

Contributions to the mechanism of natural and morbid parturition : including that of the placenta praevia. With an appendix / by J. Matthews Duncan.

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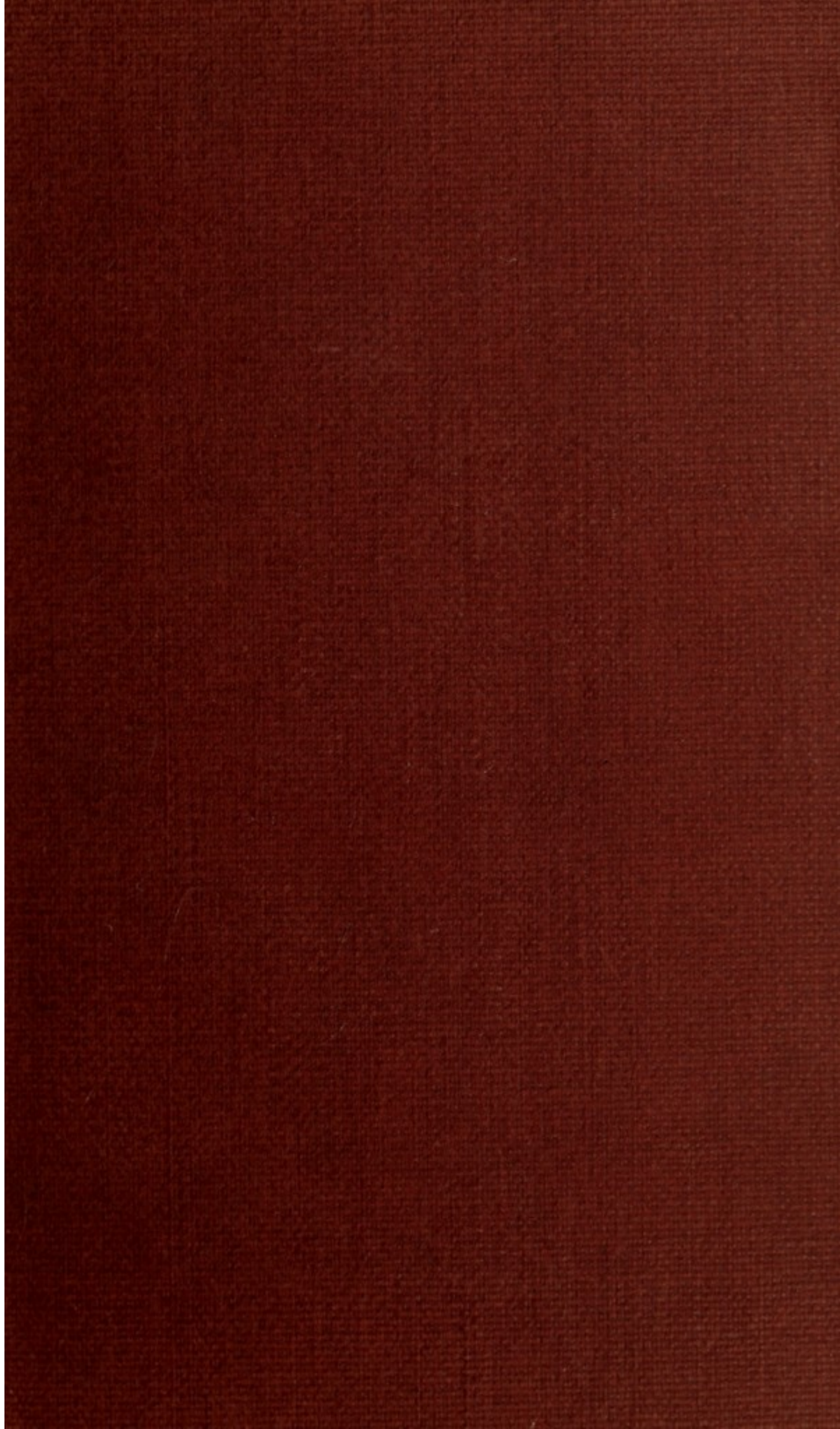
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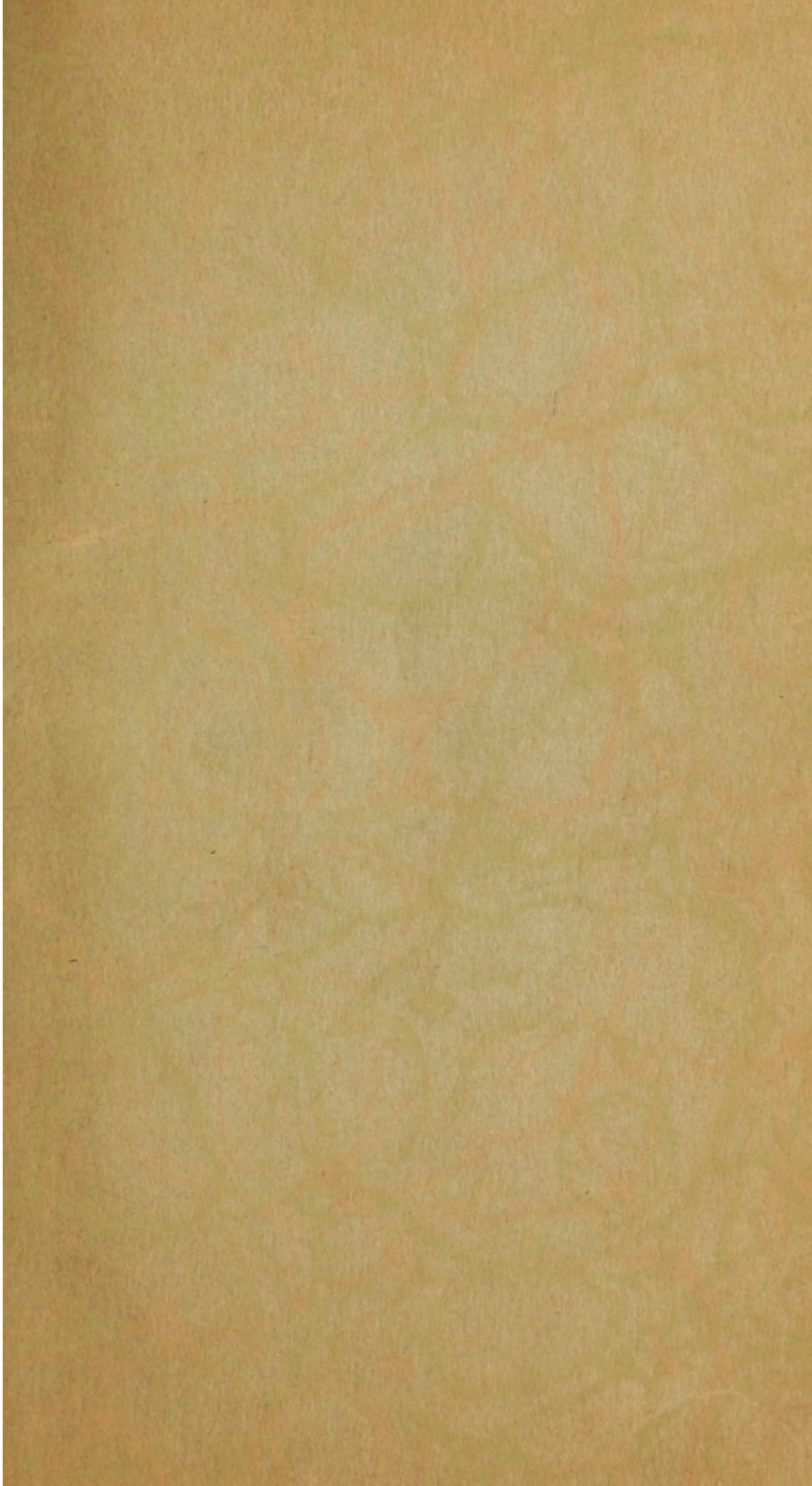


