

Address to the Medical Society of London : together with some correspondence relative to a consultation at Norwich.

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Royal College of Surgeons of England

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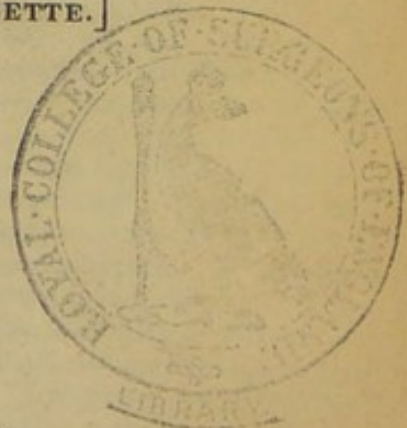


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ADDRESS
TO THE
MEDICAL SOCIETY OF LONDON:
TOGETHER WITH SOME
CORRESPONDENCE
RELATIVE TO A
CONSULTATION AT NORWICH.

[FROM THE LONDON MEDICAL GAZETTE.]



C LONDON:

PRINTED BY WILSON AND OGILVY,
57, SKINNER STREET, SNOWHILL.

A D D R E S S.

GENTLEMEN,—A very short time before the commencement of the present session I did not suppose that I should have to address you this evening. I have been reminded, however, that my respected predecessor, your late President, had opened the previous session with an interesting paper, and that I should follow so good an example. I have undertaken, therefore, at a very short notice, to bring forward a question of practical interest, trusting to your indulgence for the incomplete form that I am obliged now to submit it to your attention.

Before entering, however, upon the Duration of Pregnancy,—the subject that I propose for your consideration,—permit me very briefly to advert to one or two topics of interest to the Society. And first, I may congratulate you on the prospect that the difficulties which are in our way are likely to be removed; the clouds that threatened our financial horizon are fast disappearing; the appeal which has been made has been warmly responded to by the Fellows of this Society, and, in obedience to its resolutions, several contributions have been sent in, others are on their way, to the Society's exchequer; and it is satisfactory to add that there have been only eighteen dissentients among the whole number of Fellows.

I have also been informed that your property in Bolt Court has been let to an eligible tenant; so that the prosperity of this ancient Society, and the success of those measures which have been adopted to increase its strength and to concentrate the intellectual power of our profession, will not be impeded in their progress.

Another subject has also occupied the attention of your Council, one which, in common with the profession generally, they have looked upon with much anxiety. They have felt it their duty to submit for your approval certain resolutions which they deem essential to arrest an evil which threatens to attain a dangerous magnitude. They have proposed, and you have agreed to, a decided and public expression of opinion respecting homœopathy. You have agreed, also, on the course which this

Society expects from its Fellows when, in ordinary professional intercourse, they are brought into contact with the professors of this new mode of practice.

Under any other circumstances I am persuaded that it would be a work of supererogation to assure you that in all the sentiments these resolutions have expressed I fully agree; that I shall act upon them in their full extent; and, while I have the honour to occupy this chair, I shall endeavour to render them as efficient as it is in my power. It has happened, however, that my name has been brought before the profession, and of course before you, in a manner that I could not have supposed. I feel it, therefore, a duty towards you, when I find myself much misrepresented, to correct at least these mistakes, and, holding the position with which you have honoured me, to give you every explanation of my views. I am sure, therefore, you will pardon a slight digression respecting certain charges that have been made against me, of which, if I were guilty, I should resign this chair.

I have been charged, in the first place, with meeting a homœopath in consultation. To that charge I have already replied, and I now again repeat, in the terms of your resolution, I never have *knowingly* met a homœopath in consultation; and I may further add I never will, and that for the simplest of all reasons—viz. that a consultation is impossible between two holding principles of practice diametrically opposed. I have been asked to meet them: I have invariably refused. To another, and to me equally serious charge, I have not as yet given any public reply. It is stated that I have given, in a certificate, an opinion as to the cause of a disease which could not be true; that, if true, my opinion would have the effect of injuring the reputation of a most respectable member of our profession, and that I am bound to retract the words I have used. In reply to this charge, I beg to state that my opinion has been most completely misunderstood and misrepresented. I am now in correspondence with a gentleman upon that question, and I trust to have it in my power to place

before the profession, and before you, what my opinion really was as to the cause of that disease. I beg, also, distinctly to say that it giving that opinion, I never intended that it should be interpreted injuriously to the gentleman alluded to. It could only be made to do so by straining it from its proper meaning; and I am sorry to add that, although I would most willingly have given that gentleman any explanation of my certificate necessary to exonerate him from such charges, and me from such misrepresentation, the tone of his correspondence impressed me with the conviction that he did not desire anything of the kind; on the contrary, the same spirit that influenced the accusation that I met a homœopath in consultation seemed to prompt the desire of proving that I had given an untenable opinion, and of course a false certificate. From the correspondence which I have in my possession, I distinctly state that my opinion has been totally misrepresented; and further, that, unless by the grossest misrepresentation, it could not be made to signify any want of skill or attention, or judgment, on the part of the gentleman referred to. My opinion that a retained portion of placenta caused hydatids presumed that the placenta was morbid, that it was of necessity adherent, and that it could not be completely removed. I conjectured that the disease which rendered it so was the same as that which afterwards appeared; but in that opinion I may have erred, from ignorance of the whole of the facts of the case; some of which, I believe, were ascertained subsequently to my visit.

While I state this, I trust that the Society will discriminate between such an acknowledgment, and an admission that my opinion was under any circumstances absolutely erroneous—that it could not possibly be true. In the correspondence to which I allude I have explained what that opinion was, and I still adhere to it as being quite within the limits of truth. It was formed upon the facts which were placed before me, and if afterwards other facts were obtained which would have completely changed that opinion, I can only say I had them not. My opinion may have been correct, or it may have been incorrect, but, whether it were the one or the other, I assert that it conveyed no imputation injurious to any one's professional reputation. I feel perfectly conscious, therefore, with regard to both these charges, that there is not a shadow of foundation for either of them.

