With the Author's compliments

TO PROFESSOR SIMPSON,

OF EDINBURGH,

IN REPLY TO

HIS LETTER TO DR. COLLINS,

ON THE

"DURATION OF LABOUR AS A CAUSE OF MORTALITY

AND

DANGER TO MOTHER AND INFANT."

Second Letter.

BY ROBERT COLLINS, M.D.,

President of the King and Queen's College of Physicians in Ireland, and late Master of the Dublin Lying-in Hospital, &c., &c.

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TO PROFESSOR SIMPSON, ETC.

My Dear Sir,

I did not think it would be necessary to address you a second time, but your report of the Edinburgh Hospital, in the last number of the *Monthly Journal*, which did not reach Dublin until the middle of this month, renders it essential that I should do so.

In the letter you have addressed to me in the *Provincial, Medical, and Surgical Journal of England* for this month, in reply to my letter to you in the preceding number, you assert "I *maintain* that the protraction of labour is not a material cause of danger to the mother;" I positively deny ever having advanced any doctrine so monstrous, my observations having invariably been published in condemnation of *rash* and *mischievously hasty* measures; in support of which I endeavoured to prove, that the mortality from protracted labour was "strikingly small," and that the mortality arises chiefly from other causes. Is it credible I should state that, in cases of extreme difficulty, there is little danger to the patient? It is, indeed, an infantile assertion on your part. I thought I had contradicted this statement in terms sufficiently strong, in my last letter, to prevent its reiteration. I have referred to my Practical Treatise, in which I declare "there is no subject connected with the practice of midwifery so difficult to acquire a sound knowledge of, as the treatment of tedious and difficult labour; it is one of the most vital importance, and in the most marked manner distinguishes the experienced from the inexperienced practitioner." I also added, in my letter, that I

thought it necessary to repeat the above declaration, as, from the opinions you expressed, your readers might hastily conclude that I had no knowledge whatever of the danger of protracted labour, whereas there was no subject that had caused me greater anxiety, or occupied my thoughts more.

In support of your assertion, quoted above, you have made out a theoretical table from my data, to PROVE that when the patient is only a *few hours* in labour, and where there is *no difficulty* whatever in the delivery, the mortality is *vastly less* than in the opposite cases, in which, from *deformity* and other causes, the *utmost difficulty* is to be encountered to get the child through the pelvis. This table, indeed (as you say), needs not a word of comment. It must have surprised my medical brethren to have a *discovery* revealed, so important as that of 3,537 women delivered within *one* hour, without any difficulty, there were only eleven deaths, or one in 322; whereas, in 130 cases where the labour was above thirty-six hours, there were twenty-four deaths, or one in six. When, however, I add, that these 130 cases included all the really hazardous deliveries in the vast number of 15,850 women, many of whom were brought to the hospital, having been in labour three, four, and five days; and others, who, in consequence of great deformity, were obliged to be delivered by the crotchet, two, three, or four times; the discovery becomes a *mere innovation* on the ordinary mode of communicating our ideas. Laborious and difficult labours, caused by deformity of the pelvis, or great disproportion between the size of the pelvis and the child, if *frequently* met with, would, indeed, be productive of vast mortality. I have, however, clearly shown, that we had only ELEVEN DEATHS in labours protracted beyond forty-eight hours, out of 16,414 deliveries; so that a death from this cause does not occur, even in *hospital* practice, more than *once* in 1,492 cases—nay, more, I have satisfactorily proved, that in all the labours exceeding even *twenty-four hours*, there was only one death in 391; whereas, in the labours extending from ONE hour up to TWENTY, the deaths were in the proportion of ONE in 134. Thus, the mortality is more than *twelve* times the amount in labours *under* twenty hours, compared with the mortality in labours *above* forty-eight hours. A death from labour exceeding forty-eight hours is, indeed, a rare event—one in 1,492; whereas, in labours under twenty hours, eleven or twelve deaths occur in that number. Therefore, although

labour accompanied by malformation or disproportion, is extremely hazardous, the mortality from it is "strikingly small," notwithstanding the *imaginary proportional tables* you have given us; still more especially when we reflect, that the proportional mortality above given, of one death in 1,492 deliveries in all labours protracted beyond forty-eight hours, is the result of *hospital practice*, where the most hazardous cases are *accumulated;* and that it is notorious, as will be proved in a subsequent part of this letter, that in the same number of cases *out* of hospital, the mortality would not be more than *one-half.* A death from labours exceeding forty-eight hours, should not occur in *private practice* more than once in 2,984 deliveries; and thus, we demonstrate, scarcely ever to be met with by most practitioners. I can truly state, that I never had a death from protracted labour, and the invaluable results which I am about to publish of Dr. Joseph Clarke's practice will prove, that he never met with such an occurrence in nearly fifty years' active practice; and, will you believe it, he only used the crotchet twelve times, the forceps once, and that without effecting the delivery, in 3,847 labours. I shall not now state his mortality further than to assure you, it was not one in forty-two.

The grand point for the practitioner seriously to consider is, *how the patient can be conducted with the greatest safety* through the most laborious labours, where there is extreme difficulty in the birth, owing to deformity in the brim of the pelvis, or other causes.