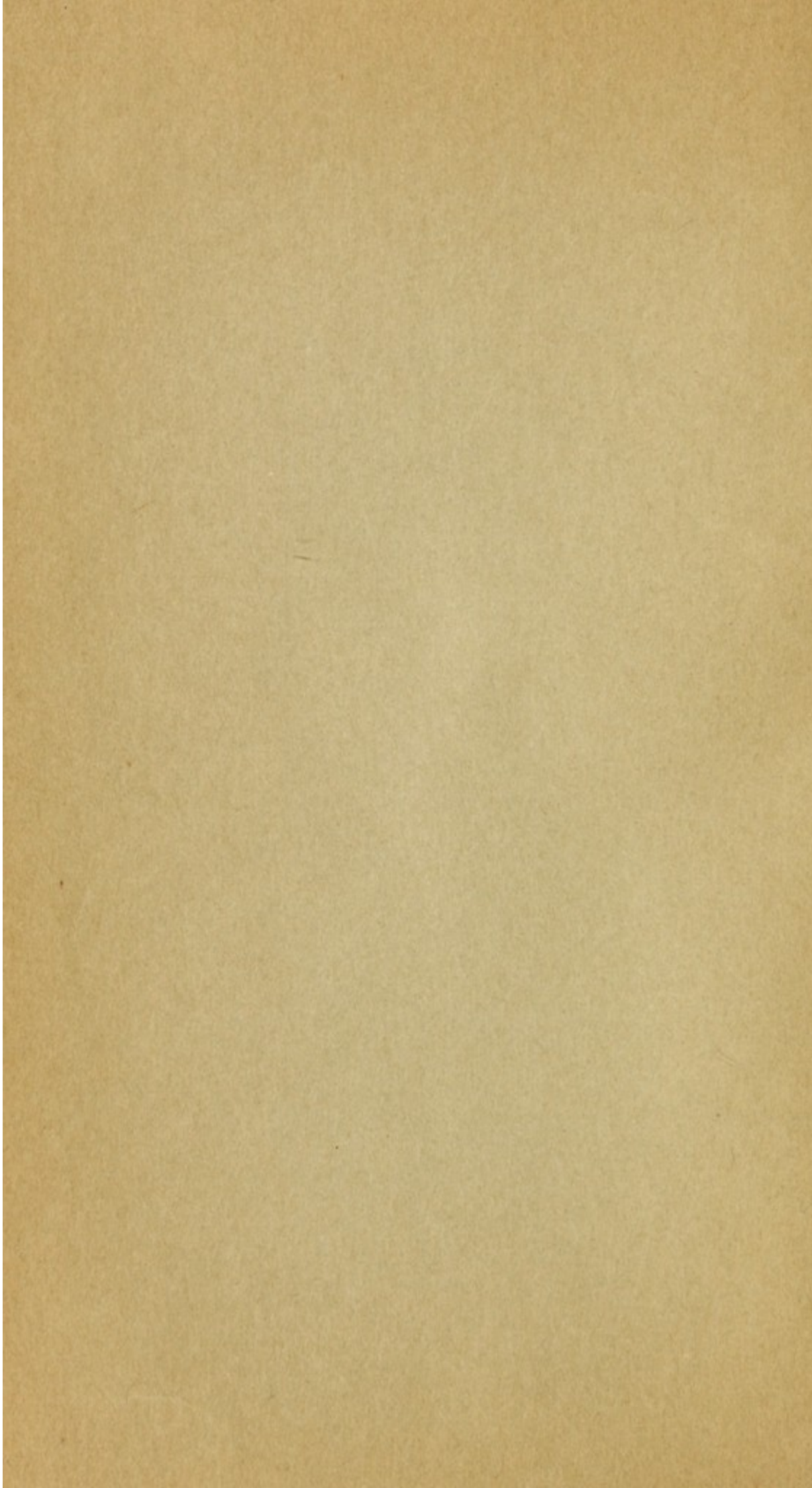
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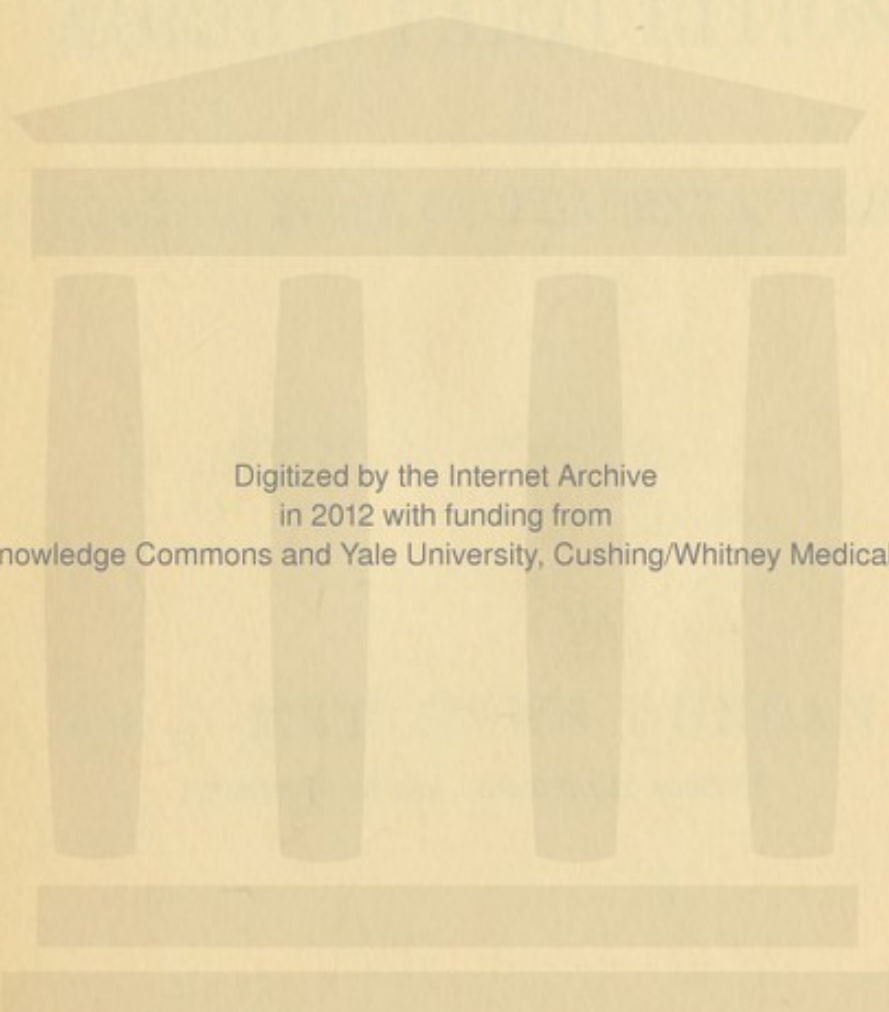