I trust the Society will pardon this allusion to a merely personal matter. I should not have thought of trespassing upon them, did I not feel it a duty to remove from their minds, as far as it is in my power

to do, the erroneous impressions that have been in circulation. With regard to homœopathy itself, I have the same opinion of it now that I always held: I believe that the professed homœopathist, like the professed mesmerist, the professed hydropathist, pursues this practice with the same object and on the same principle as the professed charlatan who sells his cures under a thousand disguises. With such men, homœopathy becomes a form of quackery the more dangerous because it assumes the garb of a science. The quack who sells his pills or his mixtures is comparatively a harmless person. Those of the public who have faith in what he tells them are alone deceived; they believe in his secret cure on the faith of his word or his advertisement, and if they please to act so foolishly, they do so at least with their eyes open. They are asked to trust a secret remedy, and they do so. But the charlatan who entangles them in the intricacies of a pseudo-science, who bewilders them in the subtleties of the absurd dogma, "*similia similibus curantur*," and presents to them his infinitesimal dose as the ultimate result of his scientific inquiries,—such a man is calculated to do a much greater evil just in proportion as the scientific garb he assumes is calculated to deceive even the judicious and reflecting.

When, therefore, you find the professed homœopathist surrounded by his publications, by his dogmata, his infinitesimal pharmacy, as infinitesimal in mind as in matter, in science as in atoms,—when you find such a one waging war against legitimate medicine—when, further, you find them undertaking to cure the most violent inflammations, the most dangerous fevers, and even surgical accidents, homœopathically,—I would say,—

Hic niger est, hunc tu Romane caveto.

But, while I hold such sentiments, I do feel that much caution and great discrimination should be observed in affixing such a title to a professional brother. I feel that it would lead to an abuse fully as great as homœopathy itself, if this stigma were affixed without just cause. I can well imagine that there are in the profession perfectly honest members, who think that there must be something in homœopathy worthy of consideration; that in the heap of chaff some valuable grains of wheat may be found which have not hitherto attracted observation, who are embarrassed by facts difficult to explain, and who may be disposed to order unusually smaller doses than they have hitherto been accustomed to prescribe, and consequently may look like homœopathists. I should be disposed to exercise great caution in putting such men in the same category as the former.

I do not think that such an act would in the least degree aid us in suppressing homœopathy: on the contrary, I would fear that the operation of another principle—one that acts powerfully on the human mind—would be the means of giving it unexpected support. An obstinate and dogged resistance to an act of injustice might make the mere inquirer into what homœopathy is, its warm advocate,—it might make him do what perhaps he never intended, and give the weight of his name and his influence to the abuse that he would have otherwise discountenanced. The spirit of inquiry natural to the human mind renders these men anxious to ascertain the true cause of the benefit received; and whether they adopt the belief that homœopathy is only another name for an expectant treatment—for, in fact, doing nothing—or whether they think that minute doses of medicine frequently repeated are more efficient than a full dose of the same agent,—in either case such men cannot be considered in the light of quacks: they should not be confounded with the homœopathic professors to whom I have alluded. I may err altogether in my judgment, but I cannot think it either just or prudent to hold up to public reprobation men equally educated as we have been, who have through life observed the same honourable course that we have marked out for ourselves—being in every respect worthy members of our profession—because they may happen to order an unusually small dose of medicine. I should rather say: wait until you have stronger, more decided, clearer evidence of the homœopathic dementia,—until you find an open acknowledgment of the principles of homœopathy as the foundation of their practice, and an admission that they have become converts to these novel doctrines. Then is the time for us to act with decision and effect.

I feel it the more necessary to urge upon the Society the importance of combining prudence with decision—caution with energy—because it is obvious that, in consequence of the spread of these false doctrines, and the manner in which they are supported, the profession is excited to the most intense degree. They are just in the same position with regard to medicine as other bodies of men have been in respect to religion: here is a danger that they may be blinded by their zeal, and, in their desire to suppress this heresy, may most unwittingly use a means that only gives to it strength. No doctrines, however untrue, were ever yet suppressed by persecution, or by any procedure that in the least degree approaches to it. On the contrary, if we would give to falsehood the same aid in its propagation that Providence in its wisdom

found necessary for truth, we have only to make a few martyrs, and the thing is accomplished. If we make one mistake in this respect,—if gentlemen who have hitherto maintained a high reputation are unjustly held up to public odium because they were guilty perhaps of ordering an unusually small dose of medicine,—we certainly do not convince them of their error, but we do what appears to me a great deal worse,—we give the homœopathist, the professed dogmatist, the apostle of a new faith in medicine—that is, to turn every chemist's shop into a pocket medicine-chest,—we give him a most powerful aid when we, as it were, authorise him to claim such men as their supporters.

In making these observations I wish not to be misunderstood: I do not mean to shelter the secret homœopathist, or to say that any such should not be called upon at once to declare their faith, to make their election: my object is rather to caution the Society against the abuse that might be made of its honest intentions. You know that professional character is the only property that a professional man has; deprive him of that and you deprive him of everything. If this, then, be true, it appears to me that we should be jealously alive to every attempt that is made to single out any of our professional brethren as objects for attack or accusation. We should be perfectly clear that the charge is true, and that on the clearest evidence, before we decide against him.

