

An explanation of the real process of 'spontaneous evolution of the foetus'.

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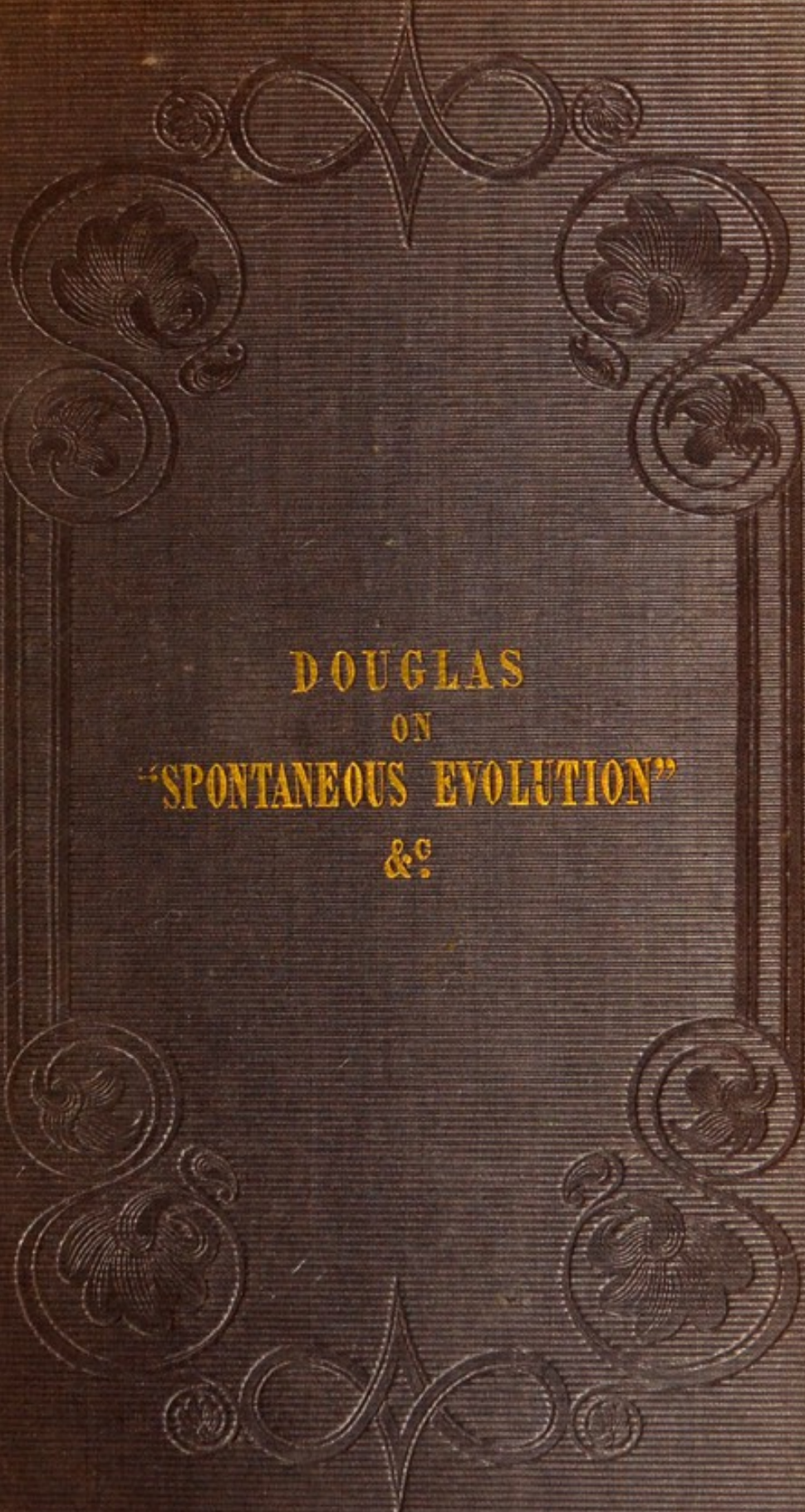
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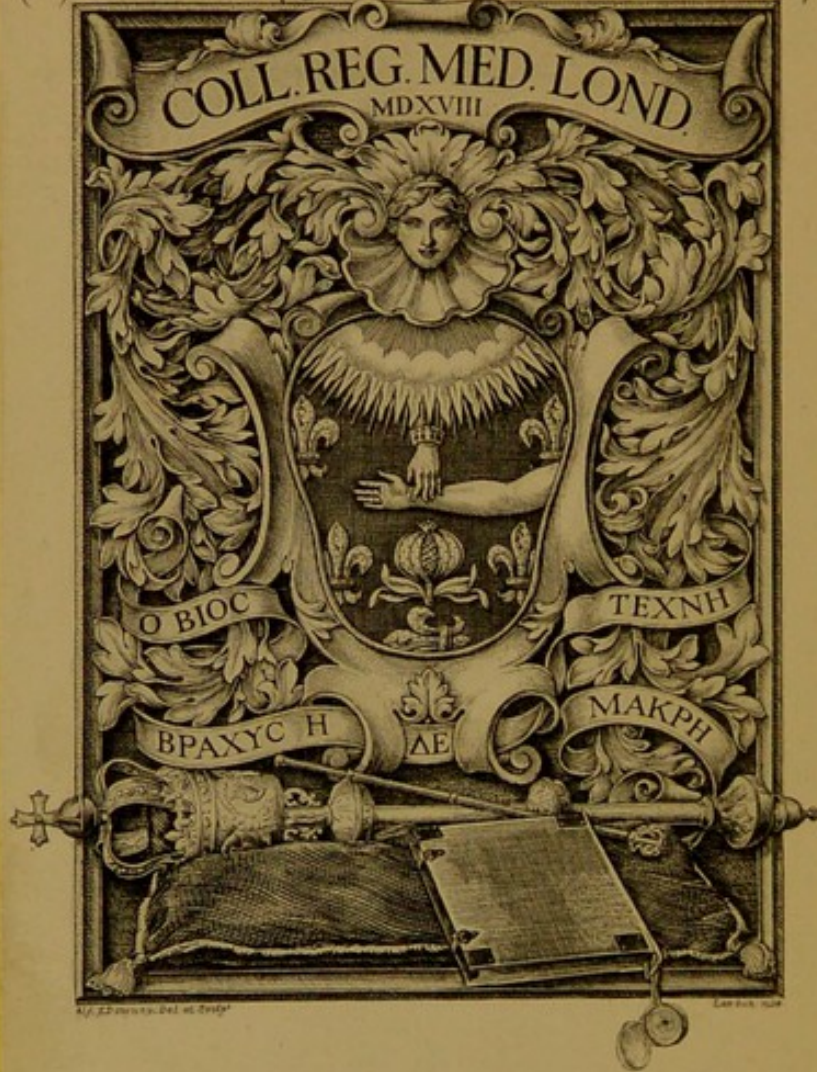
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ON
"SPONTANEOUS EVOLUTION"
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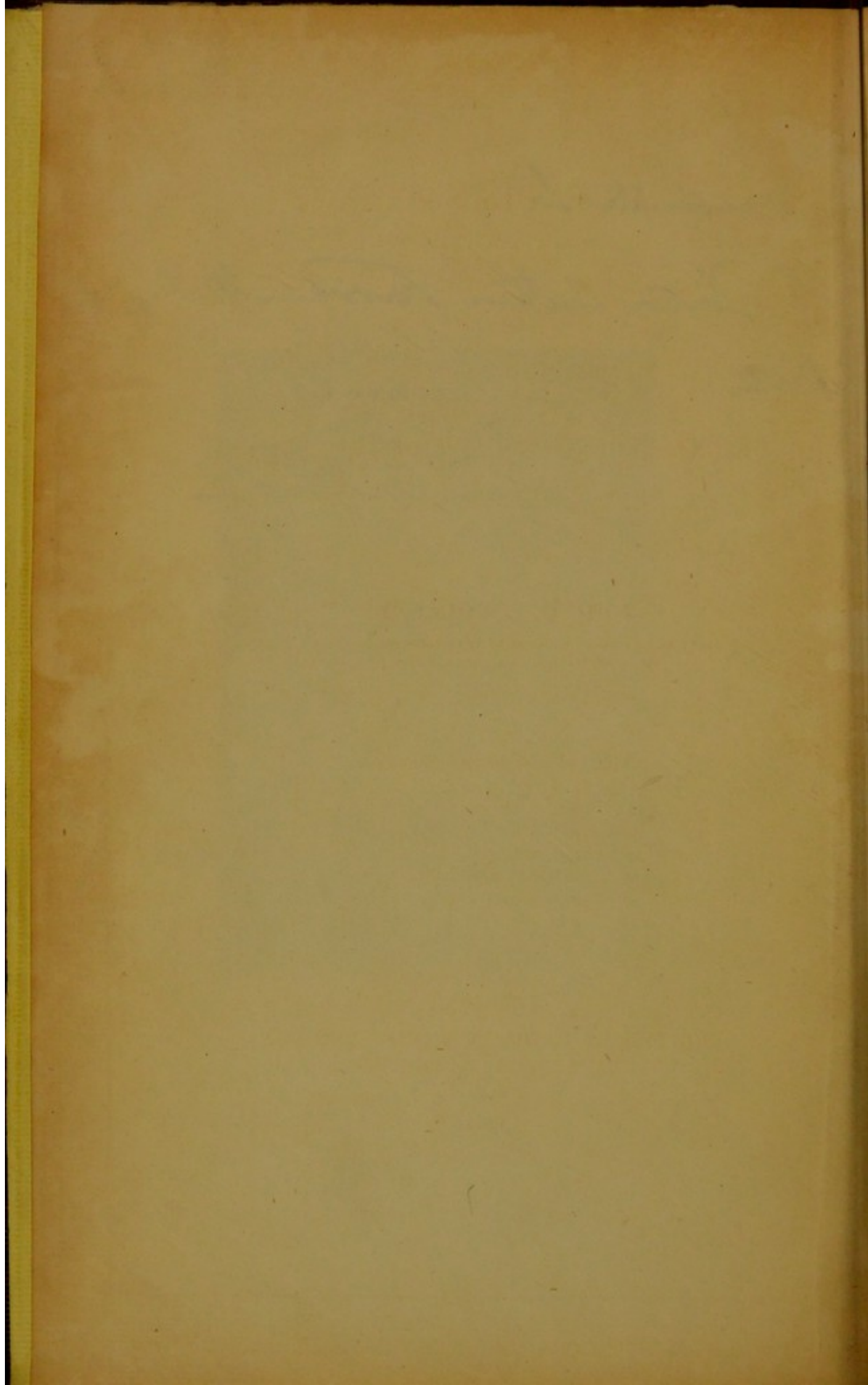


