

On turning, as an alternative for craniotomy and the long forceps / by Robert Collins.

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Note Dr. Collins's best respects
to Dr. Ferguson
3.

ON TURNING,

AS AN

ALTERNATIVE FOR CRANIOTOMY AND THE LONG FORCEPS.

By ROBERT COLLINS, M.D., DUBLIN.

TO PUBLISHED BY SIMPSON, & CO., 5, CRYSTAL PLACE.

MY DEAR SIR,—

I beg to thank you for forwarding me the proof sheet of your "Memoir on Turning, as an alternative for Craniotomy and the Long Forceps, in Deformity of the Brim of the Pelvis." You are so good as to state in the letter which accompanied the first part of your memoir, that, "If it contains any errors of statement I shall feel greatly obliged by your pointing them out that I may correct them."

It gives me pleasure to comply with your request, and I shall feel obliged by your publishing my letter as an appendix to your memoir.

The first statement I shall notice, is the *partial*, and, consequently, *erroneous* calculations you have given from my work; withholding the *total mortality*, as well as the *cause* of the fatal result, in *every case* of protracted labour met with, out of the vast number of sixteen thousand six hundred and fifty-four births, minutely recorded by me. Surely this is a great omission on your part; and thus, as you so justly condemn in others at page 9,* "you have drawn your deductions, not from the *whole* of the practice, but from *parts* only; you have not reckoned upon the certain results of the *general* collection of facts, but depended upon the fallacious results of isolated and individual instances." Again, you well remark, "the medical mind has ever been too apt to recollect and found upon those facts only which are in favour of any preconceived opinion or opinions it may chance to have adopted; and the causes of failure are too often forgotten amid the more agreeable remembrance of the cases of success," "It is on this account that the numerical method of reasoning and investigation, by obliging us to count up *all* our cases and *all* our results, whether good or bad,—whether instances of recovery or instances of death,—is no doubt destined to revolutionize, in a great degree, our modes of inquiry, particularly in surgery and midwifery, by imparting infinitely more precision and certainty to our present deductions and precepts where they are true, and shewing us in language that cannot be misunderstood, the erroneousness of our doctrine where they are not true."

In these sentiments I ~~candidly~~ agree; and the new era which my "Practical Treatise" has established in reporting the results of obstetrical practice throughout Europe, affords me extreme satisfaction. No report that is not *minute* can now command the attention of the profession; and where *all cases* and *all results* are faithfully recorded, mere extracts taken from such registries, can have no weight with those who are competent to judge for themselves.

The various errors you have fallen into by founding

your doctrines upon *partial* data, and thus totally obliterating what is really most instructive in the *general* results, should not, I think, have occurred with the ample details I had furnished, and unquestionably not without printing what has been recorded as diametrically opposed to your conclusions. You extract from my work, that during the seven years I had charge of the Dublin Lying-in Hospital 16,414 women were delivered in it; and of those 164 died, or 1 in every 100; and this you have followed up by a table, which you think proves that the deaths are increased in proportion to the duration of the labour, viz., where the duration of labour was within 1 hour, one woman in 322 died; from 2 to 3 hours, one in 231; from 4 to 6 hours, one in 134; from 7 to 12, one in 80; from 13 to 24, one in 26; from 25 to 36, one in 17; above 36 hours one in 6.

Why were these *obscure* calculations given, and the following highly important general results, and *cause* of death, in **ALL CASES** where the patient was more than **TWENTY HOURS** in labour withheld?

It is clearly stated by me that there were *forty-two* in number out of 16,414; or in the proportion of 1 in 391; and of the forty-two women who died, where the labour was above 20 hours, *one-third* of the children were born alive,—of the forty-two, *three* died of typhus fever; *nine* of puerperal fever; *one* of stricture of the intestine, with effusion into the thorax; *three* where the placenta was retained; *two* of convulsions; *one* of abdominal inflammation previous to labour; *nine* of rupture of the uterus; *one* of inflammation of the intestines, with pus in the uterine sinuses; *three* of anomalous disease; *one* of diffuse cellular inflammation; *six* of inflammation &c., subsequent to difficult labour; *one* of ulceration and sloughing of the vagina; *one* of disease of the lungs and hæmorrhage; and *one* of abdominal abscess.

Where we see from the simple statement given, that only forty-two died who were even above *twenty* hours in labour, and at the same time *look to the cause of death*; the fallacy of *extracts* in support of fancied realities is apparent.

