Table showing the Treatment and Results of Fifty-nine Cases of Placental Presentation.

No	Complete or Partial.	Treatment.	Result.
1	Complete..	Turning by the late Mr. Baker; phlebitis eighteen days after.	Died.
2	Complete..	Seen by no practitioner before death.	Died.
3	Partial ...	Placenta and child expelled	Recovered.
4	Uncertain..	Placenta and child expelled; flooding continued after.	Recovered.
5	Complete..	Turning by Dr. Lee	Recovered.
6	Partial ...	Turning by Dr. Lee	Recovered.
7	Complete..	Dying before turning by Dr. Lee ...	Died.
8	Complete..	Turning with two fingers by Dr. Lee.	Recovered.
9	Partial ...	Distortion of pelvis; craniotomy by Dr. Lee.	Recovered.
10	Partial ...	Placenta and foetus expelled	Recovered.
11	Partial ...	Membranes ruptured; turning not performed.	Recovered.
12	Complete..	Turning by Dr. Lee	Died.
13	Partial ...	Turning not performed	Died.
14	Partial ...	Turning not performed. Up to this time an epidemic prevailed.	Died.
15	Partial ...	Ergot freely given before turning by Dr. Lee.	Died.
16	Partial ...	Turning by Mr. Gaskell	Died.
17	Partial ...	Turning by Dr. Lee	Recovered.
18	Partial ...	Turning by Dr. Lee	Recovered.
19	Complete..	Turning by Dr. Lee	Recovered.
20	Partial ...	Membranes ruptured by Dr. Lee; turning not performed.	Recovered.
21	Complete..	Turning by Dr. Lee	Recovered.
22	Complete..	Turning by Dr. Boyd; phlebitis ...	Died.
23	Complete..	Craniotomy by Dr. Lee; turning not performed.	Died.
24	Complete..	Turning by Dr. Lee; great rigidity.	Died.
25	Complete..	Turning by Dr. Lee	Recovered.
26	Complete..	Great distortion; turning by Dr. H. Davis.	Died.
27	Partial ...	Membranes ruptured; turning not performed.	Recovered.
28	Complete..	Turning by Dr. Lee	Recovered.
29	Complete..	Turning by Mr. Girdwood	Died.
30	Complete..	Turning by Mr. Jonson	Recovered.
31	Partial ...	Membranes ruptured spontaneously.	Recovered.
32	Complete..	Turning by Dr. Lee	Recovered.
33	Complete..	Turning by Dr. Lee	Recovered.
34	Partial ...	Membranes ruptured by Dr. Lee ...	Recovered.
35	Complete..	Turning by Mr. Harvey	Died.
36	Complete..	Turning by Dr. Lee	Recovered.
37	Complete..	Turning by Dr. Lee	Recovered.
38	Partial ...	Membranes ruptured artificially by Dr. Lee.	Recovered.
39	Partial ...	Craniotomy by Dr. Lee	Recovered.
40	Partial ...	Craniotomy by Dr. Lee	Recovered.
41	Partial ...	Craniotomy by Dr. Lee	Recovered.
42	Complete..	Turning by Dr. Lee	Recovered.
43	Uncertain..	Perforation of placenta	Recovered.
44	Partial ...	Turning by Dr. Lee	Recovered.
45	Complete..	Turning by Mr. Byam; flooding continued.	Recovered.
46	Partial ...	Membranes ruptured; turning not performed.	Recovered.
47	Partial ...	Membranes ruptured; turning not performed.	Recovered.
48	Partial ...	Membranes ruptured; turning not performed.	Recovered.
49	Complete..	Turning by Mr. Gaskell	Recovered.
50	Complete..	Turning by Mr. Dalton	Recovered.
51	Complete..	Turning with two fingers by Dr. Lee.	Recovered.
52	Complete..	Turning with two fingers by Mr. Davidson.	Recovered.
53	Complete..	Dying when first seen; turning by Dr. Lee.	Died.
54	Complete..	Turning with two fingers by Dr. Lee.	Recovered.
55	Complete..	Placenta and foetus expelled spontaneously.	Recovered.
56	Complete..	Turning promptly performed by Mr. Balderson.	Recovered.
57	Complete..	Turning with two fingers by Dr. Lee.	Recovered.
58	Partial ...	Membranes ruptured; turning not performed.	Recovered.
59	Complete..	Placenta and child expelled; turning not performed.	Recovered.

OBSERVATIONS ON A CASE
SUPPOSED TO PROVE THAT
SECONDARY SYMPTOMS ARE CONTAGIOUS
AND CAPABLE OF TRANSMISSION.