Presented to

Doctor Arthur Farve - M. D. F. R. S.

With the Respects

of the Author -



9794

AN EXPLANATION

OF THE REAL PROCESS OF

“SPONTANEOUS EVOLUTION OF THE FŒTUS.”

BY

JOHN C. DOUGLAS, M.D.

HONORARY FELLOW OF THE COLLEGE OF PHYSICIANS, IRELAND; AND HONORARY
FELLOW OF THE MEDICAL SOCIETY OF PHILADELPHIA.

“Nullius addictus jurare in verba magistri.”

THIRD EDITION.

DUBLIN :
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1844.

LET Midwives mark what hath been written in my Observations; let them consider diligently, the several reportes not faigned, or the surmised thoughtes, nectors, or man's fantasie, sitting and meditating in his studye, but which really have been performed in the traailing woman's chambre.—And let the midwives knowe that they be nature's servantes, and not her instructors.

WILLUGHBY.

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P R E F A C E.

The increased frequency of instances of "spontaneous evolution of the foetus" in cross-birth, latterly recorded, as well in more elaborate obstetric writings, as in the periodicals of the day, has induced me to think of reprinting the second edition of my essay on the subject.

From the sedulous observations and courteous liberality of British practitioners, the accuracy of my exposition of this process, and the importance of a right conception of its progressive development, have been broadly acknowledged,—I might say, established.

As the present publication merely aims at a transcript of my second edition, and as its preface is somewhat lengthy, I will not prolong this further, than to express a hope that it may not be considered an inappropriate adjunct to the third edition, to have annexed thereto, a reprint of my paper on Hour Glass Contraction of the Uterus, as inscribed in the sixth volume of Transactions of the College of Physicians, London, 1820.

J. C. D.

16, RUTLAND SQUARE,
20th Sept. 1844.

P R E F A C E

TO SECOND EDITION—A.D. 1819.

It fell to my lot to observe, for the first time, in the year 1810, a process of parturition, which had not been previously described. The fact itself, or what I may term the result of the process, had, no doubt, been long known and acknowledged.

Having felt no inconsiderable degree of satisfaction at the discernment of the true *rati-
onale* of a process, the mechanism of which (if I may so express it), had been so long misconceived by practitioners of the first eminence in midwifery, I only waited to witness two additional instances, until I promulgated the fact, in a short pamphlet which I published in the year 1811 ; but which, I regret, was too hastily written, and not free from error. Error—inasmuch that I worded two or three para-

graphs so inadvertently as to lead some of my readers to the deduction of an inference, materially different from that which I had intended.

With the hope of obliterating any unfavourable impression which such inadvertence might have made upon the minds of the more experienced, and with the view of obviating every injurious consequence which might result from its effects upon the minds of the less experienced, I have determined to re-state the subject.

This intention, I hope, will be deemed laudable; and likewise, I trust that I will not be conceived censurable if I endeavour to pre-engage the mind of my reader, both for the importance of the subject, and for the accuracy of my explanation, in transcribing a few passages from a polite letter, which I had the gratifying honor to receive from the celebrated Doctor Denman (since dead), although I had

boldly controverted the reality of his theory of this process.

For some judicious remarks, which he was so kind as to make on my pamphlet, I feel myself much obliged ; and for the valuable information long since set forth to the world in his numerous publications on midwifery, every individual ought to feel truly grateful. But—Doctor Denman was a man, the lustre of whose fame, whether we consider him in the light of a writer, a teacher, or a practitioner, can receive no brilliancy from any applause which my pen could unfold.

His letter, to which I have alluded, begins thus :—

“ DEAR SIR,

“ I am much obliged by the favour you have done me, in sending your pamphlet, explaining with much ingenuity the evolution of the *fœtus*, a subject of considerable impor-

tance in midwifery ; and I should sooner have made this acknowledgment, had I not availed myself of the opportunity, which the quietness of this great town gives me at this season, of going into the country.

“ It would be very hard indeed if offence were taken at the difference of opinion which might be entertained of any published doctrine, as it would deny to others the privilege which all late authors assume, and every corrected edition of any work is an exercise of that privilege. For my own part, I am so far from taking offence by any freedom of criticism you have used, regarding what I had written on the subject of the evolution, that I feel obliged by what you have said upon it, in terms sufficiently flattering. On this ground, I am sure you will excuse the few observations I have ventured to make on your work.

“ When the first account of the possibility of the spontaneous evolution was published, it

was absolutely denied by every member of a society, to which I had the honour of belonging; but in the course of a few years after the promulgation of the fact, so many cases had occurred in the practice of different gentlemen, that the fact was not admitted only, but became one of the established doctrines of the schools.

“It is a great pleasure to me to think that the fact was admitted; and when the account of it was published, in a more orderly manner, I did not think it right to send it into the world without some attempt to explain how it was effected.

“In your practice I presume you have met with many instances of it; but it may be supposed that one case would not be esteemed sufficient authority for forming an axiom, and determining the general question. The fact is a distinct question; the manner of the evolution another. For the former, I am not any

longer answerable. It stands on other testimony ; but I certainly have remained responsible for the explanation of the manner, and to defend this I am not very solicitous ; yet I may observe that my explanation is not given in positive terms, beginning with ‘ I presume,’ leaving it as an opinion for future proof or disapprobation. If there be an error in the explanation, others may also err in their opinion.”

And, after having dilated considerably upon the merits and demerits of my work, he writes thus :—

“ Will you allow me to consider this long letter as a proof of my respect for a gentleman who is taking pains for the improvement of his profession. Wishing you all success,

“ I remain, dear Sir,

“ Your most obliged servant,

“ THO. DENMAN.”

AN

EXPLANATION, &c.

As I do not propose to treat, at large, of the practice of midwifery, but intend merely to consider of the management of such labours, as are termed cross-births, it cannot be deemed necessary that I should give a general view of the history, or notice the progressive improvements of this art. It will suffice that I advert briefly to the modes which have been usually adopted. for the relief of parturient women, in this variety of presentation.

From the writings of the ancients, we learn that their modes of acting, on these occasion, were not only painful and uncouth, but some of them were truly absurd. The most rational perhaps of them,

was to mutilate the *Fætus in utero*, and then to extract it piece-meal. But even this plan, although we may allow it to have been generally successful, as to the mere delivery of the woman, was certain destruction to the infant, and must have frequently offered such violence to the uterine system of the mother, as to induce inflammation and gangrene—terminating in death.

Among the more absurd expedients often had recourse to, we might notice the custom of changing the position of the patient to a variety of postures, with a view of altering the untoward situation of the *fætus* to a more favourable one ;—or, in other words, with the hope of converting a cross-presentation into a natural labour.

This object was attempted to be effected, by causing the suffering woman to turn successively, from laying upon her side to her back ; thence to rest upon her knees and elbows ; thence to be rolled, and afterward perhaps to be shaken by a number of robust assistants. And often, after a variety of similar efforts without advantage, she would be sus-

pended by the heels, with her head downward, over the bed-chamber door, or from the step of a ladder, with the design of causing the infant to present with its head to the world.

There can be no doubt that such expedients were resorted to in the earlier ages ; for Hippocrates not only relates them, but recommends them. He says —“ If the arm or leg, or both, of a living child present, they must, as soon as discovered, be returned into the womb, and the child be brought into the passage with its head downward.” To effect which, he advises to roll the woman, to shake her, and to make her jump ; and if these expedients do not succeed, he advises to extract the child with crotchets, or whatever instruments can dismember it.

Although it is easy of belief that such practice was in repute in the days of Hippocrates, yet, if we had not good authority for it, it would be scarcely credible that such absurdities had not been entirely abandoned previous to the last century.

In the writings of Dr. Smellie, who lived in that age, we find the following detail, (Case III.)

