PRACTICE IN THE PREDICTION OF THE DAY OF CONFINEMENT.

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In this brief paper I propose to pursue a purely synthetical line of argument, in order to show the accuracy of conclusions regarding the best mode of predicting the day of confinement which I had arrived at by a different process, and which I published in the *Monthly Journal of Medical Science* for March 1854.

The following were the chief results then stated, with a view to the ordinary calculation in practice:—

That the average interval between the end of last menstruation, or extending from and including the first day after the cessation of last menstruation, to parturition, is 278 days.

That the day fixed upon by this calculation is not the natural day, but only the most probable day of confinement; and that, in consequence of its variations, it is better to avoid predicting any day, but to predict the occurrence of childbirth in a week of which the said 278th day is the middle, or very nearly so.

Since these conclusions were published in 1854, there has been considerable discussion of the subject both at home and abroad; and there can be no doubt that there is a general tendency among scientific inquirers to advance in the direction which I followed,—that is, tending to show that the average duration of pregnancy is shorter than older authors generally supposed.

Many still very erroneously write, and, to a great extent, reason as if the date of conception could be made out, and as if the date of a fertilizing coitus were the date of conception. It is surely unnecessary for me to go over this ground again; for not a single argument is adduced in support of these views, and they are known to be not only not demonstrated, but to be not in accordance with our positive knowledge.

The most elaborate recent paper on this calculation which I know is by Dr Ahlfeld, and is published in the *Monatsschrift für Geburtskunde* for 1869. His theoretic conclusions differ from mine chiefly in reducing the period of pregnancy from 275 to 271 days. But I am very far from being satisfied with his data, especially with his mode of getting assurance as to the date of insemination, or, as he erroneously calls it, conception. He trusts, in my opinion, far too much in the mere statements of the females. So much is this the case, that I am disposed still to adhere to my own figure of 275 days as the nearest approach to a correct statement of the average duration of pregnancy.

Dr Ahlfeld further tries to show that the majority of women are confined in the 39th week of pregnancy,—a statement quite in accordance with his previous conclusion regarding the duration of pregnancy, and, I need not add,

not in accordance with the view of the duration of this state to which I adhere.

But prediction of the day of lying-in is an important practical matter, from whose arrangement all theory should be excluded. It is a valuable calculation of a quite empirical kind. Its successful performance does not necessarily depend at all on correctness of views as to the duration of pregnancy.

We cannot count from the beginning of pregnancy, or conception, as Ahlfeld pretends to do, because in no case do we know the day or the week in which it begins.

We cannot, except very rarely, count from a single coitus, or coitus only on a single day, because such circumstances seldom occur, and because, even when they are alleged to have occurred, we can very seldom obtain satisfactory assurance of them.

We almost invariably count from the last menses. The end of last menstruation is generally taken as the point to count from; and this is a rational proceeding, because cohabitation is, as a rule, suspended during the flow, and the female is not liable to be impregnated till after it has ceased. But, as I have already said, the calculation is purely empirical, and might, as is actually done by Cederschjold and Berthold, be made from the beginning of menstruation just as well as from the end of it. I adhere to the old plan because it is the old and generally-used plan, and because, therefore, the data from which the method of calculating the day of confinement has been elaborated have been made out by it. Had we more numerous and more carefully collected data, based upon a system of counting from the beginning of menstruation, I should be ready to give up the old one and take the new one. Both systems yield the method of calculating on purely empirical, not on rational, grounds. Authors have committed grevious errors in vainly trying to combine empirical and rational grounds for this calculation. In the present state of science this is impossible. Only confusion can arise from so doing. There can be no

objection to authors deriving evidence from this calculation for or against propositions in science; but at present science can lay down no grounds for the calculation other than the records of experience. Ahlfeld is the ablest representative of such attempts, but in practice he comes in reality to simple dependence on the date of last menstruation.

In my former papers, to which I have already referred, and which form part of my work on Fecundity, I use the great recorded experiences of Merriman and Reid. Estimating by these, I find that the 278th day after the end of last menses is the average day of delivery at the full time; and on this I proceed. No ingenuity can devise a superior plan of estimating, so long as the last menstruation forms the only generally available *terminus a quo*. The introduction by many authors of scientific views into the question of the best way of predicting the day of confinement, may be justly characterised as either at least unnecessary or else merely pedantic. Till I find a larger and more carefully compiled mass of facts, than those of Reid and Merriman, I shall adhere to my method of calculating based on the circumstance that 278 days is the average interval between menstruation and parturition; and in doing so I have science and common sense on my side.

