *specific* poison; and that its symptoms are liable to be modified by circumstances." This, no doubt, they are, as what disease is not so liable? but what the modification is to which Mr. B. would here specially refer, he does not state, neither does he specify what are the "diseases of a *highly infectious* character, with which it is capable of entering into combination." Now it appears to me, from the statements of both parties (in conjunction with facts yet to be referred to), almost self-evident, that the disease, like small-pox, being propagated by inoculation as well as by local atmospheric influence, must, according to its source, produce suitable effects; hence in some cases

(a) 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 be relied on. Mr. Thompson, one of the surgeons of the Infirmary, and Professor of Surgery, kindly favoured me with these facts, which he has himself placed on record since the publication of the first edition of this work.

local symptoms, in others constitutional, take precedence, though in many without the closest scrutiny it may be doubtful which of the two had priority (*a*). What, I may ask, occurs when we inoculate with small-pox matter? Local symptoms always precede the constitutional; but when the disease is taken without the direct insertion of its virus, then constitutional symptoms ever precede the eruption. Without the admission of this double agency in the contagious source of hospital gangrene, and the rejection

(*a*) "Surgical writers on gangrene," says Dr. Copland, "even up to the present time, have concerned themselves chiefly with the external manifestations of this lesion, without sufficient reference to the states of vascular action and of vital energy; to the changes in the organic nervous influence, in the circulating fluids and in the abdominal secretions, which both favour its occurrence, hasten its progress, and modify its conditions. Many of the surgical writers on this malady have hardly looked beyond the local origin of it, and have limited their curative measures too strictly to the gangrened part." "Hospital gangrene is always attended by adynamic fever," and "the causes commonly giving rise to typhoid or putro-adynamic fever will often occasion it, especially in the crowded wards of hospitals." Such being the case, and it being equally certain that contagion, when received into the system, requires an interval of time, more or less, for the production of its appropriate effects, it follows that, if we suppose the contagion of typhus, acting under particular circumstances on individuals duly predisposed, capable of exciting hospital gangrene, the action of that contagion would probably be perceived as early in the altered condition of the wound as in the production of constitutional symptoms. Indeed it is not unlikely that, if both were equally attended to, we should find these effects nearly simultaneous.

of a mere primary local action, how can we possibly explain the occurrence of the disease without any previous ulcer, or in wounds constantly protected from its direct influence? How, more especially, can it otherwise be possible to explain the occurrences detailed by Roederer, and which I have already given in pages 95, 96, and 97 of this work. These pages are well worthy of re-perusal, as they furnish unquestionable facts from an author who had no special theory to support. From his most interesting details nothing can be more clearly established than the supervention of hospital gangrene, so soon as any patient with the slightest wound was attacked by the "*Febris mucosa acuta maligna*," a disease which prevailed in the camps and crowded military hospitals of a town closely besieged, and reduced to great distress from a scarcity of provisions and the multitudes crammed within its walls. This febrile disease was obviously a special combination of typhus with a peculiar mucous affection of the greater and smaller intestines. His words are very distinct and remarkable, shewing that gangrene followed the slightest wound "*ex mucosi epidemici connubio:*" "*sequitur gangrena juncto simul symptomatum febris mucosæ malignæ satellitio.*" And here I cannot avoid noticing the singular combination that took place in this instance: two local diseases, the mucous affection and ulcer, neither of them contagious, are spread by contagion when combined with malignant fever, thus presenting a very singular fact in

the annals of medicine, demonstrative of the common source whence these diseases alike derived that property.

That typhoid contagion is the source most probably productive of hospital gangrene may be inferred from its symptoms and treatment, the circumstances and places of its prevalence, and the means of prevention: that it is its actual source may at least be anticipated as my opinion, from the place here assigned to the disease; and as few have hazarded a similar opinion, I cannot but feel gratified in quoting the authority of M. Delpech in its support. That gentleman, in his memoir on the subject, refers the production of typhus fever and gangrenous phagedena to the same cause, viz., the previous debilitated state of the patient, and the corrupted condition of the atmosphere of the hospitals. He thinks that, though mainly propagated by the direct application of the morbid matter to the sores, yet that it is also propagated through the medium of the atmosphere. His words are clear and unequivocal, though Mr. Blackadder sneers at an *apparent* inconsistency in the last sentence. "La contagion," says M. D., "qui produit la pourriture d'hôpital, paraît être le même qui détermine le typhus nosocomial." "Nous avons observé la pourriture combinée de toutes les manières avec le typhus, et toujours nous avons vérifié *l'indépendance fondamentale de ces deux maladies.*" In this there is no *real* inconsistency, as the passage merely evinces

this, that, though the diseases might combine, they were still *fundamentally* distinct.