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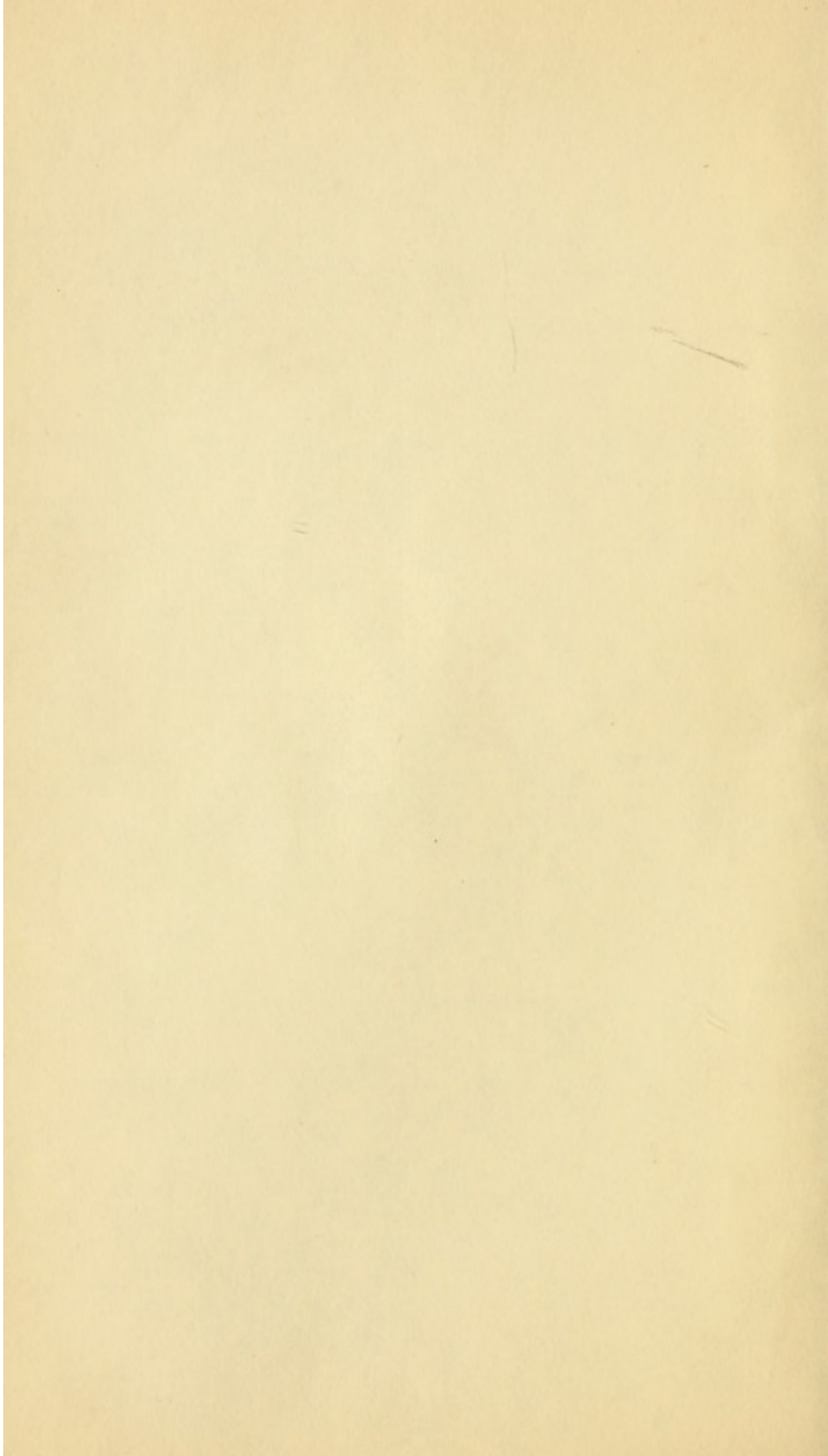
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CONTRIBUTIONS
TO THE
MECHANISM OF NATURAL AND
MORBID PARTURITION

INCLUDING THAT OF PLACENTA PRÆVIA

WITH AN APPENDIX

BY J. MATTHEWS DUNCAN

PRESIDENT OF THE OBSTETRICAL SOCIETY

EDINBURGH
ADAM AND CHARLES BLACK
1875

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TO

P. G. TAIT,

PROFESSOR OF NATURAL PHILOSOPHY IN THE
UNIVERSITY OF EDINBURGH,

A WORTHY REPRESENTATIVE OF THAT SCIENCE

ON WHICH IS BASED WHAT IS BEST

IN

MIDWIFERY.

STATE OF

IN SENATE,
JANUARY 18, 1880.

REPORT

OF

THE

COMMISSIONERS

OF THE

LANDS

AND

MINES

OF THE

STATE.

PREFACE.



THIS work consists of a collection of scattered papers (now chapters), written at various times, and published in various British and German journals. They are all devoted to the description and explanation of points in the mechanism of parturition. A few of them have already appeared in a collected form in the Author's work entitled, *Researches in Obstetrics*; and their reappearance here is called for by two considerations, namely,—the desire of the Author to have his writings on mechanism in one volume, and his desire to have further diffusion of the reprinted papers than is obtainable by their forming part of the *Researches*, a volume which will probably soon be out of print.

The book has been put together imperfectly, and for this condition the Author's only excuse is that for this kind of work he has no time to

command, and can only seize the occasional spare hours of a busy practitioner and teacher. He has to acknowledge with thanks the assistance of Dr. Hardie in passing the sheets through the press.

EDINBURGH,

December 20, 1864

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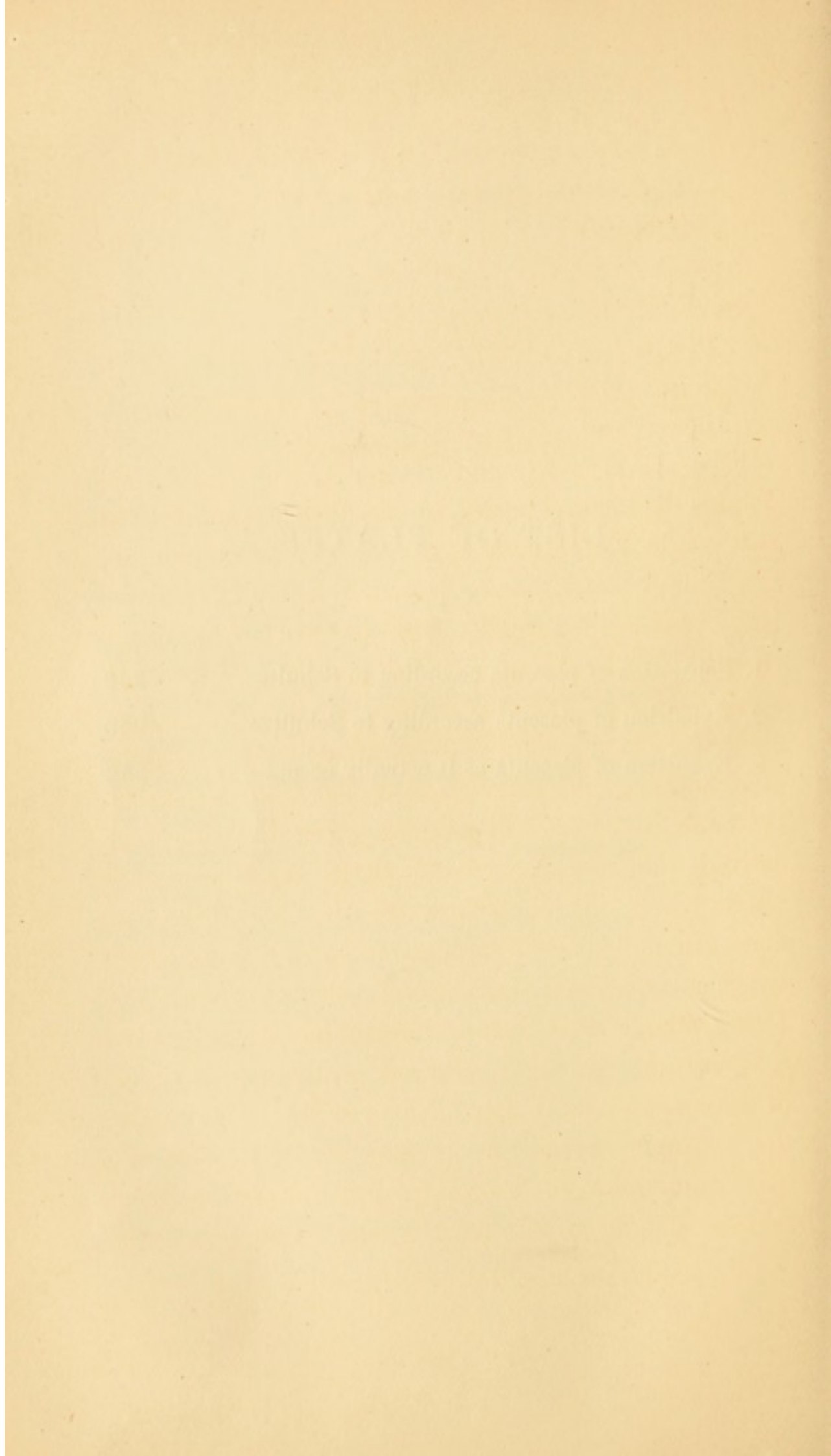


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CHAPTER I.

