

Two letters to Dr Collins ... / by J.Y. Simpson.

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TWO LETTERS

TO

DR COLLINS,

PRESIDENT OF THE KING AND QUEEN'S COLLEGE OF PHYSICIANS OF
IRELAND, &c. &c. &c.

I.

ON THE DURATION OF LABOUR AS A CAUSE OF MORTALITY
AND DANGER TO THE MOTHER AND CHILD; &c.

II.

ON THE OBSTETRICAL STATISTICS OF THE DUBLIN
HOSPITAL; &c.

BY

J. Y. SIMPSON,

PROFESSOR OF MIDWIFERY IN THE UNIVERSITY OF EDINBURGH.

EDINBURGH:

SUTHERLAND AND KNOX, GEORGE STREET.

MDCCCXLVIII.

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TWO LETTERS

DR COLLINS

REPORT ON THE CASE AND CLINICAL HISTORY OF A PATIENT

ON THE DURATION OF LABOUR AS A CAUSE OF MORTALITY
AND PAIN TO THE MOTHER AND CHILD

OF THE OBSTETRICAL STATISTICS OF THE BIRTH
HOSPITAL

J. Y. SIMPSON

LECTURER ON MIDWIFERY IN THE UNIVERSITY OF EDINBURGH

EDINBURGH:

BUTTERFIELD AND KNOX, GEORGE STREET

MDCCLXXIII

R55074

TO DR COLLINS,

LETTER I.

TO DR COLLINS

FORMERLY MASTER OF THE DUBLIN Lying-in HOSPITAL, &c. &c.

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 Training, as an Alternative for Circumcision and the Long Fore-skin, &c." published 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 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

TO DR COLLINS,

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

MY DEAR SIR,

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* 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 proportion² 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 educed 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.

POSTSCRIPT.

Table showing the Proportion of Instrumental Deliveries, and of Deliveries by the Forceps, and by Craniotomy, in different Obstetric Institutions.

Name of Reporter.	Total No. of Labours Reported.	Proportion of Instrumental Deliveries.	Proportion of Deliveries by Forceps.	Proportion of Deliveries by Craniotomy.
Siebold—Berlin.....	2093	1 in 7	1 in 7	1 in 2093
Busch—Berlin.....	2056	1 ... 11	1 ... 12	1 ... 342
Carus—Dresden.....	2549	1 ... 13	1 ... 14	1 ... 283
Nægele—Heidelberg	1711	1 ... 31	1 ... 31	1 ... 1711
Bland—Westminster	1897	1 ... 95	1 ... 158	1 ... 237
Beatty—Dublin.....	1182	1 ... 98	1 ... 131	1 ... 394
Collins—Dublin.....	16,664	1 ... 115	1 ... 617	1 ... 141
Churchill—Dublin...	1640	1 ... 117	1 ... 546	1 ... 149
Lever—London.....	4666	1 ... 137	1 ... 518	1 ... 186
Boer—Vienna.....	9589	1 ... 199	1 ... 274	1 ... 737
Lachapelle—Paris....	22,243	1 ... 252	1 ... 293	1 ... 1854
Ramsbotham—London	48,682	1 ... 322	1 ... 553	1 ... 773
Simpson—Edinburgh	1417	1 ... 354	1 ... 472	1 ... 1417

From the Monthly Journal of Medical Science for Nov. 1848, p. 337.

TO DR. COLLINS.

LETTER II.

TO DR COLLINS,

PRESIDENT OF THE KING'S AND QUEEN'S COLLEGE OF PHYSICIANS, FORMERLY
MASTER OF THE DUBLIN LYING-IN HOSPITAL, ETC. ETC. ETC.

MY DEAR SIR,

Allow me to set you right in regard to some of the *new* statements which you have made in your second letter in the Provincial Journal.* These new statements involve you only in new errors; and give me the additional trouble, and, let me add, the pain, of replying to them. You have, however, one great advantage. It is a hundred-fold more easy to make statements than to answer them. You, or any one, may at any time make in a couple of lines an assertion which may require a couple of pages properly to analyse and expose. But let us proceed.

1. You observe, "Professor Simpson asks why I did not state that during the first three years of my Mastership, out of 5629 women delivered, 106 died, or 1 in 53. In answer (you add) I could not state so and state a fact, as there were 7547 deliveries in the period stated, and 110 deaths, or 1 in 68."

The answer to this (the *only* point on which you challenge the correctness of my various computations) is abundantly simple. If error there is, it is yours, not mine. It is in your statement of facts, not in my calculations upon them. Your Mastership of the Dublin Hospital lasted for seven years, from November 1826 to November 1833. You declared in your first letter in the Provincial Journal,

* Provincial Journal for Nov. 15, 1848.

(p. 573,) that during the last four of these years 10,785 women were delivered. To use your own words, "for the four last years of my residence in the Hospital after puerperal fever disappeared, the very great number 10,785 of deliveries occurred." Further, in your second letter, you now declare that during the first three years of your residence, 7547 deliveries took place, and not, as I computed them, 5629. But if you had 10,785 deliveries during your four last years, as you stated in your first letter, and 7547 during your three first years, as you state in your second letter, then you must have had in all, not 16,414 cases, as you have always hitherto averred, but 18,332 cases; for $10,785 + 7547 = 18,332$. Or, if you had in all, during your seven years' Mastership, (as you have always hitherto stated), 16,414 cases only, and if 7547 of these occurred during the first three years of your residence, then only 8867 of them occurred during the last four years, and not, as you have before averred, 10,785; for $7547 - 8867 = 16,414$.

