= 1025.9. 65,776 grammes precipitated by ammonia; earthy phosphates = ,047 = 0.71 per 1000; 33,221 grammes precipitated by chloride of calcium and ammonia; phosphate of lime = ,126 = 3.79 per 1000.

CASE 4.—September 8th, 1845; Miss S—, aged thirty-two; melancholia; urine was acid; light-coloured; much laminar epithelium; no uric acid and no oxalate of lime deposited; specific gravity = 1027.9. 65,694 grammes precipitated by ammonia; earthy phosphates = ,09 = 1.47 per 1000; 33,285 grammes precipitated by chloride of calcium and ammonia; phosphate of lime = 1.27 = 3.81 per 1000.

CASE 5.—July 7th, 1845; Mr. W—, aged sixty-six; senile dementia; thin; wasted; sleeps badly; refuses food; urine thick, from infusoria and spermatozoa in considerable quantity; very rarely a crystal of oxalate of lime to be seen with the microscope; no albumen; specific gravity = 1021.0. 50,570 grammes precipitated by ammonia; earthy phosphates = ,036 = .71 per 1000; 33,063 grammes precipitated by chloride of calcium and ammonia; phosphate of lime = ,093 = 2.81 per 1000.

There are no marked variations among these four cases of melancholia, as regards the total amount of phosphates. In the single case of dementia, the total amount of phosphates is diminished.

Melancholia.	Earthy phosphates.	Specific grav.	Alkaline phos.	Total.
Case 1	,67 per 1000 urine	1024.3	3.36	4.03
Case 2	"	1011.3	"	2.71
Case 3	,71	1025.9	3.08	3.79
Case 4	1.47	1027.9	2.34	3.81
Senile dementia.				
Case 1	,71	1021.0	2.10	2.81

Why in Case 4 the earthy phosphates should be so high, I am unable to say. The amount of phosphates in other respects present nothing remarkable. The spermatozoa and oxalate of lime in the case of dementia are of some interest.

Conclusion.—From the five cases of "general paralysis of the insane," no very certain deduction can be made. In four of the cases the disease was in an early stage. In two of these four the total amount of phosphates is diminished; in the other two the phosphates are about the natural quantity. As regards the earthy phosphates, there is certainly no increase in their amount in the four analyses here given. The fifth case had been for many years in St. Luke's Hospital. The decomposition of the urine was probably the cause of the low specific gravity, but this would not have altered the amount of alkaline phosphates, which are certainly much below the healthy quantity.

The amount of the phosphates varies in the different cases remarkably; far too much to admit of accurate deductions from so few analyses. General paralysis being a chronic disease, I do not expect that even a very extended inquiry will give any positive results, and it is on this account I would rather direct further attention to those cases of insanity in which acute paroxysms occur, such as cases of mania. Of the four cases of mania in which I examined the urine, the first is the most interesting, because in it I think there is evidence of that increase of the amount of phosphates excreted during a paroxysm, which I hope further researches will confirm; when the patient was convalescent, the amount of phosphates is seen to be much diminished.

In two other cases of mania, in which there were no acute symptoms, the amount of phosphates is so much diminished that it approaches closely to that diminution of the phosphates which I have observed in some cases of delirium tremens. This point also requires a far more extended inquiry. Are there two states of mania—one, in which the phosphates are increased; the other, in which they are diminished? In delirium I shall show the probability of the existence of two such states. In mania, it seems reasonable to expect that the phosphates would be increased during the paroxysm, but the diminution of their amount, if proved, would be of equal interest. At present, however, the facts want to be proved, and it is desirable to do no more than notice the distinction for the purpose of directing inquiry to the subject.

The four cases of melancholia on which my analyses were made, give no marked results: all were recent cases. The contrast between the amount of alkaline phosphates in the last case of melancholia and the first case of mania, is, perhaps, worthy of observation.

The whole of these cases of insanity may, for comparison, be included in one table, thus:—

General paralysis	Earthy phosphates.	Specific gravity.	Alkaline phosphates.	Total.
Case 1	1.50 per 1000 urine	1028.6	5.40	6.90
Same case	1.17	1023.3	2.97	4.14
Case 2	,79	1022.0	1.23	2.02
Case 3	,41	1016.6	5.36	5.77
Case 4	"	1018.3	"	1.30
Case 5	"	1006.7	"	1.35
Cases of mania.				
Case 1, during attack .	1.32	1029.3	7.58	8.90
Ditto convalescent .	,67	1020.0	2.44	3.11
Case 2	,42	1023.3	4.28	4.70
Case 3	"	1025.9	"	1.26
Case 4	,74	1015.3	,38	1.12
Ditto	,72	1015.9	,46	1.18
Cases of Melancholia.				
Case 1	,67	1024.3	3.36	4.03
Case 2	"	1011.3	"	2.71
Case 3	,71	1025.9	3.08	3.79
Case 4	1.47	1027.9	2.34	3.81
Senile dementia.				
Case 1	,71	1021.0	2.10	2.81

Original Papers.