“ Being called to a watchman’s wife, the midwife told me, that the waters had come off in a large quantity, on which the arm was forced down into the birth, and the hand appeared without the external parts. She had tried different methods to make the child (as she ignorantly imagined) withdraw up its hand into the womb, and change itself into the natural position ; by dipping its hand in a bason of cold water, and also in vinegar and brandy ; but finding these trials fail, she had recourse to the last remedy, before any assistance from a man-practitioner was thought necessary : she directed the woman’s husband to take hold of her legs over his shoulders, and lift up her body, three times, with her back to his, and her head downwards, being of opinion that although the former methods failed of success, this would answer expectation.” These absurd expedients, in this case, I must remark, were the act of an ignorant female, and not that of a physician.

Such was generally the practice in cross-births, until nearly the close of the sixteenth century; at which period Ambrose Parè, having already walked several years in the footsteps of his predecessors, conceived the bold design (possibly he might have taken the hint from Celsus) of always introducing his hand into the *uterus*, taking hold of the legs of the child, and thereby completing delivery, as if the feet had originally presented. This process is termed turning;* because by it the arm, or whatever part of the trunk originally presents, recedes into the *uterus*, yielding priority of exit to the lower extremities.

On reviewing the subject at this time, it appears somewhat extraordinary that this plan had not been more generally adopted at an earlier period than

* Although the merit of introducing the practice of turning into general notice has been deservedly attributed to Parè, yet there can be little doubt that a similar method had been occasionally practised, at intervals, between the time of Celsus and that of Parè. Thus we learn from Ætius, who lived probably about the fifth century, that Philomenes discovered a method of turning and delivering children by the feet, in all unnatural presentations.

the sixteenth century ; for we find by the writings of Celsus,* that he knew children may be delivered with the feet foremost, by taking hold of their legs ; and he directs to turn children, either with their head or feet downwards, who are otherwise placed in the womb. Celsus, we must admit, speaks only of dead children ; but it might from thence have easily been inferred, that the same practice could be applied to deliver living children.

This mode of delivery adopted by Parè, although to us so evidently preferable to the means then in general use, met with the usual reception of every innovation—opposition. At length, however, its utility was acknowledged, and it has been, for many years, so generally acted upon, as to be considered by some, particularly by less experienced practitioners, the only means by which a *fœtus* can possibly be liberated in cross-birth.

* Verum intus emortuo corpori manus injecta protinus habitum ejus sentit, nam aut in caput, aut in pedes conversum est, aut in transversum jacet: fere tamen sic, ut vel manus ejus, vel pes in propinquo sit. Medici vero propositum est, ut eum manu dirigat vel in caput, vel etiam in pedes, si forte aliter compositus est.

CELSUS Lib. vii. Cap. xxix.

Such is the fate of almost every discovery or improvement in the healing art ; it first meets with opposition—is at last admitted—and then so implicitly followed as to be carried to extremes—its use to be abused—and the benefits, which, in just bounds, it might confer upon society, are defeated.

This improvement of Parè would have been of more decided advantage to the human race, if its application had been restricted to suitable cases ; for I will venture to state, that no candid man of extended practice will deny the fact of his having known women to suffer irreparable injury, even death, in consequence of the violence used by practitioners in the operation of turning.

To acquire conclusive evidence of this assertion, it is only necessary for a person to peruse the writings of any candid practitioner of the last or present century, and there will be found sufficient testimony to convince him, that the practice of turning has been too indiscriminately pursued : But, in order to spare some of my readers the trouble of

looking for information on this subject, I will here subjoin a few extracts from authors of celebrity.

La Motte, in his observation 262, page 467, says, “ Et la matrice si étroitement appliquée sur l'enfant, qu'il n'étoit pas possible d'introduire ni mes doigts ni ma main dans la matrice, pour aller en chercher les pieds ; l'épaule fermoit trop exactement le passage, joint a l'extrême grosseur du bras, et à l'étroitesse du vagin : tous ces obstacles, qui me paroissoient comme invincibles, me determinerent, apres une courte reflexion, à tordre et arracher ce bras ; ce que je fis en deux coups de main, ne doutant pas qu'après l'extraction de cette partie tronçonnée, je n'eusse une entiere liberté a mettre à execution le dessein que j'avois toujours d'aller chercher les pieds ; mais quelque liberté que me pût donner cette extraction, je n'en eus pas encore assez pour executer mon intention, quoique la malade fût sans douleur, ce qui étoit encore un grand avantage, tant pour elle que pour moi ; car quand je voulois forcer ma main à entrer a côté de ce moignon d'épaule, que ne pouvois faire retrograder, par les raisons que j'ai dites, je souffrois une si violente dou-

leur, qu'elle étoit suivie d'une impuissance absolue de remuer aucun de mes doigts, à cause que la compression, que toutes les parties en general souffroient, causoit un étranglement aux nerfs de ma main, qui interceptoit le cours des esprits ; en sorte que ces parties tomboient dans un engourdissement paralytique, ce qui m'obligea a retirer ma main plusieurs fois, afin qu'en procurant le cours aux esprits, je puisse y rendre sa premiere vigueur ; après quoi je retournois a l'ouvrage, comme auparavant, jusqu'à ce qu'enfin j'eusse forcé ce passage ; alors j'introduisis ma main dans la matrice, &c. En prenant toutes ces mesures, je finis cet accouchement, l'un des plus laborieux que j'aye jamais fait. Je crus très-certainement que je mourrois après cet accouchement, ou j'épuisai et ma science et mes forces, et après lequel je restai sans respiration ; en sorte qu'il me fallut mettre sur un matelas devant un grand feu, et me frotter avec des linges chauds pendant plus d'une heure."

And in his reflection on observation 271, page 490, where, after relating a train of similar difficulties, he says—"Je crus que cet accouchement

seroit le dernier de ma vie, tant j'étois las et épuisé, et j'eus besoin de plus de huit jours pour me remettre de l'extrême fatigue que j'y avois soufferte, sans que je puisse m'aider pendant tout ce temps-la des mains ni des bras, ne marchant même qu'avec peine."

"Les bras de cet enfant se trouverent rompus, sans que je me fusse aperçù de cet accident, jusqu'a ce que la mere fût delivrée. Ce ne fut point dans le temps que je les debarassai du passage, que cet accident arriva, mais dans le tempts du cruel et extrême effort que je fus obligé de faire pour terminer ce penible et laborieux accouchement."

The foregoing are extracts from two cases, which occurred in the practice of a French practitioner about the beginning of the last century. I shall now relate some difficulties which occurred at later periods to British practitioners.

Dr. Smellie, in case III. after relating a series of difficulties in attempting to turn, says, "this hand and arm, last introduced, being likewise cramped, I was obliged to withdraw them, and I began to

despair of succeeding without the assistance of the crotchet, but I resolved to make one effort more. My left hand being now pretty well recovered from the former fatigue, I introduced it as at first, and at last reached up to the *fundus uteri*; I now brought down one of the legs, and delivered the child, with the assistance of the noose."

"Although, while I lived in the country, I had been called to many such cases, yet I was never more fatigued; I was not able to raise my arms to my head for a day or two after this delivery; and one of the gentlemen who was present was so much frightened, that he resolved never to venture on the practice of midwifery."

And Dr. Smellie, after speaking of the fatigue he endured in the course of the delivery in case XIV. says, "the weather was remarkably cold for the season of the year; there was very little fire, and yet I sweated so much, that I was obliged to throw off my waistcoat and wig, and put on my nightgown, with a thin napkin on my head."

I could transcribe extracts from cases attended with similar difficulties, to almost endless extent ; but I think it would be superfluous to dilate further on this head. I would simply beg of the reader to reflect on the aforementioned cases ; and I would then ask him, if he can suppose it was ever intended by the Author of Nature that the uterine system of women should be subjected to such violence as therein detailed ?

La Motte in the first case relates, that his hand was so squeezed by the general compression all the parts laid under, that it was quite numbed, which forced him to withdraw it several times in order to recover its power ; and, that he had occasion repeatedly to desist from, and resume his work, before he forced a passage into the womb ; and lastly, he adds, he imagined that he should have died after this delivery, which was the most laborious he ever performed, having been put to the utmost of his skill and strength : he remained without breath, and they were forced to lay him upon a mattress before a great fire, and rub him with warm cloths for above an hour. And, in his other

case, which I have quoted, he asserts he was so much exhausted, that he did not expect ever to be able to perform another delivery.

In Dr. Smellie's first case quoted, it is evident he must have exerted great force to get his arm into the womb, as he was obliged alternately to relieve the right by the left until he accomplished his object. And, we may infer that the woman's sufferings were not inconsiderable, when one of the Doctor's pupils was thereby deterred from pursuing the profession of midwifery; and the reader may form some idea of the exertion that he used in the second case, from the circumstance of his sweating so much as to be obliged to throw off his WAISTCOAT and WIG.

As the embarrassments just recited occurred in practice many years ago, it might perhaps, by some, be conjectured that the art of midwifery has since been so much improved, that such exertions are not now required.

Happily such exertions are not now required in every case of turning; nor were they even in the

days of La Motte or Smellie; but certainly similar force is occasionally still used in cross-birth deliveries, and that not only by inexperienced practitioners, but such practice is inculcated as justifiable and necessary, when turning cannot be more easily effected, by teachers of the first eminence in Europe.