By WILLIAM ACTON, Esq.

SURGEON TO THE ISLINGTON DISPENSARY, AND FORMERLY EXTERNE
TO THE VENEREAL HOSPITALS OF PARIS.

THE subject of the correspondence included in this article has of late occupied much of public attention in the medical periodicals. To judge by the cases advanced, great uncertainty seems to exist on the contagion of secondary symptoms; and in spite of experiments which have been brought forward, proving the impossibility of inoculating with the secretion of secondary symptoms, it has been said that we cannot imitate Nature—that although our experiments do not succeed, it by no means follows that Nature cannot do what our lancets fail of effecting; and to this purpose cases are cited which are supposed to prove the position that secondary symptoms are transmissible. One gentleman with whom I have been in correspondence on this subject, states, that the evidence I have advanced in my work on venereal diseases, in the various articles in the periodicals, is wholly of a negative character; whereas, my opponents adduce positive evidence; and he will at any time rather believe the latter than the former. It is in vain for me to remind him that his countryman, Dr. Gregory, stated it as his opinion that “a hundred so-called medical facts were as many medical lies,” and that a French author has desired us “peser et non compte les faits.”

Certain it is, as I shall subsequently take occasion to show, much difference of opinion occurs on what is and what is not syphilis; and I am almost disposed to think that he would write the best treatise on syphilis who should occupy himself by showing the little value to be placed on colour alone as a diagnostic mark of syphilis.

I am daily seeing instances which induce me to believe that the pseudo-syphilis, or syphiloid diseases of authors of the last century, are similar to the cases which are now giving rise to the present difference of opinion, and that they are not syphilitic at all in many instances, and in others only allied to that disease by colour, or by the same treatment curing the one and the other complaint.

The error of the day appears to me to consist in thinking an eruption syphilitic provided it presents a copper-colour tint, notwithstanding the absence of all corroborative evidence. Hunter did not think that this symptom was alone necessary to characterize syphilis; then why should we, particularly when authors of repute have shown that the copper colour is common to many diseases?

We are only just emerging from the error of the last century, which consisted in calling that disease syphilis which yielded to mercury, and denying its existence when the complaint yielded to simple treatment. It took many years to overturn this dogma; it will probably require as long to eradicate the received idea, that a copper colour is diagnostic of syphilis, notwithstanding all other opposing circumstances.

In spite of all opposition to my views, I must still believe that the copper hue is only of importance when supported by corroborative history. Sore throat of a peculiar kind, condylomata, psoriasis palmaris, &c.; when one or all of these are present, we may safely say that the case is one of syphilis, but without one or more of these symptoms, the diagnosis is at best but doubtful.

With these preliminary observations, I submit the following correspondence to the notice of the profession, believing the case under discussion to be one of those anomalous ones, many of which may be found described in the works of Hunter and Abernethy:—

DEAR SIR,—Knowing, from your studies whilst in Paris, and your recent publications, that you are much interested in the history and nature of syphilitic diseases, I send you the following case, which, to my mind, goes far to prove the contagiousness of secondary symptoms, a fact which I believe to be questioned both by yourself and M. Ricord. I shall state the case briefly, and leave it for you to decide how far it bears upon this important point:—

A— B— contracted syphilis in May, 1845, and was under treatment for a superficial chancre, situated at the base of the glans penis. According to his own account, he took five grains of blue pill four times a day, for a week, but his system was not affected by the mineral. After exposure to wet, and much fatigue, the gland of one of the groins became inflamed, which obliged him to lay up. By the use of proper remedies, the inflammation subsided, but he was not perfectly