The method which I recommend is confessedly a rough one. The calculation itself is always what is called a rough one. My method certainly is loose and erroneous to the extent of one day in certain cases, which I have specified at page 340 of the first edition of my work on Fecundity already referred to. The plan is simply as follows:—Find the day on which the female ceased to menstruate, or the first day of being what she calls "well." Take that day nine months forwards as 275 days, unless February is included, in which case it is taken as 273 days. To this add three days in the former case, or five if February is in the count, to make up the 278. This operation is perfectly simple, and so easy of performance as to render a periodoscope quite useless.

Now, any practitioner can test this plan by his own experience, in a purely synthetical and reverse manner. He can try the plan, and then see how it has led him; whether it has led him and his patients into error or not. Since I adopted this reverse method of verifying my plan of calculation, I have found that Ahlfeld had already resorted to a similar test. It is only very slightly different from the method by which the plan of calculating was developed. The difference is stated as follows:— Cases of delivery collated yield results on which the plan is founded; instances of prediction compared with the real events test the plan.

I shall now show what my predictions on this plan have come to. No one can hope to be an absolutely good prophet in this matter, but we can be as good as possible, as nearly right as may be. The predictions to which I shall immediately make reference were all written down before the events, and remain written. I have only 153 cases to refer to, all collected within several recent years. They are few, because I did not venture on the written-down prediction unless I was satisfied that I got good information as to the day of the cessation of the menses.

I need scarcely repeat, that in practice I do not predict a day, but a week. I predicted a day in my note-book for my own use. These 153 predictions in my note-book I now analyse.

In 10 cases the day of confinement was exactly predicted, or about once in every 15 cases.

In 80 cases the confinement took place sooner than was predicted. The number of days of anticipation was, for the whole 80 cases, 590, or an average of above 7 days for each case.

In 63 cases the confinement took place later than was predicted. The number of days of protraction was, for the whole 63 cases, 535, or an average of above 8 days for each case.

In 63 cases, or more than one-third of all, the time of confinement was successfully though not exactly predicted,

the birth occurring not earlier or later than 4 days from the predicted day.

The average error was about 7½ days,—a circumstance which indicates that the prediction should not state the week of confinement but the fortnight of confinement, there being generally an error of a little above 7 days on the one side or the other of the ascertained average day.

But the most interesting result of these figures is the answer to the question, Can the calculation be improved? and the answer is, that it is, for practical purposes, perfect, or as nearly so as the present state of science permits. This near approach to perfection is shown, firstly, by the observation, that the errors on either side of the predicted day are nearly equal. If the errors on either side were exactly equal, then the calculation would be perfect; for it would thus be shown that, for the mass of cases, the exactly most probable day of confinement had been hit upon. In my 153 cases, the excess of error is on the side of anticipation. This excess is 55 days. Now, 55 days for 80 cases is less than a day of average error; and as our prediction does not pretend to even the accuracy of a day, the error may be truly regarded as trivial.

There is another, far more precise, and the only true way of analysing these or like results with a view to ascertaining whether the calculation I propose is the least erroneous possible. This method consists in ascertaining, not the average error on each side of the true point, but in observing the amount of error in each successive day on either side of the true point. This method, pointed out to me by Professor Tait, has been kindly also carried out by him, and I here give his note containing the details of it.