I think also that we should be cautious in acting upon charges that may be contained in the public journals. The accusation that is made in a public journal, whether medical or otherwise, is founded upon evidence in possession of that journal alone. The print has it in its power to bring forward just so much of the evidence as it pleases, and no more; it may tell the truth but not the whole truth, and consequently our brother may be accused, tried, and found guilty, on *ex parte* evidence alone. It may be said that the accused has the power of reply—the press is open to him, he can defend himself. This is certainly a very pleasing fiction, of which any one who ever attempted a reply to a public journal must have a pleasing consciousness. Imagine for a moment a sensitive author replying to the critiques of the *Foreign Quarterly*, or an unfortunate Irishman answering the sarcasms of the *Times*, and you may form some idea of the power of reply. The public are well aware of this, and generally appreciate correctly the castigations of the public press. It is only when these attacks are recognised by societies, whether public or private, that they become serious, and are calculated to

do mischief. While, however, I think that caution is necessary, lest we might sanction or give credence to a groundless charge, I do not complain because a journal may please to adopt such a course. The liberty of the press is sacred in this country; and although an editor may use that liberty a little too freely,—although after his own peculiar fashion he may hold a solemn court, and condemn you without a hearing,—still the injury, although great, may be pardoned for the sake of the object he has in view. The honest editor (I do not speak of those that may have sinister motives) wishes to drive the money-changers out of the temple of medicine, and, not being the fac-simile of his great Master, he lashes right and left without regarding whom he may strike. If, unhappily, the innocent are found in company with the guilty, they receive the same chastisement. I do not complain of this: we must be more careful with whom we associate: but I do feel strongly on the duty of all public bodies and public societies (who are the guardians of our profession) to act with great caution and discrimination when such charges form the basis of their proceedings.

I trust the Society will pardon these observations. I have taken the liberty to express my sentiments freely on this important subject. I should not have thought of thus trespassing upon their attention had I not a painful experience of the mischievous effect of false accusations,—did I not know the injury that must be done to the profession if its members are liable to such charges on trifling grounds,—if I did not feel sure that by these means we were giving to homœopathy an importance that it does not merit; that we were helping it to a position which I trust it may never attain,—I should not have ventured thus to address you.

When I commenced my remarks I intended them to be brief: I find, however, that they have occupied more of your time than I had anticipated. I must not therefore trespass on the space allotted to me by a lengthened discussion on the duration of pregnancy. I have another reason also for not wishing to do so at present. The duration of pregnancy is a subject which I wished to bring before the Society at a much later period, when it would be in my power to arrange the facts that are in my possession, and embody my views more perfectly than I can now do. On a future occasion I trust to be able to bring this subject again forward. At present I shall submit to the Society a very brief outline of the method that I have pursued, and its results in the attempt to resolve

this difficult problem; and I do so the more willingly because it is so completely in the power of others by adopting the same method to assist in its solution.

There are two questions to decide. Is the duration of pregnancy a fixed or a variable period? and if we decide that it is variable, what are the limits of its variation? You are aware that the most usual mode of calculating the term of pregnancy is by dating from the last appearance of the menses to the time of delivery; the gross result thus obtained is afterwards corrected by deducting as many days as is supposed necessary to avoid error as the exact time of conception. Those who believe that conception may take place at any time between two menstrual periods, generally deduct half the period, taking the middle point as the commencement of gestation. If it be 28 days they date from the 14th day after the last catamenia; so that the possible error is diminished one half. Those who assume that conception only takes place at a menstrual period date either from the last catamenia or that which should have followed only for conception. This estimate is again compared with the period of quickening, and thus a result sufficiently accurate for ordinary practical purposes is obtained. This method, however, fails in the precision necessary for legal evidence, and hence the profession have anxiously sought for another mode to determine the exact time that conception occurs. Some women are known to have peculiar sensations at the time of conception, by which they are conscious that it has taken place. Dr. Montgomery, in his valuable work on the signs of pregnancy, relates several such instances, which fix the duration at 280 days. Again, certain cases fall under the notice of those in extensive practice by which the date of conception is determined by the date of intercourse; that is, the evidence of a single intercourse being certain, the time of conception is equally certain. Sir Charles Clarke and others mention such instances, and give 280 days as the period of pregnancy. Hence, on the strength of this testimony, many are disposed to look upon the duration of pregnancy as a fixed period,—viz., 280 days. I am, however, disposed to doubt the accuracy of this assumption, because these cases form a very small number, and, if the rule were different, might easily form exceptions to that rule without in the least disturbing it. Hence it has appeared to me that statistics might be applied to the solution of the question, and for this purpose I have availed myself of the opportunities afforded me to endeavour to resolve it, or at least to aid in its resolution.

Some years since, when forming a registry of obstetric cases at University College Hospital, it occurred to me that it might be made a means of determining this question, when a sufficiently large number of cases were obtained. The following method was adopted:—When a letter for attendance was applied for, an inquiry was made as to the catamenia, the age of the applicant at its commencement, its period, and its last appearance. The applicant could have no idea of the object of these inquiries, and therefore could have no motive for deception. With regard to the last question, its cessation, some could only give the month, some were too irregular to date from, and some were precise as to the date. Those that were irregular were excluded: for instance, some who were nursing either had no change or a very irregular one; others had been always irregular before pregnancy, and therefore could not be depended on; and again, with some the catamenia had evidently continued after conception. Such cases being omitted, those that remained were noted; and when labour took place, the time of delivery was also recorded. The interval between the two dates gave what might be called the gross duration of pregnancy. The period so obtained was afterwards corrected so as to make as near an approach to accuracy as possible.

On a former occasion, when drawing up a report of the obstetric practice of University College Hospital, a table of 186 cases was formed, in which the duration of pregnancy is given. In those cases the corrections were made in the following manner:—If the period exceeded 280 days, and the woman had given the exact date when she was last unwell, an inquiry was made as to the regularity of the catamenia; but, to avoid error, the whole menstrual interval was deducted from the gross amount. For instance, if the whole duration were 328 days, and the interval of the catamenia 28 days, the last number was subtracted from the former, leaving 300 days as the true period. It was assumed that the woman might be wrong as to the exact date, and therefore it was safer to consider that conception occurred just before the catamenia which had been arrested. In the present inquiry a method slightly different has been adopted. 280 days is assumed to be the true period of pregnancy, and every case that can be made to fit is included under the table of 280 days; if it cannot, the correction is made thus:—When the month only is given, the date is taken from the last day of the month; if the precise date is given, it is assumed to be correct, and the menstrual interval being deducted, the result is presumed to be accurate. For example, if from the last

appearance of the catamenia (say April 10) to February 10, include 306 days, the true duration of pregnancy would be $306 - 28 = 278$ days; thus reducing every case of extended pregnancy to its lowest number consistently with truth.

