

On the duration of labour as a cause of mortality and danger to the mother and infant; &c; : in reply to a letter of Dr. Collins / by J.Y. Simpson.

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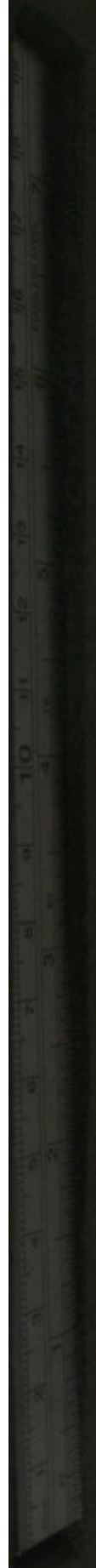
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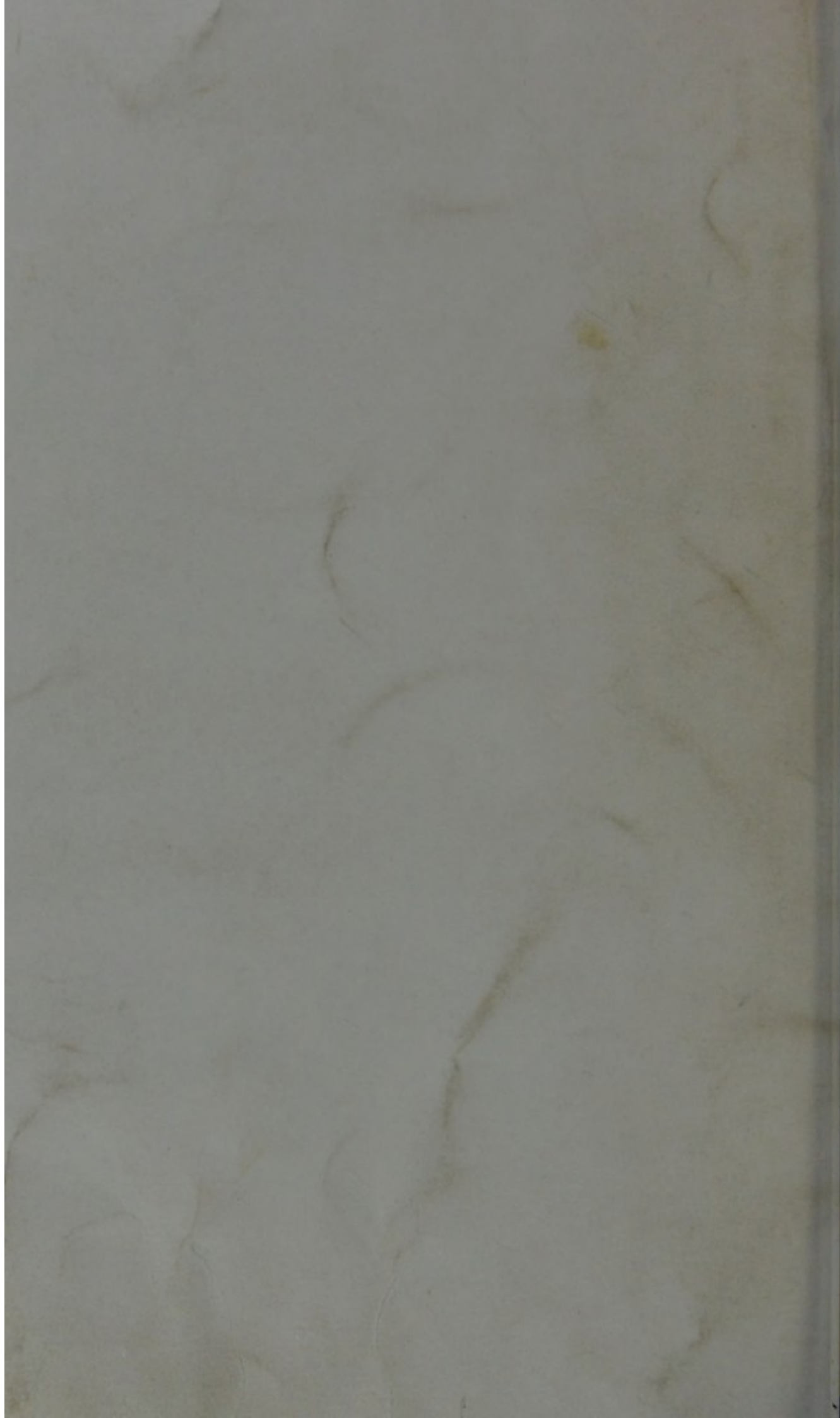
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ON THE

DURATION OF LABOUR

AS A CAUSE OF

MORTALITY AND DANGER TO THE MOTHER AND INFANT; &c.

