Puerperal fever as a private pestilence / by Oliver Wendell Holmes.

PUERPERAL FEVER,

AS A

Bind this cover in front

PRIVATE PESTILENCE.

BY

OLIVER WENDELL HOLMES, M. D.

PARKMAN PROFESSOR OF ANATOMY AND PHYSIOLOGY IN HARVARD UNIVERSITY.

BOSTON:

TICKNOR AND FIELDS.

M DCCC LV.

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THURSTON AND TORRY, PRINTERS.

TO

THE MEDICAL STUDENTS AND GRADUATES

OF HARVARD UNIVERSITY.

THE POINT AT ISSUE.

THE AFFIRMATIVE.

'The disease known as Puerperal Fever is so far contagious, as to be frequently carried from patient to patient by physicians and nurses.' — *O. W. Holmes*, 1843.

THE NEGATIVE.

'The result of the whole discussion, will, I trust, serve, not only to exalt your views of the value and dignity of our profession, but to divest your minds of the overpowering dread that you can ever become, especially to woman, under the extremely interesting circumstances of gestation and parturition, the minister of evil; that you can ever convey, in any possible manner, a horrible virus, so destructive in its effects, and so mysterious in its operations as that attributed to puerperal fever.' — *Professor Hodge*, 1852.

'I prefer to attribute them to accident, or Providence, of which I can form a conception, rather than to a contagion of which I cannot form any clear idea, at least as to this particular malady.' — *Professor Meigs*, 1852.

. . . . 'in the propagation of which they have no more to do, than with the propagation of cholera from Jessore to San Francisco, and from Mauritius to St. Petersburg.' — *Professor Meigs*, 1854.

'I arrived at that certainty in the matter, that I could venture to foretell what women would be affected with the disease, upon hearing by what midwife they were to be delivered, or by what nurse they were to be attended, during their lying-in; and, almost in every instance, my prediction was verified.' — *Gordon*, 1795.

'A certain number of deaths is caused every year by the contagion of puerperal fever, communicated by the nurses and medical attendants.' — *Farr, in Fifth Annual Report of Registrar-General of England*, 1843.

. . . . 'boards of health, if such exist, or without them, the medical institutions of a country, should have the power of coercing, or of inflicting some kind of punishment on those who recklessly go from cases of puerperal fevers to parturient or puerperal females, without using due precaution; and who, having been shown the risk, criminally encounter it, and convey pestilence and death to the persons they are employed to aid in the most interesting and suffering period of female existence.' — *Copland's Medical Dictionary*, Art. *Puerperal States and Diseases*. 1852.

'We conceive it unnecessary to go into detail to prove the contagious nature of this disease, as there are few, if any, American practitioners who do not believe in this doctrine.' — *Dr. Lee, in Additions to Article last cited.*

INTRODUCTION.

It happened, some years ago, that a discussion arose in a Medical Society, of which I was a member, involving the subject of a certain supposed cause of disease, about which something was known, a good deal suspected, and not a little feared. The discussion was suggested by a case, reported at the preceding meeting, of a physician who made an examination of the body of a patient who had died with puerperal fever, and who himself died in less than a week, apparently in consequence of a wound received at the examination, having attended several women in confinement in the mean time, all of whom, as alleged, were attacked with puerperal fever.

Whatever apprehensions and beliefs were entertained, it was plain that a fuller knowledge of the facts relating to the subject would be acceptable to all present. I therefore felt that it would be doing a good service to look into the best records I could find, and inquire of the most trustworthy practitioners I knew, to learn what experience had to teach in the matter, and arrived at the results contained in the following pages.

The Essay was read before the Boston Society for Medical Improvement, and at the request of the Society, printed in the New England Quarterly Journal of Medicine and Surgery, for April, 1843. As this Journal never obtained a large circulation, and ceased to be published after a year's existence, and as

the few copies I had struck off separately were soon lost sight of among the friends to whom they were sent, the Essay can hardly be said to have been fully brought before the Profession.

The subject of this Paper has the same profound interest for me at the present moment, as when I was first collecting the terrible evidence out of which, as it seems to me, the commonest exercise of reason could not help shaping the truth it involved. It is not merely on account of the bearing of the question,—if there is a question,—on all that is most sacred in human life and happiness, that the subject cannot lose its interest. It is because I most fully believe that a fair statement of the facts must produce its proper influence on a very large proportion of well constituted and unprejudiced minds. Individuals may, here and there, resist the practical bearing of the evidence on their own feelings or interests; some may fail to see its meaning, as some persons may be found who cannot tell red from green; but I cannot doubt that most readers will be satisfied and convinced, to loathing, long before they have finished the dark obituary calendar laid before them.

I do not know that I shall ever again have so good an opportunity of being useful as was granted me by the raising of the question which produced this Essay. For I have abundant evidence that it has made many practitioners more cautious in their relations with puerperal females, and I have no doubt it will do so still, if it has a chance of being read, though it should call out a hundred counterblasts, proving to the satisfaction of their authors that it proved nothing. And, for my own part, I had rather rescue one mother from being poisoned by her attendant, than claim to have saved forty out of fifty patients, to whom I had carried the disease. Thus, I am willing to avail myself of any hint coming from without, to offer this paper once more to the press. The occasion has presented itself, as will be seen, in a convenient, if not in a flattering form.

I send this Essay again to the Medical Profession, without the change of a word or syllable. I find, on reviewing it, that it anticipates and eliminates those secondary questions

which cannot be entertained for a moment, until the one great point of fact is peremptorily settled. In its very statement of the doctrine maintained, it avoids all discussion of the nature of the disease '*known as puerperal fever*,' and all the somewhat stale philology of the word *contagion*. It mentions, fairly enough, the names of sceptics, or unbelievers as to the reality of personal transmission; of Dewees, of Tonnellé, of Dugès, of Baudelocque and others; of course, not including those whose works were then unwritten or unpublished; nor enumerating all the continental writers who, in ignorance of the great mass of evidence accumulated by British practitioners, could hardly be called well informed on this subject. It meets all the array of negative cases, — those in which disease did not follow exposure, — by the striking example of small pox, which, although one of the most contagious of diseases, is subject to the most remarkable irregularities and seeming caprices in its transmission. It makes full allowance for other causes besides personal transmission, especially for epidemic influences. It allows for the possibility of different modes of conveyance of the destructive principle. It recognises and supports the belief that a series of cases may originate from a single primitive source which affects each new patient in turn; and especially from cases of Erysipelas. It does not undertake to discuss the theoretical aspect of the subject; that is a secondary matter of consideration. Where facts are numerous, and unquestionable, and unequivocal in their significance, theory must follow them as it best may, keeping time with their step, and not go before them, marching to the sound of its own drum and trumpet. Having thus narrowed its area to a limited practical platform of discussion, a matter of life and death, and not of phrases or theories, it covers every inch of it with a mass of evidence which I conceive a Committee of Husbands, who can count coincidences and draw conclusions as well as a Synod of Accoucheurs, would justly consider as affording ample reasons for an *unceremonious dismissal* of a practitioner, (if it is conceivable that such a step could be waited for,) after five or six funerals had marked the path of his daily visits, while other practitioners were not thus escorted. To the Profession, therefore,

I submit the paper in its original form, and leave it to take care of itself.

To the Medical Students, into whose hands this Essay may fall, some words of introduction may be appropriate, and perhaps, to a small number of them, necessary. There are some among them who, from youth, or want of training, are easily bewildered and confused in any conflict of opinions into which their studies lead them. They are liable to lose sight of the main question in collateral issues, and to be run away with by suggestive speculations. They confound belief with evidence, often trusting the first because it is expressed with energy, and slighting the latter because it is calm and unimpassioned. They are not satisfied with proof; they cannot believe a point is settled, so long as every body is not silenced. They have not learned that error is got out of the minds that cherish it, as the tænia is removed from the body, one joint, or a few joints at a time, for the most part, rarely the whole evil at once. They naturally have faith in their instructors, turning to them for truth, and taking what they may choose to give them; babes in knowledge, not yet able to tell the breast from the bottle, pumping away for the milk of truth at all that offers, were it nothing better than a Professor's shrivelled forefinger.

In the earliest and embryonic stage of professional development, any violent impression on the instructor's mind is apt to be followed by some lasting effect on that of the pupil. No mother's mark is more permanent than the mental nævi and moles, and excrescences, and mutilations, that students carry with them out of the lecture-room, if once the teeming intellect which nourishes theirs, has been scared from its propriety by any misshapen fantasy. Even an impatient or petulant expression, which to a philosopher would be a mere index of the low state of amiability of the speaker at the moment of its utterance, may pass into the young mind as an element of its future constitution, to injure its temper or corrupt its judgment. It is a duty, therefore, which we owe to this younger class of students, to clear any important truth, which may have been rendered questionable in their minds by such language, or any truth-

teller against whom they may have been prejudiced by hasty epithets, from the impressions such words have left. Until this is done, they are not ready for the question, where there is a question for them to decide. Even if we ourselves are the subjects of the prejudice, there seems to be no impropriety in showing that this prejudice is local or personal, and not an acknowledged conviction with the public at large. It may be necessary to break through our usual habits of reserve to do this, but this is a fault of the position in which others have placed us.

Two widely known and highly esteemed practitioners, Professors in two of the largest Medical Schools of the Union, teaching the branch of art which includes the Diseases of Women, and therefore speaking with authority; addressing in their lectures and printed publications large numbers of young men, many of them in the tenderest immaturity of knowledge, have recently taken ground in a formal way against the doctrine maintained in this paper.* The first of the two publications, Dr. Hodge's Lecture, while its theoretical considerations and negative experiences do not seem to me to require any further notice than such as lay ready for them in my Essay written long before, is, I am pleased to say, unobjectionable in tone and language, and may be read without offence.

This can hardly be said of the chapter of Dr. Meigs's volume which treats of Contagion in Childbed Fever. There are expressions used in it which might well put a stop to all scientific discussions, were they to form the current coin in our exchange of opinions. I leave the 'very young gentlemen,' whose careful expositions of the results of practice in more than six thousand cases, are characterized as 'the jejune and

* On the Non-Contagious Character of Puerperal Fever: An Introductory Lecture. By Hugh L. Hodge, M. D., Professor of Obstetrics in the University of Pennsylvania. Delivered Monday, October 11, 1852. Philadelphia: 1852.

On the Nature, Signs and Treatment of Childbed Fevers: in a Series of Letters Addressed to the Students of his Class. By Charles D. Meigs, M. D., Professor of Midwifery and the Diseases of Women and Children in Jefferson Medical College, Philadelphia, etc., etc. Philadelphia: 1854. Letter VI.

fizenless dreamings of sophomore writers,' to the sympathies of those 'dear young friends,' and 'dear young gentlemen,' who will judge how much to value their instructor's counsel to think for themselves, knowing what they are to expect if they happen not to think as he does.

One unpalatable expression, I suppose the laws of construction oblige me to appropriate to myself, as my reward for a certain amount of labor bestowed on the investigation of a very important question of evidence, and a statement of my own practical conclusions. I take no offence and attempt no retort. No man makes a quarrel with me over the counterpane that covers a mother, with her new-born infant at her breast! There is no epithet in the vocabulary of slight and sarcasm that can reach my personal sensibilities in such a controversy. Only just so far as a disrespectful phrase may turn the student aside from the examination of the evidence, by discrediting or dishonoring the witness, does it call for any word of notice.