ON THE MODE OF PROGRESS OF THE SCIENCE OF NATURAL PARTURITION.¹

IN this discourse I mean to direct attention, not to any original observations or new discovery in midwifery, but to the mode in which increasing knowledge of the function of natural parturition has been ripened into a science of this department of Medical study. The history of midwifery records the appearance of each new item of obstetrical knowledge, one after another, and attaches to it the honoured name of its discoverer or expounder. But the time has arrived when we may classify these discoveries and observations, and show how all this knowledge or mass of details can be arranged in logically connected order, so as to form a science of natural parturition. Doing this, we find their characters and order of succession to be full of suggestions, which are valuable in the sense of hastening future progress. If we, for the occasion, suppose, as we may justly do, that all this acquisition of knowledge and formation of a science has been effected by one great perennial

¹ An Address to the Royal Medical Society on January 19, 1872.

obstetrical mind, it will facilitate our apprehension of the point of view I wish to occupy—namely, that of an observer watching and describing the method of philosophising which has been pursued by this supposed perennial obstetrical genius, and which has at length guided him in selecting from the heaps of detached pieces of obstetrical knowledge, accumulated for ages or only recently acquired, those shapely portions which were wanted to enable him, all within the last two centuries, to lay the foundation, and continue slowly rearing, the little court of the science of natural parturition in the great temple of modern science. It is the mode in which this genius has so successfully worked that I wish to describe.

There are many departments of midwifery, and of them the chief, in every sense of the word, is what is called Natural Parturition. To it alone do I now confine my remarks. It has advanced in scientific rank far before any other department. In proportion as natural parturition is more common than morbid labour, so is this department of science more important than that of unnatural parturition. Indeed, the science of morbid labour must always lag far behind that of natural parturition, and owe much of its progress to light reflected from that greater and higher neighbour.

Modern philosophers cannot, like the great majority of their predecessors, be reproached with disdainful utility. On the contrary, the expectation of direct

practical results, or of what is in Medicine called practical bearings, is a powerful stimulant of their efforts after truth, and the attainment of improvements in practice is a chief source of their justifiable pride. But if they are wise, they do not work with a direct aim to practical results. They do not set about the search for gold by digging in the nearest field, but by the study of geology. They seek new items of knowledge with a view to the advancement of science, sure that practical good will, sooner or later, appear as the ultimate fruit of their labours; and that their discoveries and reasonings are essential preliminaries to the ripening of the full crop of beautiful practical fruit.

Cheered by such reflections, obstetricians cannot be too ardent in the pursuit of the science of midwifery. The labours of all the great, and in their times famous, Practitioners—but *mere* Practitioners—who have flourished since the world began, have done very little progressive work for the benefit of the race. That has to be slowly elaborated by an humbler sort, the men of science. Much, indeed most, of what is esteemed by the Practitioners of one age is forgotten or rejected by those of the next, and the circumstance shows its real character. To-day, we bleed like Sangrado; to-morrow, we don't bleed, but pour in alcohol. Both practices have, to the truly philosophical mind, an equally ridiculous character. The skill and wisdom of the Practitioner, so far as it really is such, is

mainly of an esoteric character. It can neither be described nor handed down. It is the individual's property. Others may attain to equal or greater skill ; they cannot inherit his.

On the other hand, Science, the grandest result of the direct efforts of the human intelligence, stands contrasted. Its achievements are certain, secure, beneficent. They may be diffused over the world without being thereby diluted. They are capable of being expressed in written language, and handed down to every coming age. They are as necessary to every coming age as to the age which saw their appearance. They are the best pabulum of that intelligence which is the most useful armament of the Practitioner ; and they are the most prolific source of those more glaring and more directly, though less widely useful, practical discoveries which alone attract the vulgar gaze. They enable the informed Physician to distinguish the real gems from those flashy pieces of therapeutical paste which dazzle, I grieve to say, not only the public, but also a very large part of the Medical Profession.

Now we see partially the rich fruitfulness for mankind of Harvey's discovery. Without it, how very ignorant should we be as to the diseases of the heart and lungs. No doubt the Practitioner, were this knowledge wanting, would look very sagacious, shake his head *secundum artem*, and write a prescription, just as he does now ; but what value could be attached to the advice or prescriptions of one so

ignorant of the primary elements of a sound judgment? The practical men of Harvey's day did not recognise the practical value of his discovery; and I am not sure that I belie even the Practitioners of the present day, when I say that, were Harvey's discovery—a set of dry mechanical details—hard reading—published for the first time now, when as yet practical results were only dimly foreseen by a few, they would turn from them without compunction, to regale themselves with the confident boasting of some vain discoverer of a new medicine or a new treatment, whose future history, unlike the ever-widening beneficence of Harvey's discovery, may be a mere drowning in the waters of Lethe, or nailing to the wall (as of some foul, injurious bird), but which may very probably deserve the praise of being as useful as any of the other and older modes of treating the same malady.

The world had to wait very long before progress or even commencement in the science of parturition was possible. Knowledge of obstetrics was, from the earliest times, gradually increasing, and heaps of it were to be found among Practitioners and in books; but a science was impossible, because the essential knowledge was awanting. Obstetricians proceeded without sufficient data—they tried the deductive method; they vainly struggled to discover and to reason aright. They did not as yet fully see the first step of scientific progress. In evidence of this, I

refer to the great work of Deventer, published in the first year of the eighteenth century. The knowledge collected in this book is very valuable, and especially noteworthy in the light of this discourse, for it preceded the dawn of obstetrical science, and its appearance and character might justly excite hope for the future.

There could be no beginning of scientific progress till, at least, the locality of the operations to be studied was known—till the common anatomy of pregnancy was described—till the rude outlines of the machine, whose action was the subject of study, were mapped out. Mundinus, Vesalius, Ruysch, Noortwyk, Smellie, and others, had already flourished since the revival of learning, and had all made some contributions to the rough anatomy of pregnancy—and it was only the rough anatomy that was as yet desiderated. But, for the science of parturition, they all together did not enough; it was reserved for William Hunter to complete this satisfactorily. His great work was published in 1774, and this grand foundation-stone of obstetrical science still remains securely fixed, unsurpassed in perfection of elaboration, and more admired than when it left his hands. The most superficial observer must easily see that this first step—this foundation—was essential to future progress in the science. It was the first in time as well as in logical order; there were no other stones ready to be placed as soon as this was laid.

Others were already in process of preparation, and they were soon required and used—being those which should properly come next.

Midwifery was now fairly started in a course of scientific progress. To advance it, obstetricians must follow William Hunter. If they do not they go astray, and lose the fruit of their labour. Deventer's motto was, "Manet post funera verum." It was scarcely appropriate to the kind of work he did; but it fits William Hunter perfectly. He completed that body of truth which was the first requirement of the scientific obstetrician, and which was truly so designated by Deventer, who says,¹ "Primo. Requiritur generalis aliqua cognitio muliebrium, quæ generationi inserviunt, partium, quales eæ, ubi vel quo loco sitæ sunt : generalis, inquam, cognitio."