IN REPLY TO A LETTER OF DR COLLINS.

BY

J. Y. SIMPSON,

PROFESSOR OF MIDWIFERY IN THE UNIVERSITY OF EDINBURGH.

[EXTRACTED FROM THE
PROVINCIAL MEDICAL AND SURGICAL JOURNAL,
FOR NOV. 1, 1848.]

EDINBURGH:
SUTHERLAND AND KNOX, 23 GEORGE STREET.

MDCCCXLVIII.

TO DR COLLINS,

FORMERLY MASTER OF THE DUBLIN LYING-IN HOSPITAL, ETC. ETC. ETC.

MY DEAR SIR,

A few months ago I took an opportunity of transmitting to you some proofs of the first five or six sections of a "Memoir on Turning, as an Alternative for Craniotomy and the Long Forceps, &c.," publishing in the *Provincial Medical Journal*. In the last Number of that Journal you have, on this subject, published a Letter, addressed to me. For your own sake, I do sincerely regret that you had not first sent this communication to myself, instead of to the Editor of the Journal, because I should have had pleasure in showing you privately the serious errors of calculation and reasoning which you have committed in it; instead of being compelled, as now, and in self-defence, to speak of them openly and publicly.

Let me premise, however, first, and once for all, that in writing the Fourth Section of the Memoir,* (the one on which you principally comment,) as in writing all the other Sections of it, I have, as far as I am aware, made no remark upon you or upon your work, calculated, in any degree, to call forth a communication from you, conceived and written in such a tone and spirit as your Letter betrays. Under various Chapters in your Treatise, you invite your readers to use your data for further calculations. I have done so. But I have done no more. And I should have felt sorry indeed if I had allowed any word or expression of disrespect to escape from me, of which you or any one could complain. I have quoted, to be sure, (and always in

* See *Provincial Journal* for Feb. 9, 1848, p. 57.

your own words,) some of the interesting cases which you have published, of delivery by the crotchet, as cases where, probably, the proposed new operation of turning would have been successful in saving human life. But certainly, in doing this, I no more attached, or dreamt of attaching, any blame to you for *not* turning in these instances, than I could mean to attach blame to you for *not* using anæsthetics in your practice twenty years ago, when the practice of anæsthesia in midwifery was unknown.

I have often already stated, and gladly and willingly repeat, that your work is a most admirable collection of obstetric data, and that it would be difficult in this respect to overpraise its candour and worth. But I believe at the same time that the generalizations which you yourself have drawn from your data, are, in most cases, incorrect. Your *individual* facts or cases are extremely accurate and valuable; but your own *deductions* from these facts or cases are equally, I believe, in most instances, extremely inaccurate and valueless.

For example:—The principal question in debate between us amounts to this. You maintain that the mere protraction of labour is not a material cause of danger to the mother, and, as a consequence, you hold that the mortality from the effects of protracted labour is, to use your own expression, “strikingly small.” On the contrary, I maintain and hold, that the mortality arising from the effects of protracted labour is strikingly great. You maintain that your published reports of 16,414 deliveries, which occurred in the Dublin Hospital when you were Master of that Institution, prove that neither the deaths of the mothers, nor the number of obstetric complications, were much or at all increased with the duration of the labour. I maintain that your published reports of these 16,414 cases prove quite the contrary. You maintain that “the test” of your own recorded “practical experience” shows you to be right in the opinions that you have formed on

the preceding points. I maintain that "the test" of your own recorded "practical experience" shows you, on the other hand, to be quite wrong in these opinions. Against, in short, the truth of your own recorded opinions, I appeal to the truth of your own recorded facts. Against your own doctrines, I merely appeal to your own data.

One fundamental mistake in your statistics and calculations has led you into all your numerous errors and inaccuracies relative to the effects arising from the morbid protraction of labour. This error has made you repeatedly express and maintain, as the supposed result of your own facts and experience, opinions which your own facts and experience, when properly interpreted, totally and altogether contradict.

The error I allude to is this:—In computing from your experience of 16,414 deliveries, the effects upon the maternal mortality of morbid prolongation of the labour, as a special or individual complication, you calculate the resulting number of deaths in relation to *the total sum of all the cases delivered*, (16,414,) instead of calculating them in relation to the *total sum of all the cases merely that were protracted* (452.) You calculate the effects of the complication relatively to your whole number of cases of labour, instead of relatively to your whole number of cases of *this one* special complication, (protraction.) An example may illustrate my meaning. I shall take it from the subject of rupture of the uterus and vagina.