You advocate the speedy delivery of the patient with the forceps, or by turning the child; but having so little practical experience of your own to submit in *proof* of the safety of this mode of proceeding, you struggle to prove, what no *physician ever doubted*, that protracted labour is *dangerous !!*

You then give us the enlightening discovery made in your table, that where the labour is protracted beyond 36 hours, the *proportion* (!) of deaths is one in six.

As you have so little information to submit respecting the results of delivery, by *turning* the child in deformity of the pelvis, let us examine the mortality resulting from the speedy mode of delivery by forceps, in the hands of some of the most experienced practitioners. These are cases which, in my humble opinion, should be attended with *trifling danger* to the patient, compared with those where the disproportion is so great as to make it *impracticable* to deliver with this instrument.

Dr. Churchill has given an instructive table on this point, from which I select the following results:—

Dr. Granville	1 death in	5
Dr. Ramsbotham	1 ,,	3½
Dr. Gooch	1 ,,	6
Mad. Lachapelle	1 ,,	5½
Dr. Boer	1 ,,	9
Dr. Ritzer	1 ,,	6½
Dr. Andree	1 ,,	8
Dr. Küster	1 ,,	4
Dr. Adelman	1 ,,	7

Here we have the mortality in some instances considerably greater, after hasty delivery with the forceps, where, as I assert, the difficulty we have to meet *is not to be compared* to the danger to be encountered where we are compelled to deliver with the crotchet. Other practitioners, given in Dr. Churchill's table, were much more successful with the forceps in private practice; but the high character of the individuals is well known to the profession, and the mortality sufficiently demonstrates what the results would be, if this method of delivery was universally adopted. Amongst the French and German practitioners, where the forceps are in constant use, the average mortality amounts to 1 in 13½, and this including all the *ordinary* cases of labour where the delivery was effected by the forceps; whereas, in 134 cases in our hospital, which you have tabulated, all of which were *protracted* from 25 to 36 hours, the mortality was only one in 17.

In my friend, Dr. Churchill's, tables, he has arranged the results of *hospital* practice with *private* practice and that of *out patients* at their own homes. This renders any general average *quite valueless*, as no comparison *as to mortality* should ever be made between hospital patients and patients out of hospital. The two are as opposite as the antipodes, and must be *scrupulously separated* to form any useful comparison. I did not think that you would have committed the *great error* in this respect throughout your letter, which I shall point out before I conclude.

The late Professor Hamilton states, that "minute information on practical points is not to be derived from the general results in hospitals, and nothing can better illustrate this, than the fact that, in 2,889 patients delivered *in* the Edinburgh Hospital, there were *eight* crotchet cases; but in 4,328 *out patients*, there were only *seven*. The explanation," he adds, "is obvious—Deformed women are sent from various distant quarters into the hospital, in consequence of its being evident, from their shape, that their labour may probably be difficult; on the other hand, the out patients afford a fair specimen of the ordinary practice in the lower ranks. Crotchet cases, accordingly, occurred in the patients delivered in the hospital once in 361, and the same cases were met with in the out patients only once in 618." In further illustration of the same argument, Professor Hamilton adds, that there "were *in* the hospital 38 forceps cases, being one in 76; while among the *out patients* there were only 28 forceps cases, or one in 154." Your own reports also *amply* testify the increased mortality in *hospital* patients, compared with *out patients:* thus you had seven deaths in 374 deliveries *in* the hospital, and only four in 1,101 *out patients.*

I need do no more than refer to the cases reported in my Treatise, where it will at once be seen the *utterly hopeless* state in which *many* of the patients who died were admitted. I have stated, that in 106 cases where the labour was extremely severe, nearly *half* of the patients had been one, two, three, four, or more days in labour, before admission into hospital, and most of them grossly mismanaged.

The cases of rupture of the uterus, to which you referred in your letter, are a good example of the increased mortality which hospital reports must necessarily exhibit. *Five* of the patients were admitted after this very fatal occurrence had taken place.

That *protracted* labour, however, is not a chief cause of this accident, it is sufficient to state, that more than *half* of those delivered in the hospital in which it occurred were not more than *twelve hours* in labour; in *three* cases only did the labour exceed 36 hours; and seven of the 34 were first pregnancies.

I should hope what I have stated will fully satisfy my professional brethren of the necessity of protesting against the comparisons you *unfairly* make between my hospital results

and the results of *private practice* and *out patients*, and that in future you will strictly confine your statements to the former.

I requested you, in my last letter, when in future you referred to the mortality of the patients under my care in hospital, and recommended a different mode of treatment, to state candidly that Dr. Collins's practice, which you propose to improve, is much the most successful on record, as you know of no report of 10,785 cases, with a mortality nearly so small as *one in* 186. You state that there is no such "laurel to pluck from my brow," either by you or any one else. In support of this assertion, you designate the *entire* deliveries in the hospital, for the long period of *four consecutive years*, and more than *eight months*, including 10,785 births, *selected ! !* cases. Surely this is too preposterous, and requires no refutation. I doubt not my readers will feel satisfied that the number of cases are *amply* sufficient to demonstrate the results as to the mortality from the *protraction* of *labour* and *all other causes*, exclusive of puerperal fever, the mortality from which has no more to do with the subject I have to discuss with you, as to the means of delivery in protracted labour, than a patient dying from cholera after delivery.