I appeal from the disparaging language by which the Professor in the Jefferson School of Philadelphia would dispose of my claims to be listened to. I appeal, not to the vote of the Society for Medical Improvement, although this was an unusual evidence of interest in the paper in question, for it was a vote passed among my own townsmen; nor to the opinion of any American, for none know better than the Professors in the great Schools of Philadelphia how cheaply the praise of native contemporary criticism is obtained. I appeal to the recorded opinions of those whom I do not know, and who do not know me, nor care for me, except for the truth that I may have uttered; to Copland, in his Medical Dictionary, who has spoken of my Essay in phrases to which the pamphlets of American 'scribblers' are seldom used from European authorities; to Ramsbotham, whose compendious eulogy is all that self-love could ask; to the Fifth Annual Report of the Registrar-General of England, in which the second-hand abstract of my Essay figures largely, and not without favorable comment, in an important appended paper. These testimonies, half forgotten until this circumstance recalled them, are dragged into

the light, not in a paroxysm of vanity, but to show that there may be food for thought in the small pamphlet which the Philadelphia Teacher treats so lightly. They were at least unsought for, and would never have been proclaimed but for the sake of securing the privilege of a decent and unprejudiced hearing.

I will take it for granted that they have so far counterpoised the depreciating language of my fellow-countryman and fellow-teacher, as to gain me a reader here and there among the youthful class of students I am now addressing. It is only for their sake that I think it necessary to analyze, or explain, or illustrate, or corroborate any portion of the following Essay. But I know that nothing can be made too plain for beginners, and as I do not expect the practitioner, or even the more mature student, to take the trouble to follow me through an Introduction which I consider wholly unnecessary and superfluous for them, I shall not hesitate to stoop to the most elementary simplicity for the benefit of the younger student. I do this more willingly, because it affords a good opportunity, as it seems to me, of exercising the untrained mind in that medical logic which does not seem to have been either taught or practised in our schools of late, to the extent that might be desired.

I will now exhibit, in a series of propositions reduced to their simplest expression, the same essential statements and conclusions as are contained in the Essay, with such commentaries and explanations as may be profitable to the inexperienced class of readers addressed.

I. It has been long believed by many competent observers, that Puerperal Fever (so called) is sometimes carried from patient to patient by medical assistants.

II. The express object of this Essay is to prove that it is so carried.

III. In order to prove this point, it is not necessary to consult any medical theorist, as to whether or not it is consistent with his preconceived notions that such a mode of transfer should exist.

IV. If the medical theorist insists on being consulted, and we see fit to indulge him, he cannot be allowed to assume that the alleged laws of contagion, *deduced from observation* in other diseases, shall be cited to disprove the alleged laws *deduced from observation* in this. Science would never make progress under such conditions. Neither the long incubation of hydrophobia, nor the protecting power of vaccination, would ever have been admitted, if the results of observation in these affections had been rejected as contradictory to the previously ascertained laws of contagion.

V. The disease in question is not a common one; producing, on the average, about three deaths in a thousand births, according to the English Registration returns which I have examined.

VI. When an unusually large number of cases of this disease occur about the same time, it is inferred, therefore, that there exists some special cause for this increased frequency. If the disease prevails extensively over a wide region of country, it is attributed without dispute to an *epidemic* influence. If it prevails in a single locality, as in a hospital, and not elsewhere, this is considered proof that some *local* cause is there active in its production.

VII. When a large number of cases of this disease occur in rapid succession, in one individual's ordinary practice, and few or none elsewhere, these cases appearing in scattered localities, in patients of the same average condition as those who escape under the care of others; there is the same reason for connecting the cause of the disease with the *person* in this instance, as with the *place* in that last mentioned.

VIII. Many series of cases, answering to these conditions, are given in this Essay, and many others will be referred to which have occurred since it was written.

IX. The alleged results of observation may be *set aside;* first, because the so-called facts are in their own nature equivocal; secondly, because they stand on insufficient authority; thirdly, because they are not sufficiently numerous. But, in this case, the disease is one of striking and well marked character; the witnesses are experts, interested in denying and

disbelieving the facts; the number of consecutive cases in many instances frightful, and the number of series of cases such, that I have no room for many of them except by mere reference.

X. These results of observation, being admitted, may, we will suppose, be *interpreted* in different methods. Thus the coincidences may be considered the effect of *chance*. I have had the chances calculated by a competent person, that a given practitioner, A., shall have sixteen fatal cases in a month, on the following data: A. to average attendance upon two hundred and fifty births in a year; three deaths in one thousand births to be assumed as the average from puerperal fever; no epidemic to be at the time prevailing. It follows, from the answer given me, that if we suppose every one of the five hundred thousand annual births of England to have been recorded during the last half century, there would not be one chance in a million million million millions, that one such series should be noted. No possible fractional error in this calculation can render the chance a working probability. Applied to dozens of series of various lengths, it is obviously an absurdity. Chance, therefore, is out of the question as an explanation of the admitted coincidences.

XI. There is, therefore, *some* relation of cause and effect, between the physician's presence and the patient's disease.

XII. Until it is proved to what *removable condition* attaching to the attendant the disease is owing, he is bound to stay away from his patients so soon as he finds himself singled out to be tracked by the disease. How long, and with what other precautions, I have suggested, without dictating, at the close of my essay. If the physician does not at once act on any reasonable suspicion of his being the medium of transfer, the families where he is engaged, if they are allowed to know the facts, should decline his services for the time. His feelings on the occasion, however interesting to himself, should not be even named in this connection. A physician who talks about *ceremony* and *gratitude*, and *services rendered*, and the *treatment he got*, surely forgets himself; it is impossible that he should seriously think of these small matters where there is

even a question whether he may not carry disease, and death, and bereavement into any one of 'his families,' as they are sometimes called.

I will now point out to the young student the mode in which he may relieve his mind of any confusion, or possibly, if *very* young, any doubt, which the perusal of Dr. Meigs's Sixth Letter may have raised in his mind.

The most prominent ideas of the Letter are, first, that the transmissible nature of puerperal fever appears improbable, and, secondly, that it would be very inconvenient, to the writer. Dr. Woodville, Physician to the Small Pox and Inoculation Hospital in London, found it improbable, and exceedingly inconvenient to himself, that cow-pox should prevent small-pox; but Dr. Jenner took the liberty to prove the fact, notwithstanding.

I will first call the young student's attention to the show of negative facts, (exposure without subsequent disease,) of which much seems to be thought. And I may, at the same time, refer him to Dr. Hodge's Lecture, where he will find the same kind of facts and reasoning. Let him now take up Watson's Lectures, the good sense and spirit of which have made his book a universal favorite, and open to the chapter on Continued Fever. He will find a paragraph containing the following sentence: 'A man might say, "I was in the battle of Waterloo, and saw many men around me fall down and die, and it was said that they were struck down by musket balls; but I know better than that, for I was there all the time, and so were many of my friends, and we were never hit by any musket balls. Musket balls, therefore, could not have been the cause of the deaths we witnessed." And if, like contagion, they were not palpable to the senses, such a person might go on to affirm that no proof existed of there being any such thing as musket balls.' Now let the student turn back to the Chapter on Hydrophobia in the same volume. He will find that John Hunter knew a case in which, of twenty-one persons bitten, only one died of the disease. He will find that one dog at Charenton was bitten at different times by thirty differ-

ent mad dogs, and outlived it all. Is there no such thing, then, as hydrophobia? Would one take no especial precautions if his wife, about to become a mother, had been bitten by a rabid animal, because so many escape? Or let him look at Underwood on Diseases of Children,* and he will find the case of a young woman who was inoculated eight times in thirty days, at the same time attending several children with small pox, and yet was not infected. But seven weeks afterwards she took the disease and died.

It would seem as if the force of this argument could hardly fail to be seen, if it were granted that every one of these series of cases were so reported as to prove that there could have been no transfer of disease. *There is not one of them* so reported, in the Lecture or the Letter, as to prove that the disease may not have been carried by the practitioner. I strongly suspect that it was so carried in some of these cases, but from the character of the very imperfect evidence, the question can never be settled without further disclosures.

Although the Letter is, as I have implied, principally taken up with secondary and collateral questions, and might therefore be set aside as in the main irrelevant, I am willing, for the student's sake, to touch some of these questions briefly, as an illustration of its logical character.

The first thing to be done, as I thought when I wrote my Essay, was to throw out all discussions of the word *contagion*, and this I did effectually by the careful wording of my statement of the subject to be discussed. My object was not to settle the etymology or definition of a word, but to show that women had often died in childbed, poisoned in some way by their medical attendants. On the other point, I, at least, have no controversy with any body, and I think the student will do well to avoid it in this connection. If I must define my position, however, as well as the term in question, I am contented with Worcester's definition; provided always this avowal do not open another side-controversy on the merits of his Dictionary, which Dr. Meigs has not cited, as compared with Webster's, which he has.

* Philadelphia, 1842, p. 244, note.

I cannot see the propriety of insisting that all the laws of the eruptive fevers must necessarily hold true of this peculiar disease of puerperal women. If there were any such propriety, the laws of the eruptive fevers must at least be stated correctly. It is not true for instance, as Dr. Meigs states, that contagion is 'no respecter of persons;' that 'it attacks all individuals alike.' To give one example: Dr. Gregory, of the Small Pox Hospital, who ought to know, says that persons pass through life apparently insensible to or unsusceptible of the small-pox virus, and that the same persons do not take the vaccine disease.

As to the short time of incubation, of which so much is made, we have no right to decide beforehand whether it shall be long or short, in the cases we are considering. A dissection wound may produce symptoms of poisoning in six hours; the bite of a rabid animal may take as many months.

After the student has read the case in Dr. Meigs's 136th paragraph, and the following one, in which he exclaims against the idea of contagion, because the patient, delivered on the 26th of December, was attacked in twenty-four hours, and died on the third day, let him read what happened at the 'Black Assizes' of 1577 and 1750. In the first case, six hundred persons sickened the same night of the exposure, and three hundred more in three days.* Of those attacked in the latter year, the exposure being on the 11th of May, Alderman Lambert died on the 13th, Under-Sheriff Cox on the 14th, and many of note before the 20th.† But these are old stories. Let the student listen then to Dr. Gerhard, whose reputation as a cautious observer, he may be supposed to know. 'The nurse was shaving a man, who died in a few hours after his entrance; he inhaled his breath, which had a nauseous taste, and in an hour afterwards was taken with nausea, cephalalgia, and singing of the ears. From that *moment* the attack began, and assumed a severe character. The assistant was supporting another patient, who died soon afterwards; he felt the pungent heat upon his skin, and was taken immediately with the symptoms of typhus.'‡ It is by notes of cases, rather than notes of admira-

* Elliotson's Practice, p. 299. † Rees's Cyc. Art. Contagion.

‡ Am. Journ. Med. Sciences, Feb. 1837, p. 299.

tion, that we must be guided, when we study the Revised Statutes of Nature, as laid down from the curule chairs of Medicine.

Let the student read Dr. Meigs's 140th paragraph soberly, and then remember that not only does he *infer*, *suspect* and *surmise*, but he actually *asserts* (page 154), 'there was poison in the house,' because three, out of five patients admitted into a ward, had puerperal fever, and died. Have I not as much right to draw a positive inference from 'Dr. A's' seventy exclusive cases, as he from the three cases in the ward of the Dublin Hospital? All practical medicine, and all action in common affairs, is founded on inferences. How does Dr. Meigs know that the patients he bled in puerperal fever would not have all got well if he had not bled them?

'You see a man discharge a gun at another; you see the flash, you hear the report, you see the person fall a lifeless corpse; and you *infer*, from all these circumstances, that there was a ball discharged from the gun, which entered his body and caused his death, because such is the usual and natural cause of such an effect. But you did not see the ball leave the gun, pass through the air, and enter the body of the slain; and your testimony to the fact of killing is, therefore, only inferential, — in other words, circumstantial. It is *possible* that no ball was in the gun; and we *infer* that there was, only because we cannot account for death on any other supposition.' *

'The question always comes to this, — Is the circumstance of intercourse with the sick followed by the appearance of the disease in a proportion of cases so much greater than any other circumstance common to any portion of the inhabitants of the place under observation, as to make it inconceivable that the succession of cases occurring in persons having that intercourse should have been the result of chance? If so, the inference is unavoidable, that that intercourse must have acted as a cause of the disease. *All observations which do not bear strictly on that point are irrelevant*, and, in the case of an epidemic first

* Chief Justice Gibson, in Am. Law Journal, vol. vi. p. 123.

appearing in a town or district, a succession of *two cases* is sometimes sufficient to furnish evidence, which, on the principle I have stated, is nearly irresistible.' *

Possibly an inexperienced youth may be awe-struck by the quotation from Cuvier. These words, or their equivalent, are certainly to be found in his Introduction. So are the words 'top not come down'! to be found in the Bible, and they were as much meant for the ladies' head-dresses, as the words of Cuvier were meant to make clinical observation wait for a permit from any body to look with its eyes and count on its fingers. Let the inquiring youth read the whole Introduction, and he will see what they mean.