It is quite clear from the writings of Dr. Denman, that he himself had occasionally used very considerable force in turning. But from the year 1772, when he first discovered that the *uterus* itself could expel the *fætus* without the interference of some accoucheur, he became more reserved in using great efforts. And, from that period, unless he could accomplish his object by moderate exertions, he either trusted to the chance of a spontaneous evolution, or he proceeded to deliver in a manner nearly similar to that, of which we shall hereafter speak.

The present celebrated professor of midwifery (Dr. James Hamilton) in the University of Edinburgh, informs his numerous class of pupils in his lectures, that he has been obliged to exert great efforts, for a considerable time, frequently upwards of

an hour, before he could thus effect delivery : and that he has occasionally suffered so much pain from the pressure which his arm sustained whilst in the *uterus*, that he has been ready to exclaim equally with the suffering woman. Indeed, I might venture to say, there are few practitioners of extended practice who have not experienced similar difficulties, and felt similar sensations.

In the year 1772, the late Dr. Denman was called to a poor woman in Oxford-street, who had been in labour all the preceding night, under the care of a midwife, an arm of the child presenting. He found there two surgeons, who had made repeated efforts to turn, but the pains were so strong as to prevent the introduction of the hand into the *uterus*. He relates,* “ I found the arm much swelled, and pushed through the external parts, in such a manner that the shoulder nearly reached the *perinæum*. The woman struggled vehemently with her pains ; and, during their continuance, I perceived the shoulder of the child to descend. I remained by the bed-

* Denman's Aphorisms, p. 61.

side till the child was expelled, and I was very much surprised to find that the breech and inferior extremities were expelled before the head, as if the case had originally been a presentation of the inferior extremities. The child was dead, but the mother recovered as soon, and as well, as she could have done after the most natural labour."

In the following year, 1773, Dr. Denman was called to another woman, whose situation was nearly similar, and where he met Surgeon Burosse in consultation. He remarks, "when I examined, I found the shoulder of the child pressed into the superior aperture of the *pelvis*. The pains were strong, and returned at short intervals. Having agreed upon the necessity of turning the child, and extracting it by the feet, I sat down and made repeated attempts to raise the shoulder, with all the force which I thought could be safely used; but the action of the *uterus* was so powerful that I was obliged to desist. No further attempts were made to turn the child. Then every pain propelled it lower into the *pelvis*, and in a little more than one hour the child was born, the breech being expelled, as in the first case.

This child was also dead, but the mother recovered in the most favourable manner.”

In the next case related (page 65) by Dr. Denman, he says, “ In all these cases the women were at the full period of *utro-gestation*, and all the children were of the usual size.” In the second case, he likewise adds, “ I now understood the progress more clearly, and attempted to explain, both in my lecture on the subject, and in the aphorisms which were printed for the use of the students, my opinion of the manner in which the body of the child turned, as it were, upon its own axis.”

Dr. Denman’s explanation, which I have extracted from his valuable system of midwifery, is as follows :—

“ As to the manner in which this evolution takes place, I presume, that after the long continued action of the *uterus*, the body of the child is brought into such a compact state as to receive the full force of every returning action. The body, in its doubled state, being too large to pass through the *pelvis*, and

the *uterus* pressing upon its inferior extremities, which are the only parts capable of being moved, they are forced gradually lower ; making room as they are pressed down for the reception of some other part into the cavity of the *uterus* which they have evacuated, until the body, turning as it were upon its own axis, the breech of the child is expelled, as in an original presentation of that part ; nor has there been any thing uncommon in the size or form of the *pelvis* of those women, to whom this case has happened, nor have the children been small, or softened by putrefaction. I believe, on the contrary, that a child of a common size, living, or but lately dead, in such a state as to possess some degree of resiliton, is the best calculated for expulsion in this manner."

Although it may be my opinion, that no practitioner will ever again have an opportunity of witnessing a process similar to that just described ; yet, I wish to retain the appellation—"spontaneous evolution," given by Dr. Denman to the event of a delivery, in arm-presentation, unaided by the *accoucheur* ; both because the fact is known by the

term, and because it appears to me more suitable than that of expulsion.

The word *expulsion* would rather convey the idea that the *fœtus* is propelled through the *os externum*, each after line of it succeeding each, in regular progression, the entire retaining the form, and every part each its relative position, as situated at the commencement of labour. Whereas—this is not the case. For, in the early stage of such a labour, the trunk of the *fœtus* is nearly perpendicular, with respect to the axis of the *pelvis*; but as it descends through the *pelvis*, it gradually approaches to the horizontal; and immediately previous to the expulsion, it lays exactly horizontal, with respect to the *os externum*. So, in fact, there is a process somewhat (although not exactly) similar to the theoretical one of Dr. Denman, but with this *material* difference, that the mechanism of it takes place in the *vagina* and *pelvis proper*; and not, as he conjectured, in the *uterus*, and between the *alæ* of the *Iliæ*.

The practicability of the process, which I propose to describe, will, perhaps, appear to some of my

readers as little worthy of credit, as Dr. Denman's explanation seems to me incompatible with unprejudiced reasoning on the subject. From a cursory view of my *rationale* I anticipate it will be inferred, that the entire trunk or body of the *fœtus* must be pressed into, and be contained at once, in the lower cavity of the *pelvis*, immediately prior to its liberation.

It is not, however, altogether required to admit this as a fact on the occasion ; but even if it were, it might be brought within the limits of belief. For, if we compress strongly, by its extreme ends, the trunk of any still-born *fœtus*, we will find its bulk not then to exceed by so much the size of the head as it usually does. And the difference of the solid contents of the body over the head is, at all times, less in excess than appearances would indicate. And why should not a *pelvis*, which we suppose perfectly capable of containing the head of a child, likewise be capable of containing its body ; the bulk, when compressed, and the solid contents of which, whether compressed or not, do not so very much exceed those of the head ?

But, in order to admit my explication of the process to be correct, it is not necessary to conceive that the trunk should be altogether compressed into a bulk not exceeding the compass of the head. For, previous to the reception of the breech into the brim of the *pelvis*, which occurs very shortly before the completion of the evolution, a great portion of one side of the thorax will have emerged from under the arch of the *pubis*. And likewise, at this period, the *perinæum* will be excessively on the stretch, suffering a degree of extension far beyond that to which it is put in usual labour.

Thus, then, we will always find, during this process, a considerable portion of the thorax to be protruded (although not disengaged) from the bony cavity of the *pelvis*, whilst the breech is getting into it; therefore the entire of the trunk is never, at one moment, altogether contained within the cavity of the *pelvis*: and consequently, my *rationale* does not require that it should be compressed into a bulk not exceeding that of the head. But I maintain that the body of an infant, under these circumstances, will be temporally much reduced in size,

and temporally very much altered in relative form.

For the information of those of my readers who may not be perfectly conversant in midwifery, I would here remark, that the effort of parturition is completed by a contractile power inherent in the *uterus*, but exerted only at particular periods, for the purpose of expelling its contents. And, in every case, the *fœtus*, whether alive or dead, is a passive body, and contributes, in no degree, to its own liberation.

As the variety of labour termed cross-birth, does not perhaps occur in the proportion of three in a thousand births; and as this more particular process—"the spontaneous evolution"—has not hitherto occurred in general practice, even in the proportion of one in thirty cross-births, it possibly might be deemed, by some persons, that the investigation of the precise nature of a process, which may not happen once in ten thousand labours, is scarcely worthy of so much discussion.

I am however of opinion, even if we were to look

forward to the evolution occurring only in this rare proportion, that a perception of its real mechanism would neither be uninteresting nor useless. But I anticipate when a knowledge of its true process shall have been more generally disseminated, and its principles better understood, that the facts will thenceforth be considerably more numerous.

I hope likewise to be able to demonstrate, that the occurrence of such an event, in any particular case, can be calculated upon equally rational principles as any other event in the routine of midwifery practice. Whereas, by a belief in Dr. Denman's theory, we never can do more than form a conjecture, or indulge in a hope. The very circumstances or condition of the woman and child, which would now lead me to expect a spontaneous delivery, would, if I believed in the Doctor's theory, lead me to despair of such an event.

The only inference which I conceive can be drawn from Dr. Denman's theoretical explanation, already quoted, is—that the shoulder of the child, after having been impacted into the *pelvis* by a series of ute-

rine actions, each successive action forcing it lower and lower; that subsequently, after some indistinct period of time, repetitions of the same propelling power, instead of producing a furtherance of the same effect, should cause another part of the child, namely the breech, to descend, and to occupy the place where the shoulder had been; and that the shoulder should, by some miraculous effort, at the same moment recede again into the *uterus*, and take possession of the place from which the breech had just been propelled. Or, to speak briefly—that the breech higher up in the *uterus*, and the shoulder lower down in the *pelvis*, had, by an unexpected uterine effort, changed places.

Now it seems to me incompatible with all received ideas of uterine action to suppose that the *uterus*, when contracting so powerfully as to force down that part of the child which was at its *fundus*, could, at the same moment, form a vacuum, into which another portion, already low down in the *pelvis*, should recede. But I will forbear to make any further remarks on this hypothesis, and I will proceed to describe, as has uniformly occurred under my

observation, this rare process of parturition, which had not been satisfactorily explained until the year eighteen hundred and eleven.