I shall conclude these details, relative to hospital gangrene, with one of some interest from Ramazzini: the statement will be found in p. 638 of his 40th chap. de Morb. Castren. In this chapter he quotes the observations of G. Erric Barnstoff, who attended the German armies during five campaigns in Hungary, a country then notorious for the great fatality of its disorders. Malignant fevers, dysentery, and wounds constituted the chief diseases of the army. After discussing 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*, quamvis vulnera essent levissima, nec mali quicquam de illis liceret suspicari; observavit enim in diuturnis arcium obsidionibus, vulnera omnia cum contusione, veluti sclopetorum, ac præsertim in capite, utut leviscula, curatu esse difficillima, et licèt summâ diligentîâ tractata, cum summo tamen chirurgorum dedecore persæpe lethalia fuisse, superveniente nimirum *inflammatione* ac postmodum *gangrenâ*, ut locus interdum suspicioni 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, dubitari cœptum, ideoque bezoordica, terrea, et absorbentia cum cephalicis vul-

nerariis præscribendo, feliciores vulnerum instituebantur curationes, non omissâ interim vulneratæ parti, remediorum ejusdem indolis applicatione." In this case the besieging and besieged suffered the same fate, both having been long molested by malignant fevers and dysentery ; and that these diseases possess some influence in the production of malignant ulcers must appear from the preceding statements(*a*).

Such are the proofs I can at present adduce in support of the opinion now 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 attention to the subject. They must satisfy us that the disease has never appeared, except under such circumstances as were

(*a*) When the foregoing observations were given in the former edition, I conceived myself the first person who had publicly maintained these sentiments ; *perhaps*, however, Dr. Jackson 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. In his *Outline of the History and Cure of Fever* (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 the subject do not appear to me very precise, nor do I think, from the context, that his meaning is at all equivalent to mine.

competent to the production of typhus fever, and many of the facts shew that they did actually co-exist; 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 cooperation of another cause, which I consider 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. This truth is 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 cooperation of a similar cause is more or less necessary in the *production* of the disease we have next to speak of, though, when once generated, they both may continue to spread without the further cooperation of the original cause(*a*).

(*a*) Van Swieten, when refuting the objection usually made to the contagion of plague and other epidemics, taken from their origin, viz., that as the first person seized with the disease must have had it without contagion, why may not all others,—replies, that diseases may be produced in the human body by manifest causes, of which causes these diseases are the effects, and that,

PUERPERAL FEVER.

Respecting this disease I shall content myself with stating a few well-ascertained facts, because I can with pleasure refer the reader to an essay, published in the year 1787, 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 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 peritonæum." In assenting to this proposition I wish it understood, that by *puerperal fever* I mean *that fever* which has been observed occasionally to prevail in lying-in wards, or under circumstances similar to those which have taken place in such wards, *a fever* which is spread by contagion, and which is very fatal. That the disease occurring in hospitals is contagious I shall take for granted, as this point

through them, those diseases induce such a change in the body of the patient 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 says, that "when this contagion is once produced, it is not necessary that the same constitution of the air continue, from whence the disorder proceeded, but the disease goes on to exercise its power, though *another* constitution of the air may prevail."

seems now universally admitted. The disease is by some termed peritonitis typhodes, and by Gooch peritoneal fever. 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 peritoneal inflammation, and which may generally be relieved by the same means; "but," says he, "when a childbed 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."—See p. 321 of the Reports, &c. All the disputes relative to the nature of puerperal fever depended partly on want of attention to these distinctions, and also in part on laying too much stress upon the symptoms and appearances of inflammation in the *compound* puerperal disease, for these would *seem* to authorize us in ranking it among the pure inflammatory affections. Accordingly, we find Dr. Hulme and others attempting to explain every phenomenon it exhibits on the *sole* ground of an inflammation of the peritoneum and omentum, the existence of which no one doubts, and the tendency to which he attributes to the constant pressure on these parts during the latter months of pregnancy. But why, on this supposition, are so few attacked? Whence the little benefit, or rather the fatality, experienced from ve-

nesection, 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 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, if we suppose that inflammation to be conjoined with contagious fever.

What are the circumstances under which this disease prevails? 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, when the wards have been over-crowded, and that strict attention was not paid to ventilation and cleanliness? The disease in its contagious form rarely appears in private practice, unless when imported. Indeed it closely resembles hospital gangrene in every circumstance necessary for its production, *with this difference*, that in one the existence of a wound, or some similar lesion, 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 adjoining; thereby clearly shewing the

analogy between the diseases, and their dependence on the same cause.