During your seven years' Mastership of the Dublin Hospital, 34 cases of rupture of the uterus and vagina occurred, and 32 of the mothers died; 2 only of the 34 survived. If asked whether rupture of the uterus were a dangerous complication or not—whether the mortality attendant upon it was "strikingly small" or "strikingly great," what answer would you return? If you argued and answered upon this special complication as you do with regard to the special complication of protraction, you would main-

tain that the mortality from rupture was strikingly small; for only 32 mothers out of 16,414, or about 1 in every 513, died of it. This would show, certainly, the proportion of death from rupture in relation to the total sum of all the cases delivered; but it would not show *what was wanted*, viz., the proportion of deaths from rupture in relation to the total sum of all the cases in which rupture happened. If you wished, in short, to state the real risk and danger attendant upon this special complication, (rupture,) you would state that 32 mothers died of it out of 34 cases in which it occurred, instead of stating that only 1 in 513 died of it. And, exactly in the same way, if asked what was the degree of maternal risk and danger attendant upon another special complication, (viz., protraction of labour beyond 20 hours,) your answer upon the same principle should not be as you have it,—42 deaths in 16,414 labours, or in the proportion of 1 in 391; but 42 deaths in 452 labours, (the *whole* number protracted beyond 20 hours); or 1 death in every 11 cases of labour prolonged to this amount of protraction.

Ask any intelligent actuary, or any medical friend properly acquainted with the principles of statistics, and they will perhaps convince you more than any more lengthened remarks of mine would, respecting the errors you have committed on this, and on various other analogous statistical questions.

But I have taken up too much time with these preliminary remarks, and I must hasten on to show the erroneous nature of the more direct criticisms, which you have pleased to offer on some of the statements and opinions contained in my memoir. The principal of these statements and criticisms I shall answer by a simple appeal to, and tabulation of, your own data; for in these data, as I have already said, there is always contained the best and most complete refutation of your own opinions. I shall notice your principal objections and statements *seriatim*.

1. *Does the maternal mortality attendant upon parturition increase in a ratio progressive with the increased duration of the labour?*

I have ventured, in the fourth section of my "Memoir on Turning," to lay down the affirmative of this, as a general obstetric law. The law itself—and I believe it to be a most important one—was originally educed by a series of calculations out of your own cases. I formerly published merely* the *results* of the calculation, without publishing the *data* on which these results were founded. For my object was not to overload my paper, or the pages of the Journal unnecessarily. As you believe, however, that these results are "obscure," and must be erroneous, because totally opposed to your opinions, and that they consist only of "partial data," and of "visionary" "theoretical calculations," (to quote your own expressions,) I shall simply, in answer to all this criticism, insert, in a tabular form, the data themselves upon which this law and its results were founded.

Out of your 16,414 cases, you noted the duration of the labour in 15,850; and among these 15,850 cases, 138 maternal deaths occurred. The following table contains, in its first column of figures, the total number of deliveries that took place under each specified period of duration; in the second column is stated the corresponding number of deaths occurring among each set of these deliveries; and in the third column is given the proportion which the deaths bear to the whole deliveries in labours of each specified degree of duration. In other words, the table reads thus:†—3537 mothers had their labours terminated within one hour from their commencement; and of these 3,537 mothers 11 died, or 1 in every 322. The labour continued from 2 to 3 hours in 6000 cases, and out of these 6000 cases 26 mothers died, or 1 in every 231; and so on.

* Provincial Journal for Feb. 9, p. 58, &c.

† The other Tables that follow are constructed on the same plan, and read in the same way.

Table showing the proportion of 138 Maternal Deaths in relation to the Duration of Labour in 15,850 cases of Delivery recorded by Dr Collins.

| Duration of Labour. | Number of Deliveries. | Number of Deaths. | Proportion of Deaths. |
|---------------------|-----------------------|-------------------|-----------------------|
| Within 1 hour | 3,537 | 11 | 1 in 322 |
| From 2 to 3 hours | 6,000 | 26 | 1 in 231 |
| From 4 to 6 hours | 3,875 | 29 | 1 in 134 |
| From 7 to 12 hours | 1,672 | 21 | 1 in 80 |
| From 13 to 24 hours | 502 | 19 | 1 in 26 |
| From 25 to 36 hours | 134 | 8 | 1 in 17 |
| Above 36 hours | 130 | 24 | 1 in 6 |
| Total.. .. . | 15,850 | 138 | 1 in 115 |

I need not, I believe, add a word of comment, for these figures themselves speak strongly enough against your opinions, and in favour of mine. But you must excuse me adding, that the data of the table are not, (as you declare,) "partial." They include *all* the 15,850 cases in which you noted the duration of labour. In the remaining 564 cases of your 16,414, you did not note the duration of the labour. But I am not answerable for that omission. And I am sure that your own gentlemanly feelings will at once prompt you to withdraw the improper accusation of my "founding doctrines upon partial data" from your work.