The second effort you make to remove my "laurel" is an equally fruitless attempt; you state that "in the Dublin Obstetric Hospital, superintended by Dr. Churchill, one mother in 219 died (puerperal fever included); one only in 274, where puerperal fever deaths were not included." If you had read Dr. Churchill's Report, you would have found that, in those contained in volumes xiii. and xv. of the *Dublin Quarterly Journal*, he has clearly stated that 128 patients were delivered *in* the hospital in the one, and 92 *in* the hospital in the other, making together 220, out of which there were three deaths, or one in 73. The remaining cases are as clearly stated to be *out patients*. In the report made by Dr. Churchill, in volume xxiii, the number of patients delivered *in* the hospital is not stated, nor can he now supply the particulars, for which I applied to him.

The third effort comes home to your own hospital, which is the place I like to meet you; here, indeed, the results speak volumes. You state that "in a printed report of the Edinburgh Maternity Hospital, then lying before you, 1 in every 134 mothers died (puerperal fever included)—1 in every 368

(puerperal fever not included).—*See Monthly Journal for Nov.* 1848." What do we find in the report referred to? This simple fact: that of 374 patients delivered in two years, *in the Maternity Hospital*, under your care, seven mothers died, or 1 in 53½; all the other deliveries were *out patients!!* So much for that report. I have, however, accidentally found another from the same Institution, under your care, in the June number of the same journal for 1847, page 934, made by you to the Medico-Chirurgical Society of Edinburgh, to which *no reference* was made. It is extremely brief—namely, "Report of the instrumental deliveries in the St. John-street Maternity Hospital, by Professor Simpson. The deliveries in two years amounted to 1,400: *one* out of every *twenty-one* mothers died!! The forceps were used three times, the crotchet once." This *exactly* amounts to the mortality which I reported under Professor Hamilton, which occurred a quarter of a century ago; so that there is not a shade of improvement—nay, it is, in reality, nearly double Professor Hamilton's frightful mortality, as his patients were delivered *in* the hospital, whereas a very large proportion of the 1,400 must have been *out patients* (although no distinction is made), in whom I have clearly shown the mortality is usually about *one-half.*

Any comment from me could add nothing to the *force* of these reports. The results having been withheld, however, when commenting so fully *on mine*, reminds me of your countryman's sage advice—

"Aye free, aff han' your story tell,
When wi' a bosom crony;
But still keep something to yoursel',
Ye scarcely tell to ony.
Conceal yoursel' as weel's you can,
Frae' critical dissection,
But keek through ev'ry other man,
Wi' sharpen'd slee inspection."

In your letter you state, in reply to my remark on the excessive mortality of one in twenty-one in the Edinburgh Hospital, under the late Professor Hamilton, "*you know that this high mortality arose from the prevalence of puerperal fever.*" I beg to say, that Dr. Hamilton positively declared that *not one case* of puerperal fever occurred in the period stated.—See the late Dr. Mackintosh's Essay, published in

1823, where he offered any proof required that the mortality was one in eighteen!!

The fourth effort you make to justify you in stating my results are not the most successful on record is, that "most of the readers of the *Provincial Journal* could show a better return than what I have done!!" This assertion is only equalled by the late Professor Hamilton's well-known appeal (!) "*to the testimony of the public opinion of the city of Edinburgh!!*" On such a foundation it will require some ingenuity to build.

The fifth and last effort you have made from "*the Registrar-general of England and Wales!*" is a similarly appropriate appeal, and must have equal weight. To make such *visionary comparisons* with the critically minute record I have given of the results in the Dublin Lying-in Hospital, requires *no exposure* beyond the mere mention of the fact.

Having thus developed the visionary records brought forward to prove that you were *not bound* to state, in future, that Dr. Collins's practice, which you propose to improve, was the most successful known (as you declared others were more successful), I must again call upon you to acknowledge this apparently unpalatable truth. I doubt not you are intimately acquainted with all the hospital results published, and a simple reference is all I require. Let us have, however, no more *wanderings*, but keep strictly within the *walls of the premises;* and until you storm the fort with more skill and happier results, the "laurel" is likely to flourish where it has been placed.

I again unequivocally assert, that the annals of medicine afford no even distant approach to the happy result of *one* death in *one hundred and eighty-six*, in any other hospital than ours, in 10,785 deliveries, and that puerperal fever has no more to do with the deaths from *protracted* labour, than it is likely to have in yours.

You state that "the accoucheur, in every case of labour, has charge of two lives: the life of the mother and the life of the infant; and that out of 16,414 mothers and 16,654 infants—viz., 33,068 lives, I lost one in every 27." You add, that out of 150 children born under your care in private practice, only one was still-born, and of 300 lives thus intrusted to you, only one in 100 was lost, whereas I lost one in 27. Why, may I ask, did you make this statement, as it were to

place me in the *shade*, and *withhold* the report of your *hospital* (to which, or some other hospital, I beg you will specially recollect, from this time forth, you must strictly confine your comparisons), where, in fair competition with you, I revel in the sunshine. This was, what I think, would be called in Scotland "too canny."

I find in your hospital report, which you state you had at the time *before you*, that out of 1,417 mothers, and 1,436 infants—viz., 2,853 lives, you lost one in 25 !! ; one in 53 of the mothers died; and every *fourteenth* child was *still-born !!* and the reader will *carefully* recollect that 1,101 of the patients, amongst whom this mortality occurred, were *out patients*, where deaths are usually about *one-half !!* the proportion of those in hospital. What more do we find in this *notable* report? The *astounding fact*, that of 313 deliveries, the labour in 53 *exceed* 24 hours, or in the vast proportion of *one* in *six !!* nay, more, of the 313 the labour in 14 cases *exceed* 36 hours, or in the large proportion of one in twenty-two !! Let us examine my report, which you have ventured to criticise with so much sentimentality.