NOTES ON THE AMOUNT OF MATERNAL MORTALITY ATTENDANT UPON PLACENTAL PRESENTATIONS, &c.

By J. Y. SIMPSON, M.D.,

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Almost every page in the past history of medicine shows that when any improvement in practice has been suggested, it has generally, at first, been met, in some quarters, with eager and strenuous opposition; and, in most instances, a considerable period of time has elapsed before it was finally allowed to settle quietly down into the form of a recognised and established truth. Among the regular routine of adverse arguments invoked on such occasions, one chief and special ground of objection urged against almost all innovations in practice, has consisted in a strong averment, that the old form of practice which it was intended to displace, was itself so satisfactory and successful as not to stand in need of reform; and all attempts to prove that the "old method" was relatively more dangerous and fatal, has usually encountered stern denial and anxious contradiction.

"It is commonly stated, (observes Dr. Gregory,*) that one-fourth of those who are attacked by small-pox in the natural way, perish." The same authority states, that while natural small-pox thus proves fatal in the proportion of one in four, inoculated small-pox proves fatal in the proportion only of one in 300 or 400. Yet at the time at which inoculation was proposed, with the view of decreasing the horrors and fatality of natural small-pox, Wagstaff, Howgrave, Sparham, and other authors of the period, rose up in virtuous indignation at this "new," "rash," "strange," "unsafe" innovation of inoculation, maintained that "natural small-pox very rarely affects life," and that the new practice should be resisted and suppressed, as "uncertain," and "dangerous," and "indefensible." And such, indeed, is one part of the line of argumentation regularly urged against almost every innovation in practice, whether it be in medicine, or in surgery, or in midwifery.

In suggesting, some time ago, a new plan of treatment for some of the most dangerous and fatal varieties of unavoidable hæmorrhage, merely by separating, and, if necessary, extracting the placenta, instead of extracting the child, I was perfectly aware that strong opposition would, of course, be offered to this proposed innovation; for (to use the language of Dr. Lee against it) it was no doubt "departing from the rule (of turning the child) which has been established in the treatment of cases of placental presentation for the last two hundred years," and "subverting the established rules of practice in the treatment of cases of such vital importance."† Nor did it require any sagacity to foresee, that one ground of opposition which would be anxiously taken up, and stoutly maintained, would consist in the old allegation, that "the old practice" was itself comparatively safe, quite sufficient, and quite successful.

When I first published upon the subject, I adduced a long series of data which led me to believe that by "the new practice" not more mothers would probably be lost than one in twelve or fourteen; whilst I endeavoured to show, from Dr. Churchill's researches and my own, that the common maternal

* Cyclopædia of Medicine, Article, "Small-pox," vol. iv. p. 740.
† Medical Gazette for September 9th, 1846.

mortality in cases of unavoidable hæmorrhage was about as high as one in three. When speaking of this point in some remarks on the treatment of placental presentations, inserted in *THE LANCET* for May 8th last, in answer to some previous observations of Mr. Barnes, I took occasion to observe, that, "in proof of the maternal mortality under the old and recognised forms of practice being greatly higher than under the proposed plan of the extraction of the placenta before the child, Mr. Barnes refers, apparently with some hesitation, to the statistics collected by Dr. Churchill and myself, as showing that one in every three mothers was usually lost in placental presentations. Among 174 cases of unavoidable hæmorrhage collected by Dr. Churchill, forty-eight mothers died. I have now before me a carefully collected list of 654 cases of placental presentations, reported by Mauriceau, Portal, Giffard, Smellie, Rigby, Clarke, and Collins, Schweighauser, Lachapelle, Drs. John and Francis Ramsbotham, Lee, Lever, and Wilson. Among these 654 cases, 180 mothers died, or 1 in $3\frac{2}{3}$. In corroboration of the correctness of the statistical view which Dr. Churchill and I have taken of the extent of maternal mortality in unavoidable hæmorrhage, I would further beg to refer Mr. Barnes to the observations of Dr. Robert Lee. In his "Midwifery Lectures," (pp. 370-1,) published in 1844, Dr. Lee states a number of statistical facts regarding uterine hæmorrhage from placental presentations, and, amongst other matters, he mentions the result to the mothers in a considerable number of cases. I shall throw all his evidence on this last point into a tabular form.

Maternal Mortality in Seventy-two Cases of Placental Presentation, noted by Dr. Lee.

Reporters.	Number of Cases Reported.	Number of Mothers Lost.
Dr. Clarke	14	1
Dr. Collins	11	2
Dr. Ramsbotham	19	8
Dr. Lee	38	14
	72	25

"Hence, according to Dr. Lee's collection of statistics, the maternal mortality in unavoidable hæmorrhage, amounting to twenty-five in seventy-two cases, is rather more than one in three. And this evidence of Dr. Lee will probably be regarded as the stronger, seeing that it is totally unprejudiced in its character; for in 1845, Dr. Lee called into doubt the accuracy of all collections of statistical data made by others, and which led to the idea, that the general maternal mortality in unavoidable hæmorrhage was so great as to approach one in three. At that time he was, I believe, unaware of the general result of his own previously published collection of statistical data relative to the point in question." (*THE LANCET*, vol. i. for 1847, p. 480.)