2. *Does the Liability to various Morbid Complications during Labour increase in proportion as the Labour is increased in its Duration?*

In one Section of my paper I had stated that the above law holds good in regard to most obstetric complications, as puerperal fever, rupture of the uterus, convulsions, retention of the placenta, &c. You demur to the truth of the law educed, and in your letter to me, you select one

complication to refute my views, and make the following statement regarding puerperal fever:—"That this intractable disease," you observe, "is *not* the result of *protracted* labour, as you erroneously state, is notorious to those who have had opportunities of observing it, *and is fully proved in my Report*, the total number of cases being 88, 71 of which were delivered within 12 hours, and 80 within 24 hours." In your Treatise you offered, some years ago, a similar opinion. "It is stated," you remark, "by several writers, that females who have suffered from tedious and fatiguing labours, are particularly liable to puerperal fever. This," you add, "does *not* accord with my experience."

Now, it *does* accord with your experience. Here, as elsewhere, your own facts and data show that very opinion to be true which you believe to be untrue. The following table is a brief way of expressing these data in reference to the 84 cases of puerperal fever, in which the previous duration of the labour was noted by you; and it sufficiently shows that the liability to this, (like other complications,) increases as the labour increases in duration:—

Table of Duration of Labour in 84 Cases of Puerperal Fever, in the Dublin Hospital, during Dr Collins's Mastership.

| Duration of Labour. | Number of Deliveries. | Number of Puerperal Cases. | Proportion of Cases. |
|---------------------|-----------------------|----------------------------|----------------------|
| Within 6 hours..... | 13,412 | 61 | 1 in 219 |
| From 7 to 12 hours | 1,672 | 10 | 1 in 167 |
| Above 12 hours..... | 766 | 13 | 1 in 59 |
| Total..... | 15,850 | 84 | 1 in 186 |

It would be difficult, I believe, to *invent* even, data more conclusive than your own, against your alleged experience on the above, and on other points.

3. *Does the Infantile Mortality attendant upon Parturition increase in a Ratio progressive with the increased Duration of the Labour?*

I have stated this as another general rule, deducible from your data. You doubt its soundness, and in answer to it express yourself as follows:—

“In reply to this *theory*, I shall only state the simple truth which you have omitted, that of 1,045 cases of still-born children accurately noted, 844 were delivered within twelve hours, and 932 within twenty-four hours; and I have added that the death of the child subsequent to birth, except in very few instances comparatively speaking, was not a consequence of injury arising from protracted labour; for of the 284, which was the total number of deaths previous to the mother leaving the hospital, the labour in 246 did not exceed twelve hours. *These unquestionable facts* (you add) *extinguish all speculative theories.*”

On this, however, as on other points, against the truth of your opinions and criticisms, I have merely to appeal to your own “unquestionable facts.” And I will do so by showing you, in the following table, that my “theory,” (as you choose to term it,) is merely a simple generalization of your own cases.

Table showing the Proportion of Still-births in reference to the Duration of Labour in 15,850 Cases of Delivery. (Dr Collins' data.)

| Duration of Labour. | Number of Deliveries. | Number of Still-born Children. | Proportion of Still-born Children. |
|-----------------------|-----------------------|--------------------------------|------------------------------------|
| Within 2 hours,..... | 7,050 | 347 | 1 in 23 |
| From 3 to 6 hours,... | 6,362 | 346 | 1 in 18 |
| From 7 to 12 hours, | 1,672 | 151 | 1 in 11 |
| From 13 to 24 hours, | 502 | 88 | 1 in 6 |
| From 25 to 36 hours, | 134 | 42 | 1 in 3 |
| Above 36 hours,..... | 130 | 71 | 1 in 2 |
| Total,..... | 15,850 | 1,045 | 1 in 15 |

The opinion which I have above quoted from your late letter to me, as to protraction of labour not influencing the life of the child, during the first two or three weeks *after* its birth, is expressed by you also in your Treatise, (p. 513.) "To prove, (you observe) that tedious labour is not often the cause, it will be sufficient to mention, as is clearly shown by the tables, that of the 284 children dead, the labour in 246 instances did not exceed twelve hours."