In 15,850 deliveries, under my care, the labour exceeded 24 hours in 264 only, or in the proportion of one in *sixty;* so that you had *ten times* as many labours protracted beyond 24 hours, as "a cause of mortality and danger to the mother and infant," as I had. Again, of 15,850 labours, I had 130 cases extending beyond 36 hours, or in the proportion of one in 122, whereas your proportion was one in 22 !! With these unquestionable *truths* before our readers, your observations on the *protraction of labour*, in the patients under *my care*, most inevitably sink below the level of deserving consideration. *Six times* as many labours exceeding 36 hours, and *ten times* as many exceeding 24 hours, "causing mortality and danger to the mother and infant," not one word of which you state, but rashly "throw stones from this house of glass ! !" What can our readers now think of your observations on the "*sufferings*" of the patient, and the number that "*perished*" from protracted labour under the care of *others*, as stated in your essay on chloroform, so widely circulated in the *domestic circles*, when your own cases were *ten-fold* more numerous ! !

The following table exhibits similar results from other reports; those of the Dublin Lying-in Hospital, by Dr. Joseph Clarke and myself, hold a remarkable position.

Table to show the *proportional* number of labours exceeding

24 and 36 hours duration, under the care of the following physicians :—

PHYSICIANS.	Total Cases of Labour.	Labours exceeding 24 hours.	Proportion of Labours above 24 hours.	Labours exceeding 36 hours.	Proportion of Labours exceeding 36 hours.
Dr. Collins	15850	264	1 in 60	130	1 in 122
Dr. Simpson	313	53	1 in 6	14	1 in 22
Dr. Joseph Clarke ...	10387	134	1 in 77		
Mr. Lever	4666	62	1 in 75		
Dr. Thomas Beatty ...	1182	69	1 in 17		
Dr. Churchill	1285	166	1 in 8		
Dr. Maunsell	839	36	1 in 23		
Dr. Merriman	2947	134	1 in 22		
Dr. Granville	640	104	1 in 6		

—See *Churchill's Midwifery*, pp. 159, 189.

You charge me, in your observations on the above subject, with criticising you for losing two mothers, in your private practice, from puerperal fever, when using anæsthetics. I made *no* observation of the kind. I stated that you lost *four* patients out of 170, or in the large proportion of one in 42.

Your theoretical "Memoir on Turning, as an alternative for Craniotomy and the Long Forceps, in deformity of the brim of the Pelvis," if thoughtlessly acted upon, must inevitably be followed by *lamentably different results* to what I have reported in the Dublin Hospital; and I do, indeed, shudder at the thought, to recommend, without practical experience of your own, the rash, and, in my humble opinion, unjustifiable proceeding of the introduction of the hand into the uterus, in the early stage of a *first* labour, to turn the child, where we can have no proof of the necessity, and where, if the necessity, as in your opinion does exist, from *deformity* of the pelvis, no hope of delivering one child, in a multitude, alive, is unquestionably a mischievous practice. I have proved that in all the labours in the hospital, which exceeded even *twenty* hours duration, and the mother died, above *one-third* of the children were born *alive;* and that of the 15 women who died subsequently to delivery with the crotchet, all but one were *first children,*

and *males*, which facts militate incalculably against your theory. When we reflect that above *one-third* of the children were born alive in all the protracted labours, which ended fatally to the mothers, and that the most extreme danger is confined to *first* labour, where, after the hazardous operation of turning is accomplished, the rigidity of the soft parts alone would, in every instance, cause so much hindrance to the passage of the child as to make it nearly hopeless to save its life; but where *malformation* of the pelvis is to be superadded, the danger to the mother and the child becomes so excessive as to make your recommendation to turn unwarrantable, so far as I am capable of forming a conclusion. Herculean efforts to attempt to drag a child through a *deformed* pelvis, in a *first* labour, must ever meet with strong condemnation from the experienced physician. You state, however, that the term "experience," which I so often repeat, is misapplied. The profession, however, can alone judge of experience by the soundness or unsoundness of the deductions drawn from the knowledge acquired by the individual. I fully agree with Bacon, that "vague and arbitrary experience astonishes rather than instructs." It is upon such grounds that I protest against your advancing theoretical opinions with so much confidence, before you have had time or opportunity to mature your judgment, and all without any substantial PROOF TO SUBMIT of the *validity of innovations*, which are directly opposed to accumulated *facts*, and the opinions of those who have had the most authentic sources for acquiring information, and who, I believe, may appeal with confidence to their professional brethren as to their *competence* to form sound deductions, although you have rashly ventured to *insinuate* that you !! possess an incomparably superior understanding.

In your own *self-satisfied* feelings (I suppose) of the numerous *discoveries* you have published for the benefit of the profession, you say "you would fain excite me, if you could, to expend more of my abilities and talents upon the real advancement of that branch of medicine which we both practice." This is thoughtful and modest, but your standing in the profession badly qualifies you to be my instructor. I shall, therefore, persevere as I have done, in communicating from time to time to the profession, such facts as I may deem worthy of their consideration, and which my judgment warrants me in believing may be of enduring benefit to our fellow-

creatures. This duty I shall ever study to fulfil, and hope, when I am no more, those who come after may not consider me to have been altogether so unworthy as your observation is intended to convey.