On this subject I must again refer the reader to Dr. Walsh's Treatise(*a*), and in particular to Dr. Gooch's Observations on Peritoneal Fever. The reader will also find some interesting information respecting the disease, 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 might be consulted on the same subject with advantage, more especially my friend Doctor Douglas, who, in the same paper referred to in the note, and in answer to a particular query, gives a statement of facts, proving the over-crowded

(*a*) There is, however, one opinion of his from which I must dissent, when he asserts that the disease is not wholly confined to lying-in women, and urges in proof thereof, a fact which, though true, is not applicable, "that 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? Dr. J. C. Douglas, in a paper inserted in vol. iii. of the Dublin Hospital Reports, and published in 1821, says: "When puerpeal fever is epidemic, I consider it contagious, but, for the most part, only to lying-in women. I do, however, believe that a woman, either pregnant or whilst nursing, or even a very delicate female for several months after lying-in, although not nursing or pregnant, might be susceptible of this disease: also, that any woman, whether married or single, might at particular periods be liable to its attacks, if much exposed to the influence of an hospital epidemic. Cases occurring under these different circumstances have happened within my own knowledge."

state of the Dublin Hospital during those years in which puerperal fever was most prevalent therein, and says: "I do firmly believe a crowded state of the hospital, and a hurried succession of patients, highly conducive to the disease." "I am likewise satisfied that a temperature below that of the usual habitations of the poor acts powerfully and frequently in inducing this disease; and it is by no means unusual to hear women, when suffering in its last stages, attribute their illness to this cause alone." "I venture to pronounce," says Dr. Douglas, "the contagious puerperal fever of the Dublin Lying-in Hospital to be neither more nor less than a malignant fever of a typhoid character, accompanied by an erysipelatous inflammation of the peritoneal covering of the stomach, intestines, and other abdominal viscera." Dr. Douglas considers part of our past ill success in treating this disease attributable to a deficiency in nosological distinction. He proposes, therefore, to divide puerperal fever into three species, viz. :

1. Synochal puerperal fever ;
2. Gastro-bilious puerperal fever ; and
3. Epidemic, or contagious puerperal fever.

These species he very accurately describes, marking their appropriate treatment. "The last," he says, "though agreeing with the others in the great leading symptoms of inflammation, pain, tumefaction, and tension of the abdomen, yet differs from them in

material characters. The sensorium here is seldom in any degree disturbed: the pulse is soft, weak, and yielding, and in quickness often exceeds 160. The eye is generally pellucid, with dilated pupil. The countenance is pale and shrunk, with an indescribable expression of anxiety: the surface of the body is usually soft and clammy, and the heat not above the natural temperature; and not only is the skin cool, with clammy exudation, but the muscles feel soft and flaccid, as if deprived of their *vis insita*. Indeed there is such a prostration of muscular strength, and depression of vital principle, from the very onset, that I must suppose the contagion to act in a manner analogous to that of the plague." I have little more to add to this lucid description and positive declaration of opinion, than to say, that I willingly subscribe to the latter, and that in so doing, I confidently refer, *first*, to the authentic facts contained in the note (a) to p. 139; and, *secondly*, to the very interesting details furnished by Mr. Robert Creely, in the *Lancet* for March, 1835, of the double epidemic which prevailed at Aylesbury in the autumn of 1831. He states that a contagious epidemic puerperal fever prevailed in that town under three forms (though he does not distinctly say that they were alike contagious), viz. the acute inflammatory, the low typhoid, and the insidious typhoid, closely re-

(a) That note consists of an extract from Drs. Barker and Cheyne's *Account of Epidemic Fever*, given on the authority of the experienced Surgeon to the Foundling Hospital, Mr. Creighton.

sembling the classification previously made by Dr. Douglas. In the two latter, death often took place on the third day of the disease: there was excessive languor, and extreme prostration of strength and spirits; rapid sinking, with a mind calm and collected. Dissection shewed the mucous membrane of vagina, uterus, and Fallopian tubes highly injected: the omentum and subjacent small intestines similarly inflamed, but to no great extent: there was no adhesion or coagulable lymph, though there was effusion into the peritoneal cavity. This puerperal fever, he says, appeared during the prevalence of an "epidemic and contagious erysipelas," in which the parts usually affected were the tonsils, uvula, and fauces; that such was the erysipelatous tendency, that wounds of all kinds, abrasions, and even common attritions of the cuticle, insured to the individual exposed to the epidemic or contagious cause, an attack of one of the three forms of erysipelas (viz. the mild, the phlegmonoid, and low typhoid); and any one such case in a house communicated the disease in its several varieties to most of the nurses and attendants. The result, he adds, was favourable in most, yet fatal to a few, whose cases emanated from contagion, and who had the erysipelas on other parts besides the fauces: these died speedily, with all the topical and general symptoms of poisoned wounds. The epidemic prevailed for nine months, and extended, principally by contagion, to the adjoining villages, and was frequently