I intend no breach of courtesy, but this is a proper place to warn the student against skimming the prefaces and introductions of works for mottos and embellishments to his thesis. He cannot learn anatomy by thrusting an exploring needle into the body. He will be very liable to misquote his author's meaning while he is picking off his outside sentences. He may make as great a blunder as that simple Prince, who praised the conductor of his orchestra for the piece just before the overture; the musician was too good a courtier to tell him that it was only the tuning of the instruments.

To the six propositions in the 142d paragraph, and the remarks about 'specific' diseases, the answer, if any is necessary, seems very simple. An inflammation of a serous membrane may give rise to secretions which act as a poison, whether that be a 'specific' poison or not, as Dr. Horner has told his young readers, and as dissectors know too well; and that poison may produce its symptoms in a few hours after the system has received it, as any may see in Druitt's Surgery, if they care to look. Puerperal peritonitis may produce such a poison, and puerperal women may be very sensible to its influences, conveyed by contact or exhalation. Whether this is so or not, facts alone can determine, and to facts we have had recourse to settle it.

* Dr. Alison.

The following statement is made by Dr. Meigs in his 142d paragraph, and developed more at length, with rhetorical amplifications, in the 134th. 'No human being, save a pregnant or parturient woman, is susceptible to the poison.' This statement is wholly incorrect, as I am sorry to have to point out to a Teacher in Dr. Meigs's position. I do not object to the erudition which quotes Willis and Fernelius, the last of whom was pleasantly said to have 'preserved the dregs of the Arabs in the honey of his Latinity.' But I could wish that more modern authorities had not been overlooked. On this point, for instance, among the numerous facts disproving the statement, the American Journal of Medical Sciences, published not far from his lecture-room, would have presented him with a respectable catalogue of such cases. Thus he might refer to Mr. Storrs's paper 'On the Contagious Effects of Puerperal Fever on the Male Subject; or on Persons not Childbearing,' (Jan. 1846,) or to Dr. Reid's case, (April, 1846,) or to Dr. Barron's statement of the children's dying of peritonitis in an epidemic of puerperal fever at the Philadelphia Hospital, (Oct. 1842,) or to various instances cited in Dr. Kneeland's article, (April, 1846.) Or if he would have referred to the New York Journal, he might have seen Prof. Austin Flint's cases. Or, if he had honored my Essay so far, he might have found striking instances of the same kind in the first of the new series of cases there reported and elsewhere. I do not see the bearing of his proposition, if it were true. But it is one of those assertions that falls in a moment before a slight examination of the facts; and I confess my surprise, that a Professor who lectures on the Diseases of Women should have ventured to make it.

Nearly seven pages are devoted to shewing that I was wrong in saying I would not be 'understood to imply that there exists a doubt in the mind of any well-informed member of the medical profession, as to the fact that puerperal fever is sometimes communicated from one person to another, both directly and indirectly.' I will devote seven lines to these seven pages, which, if I may say it without offence, are, as it seems to me, six more than are strictly necessary.

The following authors are cited as sceptics by Dr. Meigs: — *Dewees.* — I cited the same passage. Did not know half the facts. *Robert Lee.* — Believes the disease is sometimes communicable by contagion. *Tonnellé* and *Baudelocque.* — Both cited by me. *Jacquemier.* — Published three years after my Essay. *Kiwisch.* — Behindhand in knowledge of Puerperal Fever.* *Paul Dubois.* — *Scanzoni.* } Continental writers not well informed on this point.†

The story of Von Busch is of interest and value, but there is nothing in it which need perplex the student. It is not pretended that the disease is always, or even, it may be, in the majority of cases, carried about by attendants; only that it is so carried in certain cases. That it may have local and epidemic causes, as well as that depending on personal transmission, is not disputed. Remember how small pox often disappears from a community in spite of its contagious character, and the necessary exposure of many persons to those suffering from it; in both diseases contagion is only one of the coefficients of the disease.

I have already spoken of the possibility that Dr. Meigs may have been the medium of transfer of puerperal fever in some of the cases he has briefly catalogued. Of Dr. Rutter's cases I do not know how to speak. I only ask the student to read the facts stated by Dr. Condie, as given in my Essay, and say whether or not a man should allow his wife to be attended by a practitioner, in whose hands 'scarcely a female that has been delivered for weeks past has escaped an attack,' 'while no instance of the disease has occurred in the patients of any other accoucheur practising in the same district.' If I understand Dr. Meigs and Dr. Hodge, they would not warn the physician or spare the patient under such circumstances. They would 'go on,' if I understand them, not to seven, or seventy, only, but to seventy times seven, if they could find patients. If this is not what they mean, may we respectfully ask them to state

* B. & F. Med. Rev., Jan. 1842.

† See Dr. Simpson's Remarks at Meeting of Edinb. Med. Chir. Soc. (Am. Jour. Oct. 1851.)

what they do mean, to their next classes, in the name of humanity, if not of science!

I might repeat the question asked concerning Dr. Rutter's cases, with reference to those reported by Dr. Roberton. Perhaps, however, the student would like to know the opinion of a person in the habit of working at matters of this kind in a practical point of view. To satisfy him on this ground, I addressed the following question to the President of one of our principal Insurance Companies, leaving Dr. Meigs's book and my Essay in his hands at the same time.

Question. 'If such facts as Roberton's cases were before you, and the attendant had had ten, or even five fatal cases, or three, or *two* even, would you, or would you not, if insuring the life of the next patient to be taken care of by that attendant, expect an extra premium over that of an average case of childbirth?'

Answer. 'Of course I should require a very large extra premium, if I would take the risk at all.'

But I do not choose to add the expressions of indignation which the examination of the facts before him called out. I was satisfied from the effect they produced on him, that if all the hideous catalogues of cases now accumulated, were fully brought to the knowledge of the public, nothing, since the days of Burke and Hare, has raised such a cry of horror as would be shrieked in the ears of the Profession.

Dr. Meigs has elsewhere invoked 'Providence' as the alternative of accident, to account for the 'coincidences.' (Obstetrics, Phil. 1852, p. 631.) If so, Providence either acts through the agency of secondary causes, as in other diseases, or not. If through such causes, let us find out what they are, as we try to do in other cases. It may be true that offences, or diseases, will come, but 'woe unto him through whom they come,' if we catch him in the voluntary or careless act of bringing them! But if Providence does not act through secondary causes in this particular sphere of ætiology, then why does Dr. Meigs take so much pains to reason so extensively about the laws of contagion, which, on that supposition, have no more to do with this case than with the plague that destroyed the people

after David had numbered them? Above all, what becomes of the theological aspect of the question, when he asserts that a practitioner was 'only *unlucky* in meeting with the epidemic cases?' (Op. cit. p. 633.) We do not deny that the God of battles decides the fate of nations; but we like to have the biggest squadrons on our side, and we are particular that our soldiers should not only say their prayers, but also keep their powder dry. We do not deny the agency of Providence in the disaster at Norwalk, but we turn off the engineer, and charge the Company five thousand dollars a-piece for every life that is sacrificed. Why a grand jury should not bring in a bill against a physician who switches off a score of women one after the other along his private track, when he knows that there is a black gulf at the end of it, down which they are to plunge, while the great highway is clear, is more than I can answer. It is not by laying the open draw to Providence that he is to escape the charge of manslaughter.

To finish with all these lesser matters of question, I am unable to see why a female must necessarily be unattended in her confinement, because she declines the services of a particular practitioner. In all the series of cases mentioned, the death-carrying attendant was surrounded by others not tracked by disease and its consequences. Which, I would ask, is worst, — to call in another, even a rival practitioner, or to submit an unsuspecting female to a risk which an Insurance Company would have nothing to do with?

I do not expect ever to return to this subject. There is a point of mental saturation, beyond which argument cannot be forced without breeding impatient, if not harsh feelings, towards those who refuse to be convinced. If I have so far manifested neither, it is well to stop here, and leave the rest to those younger friends who may have more stomach for the dregs of a stale argument.

The extent of my prefatory remarks may lead some to think that I attach too much importance to my own Essay. Others may wonder that I should expend so many words upon the two productions referred to, the Letter and the Lecture. I do

consider my Essay of much importance so long as the doctrine it maintains is treated as a *question*, and so long as any important part of the defence of that doctrine is thought to rest on its evidence or arguments. I cannot treat as insignificant any opinions bearing on life, and interests dearer than life, proclaimed yearly to hundreds of young men, who will carry them to their legitimate results in practice.

The teachings of the two Professors in the great schools of Philadelphia are sure to be listened to, not only by their immediate pupils, but by the Profession at large. I am too much in earnest for either humility or vanity, but I do entreat those who hold the keys of life and death, to listen to me also for this once. I ask no personal favor; but I beg to be heard, in behalf of the women whose lives are at stake, until some stronger voice shall plead for them.

I trust that I have made the issue perfectly distinct and intelligible. And let it be remembered that this is no subject to be smoothed over by nicely adjusted phrases of half-assent and half-censure divided between the parties. The balance must be struck boldly and the result declared plainly. If I have been hasty, presumptuous, ill-informed, illogical; if my array of facts means nothing; if there is no reason for any caution in the view of these facts; let me be told so, on such authority that I must believe it, and I will be silent henceforth, recognizing that my mind is in a state of disorganization. If the doctrine I have maintained is a mournful truth; if to disbelieve it, and to practise on this disbelief, and to teach others so to disbelieve and practise, is to carry desolation, and to charter others to carry it, into confiding families, let it be proclaimed as plainly what is to be thought of the teachings of those who sneer at the alleged dangers, and scout the very idea of precaution. Let it be remembered that *persons* are nothing in this matter; better that twenty pamphleteers should be silenced, or as many professors unseated, than that one mother's life should be taken. There is no quarrel here between men, but there is deadly incompatibility and exterminating warfare between doctrines. *Coincidences*, meaning nothing, though a man have a monopoly of the disease for weeks or

months; or *cause and effect*, the cause being in some way connected with the person; this is the question. If I am wrong, let me be put down by such a rebuke as no rash declaimer has received since there has been a public opinion in the medical profession of America; if I am right, let doctrines which lead to professional homicide be no longer taught from the chairs of those two great Institutions. Indifference will not do here; our Journalists and Committees have no right to take up their pages with minute anatomy and tediously detailed cases, while it is a question whether or not the 'black-death' of child-bed is to be scattered broadcast by the agency of the mother's friend and adviser. Let the men who mould opinions look to it; if there is any voluntary blindness, any interested oversight, any culpable negligence, even, in such a matter, and the facts shall reach the public ear; the pestilence-carrier of the lying-in-chamber must look to God for pardon, for man will never forgive him.

THE

CONTAGIOUSNESS OF PUERPERAL FEVER.

READ BEFORE THE BOSTON SOCIETY FOR MEDICAL IMPROVEMENT, FEB. 13, 1843, AND PUBLISHED BY REQUEST OF THE SOCIETY, IN THE NEW ENGLAND JOURNAL OF MEDICINE AND SURGERY, FOR APRIL, 1843.