If we throw out of these 284 cases, the premature or twin children, as too small in size to be influenced much in their passage during labour, and those born imperfect or diseased, we have 155 children left, born at the full time, and apparently perfect in formation at birth. The following table of these cases will prove to you, contrary to your opinion, that tedious and protracted labour is a cause of danger and death to the child for some time after birth.

Table of Duration of Labour in 155 Deliveries where the Children died within a few Days after Birth. (From Dr Collins' data.)

| Duration of Labour. | Number of Deliveries. | Number of Deaths. | Proportion of Deaths. |
|---------------------|-----------------------|-------------------|-----------------------|
| Within 6 hours..... | 13,412 | 110 | 1 in 122 |
| From 7 to 12 hours | 1,672 | 24 | 1 in 70 |
| Above 12 hours..... | 766 | 21 | 1 in 36 |
| Total..... | 15,850 | 155 | 1 in 102 |

In the preceding observations I have used no arguments to refute your criticisms and statements, except your own data. Your own figures talk too eloquently against the soundness of your own deductions to require any assistance on my part. And here let me observe, that in one sense the term "experience," which you so often repeat, is misapplied by you. A man who watches and reasons upon 50

cases accurately, may form sounder deductions than a man who sees 500 or 5,000 cases, but who watches and reasons upon them inaccurately. And in its last and essential analysis, true experience really consists in true deductions from the cases observed, more than in seeing cases, however numerous, and drawing imperfect or improper deductions from them. Two practitioners of so-called experience, may each see a vast amount of similar cases, and yet, ultimately, draw the most directly opposite conclusions from them. Supposing one of these practitioners right and the other wrong, then certainly, of the latter, the so-called "vague and arbitrary experience, (as Bacon writes it,) astonishes rather than instructs." Dr Hamilton and you each had an opportunity of seeing a very large amount of obstetric cases, and you each, through thus watching facts or phenomena of exactly the same nature, came ultimately to conclusions exactly opposite in regard to the effects of morbid protraction of labour upon the safety and life of the mother. He came to the conclusion that morbid prolongation of the labour is a great cause of danger and death to the mother. You came to precisely the reverse conclusion, and attacked him with the "strong observations" which you allude to in your Letter to me. The means of settling, and settling statistically and definitely, all, or almost all, the points in dispute between you and Dr Hamilton, exist in your own work; and when the evidence of your own data is properly arranged and estimated, it will be found, I believe, as in the foregoing tables and instances, to refute your opinions, and support those of Dr Hamilton, to a degree which you could scarcely have ever dreamed of. But I feel assured that your own candour of mind, and your deservedly high standing, will yet enable you to perceive and own this, and so repel the well-known saying of Locke, that "there are few men, who once dreadfully mistake, that are willing to be set aright."

In your Letter to me, besides challenging the truth of

the generalizations which I had offered on the duration of labour as affecting the life and safety of the mother and infant, you have entered into other criticisms, and made other observations on my essay. The principal of these criticisms and observations I shall now notice. For instance:—

4. Speaking of the results of your practice when Master for seven years of the Dublin Hospital, and of the number of patients who died of puerperal fever, you observe—

“If we were *only* to deduct the deaths from this fatal disease, which may be considered *accidental*, the entire maternal mortality for seven years would then be less than one in 156. And WHY (you specially ask,) is it not stated by you, that for the four last years of my residence in the Hospital, after puerperal fever disappeared, during which period, the very great number (10,785,) of deliveries occurred, 58 only died, or in the proportion of 1 in 186?”

Why did I not state this? Simply because I had nothing whatever to do with the matter. I should have been travelling altogether out of my road if I had mentioned it. My essay was not, in any, the very slightest degree, a comment upon the good or the bad success of your practice. My calculations and inquiries were altogether independent of the numbers you happened to save or to lose. The principles I have deduced would have equally appeared whether your practice had been twice more successful, or twice less successful than it was. The proportions you lost at any specified time did not interfere in any respect with my calculations, and I had no kind of interest in the point you allude to.