Four tables have been formed, containing collectively 965 cases.

1st. Those cases in which the duration of pregnancy is above 280 days.

2d. Those that are exactly that period.

3d. Those between 260 and 280 days.

4th. Those below 260 days.

The first table (above 280 days) is by far the most important, and includes 303 cases.

The second (280 days)	378	„
The third (between 260 and 280 days)	201	„
	<hr/>	
	882	

The fourth (below 260 days), which must be considered premature deliveries . . . 83 „

Making altogether . . . 965 „

These tables give support to those formed some years back, in proving that the duration of pregnancy is not a fixed period, but, just as in the lower animals, it varies within certain limits. What those limits are is the important question to determine. I have taken 260 days, or 37 weeks, as the shortest period, because I have attended cases that have given birth to mature children at that period. I do not wish as yet to speak absolutely as to the longest period, because the first table that I have formed is yet under examination, and requires the corrections that I have alluded to. At some future opportunity I shall bring before the Society the results in a more perfect form. At present I shall only quote an instance of very protracted gestation, which I think may be considered as accurate.

Mary Abbot, æt. 40, living at 39, Gough Street, Gray's Inn Road, applied for a letter to the hospital, January 3, 1849. The catamenia had commenced at 14, and continued regularly every four weeks. She was pregnant of her fourth child, and the menses appeared last in the month of March 1848. She states that, whenever pregnant, “she always feels ill, the morning sickness being very bad, and a death-like feeling of faintness and sinking frequently recurring,” by which symptoms alone she has always been able to say she was pregnant. She perfectly remembers her last pregnancy, and the particulars respecting it. The menses ceased in the beginning of March, or, she says, “more properly in the last days of February.” It was not more than a month from this period that she began to be sensible of the same feelings of

faintness and sickness habitually present in former pregnancies. It was accompanied with much pain in the region of the uterus, and a sense of weight and bearing down. Having applied for advice, she was found to suffer from inflammation and ulceration of the womb, for which she was treated. The symptoms which she was accustomed to attribute to pregnancy were therefore rendered doubtful: it was left undecided, the woman still suffering the same sickness and faintness, until August, when she felt the child distinctly move. At the same moment she felt very faint, and nearly fell down: "she thought she was going to die." From this time the progressive enlargement left no doubt that she was pregnant, and she was very much surprised to find that she was not confined in December. Her neighbours made sport of her, and had their jokes, telling her the child was waiting for its teeth, &c. This made her very unhappy. At length she was confined, March 17, 1850, of a girl, to her great relief, as she actually believed that she would never be confined. In this case, therefore, if conception be dated from the last day in March, the duration of preg-

nancy would be 351 days; and if we deduct the catamenial period, 28 days (which in this case would evidently be erroneous), still the duration would be $351 - 28 = 323$ days. Compare this with the time of quickening (August). From the last day in August to the 17th of March would be six months and a half,—or, from the middle of August, seven months. Pregnancy was therefore prolonged two months longer than is usual after quickening.

There are other cases that I might quote, in which pregnancy was extended three weeks and a month beyond 280 days; but I fear to trespass further on your attention. I shall only say that these results, as far as they have gone, convince me that the period of gestation is not fixed; and I have great reason to think that it sometimes extends much beyond the term that is generally supposed.

I beg that the Society will pardon this imperfect paper,—imperfect, because I did not expect that I should have to address you thus early; but, being called upon, I did not hesitate to do so, trusting to your uniform kindness to forgive whatever you might see amiss.

CORRESPONDENCE.

IN consequence of some misrepresentations that have been in circulation, respecting an opinion that I had given on a case at Norwich, I have been obliged in my address to the Medical Society of London to allude to it, and to refer to my correspondence on the subject in support of my statements. A sense of duty, therefore, obliges me (although reluctantly), to publish that correspondence, which I now do without any further comment than is necessary to render it intelligible.

May 1st, 1851, I received a message by electric telegraph from Dr. Bell of Norwich, to see with him the case referred to. Having consulted with him on the best course to adopt, I gave him, at his request, a certificate stating the nature of the disease, its cause, as I supposed, and my opinion of the treatment as related to me. Soon after my return to London I received the following letter from Mr. Cooper, which instituted the subsequent correspondence.

No. I.

College Street, Norwich,
May 2, 1851.

Sir,—You were, if I mistake not, summoned to Norwich yesterday by a homœopathic practitioner in this city to visit Mrs. G., who has been a patient of mine for some years, whom I attended in her last labour about six months ago, which was accomplished in every way naturally and satisfactorily, and from which she recovered without an untoward symptom, her convalescence being more rapid than usual. She has now aborted at about the third month, and has expelled portions of a placenta with numerous hydatids.

Will you have the courtesy to inform me if the subjoined certificate is a correct copy of one you gave as your undoubted opinion of her case, and its treatment. Waiting your immediate reply, which I shall esteem a favour.

I remain, &c., &c.,
W. COOPER.

Copy of Certificate.

I believe that Mrs. G. has suffered from extreme and long-continued hæmorrhage,

the result of an hydatid degeneration of the placenta. It seems to me highly probable that it may have arisen from a portion of the placenta retained after Mrs. G.'s previous labour, which became thus disorganised. The treatment that has been adopted was judicious, nor do I think that anything has been omitted that was necessary for her safety. The great danger of such cases is the difficulty of detaching the morbid mass from the uterus, as the hæmorrhage that continues during the separation may completely exhaust the patient before it is expelled.

(Signed) EDWARD WM. MURPHY.

May 1st, 1851.

12, Henrietta Street, Cavendish Square,
May 3, 1851.