In collecting, enforcing and adding to the evidence accumulated upon this most serious subject, I would not be understood to imply that there exists a doubt in the mind of any well-informed member of the medical profession as to the fact that puerperal fever is sometimes communicated from one person to another, both directly and indirectly. In the present state of our knowledge upon this point I should consider such doubts merely as a proof that the sceptic had either not examined the evidence, or, having examined it, refused to accept its plain and unavoidable consequences. I should be sorry to think with Dr. Rigby, that it was a case of 'oblique vision;' I should be unwilling to force home the *argumentum ad hominem* of Dr. Blundell, but I would not consent to make a *question* of a momentous fact, which is no longer to be considered as a subject for trivial discussions, but to be acted upon with silent promptitude. It signifies nothing that wise and experienced practitioners have sometimes doubted the reality of the danger in question; no man has the right to doubt it any longer. No negative facts, no opposing opinions, be they what they may or whose they may, can form any answer to the series of cases now within the reach of all who choose to explore the records of medical science.

If there are some who conceive that any important end would be answered by recording such opinions, or by col-

lecting the history of all the cases they could find in which no evidence of the influence of contagion existed, I believe they are in error. Suppose a few writers of authority can be found to profess a disbelief in contagion, — and they are very few compared with those who think differently, — is it quite clear that they formed their opinions on a view of all the facts, or is it not apparent that they relied mostly on their own solitary experience? Still further, of those whose names are quoted, is it not true that scarcely a single one could by any possibility have known the half or the tenth of the facts bearing on the subject which have reached such a frightful amount within the last few years? Again, as to the utility of negative facts, as we may briefly call them, — instances, namely, in which exposure has not been followed by disease, — although, like other truths, they may be worth knowing, I do not see that they are like to shed any important light upon the subject before us. Every such instance requires a good deal of circumstantial explanation before it can be accepted. It is not enough that a practitioner should have had a single case of puerperal fever not followed by others. It must be known whether he attended others while this case was in progress, whether he went directly from one chamber to others, whether he took any, and what precautions. It is important to know that several women were exposed to infection derived from the patient, so that allowance may be made for want of predisposition. Now if of negative facts so sifted there could be accumulated a hundred for every one plain instance of communication here recorded, I trust it need not be said that we are bound to guard and watch over the hundredth tenant of our fold, though the ninety and nine may be sure of escaping the wolf at its entrance. If any one is disposed, then, to take a hundred instances of lives endangered or sacrificed out of those I have mentioned, and make it reasonably clear that within a similar time and compass *ten thousand* escaped the same exposure, I shall thank him for his industry, but I must be permitted to hold to my own practical conclusions, and beg him to adopt or at least to examine them also. Children that walk in calico before open fires are not always burned to

death; the instances to the contrary may be worth recording; but by no means if they are to be used as arguments against woollen frocks and high fenders.

I am not sure that this paper will escape another remark which it might be wished were founded in justice. It may be said that the facts are too generally known and acknowledged to require any formal argument or exposition, that there is nothing new in the positions advanced, and no need of laying additional statements before the profession. But on turning to two works, one almost universally, and the other extensively appealed to as authority in this country, I see ample reason to overlook this objection. In the last edition of Dewees's Treatise on the Diseases of Females, it is expressly said, 'In this country, under no circumstance that puerperal fever has appeared hitherto, does it afford the slightest ground for the belief that it is contagious.' In the 'Philadelphia Practice of Midwifery' not one word can be found in the chapter devoted to this disease, which would lead the reader to suspect that the idea of contagion had ever been entertained. It seems proper, therefore, to remind those who are in the habit of referring to these works for guidance, that there may possibly be some sources of danger they have slighted or omitted, quite as important as a trifling irregularity of diet, or a confined state of the bowels, and that whatever confidence a physician may have in his own mode of treatment, his services are of questionable value whenever he carries the bane as well as the antidote about his person.

The practical point to be illustrated is the following: *The disease known as Puerperal Fever is so far contagious as to be frequently carried from patient to patient by physicians and nurses.*

Let me begin by throwing out certain incidental questions, which without being absolutely essential, would render the subject more complicated, and by making such concessions and assumptions as may be fairly supposed to be without the pale of discussion.

1. It is granted that all the forms of what is called puer-

peral fever may not be, and probably are not, equally contagious or infectious. I do not enter into the distinctions that have been drawn by authors, because the facts do not appear to me sufficient to establish any absolute line of demarcation between such forms as may be propagated by contagion, and those which are never so propagated. This general result I shall only support by the authority of Dr. Ramsbotham, who gives as the result of his experience, that the same symptoms belong to what he calls the infectious and the sporadic forms of the disease, and the opinion of Armstrong in his original Essay. If others can show any such distinction, I leave it to them to do it. But there are cases enough that show the prevalence of the disease among the patients of a single practitioner when the disease was in no degree epidemic, in the proper sense of the term. I may refer to those of Mr. Roberton and of Dr. Peirson, hereafter to be cited, as examples.

2. I shall not enter into any dispute about the particular *mode* of infection, whether it be by the atmosphere the physician carries about him into the sick chamber, or by the direct application of the virus to the absorbing surfaces with which his hand comes in contact. Many facts and opinions are in favor of each of these modes of transmission. But it is obvious that in the majority of cases it must be impossible to decide by which of these channels the disease is conveyed, from the nature of the intercourse between the physician and the patient.

3. It is not pretended that the contagion of puerperal fever must always be followed by the disease. It is true of all contagious diseases that they frequently spare those who appear to be fully submitted to their influence. Even the vaccine virus, fresh from the subject, fails every day to produce its legitimate effect, though every precaution is taken to insure its action. This is still more remarkably the case with scarlet fever and some other diseases.

4. It is granted that the disease may be produced and variously modified by many causes besides contagion, and more especially by epidemic and endemic influences. But this is not peculiar to the disease in question. There is no doubt

that small pox is propagated to a great extent by contagion, yet it goes through the same periods of periodical increase and diminution which have been remarked in puerperal fever. If the question is asked how we are to reconcile the great variations in the mortality of puerperal fever in different seasons and places with the supposition of contagion, I will answer it by another question from Mr. Farr's letter to the Registrar-General. He makes the statement that '*five* die weekly of small pox in the metropolis when the disease is not epidemic,' — and adds, 'The problem for solution is, — Why do the 5 deaths become 10, 15, 20, 31, 58, 88 weekly, and then progressively fall through the same measured steps?'

5. I take it for granted, that if it can be shown that great numbers of lives have been and are sacrificed to ignorance or blindness on this point, no other error of which physicians or nurses may be occasionally suspected will be alleged in palliation of this; but that whenever and wherever they can be shown to carry disease and death instead of health and safety, the common instincts of humanity will silence every attempt to explain away their responsibility.

The treatise of Dr. Gordon, of Aberdeen, was published in the year 1795, being among the earlier special works upon the disease. A part of his testimony has been occasionally copied into other works, but his expressions are so clear, his experience is given with such manly distinctness and disinterested honesty, that it may be quoted as a model which might have been often followed with advantage.

'This disease seized such women only as were visited, or delivered, by a practitioner, or taken care of by a nurse, who had previously attended patients affected with the disease.'

'I had evident proofs of its infectious nature, and that the infection was as readily communicated as that of the small pox or measles, and operated more speedily than any other infection with which I am acquainted.'

'I had evident proofs that every person who had been with a patient in the puerperal fever became charged with an atmosphere of infection, which was communicated to every

pregnant woman who happened to come within its sphere. This is not an assertion, but a fact, admitting of demonstration, as may be seen by a perusal of the foregoing table,' — referring to a table of seventy-seven cases, in many of which the channel of propagation was evident.

He adds, 'It is a disagreeable declaration for me to mention, that I myself was the means of carrying the infection to a great number of women.' He then enumerates a number of instances in which the disease was conveyed by midwives and others to the neighboring villages, and declares that 'These facts fully prove, that the cause of the puerperal fever, of which I treat, was a specific contagion, or infection, altogether unconnected with a noxious constitution of the atmosphere.'

But his most terrible evidence is given in these words, 'I ARRIVED AT THAT CERTAINTY IN THE MATTER, THAT I COULD VENTURE TO FORETELL WHAT WOMEN WOULD BE AFFECTED WITH THE DISEASE, UPON HEARING BY WHAT MIDWIFE THEY WERE TO BE DELIVERED, OR BY WHAT NURSE THEY WERE TO BE ATTENDED, DURING THEIR LYING-IN: AND, ALMOST IN EVERY INSTANCE, MY PREDICTION WAS VERIFIED.'

Even previously to Gordon, Mr. White of Manchester had said, 'I am acquainted with two gentlemen in another town, where the whole business of midwifery is divided betwixt them, and it is very remarkable that one of them loses several patients every year of the puerperal fever, and the other never so much as meets with the disorder' — a difference which he seems to attribute to their various modes of treatment.*

Dr. Armstrong has given a number of instances in his Essay on Puerperal Fever, of the prevalence of the disease among the patients of a single practitioner. At Sunderland, 'in all, forty-three cases occurred from the first of January to the first of October, when the disease ceased; and of this number forty were witnessed by Mr. Gregson and his assistant Mr. Gregory, the remainder having been separately seen by three accoucheurs.' There is appended to the London edition

* On the Management of Lying-in Women, p. 120.

of this essay, a letter from Mr. Gregson, in which that gentleman says, in reference to the great number of cases occurring in his practice, 'The cause of this I cannot pretend fully to explain, but I should be wanting in common liberality if I were to make any hesitation in asserting, that the disease which appeared in my practice was highly contagious, and communicable from one puerperal woman to another.' 'It is customary among the lower and middle ranks of people to make frequent personal visits to puerperal women resident in the same neighborhood, and I have ample evidence for affirming that the infection of the disease was often carried about in that manner; and, however painful to my feelings, I must in candor declare, that it is very probable the contagion was conveyed, in some instances, by myself, though I took every possible care to prevent such a thing from happening, the moment that I ascertained that the distemper was infectious.' Dr. Armstrong goes on to mention six other instances within his knowledge, in which the disease had at different times and places been limited, in the same singular manner, to the practice of individuals, while it existed scarcely if at all among the patients of others around them. Two of the gentlemen became so convinced of their conveying the contagion, that they withdrew for a time from practice.

I find a brief notice, in an American Journal, of another series of cases, first mentioned by Mr. Davies, in the Medical Repository. This gentleman stated his conviction that the disease is contagious.

'In the autumn of 1822, he met with twelve cases, while his medical friends in the neighborhood did not meet with any, "or at least very few." He could attribute this circumstance to no other cause than his having been present at the examination, after death, of two cases, sometime previous, and of his having imparted the disease to his patients, notwithstanding every precaution.' *

Dr. Gooch says, 'It is not uncommon for the greater number of cases to occur in the practice of one man, whilst the

* Philad. Med. Journal for 1825, p. 408.

other practitioners of the neighborhood, who are not more skilful or more busy, meet with few or none. A practitioner opened the body of a woman who had died of puerperal fever, and continued to wear the same clothes. A lady whom he delivered a few days afterwards was attacked with and died of a similar disease; two more of his lying-in patients, in rapid succession, met with the same fate; struck by the thought, that he might have carried contagion in his clothes, he instantly changed them, and met with no more cases of the kind.* A woman in the country, who was employed as washerwoman and nurse, washed the linen of one who had died of puerperal fever; the next lying-in patient she nursed, died of the same disease; a third nursed by her met with the same fate, till the neighborhood getting afraid of her, ceased to employ her.' †

In the winter of the year 1824, 'Several instances occurred of its prevalence among the patients of particular practitioners, whilst others who were equally busy met with few or none. One instance of this kind was very remarkable. A general practitioner, in large midwifery practice, lost so many patients from puerperal fever, that he determined to deliver no more for some time, but that his partner should attend in his place. This plan was pursued for one month, during which not a case of the disease occurred in their practice. The elder practitioner being then sufficiently recovered, returned to his practice, but the first patient he attended was attacked by the disease and died. A physician, who met him in consultation soon afterwards, about a case of a different kind, and who knew nothing of his misfortune, asked him whether puerperal fever was at all prevalent in his neighborhood, on which he burst into tears, and related the above circumstances.