The last number of *THE LANCET* (Saturday, Aug. 14th) contains a communication signed "A B," and entitled "Statistical Errors respecting the Mortality in Placental Presentations." The communication is written with the object of proving to the profession, that little or no "value" can be attached to the method of determining the former mortality in unavoidable hæmorrhage adopted in the preceding extract, because, by a simple error of the press, (which I had no opportunity of correcting,) there are printed in the above table, opposite Dr. Clarke's name, the numbers 14, instead of 4. The objection and remarks of "A B" are so strange, that perhaps they deserve no such formal refutation as the present; but I notice them, lest, by possibility, they may deceive any members of the profession that happen not to examine into their character; and, besides, they are sufficiently interesting illustrations, when examined, of the weakness and recklessness of the measures to which the writer is now compelled to have recourse, in order to gain any degree of plausibility for the adverse view which he is so anxious to advocate.

No one truly desirous to arrive at the simple truth would assuredly have found any difficulty in ascertaining that the mistake alluded to (the printing of 14 instead of 4) was an error of the press only; for the final summing up of all the cases, in the concluding line of the table, is "72," and not 82, (which last it would have been, provided Dr. Clarke's cases had been written by me 14 instead of 4.) But more: The total sum, "seventy-two," is printed twice over, not in figures only, but in letters, and at full length—viz., in the deduction immediately following the table, and in the heading or title of the table itself; so that a mistake was almost impossible on the part of any one examining the matter with common care and attention.

Of all this "A B" seems to have been most fully aware,

and he has consequently been constrained to resort to one or two ingenious expedients, in order to make out the appearance of a *causa culpa*. For, first, he has *suppressed* altogether the last line of the table—viz.,

"72 25,"

—so far obliterating the proving figure, 72. *Secondly*, he has carefully omitted the deduction from the table, in which it was mentioned that the whole cases amounted to "seventy-two." And *thirdly*, he has altered the title of the table from "Maternal Mortality in Seventy-two Cases &c.," to "Maternal Mortality in Seventy-five Cases &c."

I humbly opine, that such extraordinary literary proceedings, in the way both of the *suppressio veri* and the *suggestio falsi*, really require no commentary; merely to point them out is sufficient. I willingly and silently leave in the hands of an honourable profession the verdict which should be pronounced upon such conduct. And I feel assured that most will agree with me in one essential point—namely, that any cause, such as the alleged non-fatality of unavoidable hæmorrhage, which, for its support, stands in need of the kind of argumentation followed by "A B," must be truly in a very desponding and desperate state; the weakness (not to use a stronger term) of the argument sadly betrays the weakness of the very point argued for.

Further, the author of the remarks I am commenting upon, "A B," asks if I believe Dr. Lee's table or my own the more "correct." Now I by no means cited Dr. Lee's statistics under the idea that they were at all minutely correct. I cited them as a very apposite illustration of the paradox, that, in the course of medical reasoning, a man's own theory may be sometimes flatly and unwittingly contradicted by a man's own facts.

In the table I have constructed from Dr. Lee's data, (see above,) three series of placental cases are included: 1, those of the Dublin Hospital, reported by Drs. Clarke and Collins; 2, those published by the late esteemed and venerable Dr. Ramsbotham, in his "Practical Observations;" and, 3, those reported by Dr. Lee himself as having occurred in his own practice. The returns of the first series are incorrectly given by Dr. Lee, as Drs. Clarke and Collins met with sixteen cases and four deaths,—not fifteen cases and three deaths, as stated by Dr. Lee. Again, Dr. Ramsbotham has recorded, in his chapter on unavoidable hæmorrhage, twenty-one cases of this complication, with eight deaths, instead of sixteen cases and eight deaths, as reported by Dr. Lee. Lastly, very probably, Dr. Lee's own account of his own cases is wrong, but I have not his "Lectures" near me at this moment to ascertain. At all events, however, two years ago I had occasion to point out to him that he had committed various errors in summing up his own placental cases and their results; and especially that the number of instances of placental presentation which he had then seen and published was forty-six, and not, as he erroneously averred, forty-five; and that out of twenty-four cases of turning in unavoidable hæmorrhage, detailed by him as having occurred in his private and consultation practice, ten of the mothers died, (1 in $2\frac{4}{10}$), and not nine in twenty-four, as he alleged. In answer, Dr. Lee at that time published a table professedly containing "all the cases of placental presentation which he had ever seen, and of which he had preserved the histories;" and in it, in order to show his own reckoning of the number of his own cases (forty-five) to be right, and my reckoning of them (forty-six) to be wrong, he simply dropped out one of his own previously published cases from the table; while the proof of his number of fatal cases after turning being one less than I had found them, (nine instead of ten,) was attempted to be gained by a case being declared and entered in the aforesaid table as one of "Death before turning was performed by Dr. Lee;" the fact being, that he (Dr. Lee) had already twice published this same individual case—first in his "Clinical Midwifery," and subsequently in his published Lectures—as having been delivered of a living child by turning, and as not having died till "a few days" afterwards.* But in the meantime, in want of other evidence, let us take Dr. Lee's account of the cases in his Lectures as correctly stated at thirty-eight and fourteen deaths; then the total number of placental cases observed by Drs. Clarke, Collins, Ramsbotham, and Lee, should be seventy-five with twenty-six deaths, instead of seventy-two with twenty-five deaths. The maternal mortality from Dr. Lee's data, as given by himself, amounts to 1 in $2\frac{5}{10}$. The