Sir,—In reply to your communication, I beg to say that the copy of my written opinion on Mrs. G.'s case is, to the best of my recollection, correct. I may state that in the history of her case that I received, I was given to understand that it had been necessary in Mrs. G.'s previous confinement to remove the placenta. I mention this as it does not correspond with your statement "that Mrs. G.'s last labour was accomplished in every way naturally and satisfactorily."

I remain, dear sir,

Yours, &c.,
EDWARD W. MURPHY.

No. II.

College Street, Norwich, May 5, 1851.

Dear Sir,—After your acknowledgment of the opinion given in Mrs. G.'s case, in your letter of yesterday, you will not be surprised to hear from me again. Pardon me, if I express my astonishment that you should have grounded your opinion that a portion of placenta had been left in utero in Mrs. G.'s last confinement, upon the statement of Dr. Bell rather than upon your own judgment, from facts elicited from the patient herself, or her friends; and, still further, supposing such had been the case, which I do not for one moment admit, that you should have ventured an

opinion that has reflected discredit on the practitioner in attendance at that time.

It is my intention in my present communication to deal merely with the part of your written opinion which speaks of the cause of my patient's present disease. Neither myself, nor any of my professional brethren whom I have consulted, believe your opinion to be based upon a sound principle. I will, however, give you a few further particulars, which you may possibly not have been acquainted with.

Mrs. G. once menstruated in the usual way after her last confinement, which took place six months ago; from which she was sufficiently recovered at the end of a fortnight to be out of bed, attending upon her dying infant.

She supposed herself about four months advanced in pregnancy when attacked by her present illness. Since you were consulted by Dr. Bell, a blighted but distinctly recognizable embryo had been expelled from the uterus, and before her present attack the *mammæ* became flaccid and small. Did not the alteration in the *mammæ* mark the period when the ovum became blighted?

Do not the above facts disprove the idea of the present hydatidous affection having depended upon a portion of placenta left behind at the previous confinement?

Could she have had a rapid and favourable recovery, have gone into society, have menstruated once in a healthy and natural way, if anything had been left in utero six months ago?

Is pregnancy compatible with a portion of the placenta of the previous labour becoming hydatified?

Do not hydatids more frequently originate in a blighted ovum, and very rarely, if ever, form in an unimpregnated state?

As my professional reputation is somewhat at stake, you will not, I am sure, hesitate to weigh these questions, and favour me with an answer, even if it be to the subversion of your former opinion.

I am, sir, yours obediently,

W. COOPER.

To E. W. Murphy, Esq., M.D.

12, Henrietta Street, Cavendish Square,
May 7, 1851.

Dear Sir,—In reply to your note, I beg to assure you that nothing was farther from my intention than to reflect discredit upon the practitioner in attendance upon Mrs. G., in her last confinement. I am sure your knowledge and experience in the practice of midwifery must convince you that it is quite possible that retention of the placenta may take place after delivery so as to require removal, and that an adherent portion may be left behind in the uterus, and yet the practitioner be in no way

to blame for such a result. I thought it probable such might have happened in Mrs. G.'s case, and that the retained portion became the nucleus of future disease. I trust, therefore, that you will acquit me of any intention to reflect upon you.

With regard to the astonishment you express that I should have grounded my opinion upon the statement of Dr. Bell, I can only say that it is my practice in consultation to receive the history of the case from the gentleman in attendance. You will pardon me if I decline to enter upon a discussion on the other points touched upon in your letter, as, having no personal knowledge of the facts you state, it would lead to no useful result. Dr. Bell might just as readily express astonishment that I should have adopted your statement as you have done with regard to his. I may, however, mention that when I saw her, she was considerably larger than at that period. The fundus was midway between the umbilicus and pubis, and I was informed that it was double that size before the hydatids had been expelled. I have not in my recollection any case of four months' pregnancy in which the uterus was so large, nor do I believe that the disease of the chorion increases so rapidly as to produce such an effect in so short a time. However, this is merely matter of opinion.

In conclusion, I beg you to believe that I had no intention whatever to attach blame to you, nor did I imagine that the opinion which I had given would be so interpreted.

I remain, dear sir,

Yours very truly,

EDWARD W. MURPHY.

No. III.

Norwich, May 8, 1851.

Dear Sir,—Your letter of this morning appears to me to be anything but a satisfactory solution of your reason for giving the opinion you did with regard to the cause of Mrs. Gladstone's present disease. I am quite sure you must feel that opinion to have been somewhat hastily formed. You speak ambiguously of the uterus being larger at the time of your visit than you could account for, and therefore the only inference I can draw is, that you still adhere to the idea of a portion of the placenta being retained at the previous confinement, and that the hydatid growth was of six months' duration instead of *four*. I feel you must be inclined to doubt the accuracy of the facts I have presented to your notice with regard to the history of this case, or you would not still retain such a notion. Surely it is impossible that the

patient should have recovered her confinement so rapidly, have menstruated once regularly, and have conceived again with an hydatid growth of nearly two months in her uterus. Excuse me if I now say a few words on the second part of your opinion on the case as to the judicious treatment pursued by Dr. Bell and his friend.