But since you do ask such strange questions, let me ask you one or two in turn.—*Why* is it not stated by *You* that during the first three years of your Mastership, out of 5,629 women delivered, 106 died, or one in every 53? Why have you never mentioned that in one year, (1826,) when you acted in the Hospital partly as Assistant-Physician, and partly as Master, 81 out of 2,440 women de-

livered, died, or 1 mother in every 31? Why not add that during five years subsequent to the four which you have named, 1 woman in about every 50 delivered in the Dublin Hospital, died?

You prominently state, (with what good taste I leave others to decide,) that in 1821 and 1822, in the Edinburgh Lying-in Hospital, when under the late Dr Hamilton, "1 in 21 of the patients" died. You know that this high mortality arose from the prevalence of puerperal fever, which you deem an accidental complication. Why have you not added this? Why have you not added, that in the Dublin Hospital when the same epidemic prevailed two years before,—viz. during 1819 and 1820, with all the superior endowments and superior accommodation of that large and rich hospital, the mortality was nearly as great, or about 1 in 34? You state that "unfortunately the above is the only record in existence of the mortality of this public charity"—the Edinburgh Lying-in Hospital. An official report was published, and, I think, certainly transmitted to you, the year after I succeeded Dr Hamilton, showing that out of the 930 patients of the Institution, whose deliveries had occurred immediately before and up to the date of that Report, only 1 mother had died, (1 in 930); and that one death had occurred in consequence of disease of the heart.

5. But after all, is your selected and highest return of 1 death in 186 mothers, when puerperal fever was *not* in operation, any great success? You evidently think so. You yourself write with great self-gratulation—

"Dr Collins' practice is *much the most successful on record*, as (there is) no report of 10,785 cases with a mortality nearly so small as 1 in 186." You claim from me that I should publicly state *this* as "only an act of simple justice to you"! "I feel called upon, (you further observe,) to seek the declaration of *this* important truth from you;" and you add, "I am induced to request you to blend a more equitable portion of merit to me, as

I believe you would not intentionally *pluck the laurel off my brow*, or withhold what, I hope you will agree with me in thinking, I may from you legitimately claim."

Now, I certainly have no desire to pluck, as you poetically express it, "the laurel" from your brow. Nor indeed, is there the slightest fear of myself or any one else doing it. For the truth, I humbly fear, is,—there is really no "laurel" to pluck. Your *selected* return of 1 maternal death in 186 deliveries is not greater than various other hospital reports show under the same circumstance,—viz., under the subtraction of all casualties from puerperal fever. In your Hospital during your Mastership, 1 in every 100 mothers died, (puerperal fever included;) 1 in 186 when puerperal fever had disappeared. In the Dublin Obstetric Hospital, superintended by Dr Churchill, 1 mother in 219 died, (puerperal fever included;) 1 only in 274 when puerperal fever deaths were not included. (See *Dublin Quarterly Journal*, Vols. xiii. xv. and xxiii.) In a printed report of the Edinburgh Maternity Hospital now lying before me, 1 in every 134 mothers died, (puerperal fever included;) 1 in every 368, (puerperal fevers not included.) See *Monthly Journal* for November 1848.

But we need not take a return, *selected* either as regards time or as regards the absence of puerperal fever, to show a success greater than your own. Most of the readers of your Letter in the *Provincial Journal*, could, I believe, show a better return (*without* any selected time or circumstances,) than yours, (*with* your selected time and circumstances). For in the last year (1842,) for which the Registrar-General has as yet published any returns of the relative number of births and deaths in childbed, among 517,739 births registered in England and Wales, 2687 mothers perished in childbed; or 1 in every 190 mothers died. Do you object that the average rank of life was higher than among your patients? That objection does not certainly apply to the poor patients of the London Ma-

ternity Charity. Yet out of 35,743 deliveries among the patients of that Institution, only 166 mothers died, or 1 in every 215.

And please to recollect too, that the practitioners at the London Maternity Charity, who were thus, without any selection, so much more successful in practice than the Master of the Dublin Hospital with his selections, and to whom, therefore, I fear you must surrender that "laurel" of which you suppose yourself the wearer:—please, I say, to recollect, that they are (shall I write it?) female practitioners—real petticoated midwives.

When adducing the supposed success of your own practice, I am not aware that you have anywhere stated that your general success during your seven years' Mastership, as far as regards the *maternal* mortality alone, was considerably less than that of other Masters who had preceded you in the same institution, as Dr Rock and Dr Evory. But the accoucheur, in every case of labour, has charge of two lives,—the life of the mother and the life of the infant. In your 16,414 cases you had committed to you, for instance, 33,068 lives, including 16,414 mothers, and 16,654 infants. Out of these 33,068 lives, 1 in every 27 was lost,—a mortality greater, I believe, than that which has happened to almost any of the other Masters who have had charge of the Dublin Hospital. You choose to blame and criticise me for losing, in my private practice, (during a late epidemic of puerperal fever), and when using anæsthetics, two mothers from puerperal fever, out of 150 deliveries. Out of 150 children born in these 150 cases, only one was dead. Of the 300 lives thus entrusted to me, I lost only one in 100. You lost 1 in 27.