* For full particulars of these and other similar revelations regarding various "A B" mis-representations in the question of the statistics of placental cases, I beg to refer any reader who takes an interest in so very disagreeable a subject to the *Medical Gazette* for November 7th, 1845 p. 1193, &c.

corrected data afford as nearly as possible the same result of one death in every 2-³/₁₀ of the mothers.

In the paragraph which I have quoted above, from a paper of mine on the principles of treatment in unavoidable hæmorrhage, contained in THE LANCET for 8th May last, I have stated, that at that time I had a carefully-collated table of 654 cases of placental presentation. I here will subjoin this table, under the hope, that if it contains any error or errors whatever, which "A B" or any other reader may chance to detect in it, they will kindly point them out, either in the pages of THE LANCET, or otherwise. I am anxious to have it freed from all possible mistakes before I publish it, in a more permanent form, as it is my intention to do, when time and other engagements will permit. And in seeking the aid of others in this investigation I trust my object will not be misunderstood. I do so with the sole purpose with which I hope every honourable mind in the profession is actuated, the ardent desire, namely, of ascertaining the truth and nothing but the truth: and certainly I shall feel obliged, deeply obliged, to any one who will aid me, in a proper spirit, in arriving at perfect accuracy in an inquiry in which there are confessedly no few obstacles and sources of error to overcome. So far as the figures and data of the past and present table go, they fully bear out what I stated two years ago, when writing on the same subject, that the corrections in it "do not alter in any practical degree the statistical result, regarding the amount of mortality among mothers in placental presentations. More extensive data than I have access to may alter that result, but probably not to any marked amount."

Table of Maternal Mortality in Placental Presentations:—

Reporters.	Number of Cases.	Mothers lost.
Mauriceau	18	3
Portal	14	1
Giffard	19	5
Smellie.....	18	7
Rigby	42	10
Clarke and Collins.....	16	4
Busch	13	2
Schweighauser	65	16
Lachapelle	21	10
J. Ramsbotham	129	41
F. Ramsbotham	189	49
Lever	34	8
Lee*	46	14
Wilson.....	30	10
Total	654	180

According, then, to the evidence of this table, out of 654 cases of unavoidable hæmorrhage, this complication was fatal to the mother in 180 instances, or one in every three or four of the mothers (1 in 3-⁶/₁₀) perished in connexion with it.

In reference to the construction of the preceding table, as originally published in the *Monthly Journal of Medical Science* for March, 1845, and here corrected and reproduced, I beg to state the following necessary particulars:—

1st.—The table (as I have already elsewhere remarked) was made to show the maternal mortality, in all varieties of the complication, and under all modes of treatment, whether the membranes merely were ruptured, or the child turned, or the placenta spontaneously expelled, or the mother sunk without aid of any kind.

2nd.—In collecting its materials, I proceeded rigidly upon the principle of only entering upon it the results of the practice of those individuals or institutions upon whose records I could find ten or more cases of unavoidable hæmorrhage. In this way I believed I would be more certain to arrive at an accurate statistical result, than if I made my calculation upon the collection of cases of a smaller number, scattered throughout our medical journals. I noted down all the lists of instances I could detect, in which ten or more cases were reported. In the table, as originally published, I erroneously omitted Paul Portal, because I inconsiderately relied on Dr. Lee's accuracy, when, in his "Clinical Midwifery," he stated that Portal's work contained an account of "eight" cases only of unavoidable hæmorrhage. And this instance shows the difficulty of obtaining all the desirable accuracy regarding the cases even of an individual author. For while, in 1842, Dr. Lee publicly declared Portal's treatise to contain an account of "eight" cases only of placental presentation, (see "Clinical Midwifery," p. 140;) in 1844 he declared it to contain "eight or more" cases, (see published "Lectures on Midwifery,"

* Dr. Lee has, since this table was finished, published some additional cases, but I have not the means, at present, of ascertaining either their number or result.

under hæmorrhage;) and latterly, in 1845, he declared it to contain "eighteen cases of placental presentation," see *Medical Gazette* for 1845, p. 1022.)