Why, I would again ask, is my table not given which shews the *cause* of death in all cases in the hospital, and proves that 97 of the 164 deaths arose from causes *not* the *results* of childbirth? Considerably above **ONE-THIRD** of the entire mortality during my Mastership, occurred from *puerperal fever* during the first three years. That this very intractable disease 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 eighty-eight; 71 of which were delivered within 12 hours, and 80 within 24 hours. If we were *only* to deduct the deaths from this fatal disease, which may be

* Provincial Journal, February 9th, 1848, p. 58.

considered accidental, the *entire* mortality for seven years would then be less than *one in one hundred and fifty-six*. And why is it not stated that for the *four* last years of my residence in the hospital *after puerperal fever disappeared*, during which period the very great number TEN THOUSAND SEVEN HUNDRED and EIGHTY-FIVE deliveries occurred, *fifty-eight* only died, or in the proportion of *one in ONE HUNDRED and EIGHTY-SIX*?

The annals of medicine afford no even distant approach to this favourable, and I will add happy result, in an equal number of a similar class of females.

It is clear to me, even overlooking the friendship you have at all times proffered to me, common justice demands a full acknowledgement of this *vitally* important practical test, particularly when you venture to condemn the practice of one who has had so much greater experience than yourself. When you have had similar opportunities of acquiring practical knowledge, and are enabled by a *different* mode of treatment, to publish *happier* results for the benefit of your professional brethren and the public, your opinions will then deservedly be entitled to that just respect, which the promulgation of improvements, founded upon practical experience, must ever command. I must, however, with the greatest respect, both for yourself and your professional talents, protest against the authenticity of purely theoretical opinions, given with so much confidence by a junior member of the profession, who has not yet had time nor opportunity to mature his judgment.

The evil deeds which your theoretical doctrine of turning is calculated to give rise to, among junior practitioners, strongly suggested to me what I have stated, and which, be assured, is done in good part.

Your proposal is to substitute delivery of the child by turning, instead of lessening the child's head, in the most laborious labours, where there is extreme difficulty in the birth, owing to *deformity* in the brim of the pelvis. (I do not remark upon the long forceps, as I have no pretensions to do so from practical knowledge.)

This is unquestionably a dangerous doctrine, more especially when we consider the great facilities which now exist for throwing the patient into a state of insensibility, which deprives her of the power of either expressing or feeling, the grievous injuries she may sustain from the most barbarous efforts of the unskilled practitioner, the after-consequences of which must, in a great proportion of cases, be calamitous.

That a degree of force totally inconsistent with sound practical knowledge, must be used, (after the very *hazardous* operation of turning is effected,) to get the child through a deformed pelvis, where in very many instances, even after the head is lessened, great difficulty is experienced, is obvious; and where such disproportion between the child and the pelvis exists, it is equally obvious the child cannot pass alive. Need I state to any physician of practical experience, the awful mortality which must inevitably occur to the mothers, when delivery is effected by such measures, compared with what I have so fully recorded of the practice you propose to alter.

Of 16,654 births in the hospital, delivery was effected in 79 by lessening the head, on account of *extreme* difficulty in the labour; or where the child was *dead*, and interference desirable, owing to the state of the mother; in six of the 79, delivery with the forceps was attempted, but no force consistent with safety could accomplish it. *Fifteen* only of the 79 women died, and in *none* of the

15 was death caused by any injury sustained in the actual delivery. The proportion of such deliveries, as I have fully stated, is greatly increased in hospital, by the same patient returning *two, three, or even more* times, in whom, from deformity or other circumstances, such mode of delivery is unavoidable. Another cause which *greatly* increases the proportion, is the *frequent* admission of patients after having been *two, three, or four* days in labour, and as the record of the cases shews, most of whom had been grossly mismanaged. Of 106 cases where the children were *still-born*, and the labour severe, nearly *one-half* were of this description.

It is a remarkable fact, that of the examples given by me, at page 487 of my "Practical Treatise," of *repeated* delivery of the same patient by the crotchet, but *one* woman died, thus *satisfactorily* proving, that where *death* succeeds this operation, the fatal result is not dependent on the mode of delivery, but upon the circumstances demanding such interference.