My computation, that you had during the first period of your residence 5629 cases, with 106 maternal deaths, or 1 death in every 53, was built upon a very simple calculation, and one which is quite correct, you will find, according to the data which you yourself gave. You had, (according to your own first and reiterated statement,) 16,414 deliveries in all, with 164 maternal deaths; or 1 in every 100 mothers died. Of these 16,414 deliveries 10,785 occurred "during," or, as you express it, "for the four last years of my residence in the Hospital after puerperal fever disappeared," with 58 maternal deaths, or 1 in 186. Now if you subtract 10,785 from 16,414, you will find remaining for the first three years, the number I gave, viz., 5629 deliveries; for $16,414 - 10,785 = 5629$. Again, if from the whole number of maternal deaths, 164, you subtract the declared number during the four last years, 58, you will find remaining as the number of deaths for the first three years, 106; for $164 - 58 = 106$. And 106 deaths

in 5629 deliveries, makes 1 death in 53, as I originally stated it. The whole may perhaps be more plainly expressed to you in three lines, as follows:—

During the whole 7 years, there were 16,414 cases; 164 deaths; or 1 in 100.
 During the last 4 years, there were 10,785 cases; 58 deaths; or 1 in 186.
 Hence, for the first 3 years, there were 5629 cases; 106 deaths; or 1 in 53.

I repeat, then, that when I stated you had 5629 cases, with 106 deaths, or 1 death in every 53 deliveries, during the earlier part of your Mastership, and before puerperal fever accidentally disappeared from the Hospital, I stated precisely and accurately the results which your own data afford. My calculations are correct, if your data are correct.

2. You observe:—"Professor Simpson states in reply to my remarks upon the excessive mortality of 1 in 21 in the Edinburgh Hospital (in 1821 and 1822) under the late Dr Hamilton,—'you (Dr Collins) know that this high mortality arose from the prevalence of puerperal fever.' I beg (you add) to say, that Dr Hamilton peremptorily declares, that not one case of puerperal fever occurred in the above period. See the late Dr Mackintosh's Essay, published in 1823, where *he asserts* the mortality was 1 in 18."

Upon this paragraph of yours, I must take the liberty of making two remarks. And, *first*, in the Essays of Dr Mackintosh's to which you refer me, that gentleman assuredly *nowhere* asserts that the mortality of the Edinburgh Lying-in Hospital, in 1821 or 1822, or in any other year, was 1 in 18. Dr Mackintosh knew far better than to make any such rash mis-statement of simple facts. It is *not*, as you aver, an assertion of Dr Mackintosh's; but is, I am sorry indeed to add, an assertion of Dr Collins' own. You made it for the first and only time in which it was made, in a painfully violent and intemperate attack upon the late Dr Hamilton, which you published in the

Dublin Journal for 1838, and to which you refer in your first Letter to me. And you must kindly excuse me, if I defend the respected memory of Dr Mackintosh from the odium of calumniating his brother practitioners, by having made the improper and unworthy assertion which you choose to attribute to him.

The printed Annual Reports of the Edinburgh Lying-in Hospital show, that, in 1821, 190 women were delivered within the Hospital. Of these 7 died; or 1 in 27. In 1822, 168 women were delivered within the Hospital. Of these 5 died, or 1 in 33. During these two years, out of 358 women delivered within the Hospital, 12 died, or nearly 1 in 30.

But, *secondly*, These 12 women did die, as I have stated, of epidemic puerperal fever. It grieves me, and it must, I am sure, grieve all your best friends and well-wishers, to see you anxious to deny this. As your authority, you yourself refer me to Dr Mackintosh's Essay (1823). Well, in that work, Dr Mackintosh gives the following summary of the symptoms in the seven women that died in the Hospital in 1821:—"They had," he says, "rigors; pain of abdomen and of forehead; progressive increase of pulse; difficulty of breathing; anxiously expressive countenances; tumefaction of abdomen. The lochiæ were not suppressed; on the contrary, they flowed in natural quantity and quality." (P. 39.) Two pages further on (p. 41), Dr Mackintosh states that the five patients who died in the Hospital in 1822, "had analogous symptoms." Surely no obstetric pathologist or practitioner would or could say, that these cases, with these symptoms, were not cases of "puerperal fever," as you and I, and all of us understand that generic nosological term.* On this point, I unhesitatingly appeal to Dr Collins himself, but not to the writer (*scriptor præ-*