The statistical investigations of Merriman and Casper have proved, that in England, and upon the continent, the number of maternal deaths following delivery, has decreased during the last century from about one in 50 or 80, to one in 150 or 180, from the science and art of

midwifery being everywhere more deeply and earnestly cultivated. Your vast Hospital in Dublin has now been open for nearly a century. But the returns of it show that the success of the last four Masters of it, (yourself included), has been less, both as regards the saving of maternal and of infantile life, than that of the first four Masters who took charge of it in the last century. The last four Masters lost one mother in every 75, and one child in every 17. The first four Masters lost one mother only in every 77, and one child only in every 20.

I state these things with no intention to blame,—far from it. But I do feel alarmed at the self-complacent way in which you seem to talk of the present supposed advanced state of midwifery. You accuse me of the “atrocious crime of youth.” Every day I get older, and every day I feel more and more the vast amount of work that yet remains to be done by us all in midwifery; and I would fain excite you, if I could, to expend more of your abilities and talents upon the real advancement of that branch of medicine which you and I practise. Further, you seem to suppose that the seeing an enormous number of cases is the means by which this advancement is to be accomplished, and that my want of experience (as you choose to term it,) is enough to prevent me aiding in this good work. But I beg you again to remember, that it is not the mere mass of cases seen that has ameliorated, or that will ameliorate the state of midwifery. In your Hospital upwards of 150,000 women have been delivered under the charge of different Masters. If we except, however, the names of Ould and Clarke, I cannot, at this moment, recollect that any one of your other physicians, when acting as Masters, has added a single new fact to obstetric science, or propounded a single new principle in obstetric practice.

But I have said enough on this matter; let us pass on to other topics.

6. You say that, in my "Memoir on Turning," I have ventured to condemn your practice." I am certainly not aware that I have done so. But you unscrupulously venture to condemn mine, and denounce the idea of turning in a case of contracted pelvic brim as "dangerous," &c. &c. And you condemn it on grounds which show at once that you misunderstand it. You suppose and argue that I wish to substitute turning in *all* cases of deformity of the pelvis, where the crotchet and long forceps are at present used. I never entertained or expressed such an idea. Thus, you erroneously imagine that I would have turned the children in all the 79 cases in which you perforated their heads; and you add, "I shudder at the thought." I really do not see why we should "shudder at the thought" of saving *some* of their lives instead of destroying them *all*. An old pupil of your own, your former able assistant physician at the Dublin Hospital, and my esteemed friend, Professor Murphy of London, met last December with a case (see *Lancet* for Dec. 18, 1847), in which the pelvis was three inches in the conjugate diameter of the brim,—where the head remained above it,—where he commenced *your* method of practice, viz. perforating, but the instrument failed,—and where he subsequently delivered the child alive by *my* method of practice, viz. turning; and I have quoted in my memoir several such instances. Would any man "shudder" at the thought of this child's life being saved by a new, when it was so nearly being sacrificed by an old, practice? Your countrymen have just been earnestly petitioning, and properly so, for one adult Irishman who has forfeited his life to the civil law. And why "shudder" then, at the thought of me petitioning with equal earnestness, for the lives of *some* of the next young Irishmen that may (if the old obstetric law is not changed) be doomed betimes to have their heads opened, as formerly, by the perforator; and who, as yet, have committed neither crime nor treason of any kind. You

are well aware that the Dublin returns present a much larger proportion of cases of craniotomy than those of any other hospital or school in the world.* And one of our most able and most experienced practitioners in Scotland, (Dr Wilson), when lately writing on the subject of the frequency of craniotomy, observed, not without truth, "It is to be feared that embryotomy is too lightly thought of; and our practice contrasts very strongly and unfavourably with that of criminal courts in cases involving life and death. With us the destruction of the life of a child is often determined upon, even without the formality of a regular consultation; whereas with them, such is the importance attached to life, even when that life is loaded with imputed crime, that the greatest talents in the land are summoned to judge and bear witness in the cause, so that life be not sacrificed without the most undoubted proof of guilt." (See *Dr Wilson on the Advantages of Turning in certain cases of Narrow Pelvis.*)