3rd.—In the table, as printed above, I have endeavoured to correct all the returns as assiduously and carefully as possible; and in several instances I have been able, from more lately published data, or private notes, to extend the observed number of cases of some of the authors or practitioners whose returns I originally gave in the table, as it first appeared in the *Monthly Journal of Medical Science*. I have avoided, for the present, including any returns from authors who have published for the first time on the subject, since the date of the first appearance of these researches; for though several of them would, in relation to the number of their cases, come properly within the range of the table, yet the general result might be rendered by them so far partially inaccurate in one respect, from the fact that some of these returns (as those of Mr. Newham and Mr. Russell) were published mainly, or merely because they happened to show a success greater than usual.*

The preceding remarks apply to the maternal mortality attendant upon placental presentations before the "new practice" was proposed. There is another related subject which has been latterly discussed, in some degree, in the pages of THE LANCET and elsewhere, and on which I would beg to make a few remarks. I allude to the mode or modes in which the propriety of the new practice in unavoidable hæmorrhage should be statistically proved or disproved, and the grounds on which, so far, the practice itself should be accepted or rejected.

PROPOSED MODE OF INVESTIGATING THE PROPRIETY OR IMPROPRIETY OF THE NEW TREATMENT.

At the late anniversary meeting at Derby, of the Provincial Medical and Surgical Association, Mr. Jackson, of Sheffield, in speaking of the treatment of placental presentation, observed, that "he thought they had not yet had sufficient experience of Dr. Simpson's plan, to pronounce correctly an opinion of whether it was right or wrong, and he thought, at the present moment, they could not decide upon that point of practice. They must wait to see the effect patiently, and if every gentleman present would but record the cases in which a failure occurred by the adoption of the plan proposed, it would enable them to form a good judgment of the propriety of bringing Dr. Simpson's plan into general use."

I copy these observations from the report of the proceedings of the meeting published in the *Provincial Journal* for August 11th. And whilst I do believe that the Association contains within it a ready-made machinery, which, if properly worked, might easily be made to go far to settle statistically both this and many other important points in practice, yet I entirely protest against the possibility of deciding the present matter in the way proposed by Mr. Jackson. It is assuredly not by reporting and counting the "failures" alone, that the propriety or impropriety of any new practice can be justly or rationally determined. It is by rigorously reporting and counting all the failures, as well as all the successes,—all the cases which end unfortunately equally with all those that end fortunately,—that data for a proper and impartial judgment can be alone obtained. The proportion or ratio of the successful to the unsuccessful cases will yield us the result we require. If a new method, for instance, of performing lithotomy were proposed, and in order to test its safety and propriety, as compared with the present established methods, the "failures" only were collected, would the knowledge of them alone yield any accurate or decisive result? Certainly not. It might be reported, for example, that fifty deaths, or "failures," had occurred under this new method, but that re-

* I am too far away, at present, from proper books and notes, to be able to state, with perfect precision and accuracy, all the later returns referred to in the text; but among them I remember those of Dr. Merriman, (consisting of eighty-nine cases of placental presentation, with twenty-two maternal deaths;) of Schroerer, (fifteen cases, with five deaths. of Dr. Russell, with, if I recollect aright, seven deaths in thirty-six cases: of Dr. Campbell, with twenty-two cases, and only one death; and of Mr. Newham, (thirteen cases, with one death.) These several cases amount to 175 in all; and out of these 175, there were lost, thirty-six mothers, or nearly one in five. Again, adding together the two sets of cases,—namely, those in this note, and those in the table in the text, we have 829 cases in all, with 216 maternal deaths, or nearly one mother lost out of every four, (1 in 3-⁶/₁₀.) Dr. Lee, two years ago, wrote to show that I had misrepresented Dr. F. Ramsbotham's cases, by inadvertently substituting the number of children lost for the number of mothers lost. After the matter was fully explained, Dr. Lee repeated the allegation over and over again. It is, of course, also now repeated again in the communication signed "A B." Formerly, at the very time that Dr. Lee was declaiming against my inaccuracy on this point, he himself fell into a much worse error; for, in first giving Dr. Merriman's eighty-nine cases of placental presentations to the world, he published the deaths of the mothers as sixty-seven, and the recoveries as twenty-two,—instead of the deaths as twenty-two, and the recoveries as sixty-seven.

turn would give us no information by which to judge of it as compared with other plans of lithotomy, unless we knew the number of operations in which these fifty deaths had occurred, —unless, in other words, we knew all the cases of its success, as well as all the cases of its “failure.” But suppose we had reported to us both sets of facts—both the cases of the new operation which ended fatally and those which ended safely—then we are in a condition to judge of its claims. If, for instance, the fifty cases of “failure” had occurred out of 1000 operations in all, then the result of one death in every twenty operated upon would be a result so far above the average of the common modes of lithotomy, as at once to show the superiority of the new mode. If, again, the fifty cases of “failure” had occurred out of 100 operations in all, then the result of one death in every two operated upon would, *cæteris paribus*, be a result so far below the average of the common modes of lithotomy, as at once to discountenance the new practice.