* A difference in the name makes no difference in the thing named. You call the disease "puerperal fever" in your Treatise. I sometimes give it the double name, "puerperal fever and inflammation," as marking the two principal morbid actions which co-exist, not as cause and effect, but

ceps) of the letters signed "Dr Collins," in the Provincial Journal. Dr Hamilton termed the cases "puerperal peritonitis." But *your* own author (Dr Mackintosh, p. 12,) very properly designates any attempt in this way to deny that they were not instances of puerperal fever, as "a mere *Sophistical* statement." Besides, that epidemic puerperal fever was very prevalent in Edinburgh in 1821 and 1822, is well known to every obstetrician who is at all conversant with the literature of his profession. Two large octavo volumes were published on the special Edinburgh Epidemic of those years; one by Dr Mackintosh (to which you refer;*) the other by Dr Campbell.† Both authors describe the epidemic as having attacked the patients of the Lying-in Hospital. In his work on Puerperal Diseases, your own townsman, Dr Churchill, bestows a page on the Edinburgh puerperal fever of these years 1821-22; and states (p. 289), the disease "was epidemic in the Lying-in Hospital of Edinburgh in 1821-22."‡—It carried off, at that time, many patients in private practice. In his Essay, Dr Mackintosh speaks of the late highly-esteemed Dr Kellie, as one of the "most experienced, and, in fact, greatest authorities in midwifery." That excellent and able practitioner lost in Edinburgh and Leith, 15 patients from puerperal fever, during the prevalence of the epidemic I speak of.

3. In your article in the *Dublin Journal* to which I have as co-incident effects, as I believe, of one common cause, viz. a diseased state of the blood. Various other names, as you know, have been applied to this same disease. But still I repeat, a difference in the name does not alter the disease named.

* A Treatise on Puerperal Fever, &c. (1822,) and Reply to a Pamphlet on it, (1823.) It is this last work, (1823,) to which Dr Collins refers, and from which the above extracts are taken.

† A Treatise on the Puerperal Fever, as it prevailed in Edinburgh in 1821-22. "It visited," says Dr Campbell, "this establishment, (the Lying-in-Hospital) in 1821-22, and proved fatal to several patients," p. 16.

‡ Observations on the Diseases of Pregnancy and Childbed, (Dublin, 1840,) p. 285, 286.

above referred, you say the comparison of your practice with Dr Hamilton's is like the comparison of "light with darkness;" seeing that in the Edinburgh Hospital in 1821 and 1822, 1 in 21 died, &c.* I have above shown you, that the mortality was 1 in 30; and in my former letter to you, I stated that, *under the same circumstances*, viz., the prevalence of epidemic puerperal fever, "in one year, (1826,) when you acted in the Dublin Hospital, partly as Assistant Physician, and partly as Master, 81 out of 2440 women delivered, died, or 1 mother in every 31." In your last Letter you do not gainsay the fact of this high mortality in that year; and you further confess, that you acted as Master from November 1826; but you aver that it is "contrary to the fact," that you were Assistant Physician in the earlier part of that year. It is a matter, as you must be aware, of little, or indeed of no moment, so far as regards the main question, viz. the mortality in the Hospital under puerperal fever at that time. But I made the statement in consequence of your having published in 1836 the following remark in the Preface to your "Practical Treatise:"—"These opinions [the strikingly small mortality from protracted labour, &c.] I can, (you say,) with truth state, have not been rashly formed; on the contrary, they are the result of an anxious and diligent attention to the duties of my office, as Assistant and Master in an Hospital where I resided *for a period of ten years, commencing February 1822*, during which time, 24,119 deliveries occurred."—(*Preface*, p. 2.)

4. You observe—"Professor Simpson states, that he thinks he sent me a report of the Edinburgh Hospital, which I never even heard of."

* "The *difference* in practice [between me (Dr Collins) and Dr Hamilton,] is truly remarkable, and the familiar comparison of *light with darkness* is very *appropriate*."—*Dublin Journal* for 1838, p. 320. In this extract, I beg to state that I have left the italics exactly as Dr Collins himself originally printed them.

At your own private request, I sent you a duplicate of this report a short time ago. And lest you should still miss it, or the part alluded to, let me quote it for you. It is at the bottom of p. 2, and runs thus:—"A report has been circulated against the Hospital, to the effect that the mortality amongst its patients has been of late unusually great. The very reverse of all this is the truth. From the 21st June 1839 to the 15th October 1840, 212 women were delivered within the Hospital, and all of them recovered. During the same time, 718 cases of labour occurred among the out-patients; of these only one died, and that in consequence of organic disease of the heart. It is confidently presumed, that no Lying-in Hospital in the United Kingdom can show so small a mortality within the same period. In the great Westminster and Dublin Hospitals, from 1 in 80 to 1 in 150 of the women delivered, have, for some years past, died during their confinement. Among the patients of the Edinburgh Hospital, only 1 in 900 died within the period alluded to."

I do not state this result of 1 death in 930, when puerperal fever was absent, with any view whatever of boasting of superior success over others, such as you have so repeatedly and strongly claimed for yourself, when, under the same circumstances, (viz. the absence of puerperal fever,) you lost 1 in 186. But I recall your attention to the result, for another reason, viz. in consequence of the averment or assertion that you thought fit to make in your first Letter, that 1 death in 21 "is the *only* record in existence (I quote your own words) of the mortality of this public charity," the Edinburgh Lying-in Hospital.—I am perfectly aware that a practice, under selected time and selected circumstances, such as your return of 1 in 186 in Dublin, and the above of 1 in 900 in Edinburgh, does not represent the general average mortality at other times, and under other circumstances. During your first three years' Mastership, when puerperal fever was occurring in the Dublin Hospital, you lost 1 in 53; and during the