I feel I have also another duty, little less important, to discharge, which the vast number of pupils I have instructed renders so imperative upon me, that I never can lose sight of it, which is, to continue to earnestly warn junior practitioners against the adoption of rashly formed, and purely theoretical opinions, unsupported by the *results of practical experience.* Without submitting this *grand test in proof* of what we advance, our opinions are quite undeserving of consideration; such might well be designated "gun-cotton practice!!" so extremely dangerous is it to come in contact with, and which so often *voluntarily* explodes, without leaving the slighest trace behind.

Sir Philip Crampton well states, in his "Outline of the History of Medicine," "that a *mere speculation,* however ingenious, if it be not only *unsupported* by facts, but in direct opposition to them, cannot be received as a doctrine; and any inferences which may be drawn from such a speculation, *must be as false as the foundation upon which they rest.*"

The next point I shall notice is your theoretical table of *proportions,* to show the *vast number* of children *still-born* from protracted labour.

I have stated a simple fact, that of 1,045 cases accurately noted, 844 were delivered within *twelve hours,* and 932 within 24 hours—nay, more, the tables I have given exhibit *one* child out of every 19 still-born, where the labour does not exceed *twelve hours;* whereas, there was only *one* child still-born out of every 473 births, with a labour *exceeding* 48 hours; 844 were still-born under 12 hours labour, and 35 only with the labour exceediug 48 hours. So much for fanciful *proportional* theories.

To physicians of experience it is scarcely necessary for me to observe, that, in the vast majority of *still-born* children, the medical attendant has *no means whatever in his power* to control or prevent the occurrence. The chief mortality is caused by the death of the child in the womb *previous* to labour, so as to cause its expulsion in a *putrid* condition, and likewise by the expulsion of the child *prematurely.*

Of 1,121 *still-born* children which I have reported, no less

than 527 were *putrid.* The proportion in *private practice* is still greater. That the child is seldom expelled *putrid* from *protracted* labour is well known, as, when it is so, the head yields so readily to pressure, that, unless the pelvis be very defective, or the parts very rigid, the labour is usually of short duration. This is quite obvious in the tables given by me, where it may be seen that in *all* the still-born children (113 in number) where the labour exceeded even 24 hours, 23 *only* of the children were *putrid;* and it is equally obvious from these tables, that of *all* the *mothers* who *died,* five only were delivered of *putrid* children.

It would, I think, be a waste of time to say more to prove that the mortality in still-born children, from *protracted* labour, is "strikingly small."

The mode adopted for the delivery of the child in cases of great danger, from deformity of the pelvis, or from great disproportion between the size of the child and the capacity of the pelvis, is one that you, and some other practitioners, comment upon with profound refinement of sentiment, so as to make those who adopt delivery by the crotchet appear (to those who are incompetent to estimate its advantages over other methods of delivery) in a most unenviable light; in fact, that they have no regard whatever for the life of the child. This is a subject that I have ever written upon, and instructed my pupils with feelings of responsibility I could not describe; and I feel the jocular observations you have made, as to "crime nor treason, &c.," deserving of the disapprobation of every medical man in the empire.

I have stated in my Treatise on the subject of delivery by the crotchet, "that this is an operation *no inducement* should tempt any individual to perform, except the imperative duty of *saving* the *life* of the mother, when placed in imminent danger; and that, in my opinion, *no consideration* should induce him to do so, as long as the child is alive."—See page 18, 359. With such a statement before you, I doubt not your allusions will be considered very unbecoming, and your sentiment very ethereal. After describing the difficulties which long and ample experience conscientiously satisfied me rendered delivery with the crotchet by far the most eligible, I add, "that I have no difficulty in stating, after the most anxious and minute attention to the point, that where the patient has been properly treated from the commencement of her labour; where strict

attention has been paid to keep her cool; her mind easy; where stimulants of all kinds have been prohibited, and the necessary attention paid to the state of the bowels and bladder; that, uuder such management, the *death* of the *child* takes place, in laborious and difficult labour, before the symptoms become so alarming as to cause any experienced physician to lessen the head, and this is a fact which I have ascertained beyond all doubt by the stethoscope."

You *may* say, and the late Professor Hamilton did say he would deliver otherwise, and save both mother and child with more success than I have done; but the results, in *four long years*, of one death in 21 in the Edinburgh Hospital, as referred to above (two reported by you and two under Professor Hamilton); also, the results of two years in the last Edinburgh Monthly, as reported by you, of one mother in 53 having died, and every *fourteenth* child having been *still-born;* likewise, your report of one death in every 31 for a period of 18 months, as noticed in my former letter; and, lastly, the report of one death in every 42 for the like period in your private practice, in all embracing nearly *six years* experience; such a record unspeakably warns us against measures followed by a mortality, I believe, unknown out of the Maternity in Paris, where similar means of delivery are pursued.

When we contrast the above mortality with one death in 100 in 16,414 deliveries, as I have reported in the Dublin Lying-in Hospital (including 56 deaths from puerperal fever); but still more, when we contrast it with one death in 186, including *every fatality* for a period of *four consecutive years and eight months* in the same *hospital*, and embracing 10,785 deliveries, the comparison of darkness with light is only more striking.