No theoretical reasoning, nor any other argument, without the *test of practical experience*, could for one instant cause me to listen to a statement to the effect, that if the children *had been turned* in the seventy-nine most trying and critical cases I met with, the results would have been more favourable; nay, I shudder at the thought.

What *test* could we have to place in opposition to what I call the *astounding practical fact*, that out of ALL the PROTRACTED, LABORIOUS, and TRULY DIFFICULT labours, where *delivery* was effected by the crotchet, in the unprecedented number reported, of sixteen thousand six hundred and fifty-four births, only *fifteen* proved fatal? Why is this simple and *overwhelming truth* not stated by you, simultaneously with the theoretical data you have given to support the statement you have made, that where the labour exceeded forty-eight hours, one patient in *three* died? Why did you not likewise state that only *eleven* patients died who were above forty-eight hours in labour out of the 16,414? Was it not imperative upon you to do so, as from your PROPORTIONAL extracts you have left your readers in total *ignorance*; for instead of the deaths being the *marvellously* small number of fifteen out of 16,654 births, your observations are of so truly dismal and *feeling* a character, that those who had not the clear statement I had given on the subject, which you had before you in my work, must unquestionably conclude the mortality from PROTRACTED labours, in which the crotchet was used, *frightful* indeed. I have little doubt, however, most practical physicians will agree with me, that the simple truths here stated remove the visionary gloom cast over your ~~words~~ *words*.

I have still further given accurately the CAUSE of death in each of the fifteen women who died, from which it appears that out of the fifteen, *five* died from the effects of labour *previous to admission*, and are therefore not justly to be reckoned in the hospital practice; *five* from causes not the result of *protracted* labour, and two from the effects of *hæmorrhage*, where *the hand* had been passed into the uterus; so that it is *thereby demonstrated*, that in the total number of the most trying and *hazardously-protracted* labours met with in the vast number of 16,654 births, we have, I fearlessly designate it, the INCREDIBLE MORTALITY of THREE patients, and this under the treatment which you fancy you could improve, but in support of which improvement you have so little practical experience of your own to submit to the profession.

I have stated in my introductory observations on tedious and difficult labours, that 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 labours; it is one of the most vital importance, and, in the most marked manner, distinguishes the experienced, from the inexperienced, practitioner. This information can only be obtained by diligent and persevering attendance at the patients' bedside; all other sources are comparatively worthless, and when not conjoined with practical experience, dangerous. I think it necessary to repeat the above declaration, as from the opinions you express, your readers might hastily conclude that I had no knowledge whatever of the danger of protracted labour, whereas there is no subject has caused me greater anxiety, or occupied my thoughts more.

My statements have invariably been made strictly in condemnation of *rash* and *hasty* measures, in order to prove, that where the patient is properly treated during the progress of labour, the *mortality* from the effects of *protracted labour* is strikingly small; and I do not think it possible to submit facts more unquestionable for the satisfaction of my professional brethren than I have done. Even supposing the entire fifteen women who died after delivery with the crotchet, to have died from the effects of protracted labour, this would only amount to the *one-eleventh* part of the mortality from other causes; but as I have shown that only *three* of the number can fairly be so attributed, the proportional mortality is reduced to one in fifty-four.

The strong observations I had before made upon this deeply interesting subject, and published in the *Dublin Journal of Medical Science* for March, 1837, and some of the succeeding numbers, to which I would specially refer, were made, as you are well aware, to counteract the *hasty*, and if generally acted upon, *mischievous measures*, urged at that time by the late Professor Hamilton, for the *artificial dilatation* of the mouth of the womb within twelve or fourteen hours, and the *actual delivery* of the patient within twenty-four hours from the commencement of labour.

I now apprehend, if possible, greater danger, should your theoretical proposal be acted upon, and as it holds out the very strong inducement, of at once relieving the practitioner from the fatigue of a protracted and anxious attendance, the thoughtless or careless might possibly be unable to resist this great temptation, if not *forewarned* of the fatal consequences to both mother and child.

Of the fifteen cases of protracted labour we have recorded, which proved fatal, *fourteen* were delivered of *first children*, all *males*, which clearly points out the greater size of the *male*, and greater ossification of its head, as one of the chief difficulties to be encountered. How can any practitioner pretend to know, in the *early stage* of a *first labour*, (except where the diminution is *extreme*, which is rarely to be met with,) whether the pelvis be of such a size as to permit the head to pass, or whether the ossification of the head be such as to yield to the force of uterine action?