In all the cases related by various practitioners on the subject of the evolution, it is acknowledged, that shortly before its occurrence, the shoulder of the child had been forced very low into the *pelvis*: and that the thorax had occupied so much of its cavity as to preclude the practicability of the hand of the accoucheur being passed up into the *uterus* for the purpose of turning, as is usually done in such presentations.

So far as the foregoing detail, my observations coincide with those of Dr. Denman and others; but I cannot comprehend how successive repetitions of the same propelling power, which forced the child into this situation, should subsequently, *at any period*, produce a counter-effect, causing the shoulder to retreat into the *uterus*. The fact however is—that the shoulder and thorax, thus low and impacted, instead of receding into the *uterus*, are, at each successive pain, forced still lower, until the ribs of that

side, corresponding with the protruded arm, press on the *perinæum*, and cause it to assume the same form as it would by the pressure of the forehead in a natural labour. At this period not only the entire of the arm, but the shoulder, can be perceived externally, with the clavicle laying under the arch of the *pubis*. By further uterine contractions, the ribs are forced more forward, appearing at the *os externum*, as the *vertex* would in a natural labour ; the *clavicle* having been, by degrees, forced round on the anterior part of the *pubis*, with the *acromion* looking towards the *mons veneris*.

But, in order to render as clear as possible the successive movements in this astonishing effort of nature, I will endeavour to describe, still more precisely, the situation of the *fœtus* immediately prior to its expulsion. The entire of it somewhat resembles the larger segment of a circle ; the head rests on the *pubis* internally ; the *clavicle* presses against the *pubis* externally, with the *acromion* stretching towards the *mons veneris* : the arm and shoulder are entirely protruded, with one side of the *thorax* not not only appearing at the *os externum*, but partly

without it : the lower part of the same side of the trunk presses on the *perinæum*, with the breech either in the hollow of the *sacrum*, or at the brim of the *pelvis*, ready to descend into it ; and, by a few further *uterine* efforts, the remainder of the trunk, with the lower extremities, is expelled.

And, to be still more minutely explanatory in this ultimate stage of the process, I have to state, that the breech is not expelled exactly sideways, as the upper part of the trunk had previously been ; for during the presence of that pain, by which the evolution is completed, there is a twist made, about the centre of the curve, at the lumbar *vertebræ*, when both buttocks, instead of the side of one of them, are thrown against the *perinæum*, distending it very much ; and immediately after, the breech, with the lower extremities, issues forth ; the upper and back part of it appearing first, as if the back of the child had originally formed the convex, and its front the concave side of the curve.

From this description (the accuracy of which, I have no doubt, will hereafter be verified by multi-

plied testimony,) it must be evident that the breech is not the first part of the body which appears without the *os externum*, according to Dr. Denman's theory ; because that side of the *thorax* which corresponds with the presenting arm, is previously protruded. The breech certainly is expelled momentarily before the opposite side of the *thorax*.

We will now suppose an evolution to be completed, or the entire of the *fœtus* to be without the *os externum*, except the head and one arm ; but with respect to the arm which was originally protruded, I can affirm that not one line of it, or of any other part of the child, once descended, ever withdrew again into the *uterus*. The arm and shoulder certainly, on those occasions, always withdrew from the hand of the accoucheur, placed either on the *perinæum* or at the *os externum*, in the usual direction from the axis of the *pelvis* ; but instead of returning into the *uterus*, they merely got forward on the symphysis of the *pubis* externally : and thus, whilst the practitioner's mind and hands were occupied for the safety of the excessively distended *perinæum*, he imagined that the arm and

shoulder, which only withdrew from his hand in getting forward on the *pubis* externally, receded into the *uterus*.

The probability of this error of practitioners will perhaps appear more striking to my readers, when I relate seven cases of this nature, which have fallen within my observation; and, moreover, I think I could select passages from every case, which has been detailed by Doctor Denman and others, as illustrative of his theory, to corroborate my explanation.

I refer the reader to an attentive perusal of the three cases before alluded to in Doctor Denman's aphorisms. In the first, he says—"The woman struggled vehemently with her pains, and, during their continuance, I perceived the shoulder to *descend*." In the second, he says—"Having agreed upon the necessity of turning the child, and extracting it by the feet, I sat down and made repeated attempts to raise the shoulder with all the force which I thought could be safely used; but the action of the *uterus* was so powerful, that I was

obliged to desist. Then every pain propelled it lower into the *pelvis*, and in a little more than one hour the child was born." And in the third, he says—"On examination, I found the arm protruded through the external parts, the shoulder pressing firmly upon the *perinæum*." I would here ask, whether it be more rational to infer that this shoulder and arm receded again into the *uterus*, according to Doctor Denman's theory, or followed the course which I had before, and have now again described?

As this little work already contains so much in the form of quotation, I am unwilling to transcribe any further to strengthen the validity of my explanation, although I could adduce, from cases related by several other practitioners, testimony equally conclusive in favour of my explanation, and yet written also to corroborate a very opposite theory.

The first time I had an opportunity of witnessing the process of the evolution of the *fœtus*, was in the Lying-in Hospital of this city, in the year 1810,

at which time, I was resident-assistant of that great establishment. This case occurred in ward, No. 3, and so powerful and rapid in succession were the *uterine* contractions, that the delivery was effected in less than an hour from the moment it was first perceived, by the head-midwife, to be an arm-presentation ; in fact, the evolution took place whilst I was deliberating on the employment of means to moderate the violent action of the *uterus*, before I should proceed to turn.

The second case occurred in the same year, in ward No. 2, but was more tedious in its progress.

The third case occurred in Bailly's-row, Summer-hill. On the 13th August, 1811, about noon, I was called to visit Anne M'Cormick, upon whom was waiting a midwife. I found the arm of the child presenting, and as there did not appear any circumstance strongly to contra-indicate the propriety of turning, I made some attempts to accomplish that object. I was conscious then, and I am still of opinion, that if I had persevered longer in equally forcible means as I had formerly used, and

often known to be exerted by others on similar occasions, I might have succeeded ; but I was at that time, and am still convinced, that strenuous endeavours should not be too long persevered in, and in less than twelve minutes of trial, I desisted. In about two hours from this period, and which was scarcely five hours from the commencement of the labour, the evolution was completed : this was the woman's first child, and she recovered without any untoward symptom afterwards.

On the 9th of November, 1814, at two o'clock, P.M. I was called to 45, Dorset-street, to visit M. Kerrigan, a female servant, who had been delivered about an hour before of one child, by a midwife. I had been sent for in consequence of an arm of a second presenting. On examination, I found so much of the chest of this child engaged in the *pelvis*, and likewise the action of the *uterus* so powerful, that I was deterred from making any attempt whatever to turn. The evolution, as I expected, was effected in less than an hour. The child, although one of twins, was of usual size ; the woman was much fatigued and exhausted by this

violent *uterine* effort ; but, notwithstanding, she recovered as speedily as if after the most natural labour.

On the 17th of March, 1816, at seven o'clock, A. M. I was sent for by Mrs. M——, Little Britain-street, who was in labour of her eighth child, and whom I had attended in two former confinements. On visiting, I found the labour but little advanced ; the presenting part, which I conceived to be one of the hips, being only at the brim of the *pelvis*. About three hours afterwards, I was enabled more accurately to ascertain the state of the case, and then found a shoulder, instead of a hip, considerably engaged in the *pelvis*. I made some attempts to turn, as I had done in the case of M'Cormick, but not being able to proceed with moderate efforts, I desisted, and left the completion of the delivery to nature : the evolution took place about noon, after considerable *uterine* exertions, although less violent than I had witnessed on any other occasion of the kind. Mrs. M—— recovered as quickly as she usually had in any former accouchement.

In the course of the year 1812,* some time after I had ceased to reside in the Lying-in Hospital, Dr. Thomas Ferguson, who succeeded me as assistant, afforded me an opportunity of witnessing a case of this kind. He had been called from his bed by the attending pupil of the night, in consequence of his meeting with an arm-presentation. The Doctor, on examination, found the thorax of the child so completely occupying the cavity of the *pelvis*, that he determined not to make any attempt to turn. As I had then lately written on the spontaneous evolution, he paid me the compliment to send for me on the occasion. Having made myself acquainted with the circumstances of the case, I not only agreed in opinion with him as to the propriety of waiting for an evolution, but I predicted that the process would be completed in less than an hour; and the event fully verified that prediction. This case occurred in Ward No. 6; and a number of medical gentlemen, pupils of the hospital, had the most satisfactory evi-

* Although this and the following case occurred prior in date to the two last related, yet I preferred to continue uninterruptedly the detail of those cases, which happened more immediately in my own practice.

dence, that the child was expelled precisely as I have described.

The seventh and last case which I have to notice, occurred in the same hospital, Ward, No. 7, and that in the *short space* of three days after the one just mentioned. In addition to the witnesses of that case, was here added the presence of that much respected physician, Dr. Hopkins, who was then Master of the Hospital; and it had been a subject of general regret that indisposition prevented him from being present at case No. 6.

I hope it will not be considered a defect in the relation of these cases, that I have omitted a minute description of each successive step, in the progress of each. Such description could only have been tiresome repetition, as I did not perceive the slightest variation in the mode of advancement in any two cases of the entire number; and I conceive that I have given a sufficiently minute detail of every movement, in my general explication, previous to the enumeration of the cases.