Yesterday three weeks was the first time the aid of Dr. Bell was sought in this case. Mrs. G. had then upon her a slight uterine discharge, and believed herself threatened with abortion; her symptoms varied but little until Monday, 28th May (April?) when, upon Dr. Bell making an examination per vaginam, she was told she had a polypus of the womb, and must undergo an operation; but on Tuesday he said that it was a false conception, and upon an accession of uterine pains considerable attempts were made at the dilatation of the uterus by means of an instrument, and persevered in without success until the pain could no longer be borne. On Wednesday, Dr. Bell, becoming rather alarmed, sought the assistance of his friend Mr. M'Evoy; it was then perceived that some hydatidous placenta was being expelled, and after many hours of external and internal manipulation, a part of the contents of the uterus, which I believe was submitted to your inspection, and which I have now in my possession, was expelled. Pardon me if I express my belief that you were acquainted only partially with the treatment of this case, as you could not have justified such an amount of manual interference without any alarming hæmorrhage or exhaustion to warrant it. If there was any amount of hæmorrhage where were the means taken to restrain it? No plug—no cold affusion was used—No! to take the patient's own words, there was no more flooding than she should have had after an ordinary labour. The first examination, made by Dr. Copeman and myself, discovered an os uteri irregularly dilated, tumefied, and ragged. The vagina intensely hot, and painful to the touch; and the patient with a pulse of 180 as near as it could be counted, with a putrid mass still within the uterus. As I feel the tone of both your letters implied a degree of doubt as to the correctness of the history I have given you of this case, and as I dare say it varies some little from that you received from Dr. Bell, I thought it better to substantiate my report by forwarding you Mr. Gladstone's statement. The version of the case given by Dr. Bell, and his treatment of it, would have been passed over in silence by me (notwithstanding the mal animi he has displayed in delighting—as my patient relates—in reverting con-

stantly to the management of the practitioner at the last confinement), had it not been justified by so good an authority as yourself.

I am, sir,

Yours very truly,
W. COOPER.

12, Henrietta Street, Cavendish Square,
May 13, 1851.

Dear Sir,—I regret that my letter has not given you the satisfaction you desired. I beg to repeat that my opinion in Mrs. G.'s case was not hastily formed. I mentioned what the nature of her disease was, the danger which attended it, and what seemed to me, from the history I received, its probable cause. That account is at variance in many points with that which you have sent me, and so far as my opinion was founded upon Mrs. G.'s previous history, its correctness depends upon the accuracy of the account which I received. If the placenta were retained, if it were removed, if an adherent portion were left behind, then it seems to me probable that such was the cause of the complaint. If, on the contrary, no such thing occurred, then, of course, it could not be, and we must endeavour to explain it by supposing that the ovum which entered the uterus after the last menstrual period became diseased, and that this morbid growth was the result, which increased so rapidly as to enlarge the uterus to the size it usually is at six months gestation. Such may have been the case, and if so I was in error in assigning retention of the placenta as its probable cause. But I am sure you must perceive that, when I give an opinion upon facts of which I have no personal knowledge, I must be guided by the evidence I receive, and it would be an act of great injustice to Dr. Bell to take your statement of these facts, without giving him the opportunity of justifying himself. I am extremely anxious to avoid creating discord among my professional brethren, and consequently I have declined to discuss with you these particulars, neither have I had any communication with Dr. Bell on the subject. So far as my opinion is founded upon previous history, its accuracy depends upon the correctness of that history.

There are, however, some facts that have come under my notice, upon which I can speak. I made an examination of the uterus in Mrs. G.'s case, and I neither found "the os tincæ tumefied and ragged," nor "the vagina hot and painful," nor any "putrid mass in the uterus." The mass was an hydatid chorion, which was not putrid, and part of which had been thrown off. The lady had certainly a very rapid

pulse, the pulse of hæmorrhagic exhaustion, and it was most desirable for her safety that the whole should be expelled. Dr. Bell's treatment (*which was not homœopathic*), was entirely directed to that object. You are aware that manual efforts are sometimes made to remove an hydatid mass, knowing that whatever pain may be caused it is fully compensated by the complete arrest of the hæmorrhage. When I saw Mrs. G. the hæmorrhage had ceased, but she was so exhausted, her pulse so rapid, her countenance so bloodless, that it was evident hæmorrhage had been going on for some time. She had just the appearance which this disease always presents, in consequence of the frequently repeated discharges to which it gives rise. I feel, therefore, some difficulty in understanding your assertion that there was no alarming hæmorrhage until the 30th April.

With regard to the manual efforts said to be used by Dr. Bell, I do not by any means justify "the exertion of extreme force upon the body from four in the afternoon of that day, till one in the morning of the next," but you will pardon me in expressing any opinion on these points, without having also Dr. Bell's statements of these facts.

I wish at present, that you would consider this communication to be made to you in confidence, as I have not written to Dr. Bell, and am extremely unwilling to do so, because it must have an effect that is extremely injurious among professional men, nor can I perceive that any useful purpose can be accomplished by it. If, however, you think otherwise, I shall at once communicate with Dr. Bell, and learn from him his explanation of the statement you have forwarded. With regard to the opinion I have given, I see no reason to alter anything that I have said, but merely to explain that in stating a probable cause of the disease, I must be guided by the account I receive, and either of those, yours, or Dr. Bell's, would be sufficient for the purpose.

I remain, dear sir,

Yours truly,

EDWARD W. MURPHY.

[These letters refer to a statement to which I shall presently allude. Mr. Cooper did not wait my reply, but hastened up to London, and I regret that he did not think proper to call upon me, which one would suppose to be a natural course; not doing so is a sufficient proof of the animus that guided him, inasmuch as I have been given to understand that the special object of his visit was to obtain certificates from other professional men to prove that my opinion could not be correct; that it was in

fact a pathological blunder. If this be true, it shows Mr. Cooper's desire rather to attack me than to accept any explanation that I could offer him, and will justify the suspicions which the tone of his correspondence excited. After a few days I received the following note:—]

No. IV.

10, Fenchurch Buildings.

Dear Sir,—Your communication of the 12th inst. did not reach me till yesterday, having left Norwich for London on that day. The assertion of Dr. Bell as to the cause of Mrs. G.'s present disease, and your confirmation of its extreme probability, has had so prejudicial an effect on my professional reputation, that *I am induced to bring the matter before the profession generally*. I must, therefore, beg of you to make any communication you may think right with Dr. Bell upon the subject, and consider that all correspondence which you may have the courtesy to hold with me must be done without any confidential reserve.

I am, dear sir,

Yours faithfully,

WILLIAM COOPER.