first two years of the Edinburgh Maternity Hospital,* viz. from 1844 to 1846, when puerperal fever was more or less prevalent in the house, there was exactly the same mortality of 1 in 53; 7 women dying among 574 in-patients. Among the out-patients of the same Institution, (the Maternity Hospital,) during the same period, only 4 died out of 1101 deliveries, or 1 in 275. The mortality among the in-patients was somewhat above the proper standard, and the mortality among the out-patients was somewhat below its standard, in consequence of out-patients, when severely ill, being sometimes transferred to the Hospital, and in one or two instances dying there. But I believe you will find, that the markedly greater mortality here, as elsewhere, in Hospital, than in Dispensary or in private practice, arises principally, or indeed almost entirely, from the greater liability of Hospital patients to puerperal fever; and when this "*accidental*" complication (as you term it) chances to be absent for a time from an Hospital, the rate of mortality may be decreased unusually for that time, as during the fortunate "*accidental*" absence of it in the last four years of your Mastership, and in the Edinburgh Hospital for 1839, 1840.† If, however, the Master or Captain of a sailing vessel happened to make an unusually fortunate run from Liverpool to America, or to the West Indies, in consequence of the "*accidental*" circumstance of having a fair

* To prevent confusion, it may be necessary to state, that two different Edinburgh Lying-in Hospitals are alluded to in the course of these letters; viz. 1. The old "Lying-in Hospital," Park Place, opened in 1793, and broken up in 1842, in consequence of debt and defective funds; and 2. The "Maternity Hospital," opened in St John Street in 1844, and now removed to Milton House, Canongate. See a Report of the practice in the Maternity Hospital, during its first two years, in the last number of the *Monthly Journal of Medical Science*.

† It is perhaps unnecessary to observe, that the same cleansing of wards, and other precautionary measures, which apparently prevent puerperal fever for some years, totally fail in preventing it at other times, showing still more strongly its "*accidental*" character.

wind during the latter half of the voyage, surely that is no reason for him to arrogate to himself a knowledge of seamanship superior to all other Masters and Captains, who did not happen to meet in with the same "accidental" fair wind. If such a person should in consequence talk modestly of his nautical skill being, as compared with that of his compeers, as "light with darkness," and forthwith vote to himself a "laurel crown," pray, in *your* opinion, what should his compeers really think of him? I object to telling you my opinion.

In your first Letter in the Provincial Journal, you declare the mortality in the old Edinburgh Lying-in Hospital to have been "unprecedented." At the time of your controversy with Dr Hamilton, Dr Moir, who acted for many years as physician in that Hospital, summed up with great care and labour the results of 2890 cases of delivery within the Hospital, which had occurred in the course of the fourteen previous years; and with the intention of publishing them. But other duties interfered at the time with that intention; he lost interest in the question; and the papers have since lain beside him in an unfinished state. He empowers me, however, to state, that among these 2890 cases, only 11 deaths, or about 1 in 260, occurred from other causes than puerperal fever. There was scarcely, if indeed ever, a single year of perfect freedom from attacks of puerperal fever; and 36 of the 2890 patients died from its effects, or about 1 in 83. Altogether, including the "accidental" deaths from puerperal fever, 47 women died out of the 2890; or about 1 in 61. During the same term of years 5916 of the patients of the Institution were delivered out of the Hospital; of these 5916 out-patients 24 died, or 1 in every 246; and of these 24 deaths 14 were from puerperal fever. These cases, as I have said, occurred during fourteen years, viz. from 1823 to 1837. In your Hospital in Dublin during the same fourteen years, viz. from 1823 to 1837, (and with all your superior advantages in accommodation, in attendants, in space, in

riches, in large annual Government and other endowments,) out of 34,170 delivered, 472 died, or 1 in 72; and an enormous number of the children were lost. These years include, too, your four "accidental" years in which there was no puerperal fever. If we exclude the 10,785 cases which occurred during these four "accidental" years, then in the Dublin Hospital, from 1823 to 1837, 1 in every 56 died. In the Edinburgh Hospital, at the same time, among the in-patients 1 in every 61 died. Among both the in and out-patients taken together, the maternal deaths amounted to 1 in every 124. Methinks, then, if the mortality in the old Edinburgh Lying-in Hospital was (as you assert) "unprecedented," it has, at least, been very fully "equalled" elsewhere.

But let us proceed one step further, and inquire which of the two Hospitals, the Dublin or the Edinburgh, presented the greatest *obstetric* mortality during these years.

Now you have repeatedly and at various times and places declared that puerperal fever may be considered an "accidental" complication* in Hospital practice; and hence, I take it, you believe that the deaths from this cause, being independent of the previous *obstetric* treatment of the patients, may be omitted (as you have proposed to do,) in any computation where we attempt to trace a statistical difference in the effects of different modes and forms of actual obstetric practice. In the Edinburgh Hospital, during the fourteen years above mentioned, out of 2,890 deliveries, 36 women died of puerperal fever, or 1 in 83; and 11 died from other causes, or 1 in 263. The published returns of the Dublin Hospital do not show how many died during these fourteen years of puerperal fever, and how many of other more truly obstetric causes. But your returns for seven of these years show this:—During these seven years, out of 16,414 mothers, 56 died from puerperal

* See, for example, your Treatise, p. 365; your Papers in the Dublin Journal, vol. xi. p. 42; and vol. xiii. p. 406; and your first Letter in the Provincial Journal, p. 537. (See POSTSCRIPT.)

fever, or 1 in 293; and from other causes 108, or 1 in 152. Putting then aside, in both Institutions, the deaths arising from that "accidental" cause—puerperal fever, the resulting obstetric practice is as follows:—In the Edinburgh Hospital, 1 in every 263 mothers died; in the Dublin Hospital, 1 in every 152 mothers died. Perhaps these results will be more clear if I place them in a tabular form as follows.