The reader will probably be somewhat surprised, on the perusal of these cases, at the short time in which the completion of each was effected ; in none exceeding six hours. I know the prevalent opinion to be, that such a process only occurs in very protracted labours. But although the duration of labour in these was comparatively short, yet the expelling power exerted by the *uterus* in each case was, and ever must be, on every similar occasion, prodigious.

I have already stated, that if we were to continue to consider Dr. Denman's theory of the evolution as correct, we never could apply the mere knowledge of the fact to much practical utility. According to him, we cannot have the aid of physical *data* upon which to form a *prognosis* ; we are merely permitted to hope for such a termination of labour in any particular case, where our attempts to turn had proved unsuccessful. Whereas, according to my *rationale*, you can predict the event of an evolution with as much confidence as you can that of any other occurrence in midwifery :—I must therefore now proceed to describe that condition of the *fœtus*, which would lead me to expect such a delivery.

If the arm of the *fœtus* should be almost entirely protruded, with the shoulder pressing on the *perinæum*; if a considerable portion of its thorax be in the hollow of the *sacrum*, with the *axilla* low in the *pelvis*; if, with this disposition, the *uterine* efforts be still powerful, and if the thorax be forced sensibly lower during the presence of each successive pain, the evolution may with great confidence be expected.

If, however, a practitioner be ultimately disappointed in this desired event; or if he have originally resolved not to calculate upon its occurrence, but have determined upon more speedy delivery, turning should not, at all events, be attempted, provided the situation of the child be, in any degree, near to that * which I have in the foregoing paragraph described; because it is an expedient *always* terrifying to the suffering female, and under such

* The hazard of turning in such extreme cases has been forcibly dwelt upon by several modern physicians. There is to be found, in the 40th Number of "The Medical and Physical Journal," a very interesting paper on this subject, by Dr. Sims, of London.

extreme circumstances, without any prospect of having a living child, may bring her own life either into immediate or more remote danger.

The means of delivery, then, in any case where it has been determined neither to turn nor to wait for a spontaneous evolution, are to lessen the trunk of the *fœtus*, by opening its thorax or abdomen, or both; and when thus lessened, it may be more easily expelled, after the manner of the unaided evolution; or the practitioner might still further hasten this process, by fixing a blunt-hook or crotchet, or (when practicable) his fingers, on the *pelvis* of the *fœtus*, and thereby encourage its descent.

I cannot, however, avoid to enter my protest against the doctrine recommended by some, of fixing the instrument at random on any part of the *fœtus* which happens to be nearest; because, if the physician fix it on any of the superior ribs, he might pull with herculean force, and yet not be able to bring down the body, unless he unfortunately separate it from the head, which rests over the *pubis*, and which neither can descend itself,

whilst the cavity of the *pelvis* is occupied by the trunk, nor will it permit the upper part of the thorax to be pulled down, unless the neck first give way. I therefore caution the practitioner to fix his instrument, in such cases, on the child's *pelvis*, and not on the upper part of its thorax, before he exerts much extracting force. By thus acting, he would be closely imitating that natural process, which it has been my anxious wish faithfully and accurately to describe.

The reader, however, will be careful not to confound the practice inculcated in this last page with that of the ancients, which has been so much condemned. Theirs, in cross-birth, generally was to mutilate the *fœtus* at an early period of labour, and whilst it was still high up in the *uterus*, and whether living or dead. Here, on the contrary, the practice is directed only in a few extreme cases, and when the *fœtus* is already low down in the *pelvis*; and when it must have, from circumstances not necessary here to expatiate on, been previously dead.

I am authorised* by Dr. Churchill, of this city, to say that he, some time since, accompanied by another practitioner, visited a woman in labour, and in charge of a midwife. They found the arm protruded, and the thorax so decidedly in possession of the *pelvis*, as to preclude any feasible attempt being made at turning. In this dilemma, whilst deliberating on means for eviscerating and extracting, they were agreeably surprised to find the thorax gradually to descend, until, by a succession of uterine throes, the entire of the trunk was expelled. Both the Doctor and his friend (an equally accomplished practitioner) had the most satisfactory and conclusive evidence of the progress of the evolution being, in this case, exactly as I have described it.

Unwilling to prolong to any tiresome length the detail of facts on this point, I will only further refer to

* A.D. 1844.

The case related by the late Dr. Gooch, in the Sixth Volume of Transactions of the College of Physicians, London, A.D. 1820 ; which case (I may remark) occurred in the course of a few months, after I had forwarded to him a copy of my Essay of 1819. To

Doctor Francis H. Ramsbotham's Obstetric Medicine, 1841, wherein he says :—" I have personally known seven cases of this description, in all which my assistance was desired." And to

The Lancet, 3rd August, 1844, wherein is detailed a case by T. H. Wardleworth, Esq., Bury. Mr. Wardleworth, after describing the progress and issue of the case, comments thus :—" The above case clearly illustrates the view entertained by Doctor Douglas, of Dublin, as to the mechanism of spontaneous expulsion ; which is further corroborated by Doctor Ramsbotham, in his truly excellent and practical work on midwifery : as also by several other writers, who have given testimony upon the subject of spontaneous expulsion."

Surely, I may now fairly congratulate myself on

the verification of my prediction, first published in the year 1811, that " I had no doubt of the precise accuracy of my description of this process being hereafter confirmed by the multiplied testimony of others : " and that " as the rationale or true explanation of the process becomes more widely disseminated, so would the instances of its occurrence become gradually more frequent. "

Not that a knowledge either of the fact itself, or of the rationale of its mechanism, could possibly exercise any influence in determining the incidents of cross-birth hereafter to be met with, in practice. But a knowledge both of the fact and of the rationale is calculated, in my opinion, to occasionally relieve many a practitioner from a preconceived idea of the necessity for proceeding, *vi et armis*, to effect delivery by turning. And, a knowledge of the rationale, together with a fair share of previous opportunities cultivated by observation, is calculated to enable him, in any extreme case of this untoward presentation, to form as correct a diagnosis, whether he might, in due time, expect an evolution from unaided *uterine* action ; or whether he should earlier pro-

ceed, with the aid of evisceration and extraction, to imitate that process, in the manner I have already described :—*As correct*, I conceive, as his other obstetrical knowledge now enables him, in any case of tedious labour in head-presentation, to form a sound diagnosis, whether he may safely rely, for timely delivery, upon unaided *uterine* resources ; or whether, without further delay, he ought to proceed, with the aid of forceps, vectis, or crotchet, to effect that object.

THE END.

Although the fact of a spontaneous evolution of the foetus in cross-birth is a rare occurrence in parturition, yet it is an incident incomparably less rare than that of perforation of the perinæum in head presentation ; I am, therefore, induced to append the following case of perforation of the perinæum, published in the third vol. of Dublin Hospital Reports, 1821, by John C. Douglas, M.D. &c. &c.

THERE is scarcely a systematic work on Midwifery, in which mention is not made of laceration of the *perinæum*, a casualty which not unfrequently takes place, particularly in first-births, when the *fœtus* is passing, or about to pass, through the *os externum*.

The description of accident of this nature that more usually occurs, and which is sometimes unavoidable, is a simple rent in the *perinæum*, com-

mencing at its anterior edge, and running either directly or obliquely backwards. The portion of it traversed by the rent is variable in different cases; it often does not exceed half the distance from the *fourchette* to the *anus*; it sometimes, however, runs entirely to the *sphincter ani*; and, in some rare instances, its course is continued through that muscle, and the inferior portion of the *rectum*. I may also remark, that in a few instances of simple laceration, the rent commences at a point in or near to the centre of the *perinæum*, and thence extends forward to the *vagina*.

But there is another species of laceration worthy of notice, although very rarely occurring, which Doctor Denman has not inaptly termed a perforation, or bursting of the *perinæum*. In this, the *fætus* is supposed to be protruded through the *perinæum* solely, without injuring either the *sphincter ani* muscle, or the inferior commissure of the *labia pudendi*.

The possibility of such an occurrence is doubted by some practitioners; and, I must confess, I am

rather disposed to doubt the practicability of a *fœtus* passing through a perforation in the central part of the *perinæum*. It is true, it is not distinctly stated in the detail of such cases that the perforation was central; yet, such is the inference to be drawn from the description of every case of the kind, which I have seen related. But I am of opinion that this particular kind of rent or perforation, instead of being in the centre, is usually situated towards one side of the *perinæum*, and involving also, in its necessary amplification, the *labium pudendi* of the same side with other parts, as occurred in the case which I am proceeding to relate.

Bridget Brophy, aged 23, was admitted into the Lying-in Hospital of this city on the 24th of January, 1810, at which period I was the resident assistant of that extensive establishment. She was accommodated in ward, No. 7, and occupied bed No. 72.

No symptom of labour was manifest until the evening of the 26th; and even then the pains were so slight, together with the circumstance of a first

pregnancy, as not to excite any apprehension of a speedy delivery.* It may be proper here to remark, that from the multiplicity of labours daily occurring in this Hospital, the nurse-tenders become so expert in prognostic, as to be able, very generally, to form a tolerable opinion of the progress of labour, merely from the tone of the patient's voice.

Notwithstanding the previous tranquillity, the nurse's attention was suddenly excited, about ten o'clock, P.M. by the sound of moans denoting approaching delivery. The pupil of the night was called, and he had not been many minutes at the patient's bed-side when she uttered a shriek so unusually expressive of suffering, as to induce the nurse to send for me; which she was the more willing to do, as the gentleman in attendance had only recently entered the Hospital as a pupil.†

* All the patients of the ward, she excepted, had been well from the previous day.