I have next to remark upon your statements as to the mortality which takes place in children, subsequent to birth, from the effects of *protracted* labour.

Here your theoretical table of *proportions* must be equally instructive with those I have already noticed.

Will it be believed, that out of 16,654 births, only *two* children died, previous to the mothers leaving the hospital, where the labour *exceeded* 48 hours, or in the proportion of one in 8,327 births; whereas, 246 died where the labour did not exceed 12 hours!! or in the proportion of one in 57.

I have stated, in my Treatise, that of 16,654 births, 284 died,

or in the proportion of one in 58½; which included all the deaths in children born prematurely, and in twins; also, every instance where the heart even acted, or where respiration ceased in a few seconds after birth.

Of the 284 children who died, *one hundred* were *premature* births, *thirty-two* were twins, 17 of which were premature—*seventy* children, born at the full time, died (one-half within 24 hours), and the labour in *one single instance only* exceeded 24 hours—nay, 62 of the 70 were born within 12 hours; fifty of the 284 who died, had presented preternaturally; 37 of the 284 died of trismus nascentium; in nine instances there was little doubt the mothers destroyed the children; and in nine other instances, the children were either born in an imperfect or diseased state. With such a statement before us, is it not folly to talk of protracted labour as a chief cause of mortality to the child after birth? I would specially refer to page 500 of my work, where every circumstance is *minutely* given, and which cannot fail to remove every doubt, that the mortality in children, subsequent to birth, from the effects of a *protracted* labour, is "strikingly small," notwithstanding any *delusive proportions!* to the contrary.

You ask why I did not state that, during the three first years of my mastership, out of 5,629 women delivered 106 died, or one in 53? I answer I could not state so and state a fact, as there were 7,547 delivered in the period stated, and 110 deaths (*including fifty-six* from puerperal fever), or one in *sixty-eight.* You say I was assistant physician in the hospital in 1826, which is not the fact. I was appointed master in November of that year, and my calculations are all given from that period. You then ask why I did not report the mortality for a period of five years, during a time I had nothing whatever to do with the practice of the hospital!!!

I have thus noticed all the *visionary statements* put forward by you, to prove that the protraction of labour is a chief cause of mortality to the mother and infant, and submit, for the consideration of the profession, the validity, or otherwise, of my statements.

I have demonstrated that, even in *hospital* practice, we do not meet with a death in labours exceeding 48 hours, oftener than once in 1,492 deliveries; and in *private* practice such an event is not likely to be met with more than once in 2,984 labours. The mortality, therefore, I assert *is* "strikingly

small," notwithstanding the high *proportional* fatality you have endeavoured to exhibit.

I have also demonstrated that, with regard to the infant, we have only one child *still-born* in every 473 births, with a labour exceeding 48 hours; whereas, one in 19 is still-born, where the labour does not exceed *twelve* hours.

I have similarly demonstrated, with regard to the death of the infant subsequent to birth, that only two children died previous to the mother's leaving the hospital, where the labour exceeded 48 hours, out of 16,654 births! whereas 284 died, where the labour did not exceed *twelve* hours.

The mortality, therefore, I unequivocally state to be "strikingly small" to both infant and mother, from protracted labour, no matter what *imaginary proportions* indicate.

I have demonstrated that, so far as I know, my hospital report of one death in 186, out of 10,785 deliveries, is the most successful on record, and call upon you to refer me to any similar report.

I have demonstrated that your own report of the Edinburgh Hospital proves, that the mortality has been vastly beyond what is known in any similar institution, with the exception of the Maternity in Paris.

I have demonstrated that you had the report of your own hospital before you when you commented upon the conjoint mortality to the mother and child, in my hospital practice of one in 27, and stated the mortality in your *private* practice to be one in 100; but did *not* state that, in your own hospital, every *fourteenth* child was *still-born*, and that one out of 53 of the mothers *died;* or, conjointly, in the proportion of one in *twenty-five!*

I have demonstrated that you had *six times* as many labours *protracted* beyond 36 hours as I had, and *ten times* as many exceeding 24 hours; not one word of which you mentioned in your letter.

I have demonstrated that the mortality in the Edinburgh Hospital was one in 21, for a period of *two years;* one in 53½ for a second period of *two years;* and one in thirty-one for a third period of 18 months; although your letter states that your results were more successful than mine! and that you had only one death in 134, puerperal fever included, and one in 368 in its absence!

I have demonstrated that the mortality in Dr. Churchill's

hospital was 1 in 73, in place of 1 in 219 (puerperal fever included), and 1 in 274 in its absence, as stated by you.

I have demonstrated that the excessive mortality under the late Professor Hamilton, of one death in 21, did not arise from puerperal fever, if his own statement was correct, although you state I knew it did.

I have demonstrated that I did not criticise you for losing two patients in your private practice, from puerperal fever, when using anæsthetics and that my statement was, that you lost 4 out of 170, or in the large proportion of 1 in 42.

I have demonstrated that the deliveries under my care, in the Dublin Lying-in Hospital, during the first three years of my mastership, were 7,547, and not 5,629, and that I was not assistant physician in 1826, as stated by you.

And, lastly, and especially I call upon you to prove that I have not demonstrated that your assertion that I *maintain* "the protraction of labour is *not* a cause of danger to the mother or infant," *originated* in *your fruitful imagination*, and not from any opinion advanced by me.