seen to originate there from a case of the puerperal disease. Finally, he declares it was beyond a doubt, "that the two diseases had a common origin, and that they were identical, and each capable of producing the other, every puerperal case giving rise to numerous cases of the common epidemic in the nurse and attendants. The puerperal disease was esteemed a mode of the prevailing contagious epidemic: it was erysipelas of the mucous membrane of the vagina and uterus, extending into the abdomen through the Fallopian tubes, and thence to the other parts."

Ere I conclude this subject it will not be amiss to take some notice of the opinions advanced thereon by so able a practitioner and intelligent a writer as Dr. J. Armstrong, who, after having published a work on puerperal fever as a pure inflammatory affection(*a*), thus expresses himself relative to the same disease in his work on typhus fever, p. 312: "No disease has embarrassed me more than the puerperal fever, having known it to occur *sporadically* in some cases where it could not be traced to contagion, in others *epidemically*, where the influence of the atmosphere, as well as contagion, might have justly been suspected as causes; and again, in a *third*

(*a*) This opinion has been ably refuted by Gooch in his Observations on Peritoneal Fever, in which he shews that bleeding is not the universal and sovereign remedy in puerperal fever Dr. Armstrong supposed it to be.

class of cases, in which a tainted state, a mere local infection of the atmosphere which the women breathed, really seemed to excite the disorder." Now Dr. A.'s opinion (not positively pronounced, however) being that "a fever is never contagious, except it originates from a *specific* cause (i. e. contagion), and that he was thence inclined to doubt the commonly received opinion that diseases proceeding from an ordinary cause may become contagious in their course," he very naturally concluded that none of those cases of puerperal fever, above referred to, could be propagated by contagion, except that which actually originated from contagion; and hence he was led "strongly to suspect that, when contagious, it is nothing *but the genuine typhus modified by the puerperal condition.*" Should this his "strong suspicion" be well founded, as I conceive it to be, it follows that this peculiar peritoneal or puerperal affection may, by its conjunction with typhus, become contagious, as is the case in dysentery; and it will further follow, if the proofs I have adduced be examined, that the latter frequently became so as well from the actual generation of typhoid contagion as from the conversion of other fevers, arising from ordinary causes, into typhus. This work abounds with ample proofs to that effect, and as I have already, in my work on Epidemic Fever in Ireland (p. 159, *et seq.*, and p. 276 of the Appendix), fully discussed the whole question, and given proofs in support of the opinions I maintain, I shall here

content myself by referring to *those passages* and to the subjoined note(a).

Such are the few, yet not unimportant facts I have to submit respecting the diseases I have been led to believe analogous to dysentery in the source of their contagion. The close affinity between the three last is so remarkable, that a proof applicable to one may be deemed more or less applicable to all.

(a) In the third volume of the *Medico-Chirur. Review* there is an interesting account of the prevalence of dysentery in the *Fly sloop of war*, by Mr. Jones, its Surgeon, "shewing that acute disease attended with fever may, under peculiar circumstances, assume a contagious character, though originally produced by atmospheric vicissitudes." In the course of a few months the crew had been subjected to two summers and two winters, with all their intermediate variations. The consequence was a few cases of synochus, and sixty-four cases of dysentery, in a crew of 120, among the whole of whom the latter spread with great rapidity, even after the sick were landed. The dysentery was very severe, and latterly accompanied by typhoid fever, "evincing strong proofs of a contagious character." "Nor need we wonder at this when we consider the causes which originally produced the disease, and the circumstances of crowding, &c., by which it was attended."—These and the other facts, bearing on the general question, I must now leave to the better judgment of my readers.

N. B.—The annexed Index obviates the necessity of constantly referring, as well to the titles of the works, as to the names of authors in the various quotations made in the body of this Work.

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There are yet some other names I could wish to have added to this list, but their works I have not been able to find. Of these I may mention Wepfer's *Dissertatio de Dysenteriâ Malignâ*; Baldinger (a Prussian physician highly commended by Zimmermann) on the Diseases of the Army; Frank (L.), *de Peste Dysentericâ*; and Strack's *Dissertatio de Dysenteria.*

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