This note is of some value, even in connection with the small number of cases which I have for analysis by this method, which is founded on the theory of probabilities. Were my cases ten times as numerous, it would enable us to arrive at final results. But I make careful mention of the method here, chiefly because of its extreme value as a suggestion for the use of future investigators. When

applied to a sufficient number of instances, it forms the only exact means of testing any plan of calculating. It not only tests such plan, but gives, when worked out by

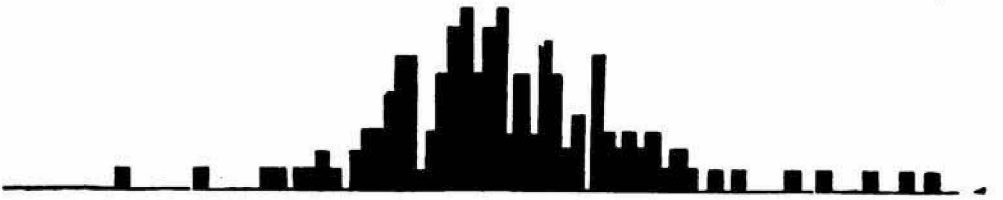

FIG. 1.—Showing Dr Matthews Duncan's data Graphically.

the method of plotting used by Professor Tait (*see* fig. 1), a correct view, at a glance, of all the errors in defect or in excess; and not only this, but also, with equal facility, a correct view of the importance of the errors. In addition to all these advantages of this method, which, so far as I know, has not yet been applied to the subject on hand, there is another, that, from a sufficient number of observations exactly made, it will enable us to elicit with certainty the true plan of calculating. It will not only show errors in an old method, it will show also how to avoid them—how to correct the old method.

The first figure here given is a mere ocular view of the errors. It puts the variations in my 153 cases not in a new light so much as before a new sense; not before the eye in written words, but before the eye in represented masses.

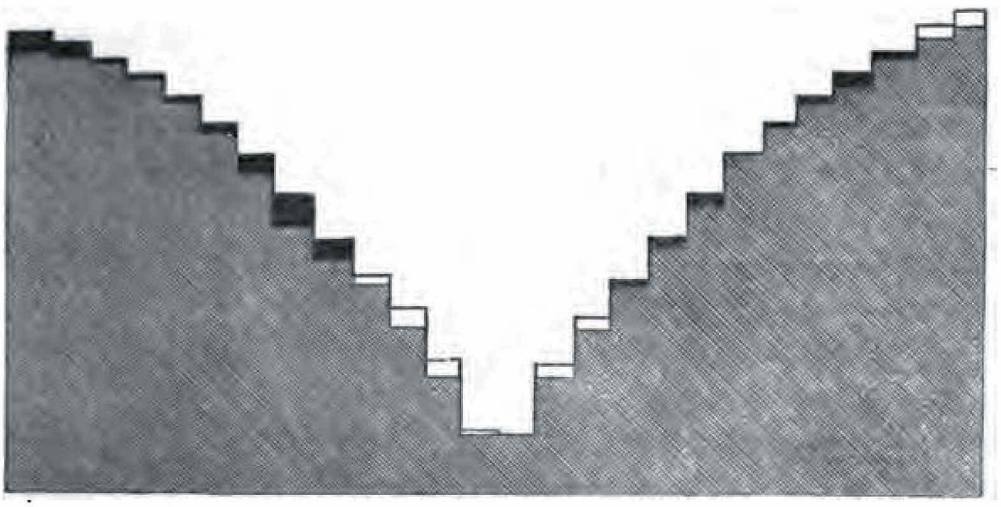

FIG. 2.—Comparison of the Data with the ordinary Law of Error.

The second figure is the important one. It requires a little explanation. The black and white parts are the errors which ought not to be—errors arising from imperfections in the plan of calculating. There must, of course, be many errors, in one sense, in these predictions of the day of confinement; but did the analysis of my 153 cases show no black and white, my plan would then be perfect. All avoidable error would then be eliminated. A different plan of calculating might be discovered, but the present plan could not be farther improved. All this can be demonstrated by the laws of probable error.

If my method of calculating were perfect of its kind, the figure would have no black and white. In order to recognise how it would then stand, the student must not merely erase the black and white parts, and put the gray in their place. The white or blank represents excess, and white must be simply erased—the white parts entirely removed from the figure. But black represents defect; black parts, therefore, are not simply erased or removed from the figure, but the black is erased, and the general gray colour put in its place. The figure, as it then would be, gives the correct amount of error—the inevitable error.

The general appearance of the figure shows that in my 153 cases of prediction, the amount of avoidable error is small. It gives at once such a view of the avoidable errors as would be very difficult and tedious to put into words.

Accumulation of cases will soon lead to the easy elaboration by this method of an absolutely correct method of calculating the term of a period whose length is indefinite.