In THE LANCET for the 22nd of May last, page 546, an anonymous author, “Auditor,” attempted a method of settling this question that was more characteristic and peculiar. “Auditor” states his confident belief “that fatal results must have repeatedly followed the extraction of the placenta before the child.” “It has (he writes) for some months been my confident expectation that a coroner’s inquest would be the means by which the truth would be divulged. This, however (he continues) has *not* been required to confirm my suspicion, for a practitioner of candour and veracity, [near Lisson-grove,] who hastily, and without reflection, had adopted the lately promulgated unsound views respecting the treatment of unavoidable uterine hæmorrhage, has met with a case that terminated fatally, and in which there was *every reason* to believe that the result would have been widely different, had the established rules of practice not been abandoned or violated in the case.” “Auditor” then goes on to describe how, in this case, the accoucheur had “peeled off the afterbirth without any great difficulty;” that, after a time, “the patient stared,” and the feet were brought down, “but the woman was dead before the head of the child could be extracted.”

Now, mark the true state of the fact, and the sequel!

In a subsequent number of THE LANCET, (Saturday, June 11, page 686,) Mr. Harding, the gentleman in whose practice the fatal case commented upon by “Auditor” had occurred, published a correct statement in regard to it. The case was one of complete placental presentation, and his patient had died after turning was performed, and before the head was extracted. But in treating it he had, to use his own words, “performed, without any great difficulty, the *old* operation” of turning, and not followed the new practice, as averred by “Auditor.” “I did *not* (observes Mr. Harding) first remove the placenta, and then extract by version, or attempt it, or even *propose* such a proceeding to my own mind.”

Most members of the medical profession, after publicly making to their brethren so very strong and very incorrect a statement as that indulged in by “Auditor,” would assuredly have offered a speedy apology for their previous strange and indecent defiance of common accuracy. Hitherto, however, none such has appeared. Nay, in the Index to the last volume of THE LANCET, stitched in and circulated with the last week’s number of the journal, under the head “Placenta Prævia” occurs this entry,—“Taken away before the child, fatal flooding from, p. 546.” This reference to “Auditor’s” allegations is not followed by any reference to the correction of these mis-statements of the case, as subsequently given by Mr. Harding himself, at p. 686. The Index so far avers, that a case of unavoidable flooding from taking away the placenta before the child, was recorded in the pages of the volume. But the pages of the volume record that the fatality thus alleged to have occurred from this practice, did assuredly *not* occur from it. The erroneous mis-statement is persisted in, the correction is omitted.

It is abundantly evident, that if medical records are to be allowed to be perverted in the way attempted by “Auditor,” our chances of arriving at correct medical conclusions would doubtlessly be very slight. But assuredly a happier and healthier spirit of investigation pervades and actuates the profession than that which I have most reluctantly, and in self-defence, felt myself called upon to expose in the course of the preceding notes. And, in conclusion, I would take the liberty of adding one remark—viz., that of the different anonymous attacks which, during the last few months, have appeared against me, and the modifications of practice I have proposed or followed, in the pages of THE LANCET, under the various “aliases” of “Spectator,” “A B C,” “Auditor,” “A B,” &c.; one and all of them could be easily shown (provided it were

worth the waste of time and space to do so) to contain the same peculiar distortion of facts and the same distortion of reasoning as characterize those two communications of “A B” and “Auditor,” which I have here more specially noticed. Lately THE LANCET has been doing good service to the practice of the profession, by denouncing all patent medicines as founded upon a system of improper and unworthy concealment. Let it take one step more in the right direction, and do a similar good service to the literature of the profession, by renouncing all anonymous attacks, as equally founded on the same system of improper and unworthy concealment. For if any gentleman has aught in any degree suitable or valuable to communicate to his professional brethren, in the way either of objection, of criticism, or of observation, surely if it is written out in a right and truth-seeking spirit, he need not hesitate to append his name to it. If the statements are so unfounded or unjust as to render it derogatory for a man to affix his signature to them, then, as certainly, they form, at the same time, for the same reason, no proper articles for filling the valuable space and pages of any medical journal.

Kilmun, Argyllshire, Aug. 26th, 1847.