well until the following October. During this time, he took no more mercury. He then considered himself cured, and from October to March, did not again expose himself to infection. During this interval, he was engaged to be married, and forgot his previous maladies. Early in the month of March, 1846, he was troubled with sore throat, and condylomata round the margin of the anus. There was no eruption about the skin. The medical man to whom he submitted his case, put him under the iodide of potassium, and he continued this treatment for six weeks, when he was perfectly recovered. The condylomata (one of which was in a state of ulceration, and very irritable, being close to the anus,) were touched with the nitrate of silver. The 15th of April was the last day in which he took any medicine, and he was married the second week in June. Nothing particular occurred until his wife miscarried in the September following, having gone with child about two months. On recovering from this, she complained of soreness in the scalp, where there were two or three nodes to be felt; one gave her particular uneasiness, being situated over the mastoid process of the right side. After this, she had sore throat, much irritation about the vagina, and an eruption over the trunk and lower extremities. This much frightened her husband, and he requested me to see her, at the same time intimating to me what his fears were. I then, for the first time, gathered from him and his former medical attendant, to whom he referred me, the details as above given. At first, I thought it was impossible there could be any grounds for his fears, as I was a faithful disciple of M. Ricord, having followed his practice at the Hôpital du Midi, in 1840, when in Paris. When, however, I saw the eruption, I began to be shaken in my opinion. It was exanthematous in its appearance, and resembled roseola or measles, the spots being crescenta and diffused, leaving interstices of healthy skin between them. The coppery hue was most marked, and I found that the eruption had been out for about a fortnight. There was nothing specific in the appearance of the throat. The tonsils were very much enlarged, and superficially ulcerated in parts. I examined the internal labia, and discovered much irritation, then produced by excoriations. I ordered small doses of iodide of mercury twice daily, an emollient gargle, and, for local application, recommended a solution of the diacetate of lead. This treatment was continued for about six weeks, and the eruption gradually disappeared, with the irritation about the vulva and vagina. The tonsils have not been reduced to their ordinary size, and I have latterly been giving small doses of iodide of potassium, in place of the iodide of mercury. The nodes have almost disappeared. I examined the state of the penis in the husband, to see whether there might be any induration remaining about the old cicatrix, but such was not the case. He appeared to be quite well. He has also been subjected to treatment since this event. The story, as above given, I can vouch for the truth of, and nothing has been concealed from me. As this is the first case which I have seen of the kind, I thought I would communicate it to you, as a matter of interest. Should you wish to hear any further details connected with it, I shall be happy to send them to you. You are at liberty to make what use you like of this case, for the furtherance of science, provided no names be mentioned, as it is in the strictest confidence that I have been consulted. If you have time to spare, I should like to hear your remarks on the contents of this notice; and in the meantime, allow me to remain, yours faithfully,

Feb. 1847.

DEAR SIR,—I have read the letter you forwarded to me with much interest, and as it is of common occurrence for me to be applied to in similar cases, I shall be happy to send your communication to *THE LANCET*, with my own remarks, provided you do not object to find your opinion (communicated to me with a scientific purpose) criticised in a manner that you are perhaps unprepared for. I have determined not to publish cases similar to yours without free permission to add my own comments, for, as you truly observe, these obscure cases are very important, and have occupied much attention both in Paris and London, and they at first sight invalidate some laws of syphilis which M. Ricord and myself may have ventured to lay down, and which we cannot give up without a struggle; although we would as readily admit the contagion of secondary symptoms as we now deny the fact of their being so, provided their occurrence could be proved.

I may, however, premise, that without seeing a patient, it is always difficult to find out any cause of error: cases are cited in books, and related in journals, which are so got up that they cannot be disputed, but yet no one believes them;

yet had they been seen by a practical man, the cases would probably have worn a very different appearance; and as you ask me my candid opinion upon your communication, I shall not hesitate to give it. In the first place, I think your diagnosis *may* have been wrong; and in saying this, allow me to observe, that you may have fallen into the error (common to many who have seen much of disease, without attending specially to syphilis) of supposing a case to be syphilitic which was not really so; and in proof that this is often done, I must beg of you to wait a few weeks, when I propose to publish the details of a large number of cases which have fallen under my notice in private practice, and which have been condemned as syphilitic for similar reasons to those given in your letter; the particulars, however, would be too long for the present occasion, and will form, I think, a very interesting and practical paper.

Now, supposing the case you have detailed to have been a case of secondary symptoms in the wife, observe what you take for granted:—That a man who does not present, and has not presented, any trace of secondary symptoms for two months, can infect his wife. This, then, is not a case of simple contagion from the contact of skin suffering from secondary symptoms with a healthy skin, as some have asserted, but renders the subject still more complicated, and, I think, improbable. Had the husband suffered under secondary symptoms subsequent to his marriage, the idea that he had communicated the disease would have been more probable. If your case proves anything, it goes to decide that a man can infect his wife, although he himself presents no traces of secondary symptoms.

Another no less extraordinary circumstance occurs in this case: supposing the wife to have secondary symptoms, how did she contract them from her husband? They do not appear to have commenced at condylomata, which authors who support your opinions have generally believed to be the means of contagion; but I find in your communication, that three nodes made their appearance on the scalp. If, then, contagion can take place between a man (who presents no mark of secondary symptoms) and his wife, we must suppose that in future such contagion will be evinced by the occurrence of nodes on the head, and not, as is usual, *simila similibus*; in other words, that so-called secondary symptoms (which are not apparent in the husband) will give rise to what we call tertiary disease in the wife. These are truly extraordinary phenomena, and perhaps you yourself are unprepared for the consequences which we must deduce from them if we admit your facts.