'Among the cases which I saw this season in consultation, four occurred in one month in the practice of one medical man, and all of them terminated fatally.' ‡

* A similar anecdote is related by Sir Benjamin Brodie, of the late Dr. John Clarke. Lancet, May 2, 1840.

† An Account of some of the most important Diseases peculiar to Women, p. 4.

‡ Ibid. p. 71.

Dr. Ramsbotham asserted, in a lecture at the London Hospital, that he had known the disease spread through a particular district, or be confined to the practice of a particular person, almost every patient being attacked with it, while others had not a single case. It seemed capable, he thought, of conveyance, not only by common modes, but through the dress of the attendants upon the patient.*

In a letter to be found in the London Medical Gazette for January, 1840, Mr. Roberton, of Manchester, makes the statement which I here give in a somewhat condensed form.

A midwife delivered a woman on the 4th of December, 1830, who died soon after with the symptoms of puerperal fever. In one month from this date the same midwife delivered thirty women, residing in different parts of an extensive suburb, of which number sixteen caught the disease and all died. These were the only cases which had occurred for a considerable time in Manchester. The other midwives connected with the same charitable institution as the woman already mentioned, are twenty-five in number, and deliver, on an average, ninety women a week, or about three hundred and eighty a month. None of these women had a case of puerperal fever. 'Yet all this time this woman was crossing the other midwives in every direction, scores of the patients of the charity being delivered by them in the very same quarters where her cases of fever were happening.'

Mr. Roberton remarks, that little more than half the women she delivered during this month took the fever; that on some days all escaped, on others only one or more out of three or four; a circumstance similar to what is seen in other infectious maladies.

Dr. Blundell says, 'Those who have never made the experiment, can have but a faint conception how difficult it is to obtain the exact truth respecting any occurrence in which feelings and interests are concerned. Omitting particulars, then, I content myself with remarking, generally, that from more than one district I have received accounts of the preva-

* Lond. Med. Gaz. May 2, 1835.

lence of puerperal fever in the practice of some individuals, while its occurrence in that of others, in the same neighborhood, was not observed. Some, as I have been told, have lost ten, twelve, or a greater number of patients, in scarcely broken succession; like their evil genius, the puerperal fever has seemed to stalk behind them wherever they went. Some have deemed it prudent to retire for a time from practice. In fine, that this fever may occur spontaneously, I admit; that its infectious nature may be plausibly disputed, I do not deny; but I add, considerately, that in my own family, I had rather that those I esteemed the most should be delivered, unaided, in a stable, by the manger-side, than that they should receive the best help, in the fairest apartment, but exposed to the vapors of this pitiless disease. Gossiping friends, wet nurses, monthly nurses, the practitioner himself, these are the channels by which, as I suspect, the infection is principally conveyed.'*

At a meeting of the Royal Medical and Chirurgical Society, Dr. King mentioned that some years since a practitioner at Woolwich lost sixteen patients from puerperal fever in the same year. He was compelled to give up practice for one or two years, his business being divided among the neighboring practitioners. No case of puerperal fever occurred afterwards, neither had any of the neighboring surgeons any cases of this disease.

At the same meeting Mr. Hutchinson mentioned the occurrence of three consecutive cases of puerperal fever, followed subsequently by two others, all in the practice of one accoucheur.†

Dr. Lee makes the following statement: 'In the last two weeks of September, 1827, five fatal cases of uterine inflammation came under our observation. All the individuals so attacked had been attended in labor by the same midwife, and no example of a febrile or inflammatory disease of a serious nature occurred during that period among the other patients

* Lect. on Midwifery, p. 395. † Lancet, May 2, 1840.

of the Westminster General Dispensary, who had been attended by the other midwives belonging to that institution.' *

The recurrence of long series of cases like those I have cited, reported by those most interested to disbelieve in contagion, scattered along through an interval of half a century, might have been thought sufficient to satisfy the minds of all inquirers that here was something more than a singular coincidence. But if, on a more extended observation, it should be found that the same ominous groups of cases, clustering about individual practitioners, were observed in a remote country, at different times, and in widely separated regions, it would seem incredible that any should be found too prejudiced or indolent to accept the solemn truth knelled into their ears by the funeral bells from both sides of the ocean — the plain conclusion that the physician and the disease entered, hand in hand, into the chamber of the unsuspecting patient.

That such series of cases have been observed in this country, and in this neighborhood, I proceed to show.

In Dr. Francis's Notes to Denman's Midwifery, a passage is cited from Dr. Hosack, in which he refers to certain puerperal cases which proved fatal to several lying-in women, and in some of which the disease was supposed to be conveyed by the accoucheurs themselves.†

A writer in the New York Medical and Physical Journal for October, 1829, in speaking of the occurrence of puerperal fever, confined to one man's practice, remarks, 'We have known cases of this kind occur, though rarely, in New York.'

I mention these little hints about the occurrence of such cases, partly because they are the first I have met with in American medical literature, but more especially because they serve to remind us that behind the fearful array of published facts, there lies a dark list of similar events, unwritten in the records of science, but long remembered by many a desolated fireside.

Certainly nothing can be more open and explicit than the

* Lond. Cyc. of Pract. Med., Art. Fever, Puerperal.

† Denman's Midwifery, p. 675, 3d Am. ed.

account given by Dr. Peirson, of Salem, of the cases seen by him. In the first nineteen days of January, 1829, he had five consecutive cases of puerperal fever, every patient he attended being attacked, and the three first cases proving fatal. In March, of the same year, he had two moderate cases, in June, another case, and in July, another, which proved fatal. 'Up to this period,' he remarks, 'I am not informed that a single case had occurred in the practice of any other physician. Since that period I have had no fatal case in my practice, although I have had several dangerous cases. I have attended in all twenty cases of this disease, of which four have been fatal. I am not aware that there has been any other case in the town of distinct puerperal peritonitis, although I am willing to admit my information may be very defective on this point. I have been told of some "mixed cases," and "morbid affections after delivery."' *

In the Quarterly Summary of the Transactions of the College of Physicians of Philadelphia,† may be found some most extraordinary developments respecting a series of cases occurring in the practice of a member of that body.

Dr. Condie called the attention of the Society to the prevalence at the present time, of puerperal fever of a peculiarly insidious and malignant character. 'In the practice of one gentleman extensively engaged as an obstetrician, nearly every female he has attended in confinement, during several weeks past, within the above limits,' (the southern sections and neighboring districts,) 'had been attacked by the fever.'

'An important query presents itself, the Doctor observed, in reference to the particular form of fever now prevalent. Is it, namely, capable of being propagated by contagion, and is a physician who has been in attendance upon a case of the disease, warranted in continuing, without interruption, his practice as an obstetrician? Dr. C., although not a believer in the contagious character of many of those affections generally supposed to be propagated in this manner, has nevertheless

* Remarks on Puerperal Fever, pp. 12 and 13.

† For May, June, and July, 1842.

become convinced by the facts that have fallen under his notice, that the puerperal fever now prevailing, is capable of being communicated by contagion. How otherwise can be explained the very curious circumstance of the disease in one district being exclusively confined to the practice of a single physician, a Fellow of this College, extensively engaged in obstetrical practice — while no instance of the disease has occurred in the patients under the care of any other accoucheur practising within the same district; scarcely a female that has been delivered for weeks past has escaped an attack?'

Dr. Rutter, the practitioner referred to, 'observed that after the occurrence of a number of cases of the disease in his practice, he had left the city and remained absent for a week, but on returning, no article of clothing he then wore having been used by him before, one of the very first cases of parturition he attended was followed by an attack of the fever, and terminated fatally; he cannot, readily, therefore, believe in the transmission of the disease from female to female, in the person or clothes of the physician.'

The meeting at which these remarks were made was held on the third of May, 1842. In a letter dated December 20, 1842, addressed to Dr. Meigs, and to be found in the Medical Examiner,* he speaks of 'those horrible cases of puerperal fever, some of which you did me the favor to see with me during the past summer,' and talks of his experience in the disease, 'now numbering nearly seventy cases, all of which have occurred within less than a twelvemonth past.'

And Dr. Meigs asserts, on the same page, 'Indeed, I believe that his practice in that department of the profession, was greater than that of any other gentleman, which was probably the cause of his seeing a greater number of the cases.' This from a professor of midwifery, who some time ago assured a gentleman whom he met in consultation, that the night on which they met was the eighteenth in succession that he himself had been summoned from his repose,† seems hardly satisfactory.

* For Jan. 21, 1843.

† Med. Examiner for Dec. 10, 1842.

I must call the attention of the inquirer most particularly to the Quarterly Report above referred to, and the letters of Dr. Meigs and Dr. Rutter, to be found in the Medical Examiner. Whatever impression they may produce upon his mind, I trust they will at least convince him that there is some reason for looking into this apparently uninviting subject.

At a meeting of the College of Physicians just mentioned, Dr. Warrington stated, that a few days after assisting at an autopsy of puerperal peritonitis, in which he laded out the contents of the abdominal cavity with his hands, he was called upon to deliver three women in rapid succession. All of these women were attacked with different forms of what is commonly called puerperal fever. Soon after these he saw two other patients, both on the same day, with the same disease. Of these five patients two died.

At the same meeting, Dr. West mentioned a fact related to him by Dr. Samuel Jackson, of Northumberland. Seven females, delivered by Dr. Jackson in rapid succession, while practising in Northumberland county, were all attacked with puerperal fever, and five of them died. 'Women,' he said, 'who had expected me to attend upon them, now becoming alarmed, removed out of my reach, and others sent for a physician residing several miles distant. These women, as well as those attended by midwives, all did well; nor did we hear of any deaths in childbed within a radius of fifty miles, excepting two, and these I afterwards ascertained to have been caused by other diseases.' He underwent, as he thought, a thorough purification, and still his next patient was attacked with the disease and died. He was led to suspect that the contagion might have been carried in the gloves which he had worn in attendance upon the previous cases. Two months or more after this he had two other cases. He could find nothing to account for these, unless it were the instruments for giving enemata, which had been used in two of the former cases, and were employed by these patients. When the first case occurred, he was attending and dressing a limb extensively mortified from erysipelas, and went immediately to the accouchement with his clothes and gloves most thoroughly imbued with its efflu-

via. And here I may mention, that this very Dr. Samuel Jackson, of Northumberland, is one of Dr. Dewees's authorities against contagion.

The three following statements are now for the first time given to the public. All of the cases referred to occurred within this State, and two of the three series in Boston and its immediate vicinity.

I. The first is a series of cases which took place during the last spring in a town at some distance from this neighborhood. A physician of that town, Dr. C., had the following consecutive cases.

No. 1, delivered March 20, died March 24.
2, " April 9, " April 14.
3, " " 10, " " 14.
4, " " 11, " " 18.
5, " " 27, " May 3.
6, " " 28, had some symptoms, recovered.
7, " May 8, " " " also recovered.

These were the only cases attended by this physician during the period referred to. 'They were all attended by him until their termination, with the exception of the patient No. 6, who fell into the hands of another physician on the 2d of May. (Dr. C. left town for a few days at this time.) Dr. C. attended cases immediately before and after the above-named periods, none of which, however, presented any peculiar symptoms of the disease.'

About the first of July, he attended another patient in a neighboring village, who died two or three days after delivery.

The first patient, it is stated, was delivered on the 20th of March. 'On the 19th, Dr. C. made the autopsy of a man who died suddenly, sick only forty-eight hours; had œdema of the thigh, and gangrene extending from a little above the ankle into the cavity of the abdomen.' Dr. C. wounded himself, very slightly, in the right hand during the autopsy. The hand was quite painful the night following, during his attendance on the patient No. 1. He did not see this patient after the 20th, be-

ing confined to the house, and very sick from the wound just mentioned, from this time until the third of April.

Several cases of erysipelas occurred in the house where the autopsy mentioned above took place, soon after the examination. There were also many cases of erysipelas in town at the time of the fatal puerperal cases which have been mentioned.

The nurse who laid out the body of the patient No. 3, was taken on the evening of the same day with sore throat and erysipelas, and died in ten days from the first attack.