Postscript.—Since writing the preceding remarks, I have seen the last number of THE LANCET for August 21st. The number contains (p. 213) another letter to the editor, which, editorially, it has been deemed right and proper to publish. The letter is principally remarkable for the evidence of lusty and hearty hate that the writer, it seems, entertains towards me, and towards Drs. Rigby and Sharpey; and throughout it betrays an amusing amount of consternation and alarm lest my hitherto very imperfect efforts to improve different parts of obstetric medicine should really, after all, have some such effect,—deprive the uterus of “its nerves,” and force back “the oldest and most experienced accoucheurs to College.” The author—“Irons”—indulges himself in very much vapid ire and impotent irony upon the subject of the uterine sound—forcibly calling back to one’s remembrance the days (not long past) when the stethoscope was roundly ridiculed as a mere mechanical wooden tube, a boxwood trumpet, &c. He mentions, and, no doubt, abhors, etherization in midwifery; but he may make his mind assured, that it *will* come into general use, if not in our days, at least in the days of our successors; and that, too, in despite of the neurotic discovery announced some months ago, in THE LANCET, by Dr. Tyler Smith, or Spectator, that it must prevent parturition, by (I write in earnest, not in jest) preventing the proper opening of the orifices of the urethra and rectum in the last stage of labour. It appears, further, that “Irons” is specially horrified with the modes of practice which I have followed, for some time past, in the more aggravated forms of amenorrhœa, of dysmenorrhœa, and of leucorrhœa; such as, in the first, exciting (when necessary) the menstrual secretion, by exciting the membrane which secretes it by the direct application of nitrate of silver. But I can sincerely assure him (and from the results of many, many cases) not only of the success and facility, but also of the perfect safety, both of this treatment, and of that dilatation and division of the os uteri for obstructive dysmenorrhœa, which he seemingly looks upon with no small “nervous” dread. The “barbarous inhumanity,” however, which of all others, apparently, according to his own confession, most particularly shocks “Irons,” is the employment of caustic potass for the purpose of removing that very frequent, often very distressing, and usually very obstinate disease—chronic inflammatory induration and ulceration of the cervix uteri. (“Irons” writes that it is for “the demolition of the os uteri;” but, of course, most know that the os and cervix uteri are different.) The expression—“barbarous inhumanities—necessarily on the lips of “Irons” matters little; for the very simplest and safest practices are always at first denounced in such terms by some men—vaccination itself having at one time been thus decried as “a gross violation of morality and humanity, &c., &c.” But I must say that, as far as the practice of reducing inflammatory induration by potassa goes, the term I have quoted is most oddly and strangely misapplied, unless by the designation, “barbarous inhumanity,” (all terms being merely conventional,) “Irons” means, what is the fact—viz., 1st, curing a patient in the space of weeks, instead of,

* I take the signature appended to the letter—“I. Irons, M.D., F.R.P.S.E.”—to be an assumed name, and I think it might be easily possible to show, by grammar, expressions, and otherwise, a very probable identity between “Irons” and the anonymous writers or writer that I have previously mentioned. At all events, one of two things holds good. 1. If the name is a true name, then the assumed title—F.R.P.S.E.—is false, there being, I believe, in reality, no such title in existence. 2. If the name is a false one, then the whole affair of deliberately placing it, or allowing it to be placed, to an article, as if it were a true name, only, I opine, renders the whole case worse and worse.

as formerly, in the space of months or years, or not at all; 2ndly, curing her without pain or suffering, (for certainly the general rule is, that the application of potassa fusa to an indurated and ulcerated cervix, in the mode I have described, gives, in reality, little or no pain;) and 3rdly, curing her in a manner that gives comparatively little or no trouble to either practitioner or patient, many patients not requiring the confinement of an hour for the treatment, and none suffering any notable consequences whatever from it, except the speedy restoration of their local and general health. It is of these "barbarous inhumanities," and the like of these, that I plead guilty. And latterly many esteemed members of the profession have not only gladly witnessed these same "inhumanities," but gladly (for the good of their suffering patients) practised the repetition and perpetration of them.

Certainly THE LANCET was, of all journals, the one which I might, *à priori*, have least expected to open its pages to such an absurd and fervent protest as that of "Irons," *versus* the use of caustic by me or others, in the treatment of induration of the cervix uteri; for, only two or three years ago, the pages of THE LANCET, and the former very talented sub-editor of it, Dr. Bennet, were the first to point out properly to British practitioners the great advantages and entire safety of this very same treatment, in this very same disease. Can it possibly be that THE LANCET, like "Irons," has latterly become anxious that, in this country, the progression of obstetric medicine should now be, like the progression of the crab—backwards?

ON THE EFFECTS OF SEA-BATHING AND CLIMATE.

By THOMAS HUNT, Esq., M.R.C.S.E., Herne Bay.

"The directions for sea-bathing are in general insufficient."

JOHN HUNTER.

THE COLD SEA-BATH

Is a therapeutic agent, not only of immense power, but it admits of no substitute: it will accomplish what no other remedy will effect. On the other hand, a cold plunge, indiscreetly resorted to in visceral disease, may prove to be a plunge into eternity. For these reasons, the cold sea-bath should only be used (by invalids) under special medical direction. Nor is any medical practitioner justified in giving directions for its use, unless he have a practical acquaintance, not only with the morbid conditions which it is calculated to rectify, but with all the circumstances which indicate or contra-indicate its use,—with the requisite dose or frequency of repetition,—with the signs of an over-dose or excessive use,—and with the indications for perseverance in its use on the one hand, or for its discontinuance on the other. All this is considered essential in administering a drug, and it holds with equal force in reference to a bath. Yet the use of the cold bath, as a remedy for disease, is perhaps less understood by the profession than any agent of equal power. The conditions of its administration, not less than the effects of its abuse, are alike subjects on which erroneous views extensively prevail.