The letter No. 3 inclosed a statement which charged Dr. Bell and another gentleman with "having attempted to dilate the womb by means of an instrument," and with having used extreme force upon the body of Mrs. G." This was certified by Mr. Gladstone. Being forwarded to Dr. Bell, I received from him another statement of a very different kind, in which Dr. Bell's treatment is given in detail, but no mention made of any instrument being used. This was also attested by Mr. Gladstone in the following words:—"The above is what I believe to be a true statement of the case of my beloved wife as it came under my own observation; and I take this opportunity of saying that whatever may be the opinion of any concerning the medical treatment which has been followed by those who have had the case under their care, I feel under the deepest obligation to Dr. Bell for his most disinterested care and kindness; and I take the first opportunity of expressing my belief that, whatever difference of opinion may exist, I have the greatest cause for thankfulness to all the medical gentlemen concerned, whose sincere object has, I believe, been to be instrumental under God in restoring my dear wife to me." The two statements appeared to me contradictory and irreconcilable.

Expecting from Mr. Cooper some public statement of his case, I was surprised to read in the *Lancet*, May 17th, an *anonymous* letter, making an attack upon Dr.

Bell and me. This letter was inserted in the leading article, which contained very severe strictures. To this attack I replied, May 31st. My letter called forth others, and among them one from Mr. Gladstone, who had evidently been under a complete misapprehension of my meaning when I stated in that letter that the documents sent to me were "contradictory and irreconcilable." In reply, I forwarded to the *Lancet* copies of the documents, with an explanation; but that journal concluded the correspondence much in the same manner it was commenced. A fragment of the letter I had written in explanation was inserted; but the chief part, and the whole of the documents sent, were omitted, stating that "This correspondence cannot be continued: it has already diverged into questions quite distinct from the object which led to its introduction in our columns. On the questions raised, we believe there cannot be two opinions in the profession. —ED. L."

Notwithstanding this determination, a letter from Dr. Ranking was inserted among the notices from correspondents, Sept. 20, 1851. This led to a renewal of the correspondence, not through the pages of the *Lancet*, but privately. I learned from it many particulars with which I was unacquainted; but being informed of them, I now feel it my duty to place the whole correspondence before the profession.

Dr. Ranking's letter to the *Lancet* was as follows:—

No. V.

Sir,—I am anxious, by the help of your pages, to inquire of Dr. Murphy whether he still adheres to the opinion expressed in his certificate, given in a late unfortunate case, that a portion of placenta remaining in utero after a labour is capable of undergoing hydatid degeneration?

I hope Dr. Murphy will excuse my asking him this question, as I am now preparing a report on midwifery for the next volume of the "Half-Yearly Abstract," and am unwilling to give further publicity to an opinion so opposed to that of all eminent obstetric authorities as emanating from a lecturer on midwifery, without first ascertaining that such are Dr. Murphy's real sentiments.—I am, sir,

Your obedient servant,
W. H. RANKING.

12, Henrietta Street, Cavendish Square,
Sept. 25, 1851.

Dear Sir,—Having been absent from London, I did not see your letter to the *Lancet* until some days after its publication. That journal had professed some time since to close the correspondence on

"the Norwich Consultation;" and I shall not reopen it by replying, through its pages, to you. Nevertheless, as my silence might be erroneously interpreted, I send you these few lines to correct what seems to me a misapprehension on your part.

The question to decide is not precisely as you have put it—viz., "Whether a portion of placenta remaining in utero after labour is capable of *undergoing* hydatid degeneration?"—but whether a placenta, *having before labour undergone* that change, and being in consequence morbidly adherent to the uterus, is capable, after delivery, of continuing the disease in the portion retained? I do not perceive any impossibility in this, although I am quite aware that a retained portion of a placenta otherwise healthy could not do so. The cases of this disease are so scattered, and their histories so imperfectly related, that it is difficult to quote a case in illustration; but that such a cause of hydatids had been ascertained, I infer from the fact that Denman, Burns, and other writers of practical experience, have mentioned a retained portion of placenta as one of the causes of hydatids. Such an opinion is not, therefore, so opposed to that of "all eminent obstetrical authorities," as you imagine.

In applying these observations to the case upon which I was called upon to give an opinion, from the history of the case which was given me, and the great size of uterus, it appeared to me that it was *probable* that a retained portion of a previously hydatid placenta, which it was impossible completely to remove, caused the subsequent symptoms. In stating a probable cause it was not intended to exclude other possible causes; the opinion that I formed rested on the facts placed before me. So far, then, as the general question is concerned, "my real sentiments" are—

1st.—That it possible that a child may be born at the full term and yet the placenta undergo hydatiform degeneration.

2nd.—That such placenta can seldom be completely removed.

3rd.—That it is probable the retained portion may continue the disease.

4th.—That the usual course of hydatid degeneration being the destruction of the ovum before it attains maturity, abortion is generally the result.

I am sure you must perceive that with such sentiments my stated opinion could not bear the interpretation given to it, or that I in any way intended to cast a reflection on Mr. Cooper's practice or treatment. I also trust that you will admit that the attacks which have been made upon me are uncalled for.

I remain, dear sir,
Yours truly,
E. W. MURPHY

No. VI.

Norwich, Oct. 1851.

Dear Sir,—I am obliged by the favor of your note this morning, but cannot feel that the points on which I was anxious to be informed have been quite satisfactorily answered.

You will excuse my repeating the question, *Whether a portion of placenta retained after labour can undergo hydatid degeneration?* not whether a portion of already hydatigenized placenta remaining can further develop: because in the case alluded to, even granting (which no one here does) that a portion of placenta *was left at all* by Mr. Cooper, that gentleman is prepared to swear on oath that the placenta removed was *in every respect healthy*. The fact, therefore, which you have countenanced when you gave a certificate to the effect that Mrs. Gladstone's symptoms arose from a portion of after-birth left in utero by Mr. Cooper, is that a portion of healthy placenta may remain and become afterwards hydatid. Another question I ought to have put is, *Do you consider that with a portion of placenta remaining in utero, fresh impregnation could take place within six or eight weeks?* Perhaps you are not aware that there was a fresh impregnation, for you admit you were obliged to rest upon the dictum of the homœopath Bell, but I assure you that such was the case, and the blighted fœtus was distinctly seen in the midst of the hydatid masses. You have therefore virtually given the profession the opinion that not only can retained placenta become hydatid, but fresh impregnation can take place, and the new placental structure also become similarly transformed.