Since the above was written, my attention has been directed to the statement made by you, in the report given in the "Edinburgh Monthly Journal" for this month, to the effect, that the report now submitted is an *extension* of that made to the Medico-Chirurgical Society, to which I have alluded.

This would, indeed, *prove* the great danger of the *protraction* of hospital results!! Thus, in the former report given by you, "one out of every *twenty-one* mothers died;" the deliveries amounted to 1,400; whereas I now make the mortality *in* the hospital, one in fifty-three.

The difference, however, is of little consequence, in a practical point of view, between a mortality of one in 21, one in 31, one in 42, one in 53, as are shown to be your results in the *various* reports noticed.

The circumstance, however, requires a full explanation, as to why you stated a mortality of "one in 21," out of 1,400 cases, and now convert the 1,400 patients into 1,475, with one death in 134!! but which is, in *reality*, one death in 53, so far as the deliveries *in* the hospital are concerned.

I am, dear sir,

Very faithfully yours,

ROBERT COLLINS.

Merrion-square, Nov. 25, 1848.

P.S.—Since the above letter was sent to press, a second letter

has been addressed to me, by Professor Simpson, which is a catalogue of mere delusions, as I shall demonstrate in this postcript, in the order he has placed them.

Delusion, No. 1.—Dr. Simpson occupies nearly three pages in an attempt to mystify the number of patients delivered in the Hospital, during the three first years of my residence. The perseverance in error must now be intentional, as I wrote to him, when his first letter was published, and directed his attention to page 378 of my Practical Treatise, where he has full information as to his misstatement. It is there clearly shown, that from February 7, 1829, to the expiration of my Mastership, (November, 1833,) a period of four years and eight months, including 10,875 deliveries, we had 58 deaths, or one in 186. These 10,875 deliveries he subtracts from tbe total deliveries, and declares the remainder, 5,629, to be the number delivered in the first three years, whereas the registry which he had proves the deliveries to be 7,547. This he endeavours to justify, by saying that, in some of the calculations I made, I gave (to avoid the statement of *fractional parts* of a year, which were minutely given *elsewhere*) the deliveries as 10,875 in the last *four* years, after puerperal fever disappeared. When he printed the present letter, however, and perseveres in stating the mortality in the three first years as one in 53, in place of one in 68, *including* 56 deaths from puerperal fever, he had my letter in his possession, pointing out the additional *eight months*, so that this exposes the first delusion.

Delusion, No. 2.—Dr. Simpson *asserts* that the late Dr Mackintosh *no where* asserts that the mortality in the Edinburgh Hospital, in 1821 and 1822, was 1 in 18; "that he knew far better than to make any such rash misstatements of simple facts, and that he is sorry, indeed, to add, that it is an assertion of Dr. Collins's own." This is genuine *mystification* on Dr. Simpson's part, as Dr. Mackintosh's Essay, to which I referred, published in 1823, clearly shows. This Essay will amply repay the reader, and verify every iota of what I stated although not in the *identical words* upon which Dr. Simpson endeavours to contradict me.

I have stated, in my letters to the late Professor Hamilton (to which I would specially refer in the Dublin Journal), that the average deliveries in the Edinburgh Hospital, for a period of 41 years, amounted to 126, which, for the years 1821-22, gives a total of 252. Dr. Hamilton acknowledged 12 women died in

the two years, and, according to *his statement, not one case* of puerperal fever occurred. This was the frightful mortality of 1 in 21, which I remarked upon; but Dr. Mackintosh, in his essay, *offers any pledge* as to the truth of his statement, that *fourteen* women died in the two years, which I am satisfied will convince my professional brethren of the undoubted truth of my assertion that he stated the mortality to be one in 18. I have added in my letter that the number I had given as delivered in 1821-22 might be slightly incorrect, owing, perhaps, to a greater proportional number being delivered in some years than others, but the difference could effect the calculation to no material extent. To take an average was the only means I had in my power to find the number delivered in 1821-22, as the annual number was withheld in the report. I find I was not far astray, as even by Dr. Simpson's return the deaths were 1 in 30, and with his number of deliveries, Dr. Mackintosh's mortality is 1 in 25. Dr. Simpson pathetically states, "It *grieves him*, and he is sure must grieve my best friends and well-wishers, to see *me anxious to deny*, that the 12 women died of puerperal fever." This is worthy of Dr. Simpson. He then details the undoubted symptoms of purperal fever, under which the patients died, from Dr. Mackintosh's essay, which was EXPRESSLY PUBLISHED to prove that Professor Hamilton's declaration, that *not one case* of purpureal fever occurred in the hospital, was *utterly at variance with the facts;* and because I have asserted, from Dr. Mackintosh's work, that Professor Hamilton *did make his peremptory denial*, Dr. Simpson good naturedly expresses his grief for me. I am now happy to relieve his mind, assuring him, that I never in my life expressed an opinion on the subject, as it would, indeed, be presumption in me to do so, when Professsor Hamilton and Dr. Mackintosh, who were on the spot, and acquainted with every particular, could not agree. I have given my authority, and Dr. Mackintosh's statements require no elucidation.