Hospital.	Total Cases.	Number of Maternal Deaths from Puerperal Fever, (an "accidental" cause.)	Deaths from <i>other</i> Obstetric and Pathological Complications and Causes.
Dublin.....	16,414	56, or 1 in 293	108, or 1 in 152
Edinburgh...	2,890	36, or 1 in 83	11, or 1 in 263

This table and the preceding statements will, I think, satisfy you that the *obstetric practice* of the Edinburgh Hospital is by no means so "frightful" in its effects as you have at different times anxiously attempted to declare it. And I have sincere satisfaction in publishing these results, for this reason; that they will show, to all unprejudiced minds, on what extravagant and erroneous grounds you proceeded in that attack of yours upon the late distinguished Dr Hamilton, to which you and I have both already referred in the course of these Letters.

5. In a postscript to your first Letter to me, you denounce the mortality of the Edinburgh Maternity Hospital, since the introduction of chloroform, as "frightful." And you come to this conclusion by perverting (no doubt unintentionally) the returns which I gave of it, in a Report on Anæsthetic Midwifery, in the October Number of the Monthly Medical Journal. The facts stated in that Report in reference to the use of chloroform in the Maternity Hospital were these. To test its usefulness, effects, mode of action, &c., in natural labour, it was stated that up to the

date of the Report it had been employed in 88 natural cases. Of these 88 mothers one died six days after delivery, from convulsions connected with albuminaria and diseased kidneys. The other 87 women recovered, their recoveries being "altogether more perfect and speedy" than usual. There were other 50 or 60 cases of natural labour in which chloroform was not used, from the labour being very speedy, &c., and all these mothers recovered. Chloroform was also used in seven morbid labours that happened to be brought into the house, viz. three cases requiring forceps, and four cases requiring turning. These patients were put under chloroform at the time of operating, to save them from the pains and shock of the operation. Two died,—one of the turning cases from rupture of the uterus; and one of the forceps cases from sloughing of the maternal passages. This last patient had been allowed to be greatly too long in labour before she was operated upon and chloroformed; and let me remark, it was the only instance I ever saw in Edinburgh of sloughing of the maternal passages; but it is an affection which is, I believe, by no means very rare in your Hospital.

Now, what do you with these data? You take the cases in which chloroform was used in natural labour; to these you add the seven cases of difficult operative delivery in which it was employed, and where the danger or death of the patient was surely not from the chloroform (for I scarcely fancy you will go the length of alleging chloroform can produce "rupture of the uterus" or "sloughing of the passages.") You thus ingeniously get 95 cases in all, with 3 deaths, and then declare 1 maternal death in 31, as "the result of anæsthesia." I will not venture to comment upon such a mode of professional argument. To carry out and perfect the same unique process of reasoning, you should have taken the 7 instrumental cases alone, and then declared 1 death in $3\frac{1}{2}$, as "the result of anæsthesia." It would have been equally fair. In the Report from which you have culled the preceding data, I have reported from various quarters 800 or 900 cases

of delivery under chloroform, and in some of them (as in the above) the chloroform was used merely *because* the cases were bad or instrumental; and yet of the whole, 7 only died—4 of the 7 from epidemic puerperal fever. I do not know if it will interest you; perhaps not; but I may add, that within the last week I have received the first volume of the Transactions of the American Medical Association. In it there is a Report by a Committee of the Association in May last, on Anæsthetics in Midwifery. From this report I quote for your information two single sentences. “The anæsthetic agents, ether and chloroform, have now been used in perhaps 2000 cases of midwifery; and, so far as the Committee have been able to learn, without a single fatal, and very few if any untoward results. The Committee, in a pretty extensive correspondence with Physicians in various parts of the country, have found an entire unanimity of opinion among those who *have tried* these agents, as to their favourable effects both in advancing the progress of the labour, and in relieving the sufferings of the patients.” (p. 228.)

6. In both your Letters you decry—or attempt to decry—the results of my private midwifery practice. When you did so in your first letter, I abstained from answering it, for I thought you had committed yourself in an unguarded and hasty moment, and would betimes see that it was wrong in a physician, and above all in you, the President of the College of Physicians of Ireland, to indulge yourself publicly in any attack upon the private practice of any member of the profession. As, however, you seem to entertain no such feelings, and repeat the attack in your second letter, let me at once answer it. In the Report on Anæsthesia above alluded to, I state that I had used anæsthetics in 150 cases of midwifery in private practice; that out of these 150 cases one child was still-born and putrid; the other 149 were born alive; and that of the mothers, 2 died from epidemic puerperal fever (which

was prevalent here at the time) ; all the others recovered. In the Report, I farther mentioned, that since anæsthesia was introduced into midwifery, I had attended 20 or 30 other cases where anæsthetics were not used, and two mothers out of this class died,*—one under an attack of puerperal convulsions and coma that suddenly came on fourteen days after delivery (she had been greatly agitated by her husband having been found dead in bed two or three mornings before); and the other dying about three weeks after delivery, from an attack of fatal syncope apparently connected with acute endocarditis. Both patients were well till a fortnight after delivery, and consequently would have been most certainly reported as recoveries instead of deaths in the Dublin Hospital, where the patients, if able, leave on the eighth or tenth day,—a means by which, you must allow me to remark, the *apparent* mortality with you is no doubt considerably diminished.† The

* If anæsthesia had happened to be used in these two cases, would you not, for one, have roundly declared the two deaths to have been the indubitable result of the anæsthesia ?