There are reasons for this deficiency of knowledge which go far both to explain and to excuse it. In the first place, a comparatively small number only of the practitioners of these islands have opportunities of witnessing and watching the operation of the cold sea-bath—those, namely, who are located at marine watering-places. Their inland brethren cannot be expected to take much interest in the study of a subject which to them must be purely theoretical. Nor is it at all necessary that they should do so. I only entreat them, as they value human life, to abstain from prescribing, at random, a remedy of which they cannot understand the value; and to admonish their patients visiting the coast, not to consign their health to the care of the ignorant proprietors of bathing machines, or their more ignorant "guides." As well might a lancet be plunged into the vein of a patient, at the dictum of the instrument-maker, or calomel be administered under the advice of the laboratory man, as for such persons to have the direction of this powerful remedy. So far as disease is concerned, every bathing establishment should be under medical superintendence. And as every watering-place may be supposed to contain medical practitioners who are practically acquainted with the subject, there can be no excuse whatever for the encouragement of the form of hydropathic quackery above described.

The application of cold sea-water to the surface of the body may be either partial or general. The general bath—the whole body being immersed—is useful, first, as a stimulus to

the skin, restoring its functions when they are deranged; secondly, as a stimulus to other organs which sympathize with the skin, their functions being imperfectly performed; and thirdly, as a general tonic to the whole system. When the skin is the subject of inflammatory disease, in whatever form of eruption, benefit is rarely to be expected from immersion in the sea. On the contrary, I have generally found the irritation increased by it, especially in squamous affections. It is sometimes useful where disease occurs in the asthenic type, as in ecthyma; but here the bath acts rather as a general tonic than as a local stimulus. Where the circulation is feeble, and there is either a relaxed condition of the capillaries, with excessive perspiration, or a deficient exhalation from inaction, a cautious use of sea-bathing is often found highly beneficial. Upon the whole, however, the main advantage of sea-bathing is seen in cases of constitutional feebleness, in strumous habits, in dyspepsia, in a low, dormant condition of the nervous system, and in habits generally relaxed from excessive fatigue, anxiety, fevers, or other causes. Its operation in these cases is well marked, especially when tonics, country air, and other restoratives have failed. The reputation of this remedy in these depressed conditions of the system will rise much higher when its administration shall have been brought more completely under the regulation of enlightened management, rather than committed to the vague chances of empirical routine.

The symptoms which specially indicate the use of the cold sea-bath are those of general debility, unattended with organic disease;—a general and prevailing sense of lassitude, aching pains in the back or limbs, low spirits, a languid circulation, and defective appetite. After a plunge into the sea, the patient experiences a refreshing sense of returning vigour, a general glow on the surface, cheerful spirits, an improved appetite, and a corresponding relief to the muscular pains.

How is this benefit to be explained? Possibly, too much credit is often given to the salt contained in the water. The quantity which can enter the system by absorption must be far too small to be effective. Twenty times as much is generally swallowed at each bath, without producing any sensible effect on the stomach. Indeed, if there were any intrinsic efficacy in the salt, a salt-bath might be procured in London, or elsewhere. The grand benefit of sea-bathing, in contradistinction from fresh-water ablutions, results from the circumstance that sea-water is in the locality of the sea-side, where the purity of the air, (and, perhaps, its impregnation with iodine and muriatic acid gas,) the relaxation from the toils of business, and the enervating habits of civic or fashionable life, and the thousand-and-one effects of change of air, all tend to promote the general health. So much advantage, indeed, is often derived from these sources, even when bathing is not practised at all, that it is, in honest truth, extremely difficult to determine exactly what share of credit the bathing can actually claim, when it is had recourse to. Yet, as the patient feels particularly invigorated immediately after the bath, it is but fair to conclude that it has done him good. In what way it acts on the physical frame I cannot explain. It first checks the cutaneous circulation, and then, by reaction, quickens it: but why should this be beneficial? It is sufficient for practical purposes, to reply, that good generally comes of it under certain conditions. What are these conditions?

The bath is sometimes prejudicial. Chilliness may succeed it, instead of the well-known salutary glow. Cold extremities, livid lips, a pale surface, pain in the chest, palpitation of the heart, headach, giddiness, bleeding at the nose, and suppressed secretions,—all these symptoms—the symptoms of an over-dose—I have repeatedly observed after cold bathing, even in cases in which the patient was previously in good health, or at least in a condition favourable for the cold bath. It is more easy to explain why the bath is sometimes hurtful than why it should ever prove so highly beneficial as it frequently is.

Water is not the natural element of man, and if the bath is continued too long, the insensible perspiration is checked, the circulation is embarrassed, the internal organs suffer from congestion, and more serious consequences sometimes follow. For any medical purpose, five minutes is long enough for the patient to remain in the water. If, from motives of luxury, or for the purpose of learning to swim, the bathing period is extended to ten, twenty, thirty, or sixty minutes, the risk is in proportion to the time.

Even the most hasty dip sometimes produces nausea, headache, and other unpleasant symptoms. Where this does not proceed from some previous affection of the digestive or other organs, (in which case it ought to have been professionally interdicted,) the mischief probably arises from some error in the mode of taking the bath.