Delusion, No. 3.—Dr. Simpson remarks, that I said he thought he had sent me a report of the Edinburgh Hospital, which "I never even heard of," and he adds, that at my own private request he sent me a duplicate of this report *a short time ago!* When Dr. Simpson's letter appeared last month I wrote to him for the report and asked him if he had sufficient proof to *satisfy himself* that it might be *strictly depended* upon as *correct!* This

veritable report! was then sent to me without one word of assurance from Dr. Simpson as to his belief of its authenticity, and the entire report, so far as regards deliveries *in* the hospital, is contained in a *note* to the *managers'* statement of the *funds*, &c., of the charity, to the effect that, from the 21st of June, 1839, to the 15th of October, 1840, 212 women were delivered within the hospital, all of whom recovered. ALL the remainder were OUT PATIENTS; and I rather think, when Dr. Mackintosh and Dr. Hamilton differed so materially as to the mortality in the *very small* number of patients delivered *in* the hospital, most of my readers will place *no confidence whatever* as to the results to the patients delivered in every quarter of the City of Edinburgh. It is absolute *trifling* with the profession to state such records as *facts!!*

Dr. Simpson has, in his present letter, given other returns of the mortality in the Edinburgh Hospital during *certain remote* periods of its management, under the late Professor Hamilton; but these can afford no information to be relied upon, as Dr. Mackintosh's essay fully proves. I have a return of the number of deliveries, both in and out of the hospital, from 1829 to 1838, which Dr. Simpson furnished me before he was elected professor, at the same time he informed me the mortality to either mother or infant was not to be had. If such could have been supplied, *my earnest and repeated entreaties* to the late Professor Hamilton, to supply those unequivocal *tests* of the success of the practice he advocated, must have brought them to light. Dr. Simpson need not now struggle to supply us with such broken and untrustworthy reeds, but should supply his own results *in* hospital, of which he has given a very scanty allowance as yet; and when he does, I shall submit with meekness to his comparisons.

Delusion, No. 4.—Dr. Simpson states, that, in a postcript to my former letter, I denounced the mortality in the Edinburgh hospital under the use of chloroform, as "frightful," and that I came to this conclusion by unintentionally perverting the returns. I deny in the most emphatic terms any perversion whatever; I stated the deaths to be three out of 95, or one in 31. What is Dr. Simpson's own version? Of 88 *natural* cases delivered under chloroform, one died; chloroform was also used in *seven morbid!* labours "that *happened!* to be brought into the house;" of these, two died. As 88 and 7 make 95, we need say no more as to perversion. Why, three cases re-

quiring the forceps, and four requiring the child to be turned, should be called *morbid labours! and not to be reckoned*, requires some explanation. How carefully did Dr. Simpson *collect*, in his *imaginary proportional tables*, ALL the *morbid labours* in 16,414 deliveries that "happened to be brought to the Dublin Hospital," many of them nearly lifeless. I made no remark whatever as to the share chloroform had in the unhappy results stated. We have on record, however, several deaths, of which there could be no doubt it was the immediate cause; and I have never even heard of *one life* having been saved by its use.

An admirable address, delivered by my friend, Professor Montgomery, at the last meeting of the Dublin Obstetric Society, which is shortly to be published, contains valuable information on this subject.

Delusion, No. 5.—Dr. Simpson ventures to state that I have decried his private practice, and that it was wrong in a physician, and, above all, the President of the College of Physicians in Ireland, to indulge himself in an attack upon the private practice of any member of the profession. I feel satisfied my professional brethren will acquit me honourably from so foul a charge. I simply stated the *facts* he had published as an *example* to others, and I felt it my duty to state the unsuccessful results. If doing so be unprofessional I plead guilty, but others must judge between us.

It is rather strange that he conspicuously contrasts his *private practice!* with my hospital reports, at the same time carefully withholding his own; and yet, when I direct attention to the same points, he questions my knowledge of professional decorum. I can state with truth, however, that there is no circumstance connected with my professional pursuits ever afforded me the same real gratification, as the feeling that, however undeserved, I have always possessed the good opinion of my brethren, and I will add, that it shall ever be my highest ambition to retain it.

Delusion, No. 6.—Dr. Simpson states, in conclusion, that this discussion was entirely commenced by me, not by him. Those who have read the medical journals, however, for several years past, and the popular essays on chloroform, with which the domestic circle has been so wantonly shocked with the "sufferings" of women in protracted labour, and the number that "perished," well know the fallacy of this assertion. From the Palace of Royalty in England, to the Viceroy in Ireland,

and to the Fellows of Trinity College, Dublin, and sundry other learned individuals, these singularly censurable Pamphlets have been supplied, plausibly founded upon the authority of Dr. Collins, when the "sufferings" of the patients under Professor Simpson, from protracted labour, which were proportionally TEN TIMES more numerous, and SIX TIMES more fatal, have been scrupulously concealed. These misdeeds, after a lengthened period of forbearance on my part, were followed by an essay "on Turning, as an alternative for Craniotomy and the long Forceps," *so fraught with danger*, that I felt it my duty to call the serious attention of the profession to the entire of these *innovations*, and I believe the *exposure* is likely to be followed by good results.

Dr. Simpson states he will not promise to answer any further communications on this subject.

As his observations, in publicly addressing me, sometimes nearly approach his private correspondence, I fear I shall be constrained to treat him in the same way I have already done with regard to the latter, and to request that when, in future, he has nothing more polite to communicate, he will save himself the trouble of writing.—*Extracted from the British Review, Dec.* 15, 1848.

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