The nurse who laid out the body of the patient No. 4, was taken on the day following with symptoms like those of this patient, and died in a week, without any external marks of erysipelas.

'No other cases of similar character with those of Dr. C. occurred in the practice of any of the physicians in the town or vicinity at the time. Deaths following confinement have occurred in the practice of other physicians during the past year, but they were not cases of puerperal fever. No post-mortem examinations were held in any of these puerperal cases.'

Some additional statements in this letter are deserving of insertion.

'A physician attended a woman in the immediate neighborhood of the cases numbered 2, 3 and 4. This patient was confined the morning of March 1st, and died on the night of March 7th. It is doubtful whether this should be considered a case of puerperal fever. She had suffered from canker, indigestion and diarrhœa for a year previous to her delivery. Her complaints were much aggravated for two or three months previous to delivery; she had become greatly emaciated, and weakened to such an extent, that it had not been expected that she would long survive her confinement, if indeed she reached that period. Her labor was easy enough; she flowed a good deal, seemed exceedingly prostrated, had ringing in the ears, and other symptoms of exhaustion; the pulse was quick and small. On the second and third day there was some tenderness and tumefaction of the abdomen, which increased some-

what on the fourth and fifth. He had cases in midwifery before and after this, which presented nothing peculiar.'

It is also mentioned in the same letter, that another physician had a case which happened last summer, and another last fall, both of which recovered.

Another gentleman reports a case last December, a second case five weeks, and another three weeks since. All these recovered. A case also occurred very recently in the practice of a physician in the village where the eighth patient of Dr. C. resides, which proved fatal. 'This patient had some patches of erysipelas on the legs and arms. The same physician has delivered three cases since, which have all done well. There have been no other cases in this town or its vicinity recently. There have been some few cases of erysipelas.' It deserves notice that the partner of Dr. C., who attended the autopsy of the man above-mentioned and took an active part in it; who also suffered very slightly from a prick under the thumb-nail received during the examination, had twelve cases of midwifery between March 26th and April 12th, all of which did well, and presented no peculiar symptoms. It should also be stated, that during these seventeen days he was in attendance on all the cases of erysipelas in the house where the autopsy had been performed.

I owe these facts to the prompt kindness of a gentleman whose intelligence and character are sufficient guaranty for their accuracy.

The two following letters were addressed to my friend Dr. Storer, by the gentleman in whose practice the cases of puerperal fever occurred. His name renders it unnecessary to refer more particularly to these gentlemen, who on their part have manifested the most perfect freedom and courtesy in affording these accounts of their painful experience.

Jan. 28, 1843.

II. . . . 'The time to which you allude was in 1830. The first case was in February, during a very cold time. She was confined the 4th, and died the 12th. Between the 10th and

28th of this month, I attended six women in labor, all of whom did well except the last, as also two who were confined March 1st and 5th. Mrs. E., confined Feb. 28th, sickened, and died March 8th. The next day, 9th, I inspected the body, and the night after attended a lady, Mrs. B., who sickened, and died 16th. The 10th, I attended another, Mrs. G., who sickened, but recovered. March 16th, I went from Mrs. G.'s room to attend a Mrs. H., who sickened, and died 21st. The 17th, I inspected Mrs. B. On the 19th, I went directly from Mrs. H.'s room to attend another lady, Mrs. G., who also sickened, and died 22d. While Mrs. B. was sick, on 15th, I went directly from her room a few rods, and attended another woman, who was not sick. Up to 20th of this month I wore the same clothes. I now refused to attend any labor, and did not till April 21st, when, having thoroughly cleansed myself, I resumed my practice, and had no more puerperal fever.

'The cases were not confined to a narrow space. The two nearest were half a mile from each other, and half that distance from my residence. The others were from two to three miles apart, and nearly that distance from my residence. There were no other cases in their immediate vicinity which came to my knowledge. The general health of all the women was pretty good, and all the labors as good as common except the first. This woman, in consequence of my not arriving in season, and the child being half born at some time before I arrived, was very much exposed to the cold at the time of confinement, and afterwards, being confined in a very open cold room. Of the six cases you perceive only one recovered.

'In the winter of 1817 two of my patients had puerperal fever, one very badly, the other not so badly. Both recovered. One other had swelled leg, or phlegmasia dolens, and one or two others did not recover as well as usual.

'In the summer of 1835 another disastrous period occurred in my practice. July 1st, I attended a lady in labor, who was afterwards quite ill and feverish; but at the time I did not consider her case a decided puerperal fever. On the 8th, I attended one who did well. On the 12th, one who was seriously sick. This was also an equivocal case, apparently

arising from constipation and irritation of the rectum. These women were ten miles apart and five from my residence. On 15th and 20th, two who did well. On 25th, I attended another. This was a severe labor, and followed by unequivocal puerperal fever, or peritonitis. She recovered. August 2d and 3d, in about twenty-four hours I attended four persons. Two of them did very well; one was attacked with some of the common symptoms, which however subsided in a day or two, and the other had decided puerperal fever, but recovered. This woman resided five miles from me. Up to this time I wore the same coat. All my other clothes had frequently been changed. On 6th, I attended two women, one of whom was not sick at all; but the other, Mrs. L., was afterwards taken ill. On 10th, I attended a lady, who did very well. I had previously changed all my clothes, and had no garment on which had been in a puerperal room. On 12th, I was called to Mrs. S., in labor. While she was ill, I left her to visit Mrs. L., one of the ladies who was confined on 6th. Mrs. L. had been more unwell than usual, but I had not considered her case any thing more than common till this visit. I had on a surtout at this visit, which, on my return to Mrs. S., I left in another room. Mrs. S. was delivered on 13th with forceps. These women both died of decided puerperal fever.

'While I attended these women in their fevers, I changed my clothes, and washed my hands in a solution of chloride of lime after each visit. I attended seven women in labor during this period, all of whom recovered without sickness.

'In my practice I have had several single cases of puerperal fever, some of whom have died and some have recovered. Until the year 1830, I had no suspicion that the disease could be communicated from one patient to another by a nurse or midwife; but I now think the foregoing facts strongly favor that idea. I was so much convinced of this fact, that I adopted the plan before related.

'I believe my own health was as good as usual at each of the above periods. I have no recollection to the contrary.

'I believe I have answered all your questions. I have been more particular on some points perhaps than necessary; but I

thought you could form your own opinion better than to take mine. In 1830, I wrote to Dr. Channing a more particular statement of my cases. If I have not answered your questions sufficiently, perhaps Dr. C. may have my letter to him, and you can find your answer there.' *

BOSTON, FEB. 3, 1843.

III. 'MY DEAR SIR,—I received a note from you last evening, requesting me to answer certain questions therein proposed, touching the cases of puerperal fever which came under my observation the past summer. It gives me pleasure to comply with your request, so far as it is in my power so to do, but owing to the hurry in preparing for a journey, the notes of the cases I had then taken were lost or mislaid. The principal *facts*, however, are too vivid upon my recollection to be soon forgotten. I think, therefore, that I shall be able to give you all the information you may require.

'All the cases that occurred in my practice, took place between the 7th of May and the 17th of June, 1842.

'They were not confined to any particular part of the city. The two first cases were patients residing at the south-end, the next was at the extreme north-end, one living in Sea-street and the other in Roxbury. The following is the order in which they occurred.

'Case 1. Mrs. —— was confined on the 7th of May, at 5 o'clock, P. M., after a natural labor of six hours. At 12 o'clock at night, on the 9th, (thirty-one hours after confinement,) she was taken with severe chill, previous to which she was as comfortable as women usually are under the circumstances. She died on the 10th.

'Case 2. Mrs. —— was confined on the 10th of June (four weeks after Mrs. C.,) at 11, A. M., after a natural, but somewhat severe labor of five hours. At 7 o'clock, on the morning of the 11th, she had a chill. Died on the 12th.

'Case 3. Mrs. ——, confined on the 14th of June, was

* In a letter to myself, this gentleman also stated, 'I do not recollect that there was any erysipelas or any other disease particularly prevalent at the time.'

comfortable until the 18th, when symptoms of puerperal fever were manifest. She died on the 20th.

'Case 4. Mrs. ——, confined June 17th, at 5 o'clock, A. M., was doing well until the morning of the 19th. She died on the evening of the 21st.

'Case 5. Mrs. —— was confined with her *fifth* child on the 17th of June, at 6 o'clock in the evening. This patient had been attacked with puerperal fever, at three of her previous confinements, but the disease yielded to depletion and other remedies without difficulty. This time, I regret to say, I was not so fortunate. She was not attacked, as were the other patients, with a chill, but complained of extreme pain in abdomen, and tenderness on pressure, almost from the moment of her confinement. In this, as in the other cases, the disease resisted all remedies, and she died in great distress on the 22d of the same month. Owing to the extreme heat of the season, and my own indisposition, none of the subjects were examined after death. Dr. Channing, who was in attendance with me on the three last cases, proposed to have a *post-mortem* examination of the subject of case No. 5, but from some cause which I do not now recollect, it was not obtained.

'You wish to know whether I wore the same clothes when attending the different cases. I cannot positively say, but I should think I did not, as the weather became warmer after the first two cases; I therefore think it probable that I made a change of at least a *part* of my dress. I have had no other case of puerperal fever in my own practice for three years, save those above related, and I do not remember to have lost a patient before with this disease. While absent, last July, I visited two patients sick with puerperal fever, with a friend of mine in the country. Both of them recovered.

'The cases that I have recorded, were not confined to any particular constitution or temperament, but it seized upon the strong and the weak, the old and the young — one being over forty years, and youngest under eighteen years of age. . . . . If the disease is of an erysipelatous nature, as many suppose, contagionists may perhaps find some ground for their belief in

the fact that for two weeks previous to my first case of puerperal fever, I had been attending a severe case of erysipelas, and the infection may have been conveyed through me to the patient; but on the other hand, why is not this the case with other physicians, or with the same physician at all times, for since my return from the country I have had a more inveterate case of erysipelas than ever before, and no difficulty whatever has attended any of my midwifery cases?'

I am assured, on unquestionable authority, that 'About three years since, a gentleman in extensive midwifery business, in a neighboring State, lost in the course of a few weeks eight patients in child-bed, seven of them being undoubted cases of puerperal fever. No other physician of the town lost a single patient of this disease during the same period.' And from what I have heard in conversation with some of our most experienced practitioners, I am inclined to think many cases of the kind might be brought to light by extensive inquiry.

This long catalogue of melancholy histories assumes a still darker aspect when we remember how kindly nature deals with the parturient female, when she is not immersed in the virulent atmosphere of an impure lying-in hospital, or poisoned in her chamber by the unsuspected breath of contagion. From all causes together, not more than four deaths in a thousand births and miscarriages, happened in England and Wales during the period embraced by the first Report of the Registrar-General.* In the second Report the mortality was shown to be about five in one thousand.† In the Dublin Lying-in Hospital, during the seven years of Dr. Collins's mastership, there was one case of puerperal fever to 178 deliveries, or less than six to the thousand, and one death from this disease in 278 cases, or between three and four to the thousand.‡ Yet

* 1st Report, p. 105.

† 2d Report, p. 73.

‡ Collins's Midwifery, p. 228, etc.

during this period the disease was endemic in the hospital, and might have gone on to rival the horrors of the pestilence of the Maternité, had not the poison been destroyed by a thorough purification.

In private practice, leaving out of view the cases that are to be ascribed to the self-acting system of propagation, it would seem that the disease must be far from common. Mr. White, of Manchester, says, 'Out of the whole number of lying-in patients whom I have delivered, (and I may safely call it a great one,) I have never lost one, nor to the best of my recollection, has one been greatly endangered, by the puerperal, miliary, low nervous, putrid malignant, or milk fever.'* Dr. Joseph Clarke informed Dr. Collins, that in the course of *forty-five* years' most extensive practice, he lost but *four* patients from this disease.† One of the most eminent practitioners of Glasgow, who has been engaged in very extensive practice for upwards of a quarter of a century, testifies that he never saw more than twelve cases of real puerperal fever.‡

I have myself been told by two gentlemen practising in this city, and having for many years a large midwifery business, that they had neither of them lost a patient from this disease, and by one of them that he had only seen it in consultation with other physicians. In five hundred cases of midwifery, of which Dr. Storer has given an abstract in the first number of this Journal, there was only one instance of fatal puerperal peritonitis.