When I say all the profession is against you, I mean that Mr. Cooper possesses certificates from almost every physician of eminence in London, as well as from several elsewhere, all of which are decidedly opposed to your presumed opinion. I trust you will not deem me impertinent in asking you to reconsider the opinion you gave Dr. Bell; by it you have not only bolstered up the reputation of a homœopath, but floun in the face of all honourable practitioners, as well as injured the reputation of a highly respectable and intelligent surgeon. That it has really done the latter I beg to assure you is the case; your opinion has been repeatedly hawked about amid the small gossip of the tea-table, and is the only support which can be adduced in favour of the treatment pursued in this most lamentable case.

Allow me to say, that by a revisal or explanation of your opinion you might regain the position you are well nigh losing in the estimation of the profession. At the Brighton meeting I was surrounded by parties anxious

to hear about the Gladstone case, and the condemnation of your share in it was universal, and given in language which augured ill for the favour in which you would be regarded for the future. Believe me, I write in good part, for though I have not the pleasure of a personal acquaintance, the character you bear for amiability and uprightness of intention render me anxious that you should not in such a cause allow your name to be tarnished as it has been.

I remain, dear sir,

Yours truly,

W. H. RANKING.

12, Henrietta Street, Cavendish Square,
Oct. 3, 1851.

Dear Sir,—I am much obliged by your note, and have only to regret that my first correspondence upon Mrs. Gladstone's case had not been with you rather than Mr. Cooper. I have no hesitation in answering both your questions. With regard to the first, I do not believe that a portion of *healthy* placenta retained after labour can undergo hydatid degeneration, but I am not at all so clear about a placenta that had previously undergone that change, if a portion be left behind in the uterus. In Mrs. G.'s case I was told that it was necessary to remove it after her delivery, that she complained of painful uneasy sensations in the left side, which continued afterwards, and that her child had only lived a fortnight. Putting all these facts together, I inferred that the placenta was morbidly adherent in her last confinement in consequence of this disease, and that it continued after her delivery progressively to show itself. In stating a retained portion of placenta as a cause, I cannot perceive why I should be understood to mean a *healthy* placenta: *I did not so state it*. The very assumed fact that a portion was left behind presumes it to have been *morbid*, because, unless with very ignorant persons, no portion of a healthy placenta could be left behind; while, on the other hand, the most adroit practitioner cannot always completely remove one that is morbid. The only difference between this and other cases of morbid adhesion was my suspicion that the morbid cause was hydatids. In making this statement I never for one moment intended to cast any reflection upon Mr. Cooper's treatment; I stated so to him, and would willingly have given him any explanation of my certificate to remove such an erroneous impression.

He, however, seemed much more anxious to convict me of an obvious mistake,—to prove that the cause which I assigned could not exist, and, as I perceive, has taken considerable pains to obtain the collective opinion of the profession against me, by placing before them *what is really not the*

question. You will not be surprised that I should feel such a course as this perfectly uncalled for by the facts of the case. As I am conscious that my certificate, if perfectly true, could cast no reflection upon Mr. Cooper; as I have given him every opportunity to remove any erroneous impression which might exist by an explanation of my meaning; and as, further, the practical effect of my opinion was to reinstate Mr. Cooper by the dismissal of Dr. Bell, I am at a loss to perceive in what respect he is an aggrieved person; still less can I understand his right to adopt such a pretext in order to make the appeal to the profession he has done, or to make use of my name in a manner so unwarrantable.

With respect to the second question, my reply is equally easy. I do not believe that a portion of placenta could remain in the uterus together with a subsequent ovum, or that a fresh impregnation could take place while it was there. If, therefore, there was any evidence of abortion existing in Mrs. G.'s case, there could have been no diseased placenta in the uterus at the same time. Every proof of abortion negatives the probable cause that I assigned: these proofs, however, were *subsequently* in possession of Mr. Cooper. When I saw the patient there was only evidence that an hydatid mass existed in the uterus. I could not find any portion of the membranes, unless, indeed, a fragment of a hydatid chorion, so thick that it looked more like placenta.

Permit me again to express my surprise that such an erroneous interpretation of my certificate should be given as to make it signify that "I have virtually given the profession the opinion that not only a retained placenta became hydatid, but that fresh impregnation can take place and the new placental structure also became similarly transformed." There is nothing in

my certificate to warrant such a meaning. In my correspondence with Mr. Cooper I distinctly pointed out my reasons for doubting that Mrs. G. was pregnant when I saw her, on the assumption that, if she were so, the cause could not have been what I stated.

When so many errors have been made I do not wonder that the whole profession should be against me. I only trust that you will act as I am persuaded your sense of justice will dictate; that you will use your best endeavour to correct these erroneous impression, and remove the odium that, as I understand from you, has been most unfairly cast upon me.

Permit me to thank you for your evidently friendly observations. I beg you to believe that nothing is more repugnant to my feelings than "to bolster up the reputation of a homœopath." I never in my life gave any countenance to irregular practitioners.

With regard to Dr. Bell, I know nothing of him more than that he was qualified to practise. In my intercourse with him I found him in every respect a gentleman. When I met him I did not know he was a homœopath; and, in our subsequent conversation, all that I could learn of his homœopathic tendencies was that he sometimes adopted it. Dr. Bell seemed to me to be just in the same position as some of our most respectable men here, nibbling at homœopathy but afraid to bite. I had no way of judging of his merits or demerits except through the press or Mr. Cooper, and the spirit of animosity was such that I did not feel authorised to adopt the opinions of either without a more certain knowledge of him.

I remain, dear sir,

Very faithfully yours,

EDWARD W. MURPHY.

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