† You must permit me to direct your attention strongly to this matter (the early dismissal of your patients) as a most important source of fallacy between your returns and those of other Hospitals and practices. In addition to the two above cases of death from causes supervening suddenly on or after the fourteenth day of delivery, let me state, that out of the nine deaths alluded to in the text, as having occurred in the course of my private practice, a third, viz., the one from acute mania, came on also upwards of a fortnight after delivery. My patient was already in the drawing-room, when some very afflicting family news were incautiously communicated to her. She was led back to her bed-room maniacal; and died in a few days. Hence, I repeat, out of these nine fatal cases, three of the deaths originated in causes and attacks coming on a fortnight after delivery; and of the nine only six would have appeared as deaths in such returns as yours. For in your "Treatise" you state (p. 500) that in most instances the mothers were dismissed "eight, nine, or ten days after delivery." I do not mean to argue that you would have returned a third more of deaths, if instead of dismissing your patients on the eighth or tenth day, you had kept them, as in some other Hospitals, for fourteen or sixteen days or longer. § But I feel quite certain of this, that by thus getting your patients' names so early removed from your Register, as the eighth or tenth day, you escape several chances of disease and death liable to come on later during the puerperal month, and you thus

first of these patients was so well as to be again under the charge of her own ordinary medical attendant, Dr Maclagan; the second had been delivered so easily and speedily, that there was not sufficient time to send a messenger for me before the child was born; and her convalescence was so perfect for the first fortnight, that I did not see her above four or five times during all that period. But to quote your own words, "*I (Dr Collins) can aver, we know of no such mortality in Ireland.*" Of course. But then we have already seen that your memory and judgment sometimes sadly betray you about averments and assertions. Let me merely add, that in considerably upwards of 1000 cases of labour which I have now had in private practice, I have lost five other cases within the puerperal month; three from puerperal fever—one from acute puerperal mania—and one from slow draining hæmorrhage after delivery, there being found on dissection by Dr Malcolm and myself, upwards of forty fibrous tumours in the uterus.

And I think I am quite entitled to add, that I have had entrusted to me, for some years past, from various parts of the United Kingdom, a number of difficult and complicated obstetric cases, amounting to far, very far above the share of morbid labours that are usually met with in any common single practice. At the present moment, for instance, I have under my care, among other such cases, two of advanced pregnancy, complicated with one of the lesions, that proved fatal in a case mentioned above, namely, the presence of *large* fibrous tumours in the uterine walls; and one of these patients has come several hundred miles to be under my professional charge, in consequence of the danger apprehended from this disease at her approaching confinement.*

more or less greatly diminish the apparent amount of mortality among your patients. Phlegmasia dolens, puerperal mania, pelvic abscess, &c., are all apt to come on after that time. (See POSTSCRIPT.)

* Allow me to make in this foot-note a remark which the re-reading of the

7. A distinguished countryman of yours, an enthusiastic Irish writer and politician—Thomas Davis—a few years ago, gravely proposed, as one of the many methods of regenerating Ireland, that you and his other countrymen should reject the English language, and talk in nothing but the old Irish or Celtic tongue. “Nothing,” says Mr Davis, (I quote from his Literary and Historical Essays, p. 177), “Nothing can make us believe that it is *natural* or honourable for the Irish to speak the speech of the alien, the invader, the Sassenagh tyrant, and to abandon the language of our kings and heroes. What! give up the tongue of Ollamh Fodhla, &c. &c.” On reading over your first strange and characteristic letter to me in the Provincial Journal, your manifest anxiety to resist my calculations, and escape my inferences, seemed to me rather analogous to the reasoning of Mr Davis. “Nothing can make us believe that it is natural or honourable for the Irish to adopt the doctrines

text strikes me as being an omission. I have alluded to the frequency with which difficult cases of Midwifery happen to be entrusted to my care. I have instanced one complication, (the co-existence of pregnancy with very large fibrous tumours,) because the two cases alluded to happened to occur to my mind at the moment of writing. Take them then as an example. Such cases are sufficiently rare, (I do not remember that you mention one among your 16,414 Hospital patients,) and I think you will not deny that they are also sufficiently dangerous. I have just turned up the work on Pregnancy by your townsman, Dr Montgomery. At p. 183, he says, “When such tumours acquire great bulk, and pregnancy occurs, they give rise to a combination which is fraught with *fearful dangers* to the unhappy sufferer.” Further, be so good as remark, that I do not object to your counting the results of my private practice, provided only you count them honestly and fairly, provided you make no selections as to time, and provided also you recollect the great over-proportion of difficult and consequently dangerous cases which my position here brings (however unworthily) under my care. To go back to the same obstetric complication as an illustration, (for one illustration is as good as another,) let me add, that in the course of the last four or five years I have had placed under my charge, during labour, (and *merely* because they were considered bad and dangerous cases,) five cases of pregnancy and delivery, in addition to the two mentioned above, complicated with these *large* uterine tumours; one of the five being the fatal case alluded to in the text.