Already was this general knowledge of parts—the *generalis cognitio* of Deventer—insufficient in some points. On these it was necessary to get information of a precise kind, and one of these was the important subject of measurements. A body had to be propelled through a passage, and it was necessary to have a good idea of their comparative dimensions. But it was long before a correct view was got; because, before it could be obtained, it was necessary to find out the adaptation of parts in the body and in the passage, and their mutual relations.

As early as the middle of the eighteenth century

¹ *Ars Obstetricandi*, 1733. Cap. ii.

Ould, Smellie, and Levret indicated in their writings some feeling of the want of knowledge of measurements, which has now grown to so large a department of descriptive obstetrics. They advanced farther than a mere vague feeling of this want, for they indicated the child's head and the pelvis as the chief parts correlated in the function of parturition whose dimensions should be studied.

The notions of Levret were far from being accurate or precise. This we may almost argue from his comparing the circumference of the brim of the pelvis with the height of the individual. But though this great author did only a little for the subject of measurements, he distinctly pressed on the minds of students the great and fertile principle which has been the guiding star of all future explorers. Eminent men, even of his own country, have expressed their difference of opinion from Levret; but every day shows more and more that Levret was right, and that his assertion that parturition is a "natural operation, truly mechanical, susceptible of geometrical demonstration,"¹ will soon be nearly realised by the labours of scientific accoucheurs. All progress in the science of natural parturition has been, and continues to be, made in the line indicated by Levret.

As we have already said, Ould, Smellie, and Levret did not do for measurements what William Hunter

¹ *Art des Accouch.*, p. 83. See also Stein, and Eichstedt, *Zeugung, Geburtsmechanismus, etc.*, S. 79.

did for anatomy, and I know no individual name that deserves the place of honour in connection with them. We have to come far on in the decades, past the time of Denman, to find them fully stated and their relations and importance understood. Even in some text-books of our own day there is great deficiency and evident want of intelligence in the authors—want of such just appreciation as is to be observed in the writings of Smellie, or such fulness and completeness as are found in the manual of Paul Dubois, published in 1849.¹

The study of anatomy, and especially of measurements, necessarily leads to observation of shapes and directions. So, from the first, there have been attempts made to determine shapes, planes, and axes. Obstetricians are familiar with the learned writings on these points by Roederer, Stein, Bakker, Carus, Naegele, and others. But the writings of these authors could scarcely be expected to be complete enough for us. They could not, in their day, foresee the direction in which midwifery, as a science, would advance, and could not meet wants which were not felt. The subject of axes, especially, is found to be so connected with that of the exercise of force, that,

¹ The succession, in order of time, of cranial measurements to pelvic is well illustrated by the circumstance that, while for a long time pelvimeters have been invented, and especially internal pelvimeters, internal craniometers have as yet never, so far as I know, been proposed, as they certainly ought to be.

even in our own day, determinations which were long considered quite accurate are subjected to reinvestigation.

Topography, measurements, shapes, axes—these were now all achieved pieces of obstetrical knowledge. None of these departments has as yet received the highest development which the inquiring mind of man can give it. But already, about the beginning of this century, not only was the want of knowledge of them felt, but the want was in a rough, yet satisfactory manner, supplied. The first stage of the progress of the science of natural parturition was completed. The passage to be traversed, and the body to be pushed through, are carefully measured and described.

Students, and even those of them who are at the same time successful pioneers of science, are very apt, when taking a retrospective view of the road over which they have travelled and of the difficulties which have been overcome, to underestimate the amount and value of the thought expended in finding the road, as well as the labour and ingenuity expended in overcoming the difficulties met with by the way. The student is like the woman who, as soon as she is delivered of the child, remembereth no more the anguish, for joy that a man is born into the world.

So, again, when the first stage of progress is finished and recognised, and the second pretty well elaborated, it is then, and not till then, easy for our supposed perennial genius of obstetricy to see what

should be the next ; and, curiously enough, what is naturally expected to be the next stage is really so. The second stage of scientific progress has been far more difficult to effect than the first ; and we may venture to predict that each succeeding stage will be more and more difficult than its predecessors. But though more difficult to effect, it is not more difficult to comprehend after it is effected—perhaps even the contrary ; while it is, and will be, certainly more glorious to its promoters and more useful to mankind. It is only in a rough way that the second stage can be said to succeed the first in point of time, as it certainly does in logical order ; but the student who enters into the details can, I believe, satisfy himself that, taking, as he must with this purpose, a wide view of the matter, the second has really followed the first in order of time.

The second great stage in the advancement of scientific midwifery is the discovery of the manner in which the foetus traverses the maternal passages, and the alterations in form thereby produced.

The passage being known, and the body that has to be pushed through it being known, and it being known that the latter is large when compared with the former, and cannot easily or at all permeate it unless it is aptly placed and pushed, and that it generally requires strenuous exertion on the mother's part to force it on, it is evident that the next point to be studied is—How does it pass ?

The answer to this great question involves a mass of details which have been, for the most part, well made out ; yet many remain for investigation or for reinvestigation and confirmation, or the reverse. It had, for example, to be observed how the foetus lies before it starts on its course, or, when it starts ; how it is adapted to, or lies in, the passages at every stage of its course ; how it emerges from the passages into the world ; what changes in attitude it has in its course undergone, and when they took place ; what changes in form it has undergone ; how the placenta is separated, how it is expelled. When William Hunter's work was published none of these points was generally known ; most of them were entirely undecided ; some are not yet decided.

The second stage is peculiarly interesting to the obstetrical historian ; for he observes that it was only after it had made decided progress that obstetricians generally began to see that the subject of their studies was not a mere heap of blocks and chips of knowledge, but that there was a true science of natural parturition. To speak figuratively, it may be said for the world, and especially for Great Britain, that it was only when Naegele's paper on the " Mechanism of Birth " appeared, that the little plant of obstetrical science showed itself above ground. A new enthusiasm entered the Medical mind, and it has not died out, and will not, for it has living substantial roots. Every step in advance increases the growth,

and stimulates the cultivators. The epoch of this new and generous enthusiasm can be easily fixed by historical marks ; but it is also easy now to make manifest its weak juvenility. This is aptly illustrated by the very name of the memoir by Naegele—the “ Mechanism of Birth,” or rather the “ mechanism of labour ;” by which latter title the subject of it has been, and unfortunately is still, designated among the English-speaking nations. Naegele and his contemporaries and followers were fired with enthusiasm by the discovery of a new science. Their *eureka* was designated mechanism. They saw now, as a profession, for the first time, the grand idea of Levret, and they gave this name to the limited branch of investigation which they had cherished. The whole plant was mechanical ; they erroneously designated as “ mechanism ” one early leaf of it. Had William Hunter seen as far as Naegele, he might with equal justice, and perhaps he would, have named his volume of plates “ the mechanism of labour.” The mechanism of labour is a grand idea, for whose just application as the title of a book or of a chapter in a book we have still long to wait. We are still only working out the elementary parts of this mechanism. The second stage of the progress of the science of parturition has no more, perhaps less, claim than any other to this special name ; and it is desirable that it should be at once denuded of an honour of which it is not worthy. To say that the course described

by a train as it passes along the rails is the mechanism of railway travelling, is utterly absurd. But Naegele may well be pardoned this fault, seeing that he was the means of a great revival of the obstetric genius, and christened his work by a name which, though erroneously applied, expressed the grand basic idea of the whole modern progress of obstetrics.