We totally disbelieve in the *accuracy* of the minute measurements given by some writers, of the brim and outlet of the pelvis in the living subject; and even were we certain of its capacity, a child of 7lbs. or 8lbs. weight would pass with *facility*, where one of 10lbs., 12lbs., or 13lbs., could, by *no possibility*, be delivered without diminution.

To turn a child in the *early stage* of a *first labour*,

where we can have no knowledge as to the practicability or impracticability of the natural effort to accomplish the delivery, is in my opinion wholly unjustifiable. Our registry *proves* that the mortality is ~~merely~~ confined to *first pregnancies*, where there has been no previous knowledge of the capabilities of the pelvis, and where, under ordinary circumstances, the rigidity of the soft parts, renders turning hazardous to both mother and child; but where deformity of the pelvis exists, or where the child is extremely large, as is invariably the case in very protracted labours, the results must be disastrous. I venture to prognosticate, that out of 79 such *trying* cases as we have reported, the mortality, instead of 15, would be much nearer to one-half of the total number of mothers; and a living child would be a rare event. Over and above the sad evils resulting from its adoption in *really laborious labours*, we shall have innumerable bad consequences from the turning of the child, in order to *expedite* the labour, *where no deformity or disproportion whatever exists*. In truth I see no limit to such mischievous proceedings, where chloroform is used, and the attendant is not conscious of the real dangers of turning the child.

Perhaps you are not aware of the *fact*, that in hospital, when puerperal fever prevails, the introduction of the hand into the uterus for *any* purpose, is usually followed by the most fatal results,—so much so that few escape. This should be carefully recollected, and when a tendency to puerperal peritonitis exists out of hospital, the risk is very great.

I shall now notice your statement that “the infantile mortality attendant upon parturition increases in a ratio progressive with the increased duration of the labour.”

In reply to this theory, I shall only state the simple truth which you have *omitted*, that of 1045 cases of still-born children accurately noted; *eight hundred* and *forty-four* were delivered within *twelve hours*, and *nine hundred* and *thirty-two* 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 extinguish all speculative theories. I have also stated, when we consider the class of patients admitted into the Institution, where *extreme* poverty is the only passport demanded,—and the *very great number* admitted after having been *one, two, three, or more* days in labour, most of whom are grossly mismanaged,—besides the numerous cases sent in actually *almost dead*, as the reported cases witness, the success of the treatment pursued will be still *more* apparent.

That your opinions should totally disagree with mine, on the advantages to be derived from the use of the stethoscope, as a means of ascertaining the life or death of the child, in *trying cases of protracted labour*, is what I had no doubt of; and when you state the mighty boon which auscultation offers us in such cases to be, the delivery of the infant with the *long forceps* so as to *preserve* its life, few will wonder at our disagreement! Had I not made a better use of this invaluable assistant in the 79 cases we met with, the mortality would not have stopped at fifteen, nor twice that number.

I could not picture to my mind a greater outrage in practice, than the attempt to drag a child through a deformed pelvis, or an extraordinary large child through

merely

an ordinary pelvis, with the long forceps, *more especially* as I have proved that the most trying cases are met with in *first labours*, where no relaxation of the soft parts can take place so long as the head remains at the brim of the pelvis; also, that in most laborious labours the pelvis measures little more than *three inches* from the pubes to sacrum; in others less than this; and that when we consider the blades of the smallest sized long forceps used in Britain, even when *completely closed*, measure from $3\frac{1}{2}$ inches to $3\frac{3}{4}$, it is clear that were the bones of the pelvis denuded of their *soft parts*, there would not be space to admit of their application. Your observations upon my recommendation of the use of the forceps in prolapsus of the umbilical cord, when *the child is so situated as the head can be reached with safety*, are strongly misapplied, as in the one case there is ample space to use the instrument without injury to either mother or child, whereas in the other I have already shown this to be impracticable, without exposing the patient to unjustifiable danger. It is to be supposed, if you had not some experience of the mischievous effects of the long forceps under such circumstances, you would not now be so anxious to abolish the use of them; but as we say in Ireland, you have leaped "out of the frying pan into the fire."