and computations of the alien, the invader, &c. &c." But in this thought, I fear I did your countrymen, and perhaps you, injustice. From several of them, as from others, I have had letters confessing the importance and soundness of the laws and generalizations which I have educed from your data, and regretting the very untenable ground which you had taken up. And since you yourself only challenge *one* of my calculations as wrong, (and yet the error there, if any, as I have already shown, is yours), I must believe that you could find no flaw in the various other and far more important computations which my first letter to you contained; otherwise you would gladly have pointed it out.

Amid the various irrelevant subjects into which you branched off in your first and second letters, let us not forget that the *main and original question* to which these unchallenged figures refer, was the question of the effect of protraction in increasing the mortality accompanying parturition both to mother and child. Dr Hamilton declared it as the result of his observations, that besides this, the protraction of labour produces in the mother also an increasing liability to rupture of the uterus, to retention of the placenta, to uterine inertia, and hemorrhage, to febrile and inflammatory affections, &c. All this you asserted was not the fact. But all this and more your own data show, by the clearest and simplest *statistical evidence*, to be the fact. Nay, if you had actually invented cases and figures to disprove your own doctrines on these points, you could not have well suggested more strong figures for the purpose than are to be found in your own Practical Treatise. I have already, in my first Letter, given you a specimen relative to puerperal fever, &c. When our busy season here is over, I shall send you a copy of tables on these points, that I calculated from your work, and laid before the Edinburgh Medico-Chirurgical Society two or three years ago, and which I certainly would have published ere now, had I not kept them back purely in deference to your very strong and marked feelings, on a sub-

ject on which I knew you had already so deeply and unfortunately committed yourself.

Before closing, permit me one word more. I beg you again to remember, that the present discussion was, in the first instance, entirely commenced on your part; not on mine. I have hitherto stood mainly on the defensive. If, in writing hurriedly, I have used at any time any expression that is in any way improper, I beg to express my sincere regret for it. And if I have made unwittingly any statement that is erroneous, I will be happy to correct it. But remember, I do not promise to answer any further communications on the present subject. If you *will* go wrong, I am not surely to be taxed to keep you right. For, my dear Sir, I have at this season, really much more important work to do, and much more important duties to attend to, than to prevent the President of the Irish College of Physicians from committing open irregularities and deviations in statistical and other professional matters. If you choose still to persist in this course, I must, of necessity, transfer the answering and further charge of you to some of my young and idler medical friends, who will doubtless, however, take all due and proper care of you, always and provided they may deem the correction of your statements worth their trouble.

And sincerely trusting that what has occurred will in future teach you to be more wary and cautious in blindly rushing into any unnecessary and uncalled for attacks upon your professional brethren, believe me, with truly the best wishes for your welfare,

Very faithfully yours,

J. Y. SIMPSON.

EDINBURGH, November 18, 1848.

POSTSCRIPT.

Since the preceding remarks were written, my attention has been called to two published statements so strongly corroborative of some of the observations that I have made in the above Letter, that I will transcribe them for you in a Postscript. The statements in question refer to, 1st, The fallacy in your mortality returns arising from the early dismissal of the patients from the Dublin Hospital; and, 2d, The non propriety (according to your own expressed views) of counting puerperal fever deaths as any evidence of the want of success in actual obstetric practice.

1st, *The fallacy in your mortality returns arising from the early dismissal of the patients from the Dublin Lying-in Hospital.* I have already insisted on this important point, and directed your attention to this source of statistical error at pp. 40 and 41, *foot-note*. By dismissing your patients "eight, nine, or ten days after delivery," (to quote your own words,) I showed you that you thus escaped several chances of disease and death to which women are still subject *after* that date, though seemingly well *up* to that time, such as phlegmasia dolens, puerperal mania, pelvic abscess, &c., and that you thus, no doubt, diminished, in a greater or less degree, the *apparent* amount of mortality among the mothers delivered.

In the excellent Report of the Dublin Hospital, published last year by Drs Hardy and M'Clintock, there is incidentally given an observation on this point, so apposite, and at the same time so confirmatory of my own remarks on this matter, that I take the liberty of quoting it for your consideration. It occurs in their Work at p. 57 :—"As phlegmasia dolens (they say) more commonly makes its appearance *after* than before the end of the first week, it is not often met with in the Hospital, the patients being discharged from thence on the *eighth day*, if perfectly well. Of twenty-two cases of this disease, reported by Dr R. Lee, in seven 'it commenced between the fourth and twelfth days after delivery, and in the remaining *fifteen* it appeared subsequent to the end of the second week after parturition.'"