I would point out another extraordinary circumstance unknown to persons who have had great experience in syphilitic disease; I allude to these said nodes. Your letter states that they occurred about three months after possible infection. This is likewise an almost unprecedented circumstance. You further state, that they were followed by an exanthematous eruption. Now generally, the course of nodes is this: secondary symptoms follow primary ones at intervals varying from six weeks to three months; and tertiary symptoms, or nodes, follow secondary symptoms at the end of many months, or even a year. Now, exactly an opposite course occurred in your recital. So wedded am I to my own opinions, founded on the observation of a vast number of cases, which I have now watched for many years, that I would disbelieve my own senses, and consider that I myself had observed these cases imperfectly, rather than believe one case ran counter to all recorded instances, or all syphilitic cases that I myself had witnessed.

Now, allow me to recal your attention to the reasons for considering the symptoms in the wife as those of syphilis—what are they? You admit, yourself, that the throat presented nothing specific—I should say as much of the exanthematous eruption. I have alluded elsewhere, both in my work on venereal diseases, and in the pages of *THE LANCET*, to the little value of the coppery hue; and I find no traces of condylomata, or psoriasis palmaris, and other characteristic signs of syphilis mentioned as occurring in the case you cite. Now, had the husband never had syphilis, would you have condemned the wife? I think not; and as happens often in syphilis, the history of cases biasses the medical man. It is always to be regretted, when errors have been committed; and it is usually a thankless task to point them out, particularly when the case is not at hand to prove or disprove statements; but as you ask me to state my opinion, I give it you, the more readily, as I believe your case is far from uncommon, and its recital may clear up certain obscure points in diagnosis. If, therefore, you do not object to my views, or their publication, I will, on your sending back this letter, forward them to *THE LANCET*, together with some additional cases bearing on

the question, which press of business prevents me giving at the present moment.—Pray excuse this hasty letter, and believe me to be, yours faithfully,
W. ACTON.

Queen Anne-street, Cavendish-square, Feb. 1847.

P.S.—You may probably ask me, what was the case, if not one of syphilis? I would not venture to give it a name; but should we call that syphilis which we are unable to class under any other disease? and yet I fear this has been often done.

DEAR SIR,—In reply to your comments on the communication which I sent you, I beg to offer the following remarks, and, by way of preface, allow me to say that most of your objections had suggested themselves to my mind ere I decided upon the conclusion to which I at length came. I was therefore prepared in your answer for one of two modes of your meeting the case, by your questioning either the accuracy of my diagnosis, or the truth of the story told by the party concerned. It is to the former of these points that you direct your observations, and I conclude that the latter is not doubted by you.

Now, your chief arguments are founded upon the supposition that the conclusions which you have come to relative to the nature of syphilis are decisive, and you invalidate the opinion which I had formed, by showing its nonconformity with certain laws which you take for granted as definitely fixed, but which my case (if it be such as I suppose it to be) proves not to be absolute in their application. You, in fact, draw your inferences from premises which you presuppose to be true, but which I am not so ready to grant now as I might have been twelve months ago. It is of course self-evident, that if the great rules by which you test the accuracy of my conclusion be so perfect as to admit of no exceptions, and so true as to constitute axioms, then the question between us is at once settled. It is, however, just this very point upon which a doubt exists in my mind, and therefore it was that I sent you the details of a case which apparently proved that the rules of syphilis, like most other rules, had their exception. If it be impossible that secondary syphilis can be communicated by contagion; if the like disease in one must produce the like in another, (an axiom that I have always questioned, believing that the character of syphilis is often dependent upon peculiarity of constitution;) if it be impossible to have periosteal pains and swellings after three months of supposed infection, or for an individual apparently cured of secondary symptoms, though perhaps not really so, to infect another; if by the laws of syphilis this be impossible, then the case before us is not syphilitic in its character.