In the view of these facts, it does appear a singular coincidence, that one man or woman should have ten, twenty, thirty, or seventy cases of this rare disease, following their footsteps with the keenness of a beagle, through the streets and lanes of a crowded city, while the scores that cross the same paths on the same errands know it only by name. It is a series of similar coincidences that has led us to consider the dagger, the musket, and certain innocent looking white pow-

* Op. cit. p. 115.

† Collins's Treatise on Midwifery, p. 228.

‡ Lancet, May 4, 1833.

ders, as having some little claim to be regarded as dangerous. It is the practical inattention to similar coincidences that has given rise to the unpleasant but often necessary documents called *indictments*, that has sharpened a form of the cephalotome sometimes employed in the case of adults, and adjusted that modification of the fillet which delivers the world of those who happen to be too much in the way while such striking coincidences are taking place.

I shall now mention a few instances in which the disease appears to have been conveyed by the process of direct inoculation.

Dr. Campbell, of Edinburgh, states that in October, 1821, he assisted at the *post-mortem* examination of a patient who died with puerperal fever. He carried the pelvic viscera in his pocket to the class room. The same evening he attended a woman in labor without previously changing his clothes; this patient died. The next morning he delivered a woman with the forceps; she died also, and of many others who were seized with the disease within a few weeks, three shared the same fate in succession.

In June, 1823, he assisted some of his pupils at the autopsy of a case of puerperal fever. He was unable to wash his hands with proper care, for want of the necessary accommodations. On getting home he found two patients required his assistance. He went without further ablution, or changing his clothes; both these patients died with puerperal fever.* This same Dr. Campbell is one of Dr. Churchill's authorities against contagion.

Mr. Roberton says that in one instance within his knowledge, a practitioner passed the catheter for a patient with puerperal fever late in the evening; the same night he attended a lady who had the symptoms of the disease on the second day. In another instance a surgeon was called while in the act of inspecting the body of a woman who had died of this fever, to attend a labor; within forty-eight hours this patient was seized with the fever.†

* London Med. Gaz. Dec. 10th, 1831. † Ibid. for Jan. 1832.

On the 16th of March, 1831, a medical practitioner examined the body of a woman who had died a few days after delivery, from puerperal peritonitis. On the evening of the 17th he delivered a patient who was seized with puerperal fever on the 19th, and died on the 24th. Between this period and the 6th of April, the same practitioner attended two other patients, both of whom were attacked with the same disease and died.*

In the autumn of 1829, a physician was present at the examination of a case of puerperal fever, dissected out the organs, and assisted in sewing up the body. He had scarcely reached home when he was summoned to attend a young lady in labor. In sixteen hours she was attacked with the symptoms of puerperal fever, and narrowly escaped with her life.†

In December, 1830, a midwife who had attended two fatal cases of puerperal fever at the British Lying-in Hospital, examined a patient who had just been admitted, to ascertain if labor had commenced. This patient remained two days in the expectation that labor would come on, when she returned home and was then suddenly taken in labor, and delivered before she could set out for the hospital. She went on favorably for two days, and was then taken with puerperal fever and died in thirty-six hours.‡

'A young practitioner, contrary to advice, examined the body of a patient who had died from puerperal fever; there was no epidemic at the time; the case appeared to be purely sporadic. He delivered three other women shortly afterwards; they all died with puerperal fever, the symptoms of which broke out very soon after labor. The patients of his colleague did well, except one, where he assisted to remove some coagula from the uterus; she was attacked in the same manner as those whom he had attended, and died also.' The writer in the British and Foreign Medical Review, from whom I quote this statement — and who is no other than Dr. Rigby,

* London Cyc. of Pract. Med., Art. Fever, Puerperal.
† Ibid. ‡ Ibid.

— adds, 'We trust that this fact alone will forever silence such doubts, and stamp the well-merited epithet of "criminal," as above quoted, upon such attempts.'*

From the cases given by Mr. Ingleby, I select the following. Two gentlemen, after having been engaged in conducting the *post-mortem* examination of a case of puerperal fever, went in the same dress, each respectively, to a case of midwifery. 'The one patient was seized with the rigor about thirty hours afterwards. The other patient was seized with a rigor the third morning after delivery. *One recovered, one died.*'† One of these same gentlemen attended another woman in the same clothes two days after the autopsy referred to. 'The rigor did not take place until the evening of the fifth day from the first visit. *Result fatal.*' These cases belonged to a series of seven, the first of which was thought to have originated in a case of erysipelas. 'Several cases of a mild character followed the foregoing seven, and their nature being now most unequivocal, my friend declined visiting all midwifery cases for a time, and there was no recurrence of the disease.' These cases occurred in 1833. Five of them proved fatal. Mr. Ingleby gives another series of seven cases which occurred to a practitioner in 1836, the first of which was also attributed to his having opened several erysipelatous abscesses a short time previously.

I need not refer to the case lately read before this Society, in which a physician went, soon after performing an autopsy of a case of puerperal fever, to a woman in labor, who was seized with the same disease and perished. The forfeit of that error has been already paid.

At a meeting of the Medical and Chirurgical Society before referred to, Dr. Merriman related an instance occurring in his own practice, which excites a reasonable suspicion that two lives were sacrificed to a still less dangerous experiment. He was at the examination of a case of puerperal fever at two o'clock in the afternoon. *He took care not to touch the body.*

* Brit. and For. Medical Review, for Jan. 1842, p. 112.

† Edin. Med. and Surg. Journal, April, 1838.

At 9 o'clock the same evening he attended a woman in labor; she was so nearly delivered that he had scarcely any thing to do. The next morning she had severe rigors, and in forty-eight hours she was a corpse. Her infant had erysipelas and died in two days.*

In connection with the facts which have been stated, it seems proper to allude to the dangerous and often fatal effects which have followed from wounds received in the post-mortem examination of patients who have died of puerperal fever. The fact that such wounds are attended with peculiar risk has been long noticed. I find that Chaussier was in the habit of cautioning his students against the danger to which they were exposed in these dissections.† The head *pharmacien* of the Hotel Dieu, in his analysis of the fluid effused in puerperal peritonitis, says that practitioners are convinced of its deleterious qualities, and that it is very dangerous to apply it to the denuded skin.‡ Sir Benjamin Brodie speaks of it as being well known that the inoculation of lymph or pus from the peritoneum of a puerperal patient is often attended with dangerous and even fatal symptoms. Three cases in confirmation of this statement, two of them fatal, have been reported to this Society within a few months.

Of about fifty cases of injuries of this kind, of various degrees of severity, which I have collected from different sources, at least twelve were instances of infection from puerperal peritonitis. Some of the others are so stated as to render it probable that they may have been of the same nature. Five other cases were of peritoneal inflammation: three in males. Three were of what was called enteritis, in one instance complicated with erysipelas; but it is well known that this term has been often used to signify inflammation of the peritoneum covering the intestines. On the other hand, no case of typhus or typhoid fever is mentioned as giving rise to dangerous consequences, with the exception of the single instance of an undertaker mentioned by Mr. Travers, who seems to have been

* Lancet, May 2d, 1840.

† Stein, l'Art de Accoucher, 1794. Dict. des Sciences Médicales, Art. Puerperal.

‡ Journal de Pharmacie, Jan. 1836.

poisoned by a fluid which exuded from the body. The other accidents were produced by dissection, or some other mode of contact with bodies of patients who had died of various affections. They also differed much in severity, the cases of puerperal origin being among the most formidable and fatal. Now a moment's reflection will show that the number of cases of serious consequences ensuing from the dissection of the bodies of those who had perished of puerperal fever, is so vastly disproportioned to the relatively small number of autopsies made in this complaint as compared with typhus, or pneumonia, (from which last disease not one case of poisoning happened,) and still more from all diseases put together, that the conclusion is irresistible that a most fearful morbid poison is often generated in the course of this disease. Whether or not it is *sui generis*, confined to this disease, or produced in some others, as for instance erysipelas, I need not stop to inquire.

In connection with this may be taken the following statement of Dr. Rigby. 'That the discharges from a patient under puerperal fever are in the highest degree contagious, we have abundant evidence in the history of lying-in hospitals. The puerperal abscesses are also contagious, and may be communicated to healthy lying-in women by washing with the same sponge; this fact has been repeatedly proved in the Vienna Hospital; but they are equally communicable to women not pregnant; on more than one occasion the women engaged in washing the soiled bed-linen of the General Lying-in Hospital have been attacked with abscess in the fingers or hands, attended with rapidly spreading inflammation of the cellular tissue.' *

Now add to all this the undisputed fact, that within the walls of lying-in hospitals there is often generated a miasm, palpable as the chlorine used to destroy it, tenacious so as in some cases almost to defy extirpation, deadly in some institutions as the plague; which has killed women in a private hospital of London so fast that they were buried two in one coffin to conceal its horrors; which enabled Tonnellé to record two hundred and

* System of Midwifery, p. 292.

twenty-two autopsies at the Maternité of Paris; which has led Dr. Lee to express his deliberate conviction that the loss of life occasioned by these institutions completely defeats the objects of their founders; and out of this train of cumulative evidence, the multiplied groups of cases clustering about individuals, the deadly results of autopsies, the inoculation by fluids from the living patient, the murderous poison of hospitals, — does there not result a conclusion that laughs all sophistry to scorn, and renders all argument an insult?

I have had occasion to mention some instances in which there was an apparent relation between puerperal fever and erysipelas. The length to which this paper has extended does not allow me to enter into the consideration of this most important subject. I will only say, that the evidence appears to me altogether satisfactory that some most fatal series of puerperal fever have been produced by an infection originating in the matter or effluvia of erysipelas. In evidence of some connection between the two diseases, I need not go back to the older authors, as Pouteau or Gordon, but will content myself with giving the following references, with their dates; from which it will be seen that the testimony has been constantly coming before the profession for the last few years.

London Cyclopedia of Practical Medicine — article Puerperal Fever. 1833.

Mr. Ceeley's Account of the Puerperal Fever at Aylesbury, Lancet, 1835.

Dr. Ramsbotham's Lecture. London Medical Gazette, 1835.

Mr. Yates Ackerly's Letter in the same Journal, 1838.

Mr. Ingleby on Epidemic Puerperal Fever. Edinburgh Medical and Surgical Journal, 1838.

Mr. Paley's Letter. London Medical Gazette, 1839.

Remarks at the Medical and Chirurgical Society. Lancet. 1840.

Dr. Rigby's System of Midwifery. 1841.

Nunneley on Erysipelas — a work which contains a large number of references on the subject. 1841.

British and Foreign Quarterly Review, 1842.

Dr. S. Jackson, of Northumberland, as already quoted from the Summary of the College of Physicians, 1842.

And lastly, a startling series of cases by Mr. Storrs, of Doncaster, to be found in the American Journal of the Medical Sciences for January, 1843.

The relation of puerperal fever with other continued fevers, would seem to be remote and rarely obvious. Hey refers to two cases of synochus occurring in the Royal Infirmary of Edinburgh, in women who had attended upon puerperal patients. Dr. Collins refers to several instances in which puerperal fever has appeared to originate from a continued proximity to patients suffering with typhus.*

Such occurrences as those just mentioned, though most important to be remembered and guarded against, hardly attract our notice in the midst of the gloomy facts by which they are surrounded. Of these facts, at the risk of fatiguing repetitions, I have summoned a sufficient number, as I believe, to convince the most incredulous, that every attempt to disguise the truth which underlies them all, is useless.