There are many names worthily connected with the second stage of our science's progress. Among these may be placed Ould, Smellie, Roederer, Solayres, Saxtorph, Naegele. All these, and many others, have contributed to its progress. But if one name is to be singled out for the place of honour, it is that of Solayres of Renhac, who flourished at Montpellier, was the teacher of Baudelocque, and in 1771 published his dissertation "De Partu Viribus Maternis Absoluto."

Every point in the whole of this progress of the foetus and placenta has been the scene of contests most interesting to watch. But this is not the place for historical sketches of them; and, besides, this piece of historical work has already been very thoroughly done. Truths have been attacked, errors introduced, errors corrected, errors revived, truths triumphant; and, after all, knowledge has increased, and has, with general acceptance, come to be regarded as positive. Now, the great outlines of the subject are agreed upon, and, for me at least, well settled. But the obstetrical student of the present day knows

that much is still wanting in this department, and much confirmation of what is generally regarded as secure knowledge. The works of Küneke and of Schatz, full as they are of observation, hypothesis, and clever reasoning, are too recent, and have too much shaken the obstetrical mind of the world to allow us to regard the second stage in the progress of scientific obstetrics with complacency as a work quite done and finished. We are justly proud that it is a work whose great features are easily seen, and whose place is well known and universally recognised.

In recent years an interesting feature has been added to this department of obstetrics, in the description of changes of form or shears undergone by the propelled foetus. It had been long known that the head was changed in form during labour, but the change that was chiefly dwelt upon was produced by alteration of the condition of the soft parts. The new investigations refer to shears and other changes undergone by the plastic cranium of the foetus, and are an important addition to our knowledge, for which we are indebted to Dohrn, Barnes, Olshausen, Ahlfeld, and others.

If the second great stage of progress of the science of natural parturition is not nearly completed, if its maturity is not as that of the first stage, still it is old and highly developed compared with the third stage. It may be truly said, that as yet the Profession does not desiderate the increments of knowledge

destined to be produced by the promoters of the third stage. The second stage has come nearly to completion during the first half of the nineteenth century. The third stage has only made a small beginning in the second half of the same century; but as the number, the zeal, and the attainments of the modern students of midwifery have increased above those of past times, we may expect a rapid increase of our knowledge and new eras for our cherished and beneficent science.

The first department of the science of natural parturition is occupied by the description of the passage and of the body to be passed. The second department is occupied with the description of the mode in which the foetus passes, and the physical changes it undergoes during its passage. The third department of scientific midwifery is engaged with the forces employed to do the work. The sequence of the three stages is natural and logical, and also historical. The first two stages or departments are comparatively simple—they are mainly descriptive; but the third involves problems of much higher complexity than the first two. It taxes other faculties than that of observation; and its results, both in theory and in practical usefulness, will be much more conspicuous. It is destined to comprehend the knowledge of the amount of the efficient powers of parturition; their direction and application to the passages, to the bag of membranes, and to the foetus at

every part of its course. When the physicist considers the nature of the passages, their curvature, and their development during the process of labour, as well as the other factors in this function, he may be excused if he almost despairs of making progress. Many students are, however, already diligently occupied with work in this field ; measuring forces, ascertaining angles, observing levers, studying the influence of curves. As might be expected in the present day, these students are chiefly to be found in the German schools ; but there are cultivators of this department among the French and among ourselves. The Germans are, however, pre-eminent, and among them we may name Hecker, Poppel, Kehrer, Schroeder, Schultze, Winkler, and Schatz.

These three great stages appear to me to comprehend all that can be well called a constructed science in Midwifery—an ambitious title which I use and apply as it is used in describing the advanced parts of physics and of chemistry. Many departments of Medicine are regarded justly as in some sense scientific, while there is as yet no constructed science of them, nothing so advanced as in the part of obstetric science which I have been describing. Among these departments I may cite as examples the great subjects of therapeutics and of lunacy. These are replete with valuable knowledge of a scientific kind, but they get no farther. They lean upon various neighbouring scientific departments of knowledge ; but they are

themselves destitute of scientific structure as departments of Medicine. There is in these subjects as yet only blind and often frantic groping for scientific order ; there is no good foundation of a science of either of them laid.

These remarks I make partly in order to explain how it comes to pass that my sketch of the constructed science of natural parturition appears to be, and really is, so limited. In truth it goes no farther ; and obstetricians may, perhaps, be proud that it goes so far as it does. But in addition to this science of natural parturition, there is a very great accumulation of valuable pieces of obstetrical knowledge, all destined (I hope at an early day) to take their place in the constructed science, but as yet too chaotic for that ultimate destination.

There are two collections of obstetric knowledge in two great fields of obstetric labour, which deserve mention in this place. They are included in the physiology of parturition, but are engaged with matters which are not exclusively or purely obstetrical. I allude to the histology of the uterus and the functions of the various kinds of nerves supplying it. Although much has been done, and continues to be done, in these departments, yet ignorance may be said to prevail. We know nothing certain of the cause of labour, of the intermissions of pains, of the part of the nervous system presiding over the function of birth, of the connection of the nerves with the

muscular fibres, their nuclei, or their nucleoli ; and we know the history of the decidua vera and of the serotina very imperfectly.

Besides these two great masses of knowledge in the science of natural parturition, who will say how many more there are ? Who will tell us how great is our ignorance ? Each department was an unknown and probably unimagined territory to the most advanced obstetricians of earlier days ; and there can be no doubt that new territories for exploration will be discovered, of which, as yet, the obstetric mind has formed not the slightest conception. Each stage of advance involves increased difficulty of progress, and demands increased education in its promoters. The obstetricians of the future must be more talented and as laborious as their predecessors ; they are certain to be better informed.

All honest work contributes to progress ; for if the result is not a new piece of knowledge for all obstetricians, it is probably new for some, and the time expended on it is not lost, for it has at least contributed to increase the intelligence of one obstetrician, and has added to the common stock of intelligence in the community of obstetricians.

The work most likely to be successful is that done in a spirit of utmost humility by a man well informed as to the limit of present knowledge. If I may presume to criticise the ingenuous labours of my contemporaries, I should say that the prevalent error is

an overweening ambition. They try to go farther than it is as yet possible to go—faster than is consistent with safety—to establish a theory or a treatment without sufficient grounds. They should learn to do a small thing well before they try high tasks. They should study the history of midwifery, and, observing how small and easily made the greatest discoveries now appear to us, though they cost great labour to their authors, they may justly reflect that when the next great discoveries are made they will have the same appearance to our successors, but cannot at present offer such an aspect to ourselves. It may be thought that there was little ingenuity or merit in measuring the pelvis or the foetal head, or in telling what part of the head first emerged from the mother's body; yet the world waited for more than a thousand years before these simple things were done, and one of them is even now the subject of respectable controversy!

After all my discourse is finished, I think I hear sounding around me, and finding an echo even within my own microcosm, the question—Can it be that our great and proud medical discoveries, brought to the birth after such painful and tedious labours, are so simple that a child may comprehend them? Can it be that our highest medical science of the nineteenth century is so elementary, so mechanical? Yet the more the question is forced home the more clearly does the affirmative answer come back; and it is

amply confirmed by a wide circumspective view, as indicated by the answers to other like questions. Of what consist the greatest discoveries of man, if not in the discovery and application of the simplest mechanism? Regard the steam-engine and its applications, telegraphy, photography. Regard the ophthalmoscope, the laryngoscope, the stethoscope—a simple bit of wood, whittled into convenient shape—or any sound-conductor, to convey as much as possible of sound to the ear; yet a grand invention of recent times. In what consists the greatest modern scientific progress? Regard the already extensive and prospectively total reabsorption of chemistry by physics as an outlying or surrounding province. Regard the advancing researches in the nervous system, ever bringing the human frame nearer and nearer to the conditions of an as yet inconceivably fine automatic electro-magnetic machine. Regard the securest and best knowledge of the physician in the departments of dropsies, of hæmorrhages, of embolisms, of unnatural labours. Before their exploration, all these were inaccessible Alps, utterly hidden or only dimly seen through thick clouds of ignorance; now, they are green pastures by still waters, where a child may be safely led, and where a man may humbly step with courage, striving to subdue the next obstacle to his progress.