Now in my communication to you, I gave a simple recital of the case as it presented itself to my observation; I took nothing for granted, but simply stated facts, and my conclusion from them, and I leave it for others to decide how far the history and the symptoms of the disease warranted the inference which I drew, and how far I should be justified in turning a deaf ear to my own convictions, because they invalidated a theory which I had hitherto judged to be right. If there be certain general rules whereby one is to be led in arriving at a correct diagnosis,—if there be certain symptoms which are characteristic of certain diseases,—if the history of a case is to be our guide, we are not to reject the conclusion to which they may lead us, because that conclusion may question the accuracy of certain theories. A female of good health and strong constitution marries. From no assignable cause she miscarries. Her miscarriage is followed by sore throat and a copper-coloured exanthematous eruption, (the latter so peculiar in its tint as to attract the notice of an ordinary observer,) which persists for nearly two months, and then disappears under the apparent influence of the iodide of mercury. Pain is complained of at night in the scalp, when there are to be felt two or three hard swellings; and the question asked is—what may this case be? Is it not one of secondary symptoms? I answer in the affirmative. But you deny the accuracy of my judgment, as the history of the case will not bear it out, and the laws of syphilis declare it to be impossible. You allow that it is difficult to know to what class of diseases it may belong; but you ask, must it therefore be syphilitic? I came to my conclusion through no such unsound premises; but in this case, as in every other of doubt, was guided by certain general features which developed themselves in the disorder, and gave it a peculiar character. So faithful a follower was I of M. Ricord and yourself, that I unhesitatingly replied, when the possibility of syphilis was first mentioned, that such could not, under the peculiar circumstances, be the case. It required, therefore, no little reasoning with myself to argue me out of my former belief,

to which I gave the benefit of the doubt, till the character of external appearances no longer warranted it. If these were not such as indicated the nature of the disease, my studies at the Hôpital St. Louis, and the lessons I there learnt, have taught me nothing. If the copper colour of an eruption is not to be relied on as diagnostic of its peculiar nature, I know not what is; for whether it be a case of psoriasis, lichen, lepra, or roseola, what, I would ask, gives them their specific stamp, if this does not? Let the profession generally answer this question—Do we ever see copper-coloured eruptions, with sore throats, in patients whose history places them beyond the suspicion and the possibility of being tainted with the syphilitic virus? I think not; and it does appear to me, that if once we deny the specific character of the coppery hue, one great means of diagnosis is gone. You ask me—“Had the husband never had syphilis, would you have condemned the wife?” I answer, that tacitly I should, provided the husband was a person on whose word I could rely with confidence. But let me ask in return, whether, supposing such a case as this had presented itself to your notice in one of the lower orders of society, amongst whom virtue is not so highly valued as amongst the higher, would you have then been inclined to question the nature of the disease, though you might have questioned the accuracy of the story? How the secondary symptoms could be communicated in this or any other case, I do not pretend to determine; for theory upon theory might suggest itself, supposing the reality of the case and its circumstances be satisfactorily established. You are perfectly right in not yielding your point upon the mere hearsay of another; but still cases like the above, though perhaps very rare, may, in the course of time, present themselves to your notice, when you will be able to reason better upon them, and judge more dispassionately of their merits, than by reading the mere dry details on paper of what another has seen, all the while knowing how prone the mind of the inexperienced is to be deceived in points, the difficulties of which can alone be met, and satisfactorily answered, by him whose practice has made him perfect in unravelling them. The question is very important, and one well adapted for discussion. I have to thank you for your kind and prompt attention to it, and your opinion offered on it. Should anything be further developed in the course of time in connexion with this case, I shall again write you on the subject.—Believe me to remain, yours faithfully,

Reviews.

Body and Soul; or, Life, Mind, and Matter. By GEORGE REDFORD, M.R.C.S.E. London: Churchill. pp. 232.

THE first eight words, so pregnant with meaning, forming the title of this book, convey a tolerable notion of the nature of its contents; and they will prepare the reader for the discussion of the numerous knotty points which occur in that domain of science, which borders upon metaphysics on the one hand, and physiology on the other. The vulgar, who know a little, and the severe rationalists, who know a great deal, have long held up the mysteries of life, as well as those which attend it in disease, as a sort of opprobrium to the science of medicine. They think that we ought to be able to explain, or at any rate, account for, everything which falls within the immense range of our mighty study, as is done in the exact sciences, but the best amongst us are mindful of the evil that has arisen from a desire, on the part of many physiologists, to explain away difficulties, and place physiology nearer the exact sciences, by reducing nature to the rule of favourite theories and fancied laws. We must always remember how very little in our science can be actually proved to be true. We are now only beginning to comprehend the physiology of life, and its great antagonist, disease; but, brilliant as the results of the last twenty years have been, we seem, as yet, only to have arrived at a position parallel to that which Newton so beautifully described, in reference to the inductive sciences in his hands; we have gathered a few pebbles from the shore of a boundless ocean which lies beyond.

In the work before us, the various theories concerning life and mind are pretty fully discussed, with an extensive appli-