2d, *The non propriety (according to your own professed views) of counting puerperal fever deaths as any evidence of the want of success in actual obstetric practice.* At p. 36, &c., I have quoted

your own reiterated statement, (see foot-note, p. 36,) that in Hospital obstetric practice, puerperal fever should be considered as "accidental;" and, consequently, the deaths arising from it are not to be reckoned as any criterion of the actual goodness or badness of the obstetric practice that has been followed in the delivery of the patients. In one published statement of yours, to which my attention has been directed, this opinion is declared still more explicitly. When Dr Hamilton, our former eminent Professor of Midwifery in this University, died, a number of competitors came forward for his Chair. One of these competitors, having the very highest claims for the vacant Professorship, was the incumbent Master of the Dublin Lying-in Hospital, your distinguished townsman, Dr Every Kennedy. In a Testimonial, written by Dr Craigie, the Editor of the *Edinburgh Medical Journal*, and published by Dr Kennedy, Dr Craigie announced (what was new to myself at the time as well as to others,) that he "had heard it urged as an objection to Dr Kennedy, that the mortality in the Dublin Hospital had been increased during his Mastership." "The mortality (argued Dr Craigie) taking place in Lying-in Hospitals, has been known ever since they were established, to depend on causes totally beyond the control of the Physician." You subsequently wrote to the Lord Provost, as principal Patron of the vacant Chair, a letter on the subject, which Dr Kennedy also published. From this letter of yours I extract the following paragraphs:—

"In Dr Craigie's Testimonial, he states, it has been reported publicly, in order to detract from Dr Kennedy's high character, that the mortality in the Dublin Lying-in Hospital has increased during the period he has had the medical superintendence. I cannot avoid describing this charge as one *totally devoid of justice*, and unworthy of the least attention of the Patrons. It must have been made by an individual completely unacquainted with the chief cause of the mortality in the Dublin Lying-in Hospital, as well as in that of every other Lying-in Hospital; or to serve a sinister purpose.

"All Physicians who have ever had the charge of a Lying-in Hospital are but too well acquainted with the fact, that puerperal fever is the great source of the mortality in such Institutions.

"It has been SOLELY owing to the occasional prevalence of this

disease, that the mortality in our Hospital has been greater since Dr Kennedy's appointment than for four years preceding his Mastership, during which there was not a single case of puerperal fever. The mortality, however, since Dr Kennedy's period in office, has not been more than ONE-HALF of that in several former periods in our Hospital, &c."*

Now in all these sentiments, I think you expressed an honest and conscientious opinion. But permit me one or two remarks. You maintain, and properly, that to object to Dr Kennedy's obstetric skill or abilities, on the ground that, in his Hospital practice, he had met with a large mortality in consequence of the prevalence of puerperal fever among his patients, is "totally devoid of justice." But on exactly the same grounds, your decrying Dr Hamilton's obstetric skill and abilities, when he lost a larger number than usual of Hospital patients in 1821-22, from the same cause, was certainly also, and equally, "totally devoid of justice." You surely will not maintain that Dr Hamilton's Hospital practice should be tried by a standard quite different from that which you publicly desired the Patrons to apply to the practice of one of the candidates for Dr Hamilton's chair. If, as you declared to the Patrons here, it is "totally devoid of justice" to judge of the Dublin Hospital in this way, surely it was "totally devoid of justice" in you to judge of the Edinburgh Hospital in this same way. And yet it was solely by throwing aside this justice that in your paper in the Dublin Journal for 1838, to which we have so often referred, you formed for yourself any grounds for attacking the results of Dr Hamilton's Hospital practice, during Dr Hamilton's own lifetime. The rule that applies to one man and one hospital, must be meted out to other men and other hospitals. And I have already shown, that if we do not take into account the deaths from puerperal fever, the results of the obstetric practice in the Edinburgh Hospital from 1823 to 1837, were 1 death in every 263 mothers; and, consequently, more favourable than the corresponding practice of the Dublin Hospital, during the same period, where, independently of puerperal fever, there was 1 death in every 152 mothers.

Permit me to call your attention to another part of your statement. You say, "The mortality since Dr Kennedy's period in

* See "Statement, Testimonials, and other Documents in favour of Dr Evory Kennedy," p. 96.

office has not been more than ONE HALF* of several former periods in our Hospital." The published Register of the Hospital which you lately were so good as send me, shows, that in the seven years of Dr Kennedy's Mastership, 13,167 women were delivered in the Hospital. Of these women, 224 died, or 1 in every 58. But as this, according to your own account, is not more than *one half* of the mortality in "several former periods" of the Dublin Hospital, then it necessarily follows, that in these "several former periods," the mortality in the Hospital has been as high, at least, as 1 in 29. Knowing *this*, surely Dr Hamilton did not deserve to be reviled by you for losing, in a smaller, far less commodious, and far more crowded hospital, in two years, (1821 and 1822,) patients to the amount of 1 in 30; and that too during one of the most severe puerperal epidemics that has occurred in Scotland since the commencement of the present century.

Altogether, then, I have strong hopes that your own sense of charity and justice, which urged you to address the Patrons of the University of Edinburgh with the preceding argument in favour of Dr Kennedy, will yet enable you to make the *amende honorable* to the memory of Dr Hamilton, and acquit him (on exactly the same grounds as you wished the Patrons of the University to acquit Dr Kennedy) from all obstetrical blame or demerit, on account of losing a proportion of Hospital patients from the "accidental" occurrence of puerperal fever. And, my dear Sir, you may rest assured of this fact, that an ingenuous confession and retraction of error, will never detract from your character, nor from the character of any other professional man. J. Y. S.

* This word is printed thus in small capitals in your own published Letter.