† Only for the circumstance of the nurse having been taken at unawares, the patient would have been removed for delivery, according to the custom of the Hospital, to a couch situated near the fire-place.

In proceeding to make myself acquainted with the nature of this case, I found that the head of the *fætus* had protruded ; but, instead of its being in the usual position, it was closely applied to the inner, with an inclination to the posterior part of the patient's left thigh : she was lying in the usual posture, on her left side. While I was reflecting on these circumstances an uterine effort succeeded, by which the remainder of the *fætus* was protruded. It was of the male sex—briskly alive—perfectly formed, and without any peculiarity.

I still could not comprehend this anomalism, and having called for a candle to enable me to view the parts, I beheld a shocking laceration, which had the appearance of a large wound inflicted by external violence. Although I have denominated this a case of perforation of the *perinæum*, yet the opening was only partly comprised in the *perinæum* laterally, partly in the left *labium pudendi*, but chiefly in the integuments of the thigh.

Having disengaged the infant, I put back the *funis* through the wound into the *vagina*, and brought

it out at the *os externum*, with the intent of thus extracting the *placenta*. The *placenta*, however, seemed determinately disposed to follow the track of the child ; its gravity, the dependent situation of the artificial outlet, together with the circumstance of that being much more capacious than the natural passage, all contributed to this effect. I therefore allowed the *placenta* to fall out through the laceration, and the end of the *funis* was thus drawn again within the *sphincter vaginæ*, and followed the *placenta*.

To this procedure I had but little objection, as it was evident that the transit of the *placenta* would not enlarge an opening through which the child had already passed ; and besides, I had scarcely a choice, as I found it would require the exertion of more extracting force than could, under the circumstances, be judiciously used to pull the *placenta* through the *os externum*, which was little disposed to dilatation.

Notwithstanding this extensive injury no extraordinary activity of after-treatment was required to

obviate febrile action or inflammation, nor were any other than fomentations and other simple topical applications required ; even in the course of one day, the parts around had so considerably contracted, that the gash did not present half so formidable an appearance as it did, shortly after delivery.

The only after-occurrence worth notice was that, at the expiration of seven weeks, I found the space left by destruction of substance not likely to be entirely filled up by granulation, the opening yet remaining being nearly equal in size to the natural *os externum*, and separated from it only by the *sphincter vaginae*. The part of this muscle, which formed the barrier between the two apertures, had become much weakened and faded, in consequence of being denuded and separated from its natural attachments, on its external border. I therefore snipped it at a point about two-thirds distant from the superior, and one-third from the inferior commissure of the *os externum*. This was only anticipating a process which was rapidly advancing, as it was evident, from the daily fading of the *sphincter*,

which was destitute of support and nourishment at this part, that it would shortly give way spontaneously.

From this moment the granulations shot up from the bottom of the wound with increased rapidity ; and the woman was discharged from the hospital, in good health, on the 26th of March, just two months from the day of her delivery ; and I may add, with scarcely a shade of wider deviation from the normal condition of parts, than usually obtains after the more ordinary laceration of *perinæum*, in labour.

Although it may have been regretted that the attendance on this patient devolved in the first instance on a pupil of little experience, yet I am of opinion that the casualty which occurred could not, under any management, have been obviated. This opinion I formed from the undilatable state in which I found the *os externum*, when I made some attempt to extract the *placenta* through it, and it was strengthened by the circumstance of an experienced nurse having been taken so completely unawares. I like-

wise feel satisfied, that what may be termed real labour had commenced only a short time before the pupil was called, and that probably during the pain, when the nurse sent for me, the head of the *fœtus* pierced the *vagina* or internal coat of the *perinæum* ; and that instead of continuing its progress (as may have been usual in such accidents) without cessation through the external *tunic*, it was, from the rigidity of this, forced down between the *tunics* of the *perinæum* and the folds of the left *labium pudendi*, and under the integuments of the thigh ; and probably I do not err in conjecturing that, during the succeeding pain, the head was forced through the external *tunic* of the *perinæum*, part of the left *labium*, and the integuments of part of the thigh, in which situation I found it on my arrival ; after which, with scarcely a perceptible interval of time, the remainder of the child, by a single *uterine* effort, was expelled.

After the foregoing detail had been committed to paper, an experienced practitioner in midwifery related to me an occurrence, which would seem to contain the rationale of the child's head having

pierced the *vagina* much higher up, than before it could have met with any resistance from the *os externum* to give it such a direction.

In the case to which I allude, the expulsive efforts of the *uterus*, are represented to have been very powerful, while the *os uteri* was neither dilated nor disposed to dilate. The head of the *fœtus* thus bearing upon the *cervix*, burst through it during the presence of a strong pain ; the laceration took place on the finger of the accoucheur, and was continued down the almost entire length of the *vagina*.

The difference in the two cases is, that in the one, which I have detailed, the *fœtus* burst, as I now suppose, through the *cervix uteri*, and through the *vagina*, passing completely behind the latter, at or near to the *sacro-iliac synchondrosis* ; whereas, in the case related to me, the *fœtus* burst through the *cervix uteri posteriorly*, and only lacerated the *vagina*, without passing through the latter, and was ultimately expelled through the natural *os externum*.

Observations on the Hour-glass contraction
of the Uterus. By John C. Douglas,
M.D. Licentiate of the College of Physi-
cians, Ireland. From the sixth volume of
the Medical Transactions of the Royal
College of Physicians, London.

Read at the College, March 27, 1820,

“I cannot help regarding the neck of the *uterus*
as a distinct and independent part from the body
and *fundus*; and as having its own peculiar laws
and actions; and that this separation of powers is
absolutely necessary to the explanation of some of
the phenomena exhibited by health and disease, and
the influence of certain agents on these parts.”

“W. P. DEWEES.”

It will readily be recollected by those gentlemen,

who were pupils in the Lying-in Hospital of this city (Dublin,) during the years 1810 and 1811, and by many others, whom I have, on various occasions, addressed on the subject of midwifery, that I have long entertained an opinion similar to the above doctrine, and hereby endeavoured to explain some of the more frequent causes of protracted labours. But I more particularly insisted upon an independency of action, when advocating my views of that condition of the *uterus*, termed hour-glass contraction. A more satisfactory explanation of which is the object of this paper.

On reflection, every one must feel disposed to admit, that we early imbibe habits of receiving the opinions of our teachers and seniors, without venturing to question their propriety or to examine their solidity. This error has perhaps been the chief of many causes, which have retarded the progress of improvement in medical, as well as in other sciences; and has led to the retaining of many absurd opinions, which ought to have been long since exploded.

Whether the present exposition will have any claim to the rank of a doctrine destined to supersede an erroneous theory, by explaining more satisfactorily the nature of this untoward occurrence in midwifery, must rest with the judgment of the experienced practitioner.

We all know that the *placenta* is usually expelled within a short period after the birth of the child, without any particular interference on the part of the accoucheur. Cases of retention* are not now very numerous; its recurrence gradually becoming less frequent, in consequence of increasing improvement in this branch of the medical profession.

It has been a maxim universal with authors and teachers of midwifery, that detentions of *placenta* are attributable to three causes, and are occasioned indifferently by any one of them, viz. irregular, or hour-glass contraction of the *uterus*, morbid adhesion of the *placenta*, and inaction of the *uterus*.

* By the term retention or detention is to be understood, untoward or protracted delay of the *placenta*, after the birth of the child.

I do not recollect any computation of the relative numbers occasioned by these respectively; but I believe it to be the received opinion, that a greater number result from this supposed hour-glass contraction, than from either of the other two causes, or perhaps than from both combined.

In questioning the accuracy of this opinion, I do not wish it to be thought that I have not, in the course of practice, met with the hour-glass contraction of the *uterus*: I freely confess, I frequently experienced its existence, and in consequence thereof, I have often felt difficulty in reaching the *placenta*.

But, notwithstanding this concession, I presume to adduce as my opinion, that a *placenta* has rarely, if ever, been primarily retained, by this cause. I might, therefore, in a pathological sense, reduce the causes of detention from three to two: to morbid adhesion of the *placenta*, and to inaction of the *uterus*.

If this position be correct, it naturally follows

that the occurrence of this hour-glass contraction should only be considered as a secondary cause of detention ; its formation being merely the result of the undecided manner in which the practitioner introduces, or attempts to introduce, his hand, with the intent to extract a *placenta* that had been retained by one of the other two causes, which I might denominate primary causes.

It is not, however, my intention here to contend for any new arrangement of assignable causes of detention. In the first place, because I cannot positively assert that the hour-glass contraction never was a primary cause, although such be my opinion. And, secondly, because, as such contractions will hereafter occasionally occur, although less frequently than heretofore, from heedless or injudicious management ; it may be desirable that the precepts inculcated for such events, should remain extant.

And yet, I may remark, notwithstanding there are particular rules of conduct prescribed for the accoucheur on such occasions, he must in practice

ever act from the incidental circumstances of individual cases, and not be entirely guided by insulated rules, or nosological distinctions.