It is true that some of the historians of the disease, especially Hulme, Hull and Leake, in England; Tonnellé, Dugès and Baudelocque, in France, profess not to have found puerperal fever contagious. At the most they give us mere negative facts, worthless against an extent of evidence which now overlaps the widest range of doubt, and doubles upon itself in the redundancy of superfluous demonstration. Examined in detail, this and much of the show of testimony brought up to stare the daylight of conviction out of countenance, proves to be in a great measure unmeaning and inapplicable, as might be easily shown were it necessary. Nor do I feel the necessity of enforcing the conclusion which arises spontaneously from the facts which have been enumerated, by formally citing the opinions of those grave authorities who have for the last half century been sounding the unwelcome truth it has cost so many lives to establish.

* Treatise on Midwifery, p. 228.

'It is to the British practitioner,' says Dr. Rigby, 'that we are indebted for strongly insisting upon this important and dangerous character of puerperal fever.' *

The names of Gordon, John Clarke, Denman, Burns, Young,† Hamilton,‡ Haighton,§ Good,‖ Waller,¶ Blundell, Gooch, Ramsbotham, Douglas,** Lee, Ingleby, Locock,†† Abercrombie,‡‡ Alison,§§ Travers,‖‖ Rigby, and Watson,¶¶ many of whose writings I have already referred to, may have some influence with those who prefer the weight of authorities to the simple deductions of their own reason from the facts laid before them. A few continental writers have adopted similar conclusions.*** It gives me pleasure to remember, that while the doctrine has been unceremoniously discredited in one of the leading Journals,††† and made very light of by teachers in two of the principal Medical Schools of this country, Dr. Channing has for many years inculcated and enforced by examples, the danger to be apprehended and the precautions to be taken in the disease under consideration.

I have no wish to express any harsh feeling with regard to the painful subject that has come before us. If there are any so far excited by the story of these dreadful events, that they ask for some word of indignant remonstrance, to show that science does not turn the hearts of its followers into ice or stone, let me remind them that such words have been uttered

* British and Foreign Med. Review for Jan. 1842.

† Encyc. Britannica, xiii. 467, Art. Medicine.

‡ Outlines of Midwifery, p. 109.

§ Oral Lectures, &c.

‖ Study of Medicine, ii. 195.

¶ Medical and Physical Journal, July, 1830.

** Dublin Hospital Reports, for 1822.

†† Library of Practical Medicine, i. 373.

‡‡ Researches on Diseases of the Stomach, &c., p. 181.

§§ Library of Practical Medicine, vol. i. p. 96.

‖‖ Further Researches on Constitutional Irritation, p. 128.

¶¶ Lond. Medical Gazette, Feb. 1842.

*** See British and Foreign Medical Review, vol. iii. p. 525, and vol. iv. p. 517. Also, Ed. Med. and Surg. Journal for July, 1824, and American Journal of Med. Sciences for Jan. 1841.

††† Phil. Med. Journal, vol. xii. p. 364.

by those who speak with an authority I could not claim.* It is as a lesson rather than as a reproach that I call up the memory of these irreparable errors and wrongs. No tongue can tell the heart-breaking calamity they have caused; they have closed the eyes just opened upon a new world of love and happiness; they have bowed the strength of manhood into the dust; they have cast the helplessness of infancy into the stranger's arms, or bequeathed it, with less cruelty, the death of its dying parent. There is no tone deep enough for regret, and no voice loud enough for warning. The woman about to become a mother, or with her new-born infant upon her bosom, should be the object of trembling care and sympathy wherever she bears her tender burden, or stretches her aching limbs. The very outcast of the streets has pity upon her sister in degradation, when the seal of promised maternity is impressed upon her. The remorseless vengeance of the law, brought down upon its victim by a machinery as sure as destiny, is arrested in its fall at a word which reveals her transient claim for mercy. The solemn prayer of the liturgy singles out her sorrows from the multiplied trials of life, to plead for her in the hour of peril. God forbid that any member of the profession to which she trusts her life, doubly precious at that eventful period, should hazard it negligently, unadvisedly, or selfishly!

There may be some among those whom I address, who are disposed to ask the question, What course are we to follow in relation to this matter? The facts are before them, and the answer must be left to their own judgment and conscience. If any should care to know my own conclusions, they are the following; and in taking the liberty to state them very freely and broadly, I would ask the inquirer to examine them as freely in the light of the evidence which has been laid before him.

1. A physician holding himself in readiness to attend cases of midwifery, should never take any active part in the post-mortem examination of cases of puerperal fever.

* Dr. Blundell and Dr. Rigby in the works already cited.

2. If a physician is present at such autopsies, he should use thorough ablution, change every article of dress, and allow twenty-four hours or more to elapse before attending to any case of midwifery. It may be well to extend the same caution to cases of simple peritonitis.

3. Similar precautions should be taken after the autopsy or surgical treatment of cases of erysipelas, if the physician is obliged to unite such offices with his obstetrical duties, which is in the highest degree inexpedient.

4. On the occurrence of a single case of puerperal fever in his practice, the physician is bound to consider the next female he attends in labor, unless some weeks, at least, have elapsed, as in danger of being infected by him, and it is his duty to take every precaution to diminish her risk of disease and death.

5. If within a short period two cases of puerperal fever happen close to each other, in the practice of the same physician, the disease not existing or prevailing in the neighborhood, he would do wisely to relinquish his obstetrical practice for at least one month, and endeavor to free himself by every available means from any noxious influence he may carry about with him.

6. The occurrence of three or more closely connected cases, in the practice of one individual, no others existing in the neighborhood, and no other sufficient cause being alleged for the coincidence, is *primâ facie* evidence that he is the vehicle of contagion.

7. It is the duty of the physician to take every precaution that the disease shall not be introduced by nurses or other assistants, by making proper inquiries concerning them, and giving timely warning of every suspected source of danger.

8. Whatever indulgence may be granted to those who have heretofore been the ignorant causes of so much misery, the time has come when the existence of a *private pestilence* in the sphere of a single physician should be looked upon not as a misfortune but a crime; and in the knowledge of such occurrences, the duties of the practitioner to his profession, should give way to his paramount obligations to society.

ADDITIONAL REFERENCES AND CASES.

Fifth Annual Report of the Registrar-General of England. London, 1843. Appendix. Letter from William Farr, Esq. — Several new series of cases are given in the Letter of Mr. Storrs, contained in the Appendix to this Report. Mr. Storrs suggests precautions similar to those I have laid down, and these precautions are strongly enforced by Mr. Farr, who is, therefore, entitled to the same epithet as myself.

Hall and Dexter, in Am. Journal of Med. Sc. for January, 1844. — Cases of puerperal fever seeming to originate in erysipelas.

Elkington, of Birmingham, in Provincial Med. Journal, cited in Am. Journ. Med. Sc. for April, 1844. — Six cases in less than a fortnight, seeming to originate in a case of erysipelas.

West's Reports, in Brit. and For. Med. Review for October, 1845, and January, 1847. — Affection of the arm, resembling malignant pustule, after removing the placenta of a patient who died from puerperal fever. Reference to cases at Würzburg, as proving contagion, and to Keiller's cases in the Monthly Journal for February, 1846, as showing connection of puerperal fever and erysipelas.

Kneeland. — Contagiousness of Puerperal Fever. Am. Jour. Med. Sc., January, 1846. Also, Connection between Puerperal Fever and Epidemic Erysipelas. Ibid. April, 1846.

Robert Storrs. — Contagious Effects of Puerperal Fever on the Male Subject; or on Persons not Child-bearing, (from Provincial Med. and Surg. Journal.) Am. Jour. Med. Sc., January, 1846. Numerous cases. See also Dr. Reid's case in same Journal for April, 1846.

Routh's paper in Proc. of Royal Med. Chir. Soc., Am. Jour. Med. Sc., April, 1849, also in B. and F. Med. Chir. Review, April, 1850.

Hill, of Leuchars. — A Series of Cases Illustrating the Contagious Nature of Erysipelas and of Puerperal Fever, and their Intimate Pathological Connection. (From Monthly Journal of Med. Sc.) Am. Jour. Med. Sc., July, 1850.

Skoda on the Causes of Puerperal Fever. (Peritonitis in rabbits, from inoculation with different morbid secretions.) Am. Jour. Med. Sc., October, 1850.

Arneth. — Paper read before the National Academy of Medicine. Annales d'Hygiène, tome lxv. 2e Partie. (Means of Disinfection proposed by M. 'Semmeliveis' (Semmelweiss). Lotions of chloride of lime and use of nail-brush before admission to lying-in wards. Alleged sudden and great decrease of mortality from puerperal fever. Cause of disease attributed to inoculation with cadaveric matters.) See also *Routh's* paper, mentioned above.

Moir. — Remarks at a Meeting of the Edinburgh Medico-Chirurgical Society. Refers to cases of Dr. Kellie, of Leith. *Sixteen* in succession, *all fatal*. Also to

several instances of individual pupils having had a succession of cases in various quarters of the town, while others, practising as extensively in the same localities, had none. Also to several special cases not mentioned elsewhere. Am. Jour. Med. Sc. for October, 1851. (From New Monthly Journal of Med. Science.)

Simpson.— Observations at a Meeting of the Edinburgh Obstetrical Society. (An 'eminent gentleman,' according to Dr. Meigs, whose 'name is as well known in America as in (his) native land.' Obstetrics. Phil. 1852. pp. 368, 375.) The student is referred to this paper for a valuable *resumé* of many of the facts, and the necessary inferences, relating to this subject. Also for another series of cases, Mr. Sidey's, five or six in rapid succession. Dr. Simpson attended the dissection of two of Dr. Sidey's cases, and freely handled the diseased parts. His next four cases of midwifery were affected with puerperal fever, and it was the first time he had seen it in practice. As Dr. Simpson is *a gentleman* (Dr. Meigs, as above,) and as 'a gentleman's hands are clean,' (Dr. Meigs's Sixth Letter,) it follows that a gentleman with clean hands may carry the disease. Am. Jour. Med. Sc., Oct. 1851.

Peddie. — The five or six cases of Dr. Sidey, followed by the four of Dr. Simpson, did not end the series. A practitioner in Leith having examined, in Dr. Simpson's house, a portion of the uterus obtained from one of the patients, had immediately afterwards three fatal cases of puerperal fever. Dr. Peddie referred to two distinct series of consecutive cases in his own practice. He had since taken precautions, and not met with any such cases. Am. Jour. Med. Sc., Oct. 1851.

Copland. — Considers it proved that Puerperal Fever may be propagated by the hands and the clothes, or either, of a third person, the bed clothes or body clothes of a patient. Mentions a new series of cases, one of which he saw, with the practitioner who had attended them. She was *the sixth* he had had within a few days. *All died.* Dr. Copland insisted that contagion had caused these cases; advised precautionary measures, and the practitioner had no other cases for a considerable time. Considers it *criminal*, after the evidence adduced, — which he could have quadrupled, — and the weight of authority brought forward, for a practitioner to be the medium of transmitting contagion and death to his patients. Dr. Copland lays down rules similar to those suggested by myself, and is therefore entitled to the same epithet for so doing. Medical Dictionary, New York, 1852. Article, Puerperal States and Diseases.

If there is any appetite for facts so craving as to be yet unappeased, — *lassata, necdum satiata*, — more can be obtained. Dr. Hodge remarks that 'the frequency and importance of this singular circumstance (that the disease is occasionally more prevalent with one practitioner than another,) has been exceedingly overrated.' More than thirty strings of cases, more than two hundred and fifty sufferers from puerperal fever, more than one hundred and thirty deaths appear as the results of a sparing estimate of such among the facts I have gleaned as could be numerically valued. These facts constitute, we may take it for granted, but a small fraction of those that have actually occurred. The number of them might be greater, but ''t is enough, 't will serve,' in Mercutio's modest phrase, so far as frequency is concerned. For a just estimate of the importance of the singular circumstance, it might be proper to consult the languid survivors, the widowed husbands, and the motherless children, as well as 'the unfortunate accoucheur.'