CHAPTER II.

LONG DELAY OF LABOUR AFTER DISCHARGE OF THE LIQUOR AMNII.¹

THIS chapter is not intended as a comprehensive discussion of premature rupture of the membranes, like that of Hugenberger;² nor does it discuss the uses of the bag of membranes, nor the replacement of it when prematurely ruptured by a bag distended with fluid, like those now used to induce premature labour, nor any other kind of replacement of the fluid, such as is practised in veterinary obstetrics, and with which the names of Wellenbergh, Kervel, Schlichting, Walbaum, and others,³ are connected. It is devoted to the single point of long delay of supervention of labour after discharge of the amniotic fluid.

Mrs. S.—, æt. 22, mother of two healthy children born at the full time, was unfortunate in her third pregnancy, the events of which I now relate. She had her last monthly illness in the end of September

¹ Read before the Obstetrical Society of London, June 5, 1872.

² *St. Petersburger medicin. Zeitschrift*, 1872.

³ See Emm. Stein. *Reflexions sur l'implantation de l'arrière-faix sur le col*, etc.

1871, and expected her confinement in June 1872. On February 23d she quickened, and stirrage continued to be felt till the child was born. (The size of the child when born and the date of the quickening render it probable that conception did not occur till near the end of October.) On March 10th there occurred during the night a copious flow of liquor amnii, and slight irregular pains were felt. The uterus at this time was bulky, filled the lower belly, and its upper margin was considerably above the level of the umbilicus. The liquor amnii continued to discharge freely, but not constantly, and irregular slight pains were often felt, generally during the night. The uterus gradually diminished in bulk and increased in hardness, and at last, and not later than a fortnight after March 10th, it felt not much bigger than a large adult foetal head, as hard as a fibroid, and did not occupy the whole hypogastric region, but lay much inclined to the left side, its fundus being somewhat below the level of the umbilicus. (For more than a week before the coming on of labour, Mrs. S—— thought that the uterine tumour was slightly increasing in size.) On the 20th April some shortening of the cervix could be felt by the examining finger, and this shortening slowly became greater without any increase of the occasional sensible pains. On the night of April 25th regular pains came on. The child presented the breech. No bag of membranes could be felt, and no gush or consider-

able quantity of liquor amnii was discharged during labour. When the breech distended the orifice of the vagina the child was extracted alive. The placenta was soon taken away thereafter; it was carefully examined and found to be natural, presenting the characters of the afterbirth of one child. The membranes also were natural, and were ruptured at the antiplacental pole of the ovum. The child was of the size and had the characters of a six months' fœtus. (It cried feebly and made violent respiratory struggles; but from the weakness of the thoracic skeleton they were nearly quite in vain, for the sternum and its adjacent costal cartilages were at each inspiratory effort violently drawn in, and as a consequence a great depression formed in the epigastric and sternal regions.) The child lived for a short time. (Its lungs were found to contain a little air, which could be squeezed out of them under water and so seen, but no part of the lungs floated in water.)

The child weighed 1 lb. 15 oz., and was thirteen inches in length. Its eyelids were closed; the vernix was everywhere rubbed away except on the nucha; the testicles had not descended into the scrotum. When born, it lay with its legs extended on the thighs, and the thighs bent so that the limbs were adpressed to the body. Besides this position, the limbs and features of the child showed signs of compression, which can scarcely be described, except so far as the auricles are concerned. These were pressed

flat on the skull, and by the vernix caseosa made to adhere so closely that a careless observer might have thought them absent.

[There appeared to me no doubt as to the facts of this case. So far as possible, everything came under my own direct observation. Pregnancy must therefore be regarded as having continued for forty-five days after the rupture of the membranes and the discharge of some of the waters. This long delay of the coming on of labour is not the only remarkable feature of the case, but also the circumstance that the foetus continued to live in a uterus so contracted as this was for weeks before its birth. In these respects the case is instructive.]

Numerous similar cases are recorded, but most of them are defective in the supply of such circumstantial data as contribute not only to their fulness but to their confirmation. In a matter like this, where various explanations of the source of the discharge may be suggested, and where there is much room for error, it is necessary, in order to the greatest sufficiency of a recorded case, that every detail which can contribute thereto be pressed into its history. Some cases are, indeed, so remarkable and so rare that in the meantime no amount of circumstantial or indirect evidence is sufficient to ensure their being received as true representations of nature. Among such I place the following, which I give in the words of Burns:¹

¹ *Principles of Midwifery*, tenth edition, p. 283.

—“Dr Pentland relates a very distinct case, where the liquor was, in the third or fourth month, discharged in a fit of coughing. The belly fell, but she still went on to the full time, and had a good labour.”¹ In order to explain such extraordinary long interval between the discharge of the liquor and the coming on of labour, Burns resorts to an hypothesis which appears to me to be not only without any rational grounds, but contrary to all we know ; and the resort to it is good evidence against the reality of the occurrence which he so attempts to explain. The hypothesis is that the torn membranes may be healed or the hole in them closed ; and it is said to be necessary to suppose such healing, because, when labour comes on, a discharge of liquor amnii takes place. “The aperture (says Burns) seems to close, if gestation go on, for during labour a discharge of water takes place.” At present I believe that obstetricians would reject all such hypotheses, and hold that entirety of membranes is a sign that they had never been ruptured, and that the discharge of liquor amnii in any considerable quantity during labour, affords at least a presumption against the belief that the rupture of the membranes had taken place long before labour began.

Dr. Pentland's case is, indeed, a good example of an incomplete and unsatisfactory report, for there is room for supposing that the water came from the

¹ *Dublin Medical and Phys. Essays*, No. 1, art. 3.

bladder. The waters are described as bursting in a fit of coughing, and the urine as coming away involuntarily by the force of coughing when the patient was in the horizontal posture. After delivery, the urine was discharged involuntarily : and there is no account of the state of the membranes in early labour.

Among recent cases of long interval between discharge of liquor amnii and the coming on of labour are the following :—Mr. Bradley, of Manchester,¹ relates a case of six weeks of interval, the child being described as if born alive. Mr. Norton, of London,² relates a case of seven weeks' interval. Mr. Bassett, of Birmingham, relates a case³ where the interval was six weeks. Dr. Swayne, of Bristol,⁴ gives a case of a month's interval. Mr. Cox, of Winchcombe,⁵ gives a case of delay of labour for thirty-three days after rupture of the membranes. Dr. Thorburn, of Manchester, records a case of six weeks' delay, after which the child was born alive.⁶

There are various conditions which may be mistaken for premature or periodical evacuation of liquor amnii.⁷ Among these are discharges of urine from the bladder, watery discharges such as are sometimes observed in virgins, and whose source may be

¹ *Brit. Med. Jour.* vol. ii. 1871, p. 612. ² *Ibid.* vol. ii. 1871, p. 667. ³ *Ibid.* vol. i. 1872, p. 155. ⁴ *Ibid.* vol. i. 1872, p. 184.

⁵ *Ibid.* vol. i. 1872, p. 367. ⁶ *Ibid.* May 18, 1870, p. 520.

⁷ On this subject, see some remarks by Barnes. *Diseases of Women*, p. 81.