With regard to the validity of my position, I conceive it to be but of trifling moment; whether, in any case of hour-glass form, we admit that the muscular fibres of the *uterus* had been irritated into this contraction, by the protracted presence of the practitioner's hand within the *cervix uteri*; or, whether we suppose the stricture had been occasioned by his injudiciously irritating the *vagina*, in minor attempts to bring down a *placenta*, that had been detained by some unavoidable cause. The inference, in either case, would be the same, that this hour-glass contraction had not been the original cause of retention.

Anatomists and teachers of midwifery divide the *uterus* by imaginary circular lines into three parts, viz. into the *fundus*, the body, and the *cervix*; the upper section is termed its *fundus*, the lower its *cervix*, and the intermediate (the limits of which are not positively defined), its body.

As we are left at liberty to dispose of those lines on the *uterus* as we please, I would so arrange them as to apportion two-sevenths to the *fundus*, three-sevenths to its body, and two-sevenths to the *cervix*. By this division, the upper and lower sections on the long diameter of the *uterus* are of equal length ; and the middle section is equal to one and half of either.

The foregoing division would apply to the *uterus* itself in every condition, but as the *uterus* and *vagina*, at the period of delivery, form one continuous and uninterrupted canal, I would then divide the combined two into four portions, and of the entire, assign two-tenths to the *fundus*, three-tenths to the body, two-tenths to the *cervix uteri*, and three-tenths to the *vagina*. In this arrangement, the two upper sections, the *fundus* and body of the *uterus*, are equal to the two lower, the *cervix uteri* and *vagina*.

This combined anatomical view of the *uterus* and *vagina* at the period of delivery, instead of considering of each apart, appears to me requisite for a

correct conception of many of the phenomena of labour.

And such arrangement appears naturally to exist in the constitution of the parts themselves. For we find the *fundus* and body thus to comprise the entire of the thickly muscular substance of the *uterus*, whilst the portion allotted to the *cervix* partakes more nearly of the structure of the *vagina* than of the upper portions of the *uterus*.

Thence the propriety of anatomically “regarding the neck of the *uterus* as a distinct and independent part from the body and *fundus* ;” and I myself think that instead of considering, at the period of labour, the entire of the *uterus* as one organ, and the *vagina* as another, it would be more judicious and natural to consider the *fundus* and body of the *uterus* as one, because possessing a common structure, and the *cervix uteri* and *vagina* as another, being also nearly of a common structure.

And I trust the eligibility of this arrangement will not be less perspicuous, in a physiological point

of view ; as the functions of the *cervix uteri*, during labour, are the very reverse of those of the *fundus* and body. The *fundus* and body exert a contractile and expulsive action ; whilst, on the contrary, the *cervix* relaxes in sympathy with the *vagina*. The *cervix* relaxes and must relax, otherwise the *fœtus* could not get exit ; and the *fundus* and body must contract, otherwise the *fœtus* would not be expelled.

Although any discussion on the general subject of labour would be a deviation from the avowed object of this paper ; yet as connected with the physiology of the *uterus*, I cannot omit to remark, that I have long considered the protraction of tedious labours to arise more frequently, from want of yielding elasticity in the *cervix uteri* and *vagina*, than from deficiency of expulsive action in the body and *fundus*.

To my admission already expressed, that I have met with the hour-glass form of the *uterus*, in the course of practice, I will here add, that I have even felt the lower chamber (as it is termed) so

nearly arched by the contraction, as to convey the impression of my hand having there reached the *fundus* itself. But, notwithstanding this fact, I object to the generally received opinion of such stricture being produced by any spontaneous action of the *uterus*. I would now beg simply to ask the candid practitioner, did he, in any case, after having overcome this stricture, find the *placenta* in the upper chamber detached?

If he answer no, an answer which I anticipate, that answer would be almost tantamount to a confirmation of my doctrine. The utmost that the most obstinate could then contend for, would be that this hour-glass contraction is occasionally a concurrent cause of detention. But, if independently of the contraction, there invariably exist a cause, and in itself, sufficient to account for the retention; such fact must indubitably smooth the way, for the reception of my explanation.

Practitioners have observed, in the operation of turning the *fœtus* in cross birth, it frequently is not necessary to introduce the fore-arm further within the

os externum, than midway or so from the wrist to the elbow. Hence it is inferred, if only so much of it be required to be introduced in an operation, when the *uterus* is distended with a *fœtus*, not more, if so much, will be required merely to bring away a *placenta*, when the principal cause of distention no longer remains.

But, although the principal cause of distention be removed by the birth of the child ; yet, the *uterus* at this time is not in length decreased. Moreover, in cross-birth we can often effect delivery without our hand going nearly so high as the *fundus*. Whereas, in the operation in question, it is always requisite that the convex hand should press against the farthest boundary of its cavity. Our object being not only to extract the *placenta*, but also to excite the *uterus* into regular and wholesome contraction.

The practitioner, however, instead of effecting this object introduces, (I mean occasionally) his hand only so far as to be within the *cervix uteri* ; and whilst searching there in vain for the *placenta*,

he unconsciously irritates the lower edge of the thickly muscular part of the *uterus* into action. The hour-glass contraction is the usual result of this irritation ; and, by the time the practitioner has discovered his error, a barrier is thus opposed to the further progress of his hand.

In some time after I had adopted in theory the opinion I am here advocating, I availed myself of an opportunity in a case of retention, to introduce my hand only so far as to be within the *cervix uteri* ; and, having delayed it there designedly, as I had on former occasions done inadvertently, I found the *uterine* cavity gradually assuming the hour-glass form. I then quickly passed up my hand to the *fundus*, without allowing the constriction to close so much as to materially impede its progress.

Having advanced so much in favour of my opinion of the cause, I shall now say a few words on the locality, of this stricture. Which locality I consider not to be at the central circumference of the *uterus*, as is more generally supposed ; but at the centre of the *cone* formed conjointly of the *uterus* and *vagina*.

It had always seemed to me an enigma, that this apparent inclination to oppose the hand of the accoucheur, thereby subjecting the suffering female to much additional pain, should be inherent in a circular band of fibres at the centre of the *uterus* ; and that the same structure of substance, both above and below this belt, should remain quiescent and relaxed.

But this stricture does not form from the middle circumference of the *uterus* ; it is formed by the lowest verge of its thickly muscular substance at the line of demarcation of its body and *cervix*. Which line in my arrangement of parts, is at an equal distance from the *os externum* of the *vagina*, and the farthest part of the *fundus uteri*.

Thus, then, it would appear, that the upper chamber comprises in its formation the entire of the body and *fundus* ; whilst the lower chamber engages only the *cervix uteri* and *vagina* ; and yet, these two compartments are, as nearly as may be, equal in capacity.

I ascertained the foregoing, in the following manner. Having in a case of this kind, overcome the stricture and gotten my hand into the upper chamber, it soon contracted and threw off the *placenta*; and shortly after, its capacity was so much diminished as merely to contain my fingers. I was then enabled by pressing my thumb against the thinner structure, to take (as it were) hold of the edge of the thickly muscular substance evidently terminating there; and, I could easily imagine from the point of my arm then at the *os externum*, that my finger and thumb rested on that part where the stricture had been. And likewise, on withdrawing my fingers from the contracted upper chamber, and rolling my hand within the lower, I found it equally capacious, as it had been previously to the diminution of the upper. The stimulus of my hand not similarly exciting it to contract.

Can any stronger facts be required to prove that the entire of the thickly muscular part of the *uterus* had been engaged in the formation of the upper chamber. For, if the lower, or any part of it, had been of similar structure with the upper, surely it

would have contracted upon application of the same stimulus, the hand.

I will now briefly state it to be my opinion :—

That the remote cause of the *uterus* assuming the hour-glass form, is a miscalculation of the distance (which is not less than fifteen inches) at this period, from the *os externum* to the *fundus* of the *uterus*.

That the exciting cause is irritation, produced either in the *vagina*, by injudicious pulling at the *umbilical* cord ; or, in the *cervix uteri*, by the accoucheur's hand searching there in vain for the *placenta*.

That the proximate cause is a spasmodic constriction of the muscular fibres of the *uterus* at the lower verge (not at the centre) of that section termed its body, and just where it ceases to be thickly muscular.

Thence, I conclude, that this hour-glass contrac-

tion is not produced by any principle of action inherent in the *uterus* itself; and that whenever it does occur, it is caused by mismanagement.

Therefore, in order to avoid such occurrences, the practitioner should always refrain from exciting unnecessary irritation.

And, in those few cases of unavoidable retention of the *placenta*, wherein it may be necessary for the accoucheur materially to interfere, he should, having first cautiously inserted it within the *vagina*, push his hand briskly up to the very *fundus* of the *uterus*. And, in this operation, he should direct the hand forward towards the *umbilicus*; ever bearing in recollection that the axis of the *uterus*, as well as the axis of the *pelvis*, inclines at a considerable angle to the axis of the trunk of the body.

and is not intended by any person to be
used in the future, and that the
said money is to be used by the management.

The management of the said
company, in order to avoid such
consequences, has decided to
revoke the said authority.

It is hereby declared that the
said authority is hereby revoked
and that the said person is
no longer authorized to act
in the name of the company.

The management of the company
has decided to revoke the
said authority, and that the
said person is no longer
authorized to act in the
name of the company.

It is hereby declared that the
said authority is hereby revoked
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in the name of the company.

The management of the company
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