7. You believe that I wish to give up the long forceps, and substitute turning for them, and that I thus leap, (as you elegantly express it), "out of the frying-pan into the fire." Now, I do not any more imagine that turning should be a substitute for the long forceps in all cases, than that it should be a substitute for the crotchet in all cases. There are cases in which each of these three means is the appropriate plan of treatment. That I have not given up the long forceps, you will find by a paper of mine on the subject, published in the *Monthly Journal of Medical Science* only two months back. In your letter to me, you object to the long forceps in the following terms: "In most laborious labours the pelvis measures little more than three inches from the pubes to the sacrum; in others less than this. And when we consider the blades of the small-

* See Table in Postscript, p. 23.

est-sized long forceps used in Britain, even when completely closed, measure from $3\frac{1}{8}$ inches to $3\frac{1}{2}$, it is clear that where the bones of the pelvis were denuded of their soft parts, there would not be space to admit of their application." Now, here, you commit two most extraordinary errors in relation to the mechanism and application of the long forceps. They are not applied antero-posteriorly,—that is, in the *most* narrow and *most* occupied diameter of the pelvis. The contracted pelvic brim is cordate or elliptico-cordate; and, (in this part of the world at least,) children's heads are *not* of that shape, but round or oval. Consequently, in entering the pelvic brim, they leave and must leave, laterally or diagonally, a free space on either side. It is in these free spaces that the blades of the long forceps are passed up by all who use them.

Surely the rejection altogether of the benefits of the long forceps from the practice of the Dublin Hospital, when under your care, was based on grounds more valid than the two very strange and very grave errors which you have here expressed on an important yet simple point in obstetric anatomy and obstetric surgery.

8. In relation to the operation of craniotomy, you try to show that the whole mortality from it was small. "Fifteen *only*," you say, "out of 79 died." That was 1 death in every 5 operations. Was that a *small* mortality? When the same operation was performed after labours of 48 hours' duration, my observations in regard to its mortality are, you state, "*frightful* indeed;" but, notwithstanding, you aver that the resulting mortality was "*marvellously* small," for you argue the deaths were only 11 out of the 16,414 deliveries," (here reasoning again according to your old and erroneous mode of calculation.) There were 11 maternal deaths out of 27 craniotomy operations; or of the 27 women delivered by the crotchet at that late period of the labour, 1 *in every* $2\frac{1}{2}$ died. Whether is this mortality

“*marvellously* small” or “*frightful* indeed”?—I willingly leave you and our readers to determine.

9. You give a statement of the supposed *Causes* of death in the 42 women who died after labours protracted beyond 20 hours. You do so evidently with the view of showing that they did not die of affections brought on by the protraction. But the whole list is a list merely of such injuries and diseases as tedious labour does produce,—viz., fever, inflammation, sloughing, rupture, &c. There is *one* striking exception in your table,—viz., *one death* entered from “Stricture of intestine.” Stricture—chronic stricture of the intestines,—was, you state, found on dissection; but *how* that stricture did or could possibly produce death without ileus, &c., you do not state. At last you ingeniously argue (or what you term “demonstrate,”) that you lost only “three” patients instead of “forty-two,” from the effects of protracted labour. But I feel sure, that to the members of the Medical Profession of England, it is utterly needless to expose such trifling with facts. Long ago surgeons always used to argue in regard to their lithotomy and other cases, that the deaths were from inflammation of the bladder, or inflammation of the intestines, or disease of the kidneys, or of the liver, or—anything, in fact, except the operation itself. Modern surgery does not admit of such pathological casuistry. Nor does modern midwifery.

If my time and space were not both alike exhausted, I would notice and correct other statements which you have made in your letter,—such as your declared idea that your “Practical Treatise” established a “*new era*,” (is this expression not a misprint?);—your erroneous apprehension of the uses that I attribute to auscultation during labour;—your allegation that “*a child cannot pass alive*” by turning in a deformed pelvis; your incorrect misrepresen-

tation, (not, I trust, a wilful one,) of the results of anæsthetic midwifery in my own practice, and in the practice of the Maternity Hospital; &c., &c.

But I have already said enough, I believe, to put on their guard your readers and mine as to the weight of your mere opinions and deductions on these as on other matters. And apologizing for the very hasty, and consequently, very imperfect way in which these remarks have been thrown together, I beg you to believe me,

MY DEAR SIR,

Very faithfully yours,

J. Y. SIMPSON.

EDINBURGH, October 25, 1848.