"Your data," says Professor Tait, "though numerous in the sense of having been collected from your own observations, are rather scanty for the application of mathematical methods. I have, therefore, confined myself to a very simple species of interpolation, which seems to be sufficient to extract from them their most important contents.

"When the numbers are plotted, as in the first figure, we notice some strange irregularities, the most singular of which are actual *minima*—seven and fourteen days before, and eight and fifteen days after, your typical period. What these may mean (if they are *real*, and not due to mere defect of data) I cannot conjecture. If we suppose them due to defect of data, as I have no reason to doubt, there is still the curious fact that the errors in excess of the period are not merely more numerous than those in defect, but *they extend farther in time.* This must, I feel sure, be due to miscalculation on the part of some of the patients.

"By a tentative process, I find that all your numbers, irregular as they at first sight appear, with the exception of those last mentioned (which, for the reason given, I consider myself entitled to reject), accord fairly enough with the ordinary law of probability of error, provided we assign, as the true period, the second day before that given by your rule. Thus we obtain the following series, which is graphically represented in the second figure, white representing excess, and black defect of observation as compared with calculation:—

Days.	1	2	3	4	5	6	7	8	9	10	11	12	13	
Within.....	16	35	50	59	68	71	85	97	105	111	116	118	119	etc.
Beyond.....	15	34	47	56	65	76	90	98	104	108	115	125	130	etc.
Calculated.	15	30	44	57	69	80	90	99	106	112	117	121	124	etc.

The second and third lines are found for separate days by adding together each successive pair of your numbers, and the fourth is roughly calculated from the ordinary tables of Probability of Error. It would be easy to make the coincidence more exact, but the labour of the necessary calculation would hardly be justified by the extent of the data."

I shall now briefly compare some of my analytical state-

ments with similar statements regarding the data of Ahlfeld, made in the elaborate paper already referred to.

Ahlfeld predicted the day of confinement by his own method in 1014 cases, of which he has the details in 915 instances.

In 30 of Ahlfeld's cases the day of confinement was exactly predicted, or about once in every 30 cases. My success was twice as great, the prediction in my cases proving exactly true about once in every 15 cases.

In 205 of Ahlfeld's cases the time of confinement was successfully though not exactly predicted—the event occurring not earlier or later than four days from the predicted day. This was a success in much less than one-fourth of his cases. My similar success was in 63 cases, or more than one-third of the whole.

Further, Ahlfeld points out that 465 of his cases, or less than one-half, showed not above 11 days of error in the prediction. Of my cases 120 showed not above 11 days of error in the prediction, or considerably more than two-thirds of the whole.

It is thus seen that, so far as the limited number of cases can show it, my plan surpasses Ahlfeld's to a great degree.

I may add that, with a view to comparing his own plan with Naegelé's, Ahlfeld calculated (not predicted) the day of confinement for 258 cases, of which he possessed all the necessary details, including, of course, the day of confinement. He found the average error to be, for his own method and for Naegelé's, nearly 10 days. Mine was only 7½. His own method proved a little more accurate than Naegelé's.

Naegelé's plan is to fix upon the seventh day from the first of the last menstrual period, and to predict the same day of the third next month, counting backwards.

Ahlfeld's plan seems to be to fix upon the seventh or eighth day from the beginning of menstruation as the day of conception, and to add 271 days.

Before concluding, it is necessary not to omit mention

of a correction of one of my own practical recommendations. I say that the accoucheur may venture to predict the week of confinement, or to fix upon a day which is the middle of the week in which a woman is to be confined. Now, as the average error is about 7 days on each side of the event, it is evident that the accoucheur should not predict confinement in a certain week, but in a certain fortnight, or fix upon a day which is in the middle of the fortnight in which a woman is to be confined.*

Dr Thomson said he had met with three cases in which, after a single coitus, confinement took place at the end of 273 days.

Dr Duncan stated that some time ago he had collected the particulars of a number of such cases, and the conclusion he then arrived at was that 275 days was the time, and it was very much the same in cases where women dated from the day of marriage. The tendency, however, amongst authors seems to be to shorten the period. It was rather curious and interesting to know that in the calendar the time between the Festival of the Annunciation and Festival of the Nativity was exactly 275 days.