As you so fairly state that I am the only writer who furnished the profession with data showing the duration of labour, either in natural or operative cases, and that my "Practical Treatise on Midwifery" is a work, the great value and candour of the facts contained in which it would be difficult to overpraise, I would solicit from you, in return for the information which cost me so much labour to supply to my professional brethren, that henceforth, and upon all occasions, either in your lectures as a University Professor, or in your writings, you would plainly state the *total number of cases* under each head you refer to, and at the same time give the *No. of the case* which is affixed to each, in order to enable the profession to judge for themselves of the correctness of reference.

I would also *claim* from you, as a reward for my industry, that when you refer to the mortality of the patients under my care, and recommend a *different mode of treatment*, you would honourably state, that Dr. Collins' 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 one hundred and eighty-six*. This is only an act of *simple justice*, and I should not object to your *then adding*, if Dr. Collins had adopted *my treatment*, *none of the patients* would have died.

I feel called upon to seek the declaration of this *important truth* from you in *future*, as in your essay lately published on "The Use and Effects of Chloroform in Obstetric Practice," you set forth, in the most feeling manner, the "*sufferings*" of the patients, and the number that "*perished*" from prolonged labour under my care; whereas, if you had fairly stated the facts I had so clearly adduced, your theoretical calculations must have proved visionary. I would not now allude to this essay had its circulation been confined to our professional brethren, (to which I have no doubt all medical men agree with me in thinking it should have been strictly limited,) but finding it has been introduced into the domestic circles, where I cannot consent to correct the statements it contains, I am

induced to request you to blend with your popular writings 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.

I am, dear Sir,

Very faithfully yours,

ROBERT COLLINS.

Merrion Square, Dublin,
Oct. 6th, 1848.

P.S. Since the above letter was forwarded for publication, Professor Simpson has been so good as to send me a Report on "Anæsthetic Midwifery," which he has just published in the *Edinburgh Monthly Journal of Medical Science*, detailing the results of cases that occurred in his private practice, and also under his care in the Edinburgh Lying-in Hospital, from January, 1847, to October, 1848, being a period of one year and nine months. From this Report it appears that in Dr. Simpson's private practice about 150 patients were delivered in a state of anæsthesia, and that there were 20 or 30 other cases of labour in which anæsthesia was not employed. The total number of those cases we may calculate as 170, four of which died, or in the large proportion of 1 in 42. In reference to this result, I can aver that we know of no such mortality in Ireland. I have been more than a quarter of a century engaged in the profession, with a reasonable share of practice in the highest ranks of society, and during the whole of that time I have not met with a greater number of deaths in my midwifery practice, than Dr. Simpson has reported in the twenty-one months; and I have now nearly ready for publication, the entire results of the late Doctor Joseph Clarke's private practice in this metropolis, extending over a period of nearly fifty years, and exhibiting an accurate and unprecedented record of three thousand eight hundred and forty-seven cases, which fully corroborates my statement. The information I have to publish will, I entertain no doubt, prove of extreme interest to the profession, as hitherto we have been left in total ignorance of the accurate results of extensive private practice in the higher ranks of society.

The following account of the results of anæsthesia in the practice of the Edinburgh Lying-in Hospital, has been furnished to Dr. Simpson, by his resident assistants, Dr. Duncan and Mr. Norris:—

The use of anæsthesia in labour became general in the hospital shortly after the discovery of chloroform, since which, 95 women in all have been delivered in the house under its influence; among these, 88 were natural, and seven were morbid labours. Of the 95 cases, three mothers died, or in the frightful proportion of one in 31; of these three cases, one died of convulsions, one from sloughing of the maternal passages, subsequent to the long forceps, and one from rupture of the uterus, where the child had been turned. Fifty cases were delivered in the hospital without chloroform, but as the result to the mother and child is not given, they are not to be included. When we compare the above mortality with what I have reported of the Dublin Lying-in Hospital, under similar circumstances,—namely, one death in 186 cases, I am satisfied my professional brethren will admit the justice of the acknowledgment I have demanded from Dr. Simpson, on all future occasions, when he thinks fit to criticise my practice, and, on points of such vital importance, to introduce my work as his authority.

The fatal results in the Edinburgh Lying-in Hospital, as now furnished by Dr. Simpson, closely approach the unprecedented mortality of the same Institution in 1821 and 1822, of one in 21,* which unfortunately is the only record in existence of the mortality in this public charity.

Had the deaths been one in 31 during my Mastership of the Dublin Lying-in Hospital, I should have had the melancholy duty of reporting the appalling number of 529 deaths instead of 164.

* *Dublin Medical Journal*, 1837 and 1838.