Cowper's glands or the cervix uteri, discharges from the uterus of a fluid occupying the anatomical position of hydroperionic fluid, discharge of liquor chorii or false waters ; discharge of the liquor amnii of one ovum in a case of plural pregnancy ; discharge of the fluid in a cyst described as occurring between the chorion and the amnion. Examples of these discharges I have observed in circumstances which showed that there was much doubt and difficulty as to their source, or even positive error. In the case which I have related, it appears to me impossible to accept any of these views as to the source of the fluid discharged. That it was the liquor amnii is proved by the subsidence of the uterine tumour, by the diminution of its bulk, by the increase of its hardness, by the complete absence of discharge of liquor amnii at the time of labour, by the compressed state of the child, and by the almost complete rubbing off of the vernix caseosa. Another occasionally present item of evidence described by Burns was absent in my case. "I have known (says he) a discharge of the water take place at short intervals for some weeks, and then the funis umbilicalis protruded, without any exertion or any pains to rupture the membranes, which is a demonstration that the membranes had been previously open, and that the discharge of liquor did not speedily excite labour."

There is abundant evidence to prove that, as a general rule, the discharge of the liquor amnii is fol-

lowed by labour within a few days. But some authors, and among these Dr. Radford, very recently,¹ record it as the result of their experience that the rule which I have designated general is invariable, and throw doubt on the accuracy of observations which are adduced as evidence of the occasional prolongation of pregnancy for several weeks after such partial evacuation of the uterus. That such prolongation of pregnancy does occasionally occur can be absolutely proved. A medical friend of my own, in charge of a ward for diseases of women in a great public institution, mistaking a pregnancy for an ovarian dropsy, set about the operation of paracentesis in the usual way, and only desisted after a large quantity of liquor amnii had been drawn off, admonished by the motions of the fœtus striking against the canula. The woman and her child were none the worse of the operation. The uterus again enlarged, and pregnancy was continued for about a month afterwards. This is not the only case of the kind that I have known ; and, indeed, reflecting on such cases of error, and on others of intentional tapping of the uterus through the abdominal wall, I have more than once contemplated a like operation in cases of hydramnios.

It appears to me to be probable that in the common cases of rupture of the membranes and discharge of liquor amnii, where labour does not supervene at once, or within a few hours, the delay is only until

¹ *Brit. Med. Jour.*, February 3, 1872, p. 127.

the evacuation is almost complete, or, in other words, till the uterus is in close contact with the fœtus and irritated by its hard irregular surface, and perhaps by its movements. The liquor amnii, as in the case which I have narrated, and in others which I could cite, is not always all, or nearly all, discharged at once. Several days may elapse, and many gushes occur, before the discharge is as nearly complete as it can be without complete evacuation of the uterus. If the uterus in action causes the rupture of the membranes, it will then probably evacuate itself almost completely of liquor amnii, and labour will probably be commenced very soon. But when the discharge takes place under other circumstances, there may be retention of a considerable portion of the liquor, and its partial discharge in successive gushes may be produced by continued contraction, the result of the ordinary uterine contractions of pregnancy described by Braxton Hicks,¹ or by other causes, such as change of position, bearing down, or other muscular exertions.

In the case which I have narrated, the discharge of liquor amnii, in occasional gushes, continued till labour came on, and long after it was evident that the uterus had been, for the time, as completely evacuated of this fluid as it could be. But this circumstance is easily explained by the accumulation of newly secreted liquor amnii. Indeed, while excessive secretion may have been the cause of the rupture of

¹ *Transactions of Obstetrical Society of London*, vol. xiii.

the membranes, continued secretion must have been the source of the discharges which took place after the uterus reached its smallest dimensions. The amniotic membrane, whose structure has been recently so signally illustrated by the histological researches of Winkler,¹ has the power of secretion and absorption in a high degree, and clinical examples of both of these processes are not infrequent. That freshly secreted liquor amnii may accumulate in the uterus is not more remarkable than its only partial evacuation in cases of accidental rupture of the bag during pregnancy, or of ordinary rupture in the early stage of natural labour, or than the accumulation of urine in the bladder in some cases of vesico-vaginal fistula of considerable size. In my case the child survived while there was evidence of continued secretion by the amnion, and probably the life of the fœtus and its amnion is a necessary condition of continued secretion, but the settlement of this question by direct observations is desirable.

Two difficulties in the way of accepting the evidence adduced in favour of long delay of labour after nearly complete discharge of the liquor amnii are forcibly expressed by Mr. Bradley and Dr. Whitehead.² They consist in the belief that such sudden and nearly complete discharge of amniotic fluid would

¹ *Textur, Structur, und Zell-leben in den Adnexen des menschlichen Eies*, Jena, 1870 ; see also *Archiv f. Gynæk.*, Band iv. S. 253.

² *Brit. Med. Jour.*, January 20, 1872.

certainly bring on labour, and that the death of the foetus should always follow such evacuation of the uterus before many days have elapsed.¹ The former view is expressed by Mr. Bradley in the following words :—“I should think it most likely that one essential condition of such cases is that the rent in the membranes should be situated high up, so that all the liquor amnii does not drain away at once ; for if such were the case, and the uterus were to firmly embrace the contained foetus, it can scarcely be doubted that uterine contractions would ensue, followed by expulsion of the child.” Bradley’s opinion is confirmed, and the latter view also expressed by Whitehead in the following terms :—“I do not think the foetus could live many days in a sac without fluid, and compressed by firm, tonic, muscular action.”

X } Against such difficulties it is sufficient to adduce the evidence of well-attested facts, including those of uterine tapping, which I have in this paper mentioned. It is admitted that cases of prolongation of pregnancy after discharge of the liquor amnii for more than a few days are rare. It is only the occurrence of rare cases that has to be proved, and, as far as possible, accounted for. Now, so far as the nearly complete absence of liquor amnii and the firm compression of the foetus are concerned, we have an

¹ On this point see Hugenberger (*loc. cit.*), who discusses the opinions of Deventer and Denman.

analogical illustration in a rare set of cases, of which I have seen unmistakable instances, and of which many examples are recorded. These are cases of missed abortion, missed miscarriage, missed labour. In such the liquor amnii becomes absorbed; the uterus diminishes in bulk, becomes harder; the foetus and membranes are compressed, not, however, by "firm tonic muscular action," and the super-vention of labour may be delayed for weeks or months.

The survival of the foetus in the cases which I have recorded or referred to is certainly very remarkable, and it would, no doubt, have been impossible if the uterus had been firmly and actively contracted. Firm tonic muscular action of the uterus after discharge of the liquor amnii would, no doubt, soon destroy foetal life, and it would also lead, without much delay, to evacuation of all the uterine contents. That it was not firmly and actively contracted is proved by the absence of labour and the continuance of foetal life.¹ Firm compression of the foetus may take place without active uterine contraction, and it is firm compression by active uterine contraction that is incompatible with the continuance of pregnancy or of foetal life for any considerable time, not such mere

¹ Schatz (*Archiv f. Gynæk.*, Band iv., S. 50) makes an ingenious attempt to explain the survival of the foetus in breech cases, a class in which he alleges the premature discharge of liquor amnii to be less dangerous than in cranial cases.

firm compression as is seen in a case of missed miscarriage or missed labour. In such there is only the continued contraction necessary to keep the organ in contact with its diminishing contents, a kind of contraction that is observed during delivery, and is distinct from active uterine pains.

A high position of a rent in the membranes, and some sort of valvular action, are cited by some authors in order to explain the partial discharge of the liquor amnii and its repeated discharges; and it is well known that, with a view to desirable obstetric results, it has been recommended to puncture the membranes high up in order to secure only partial discharge of amniotic fluid. But such explanations are, I believe, chimerical. I know of no evidence whatever to show that accidental high rupture of the membranes was ever followed by the partial and repeated discharges supposed or expected to ensue. On the contrary, I have known high puncture of the membranes, as in the cases of paracentesis of the pregnant uterus previously spoken of, followed by no discharge at all along the natural genital passages. It is easy to suppose a high puncture of the membranes and partial withdrawal of the liquor amnii; but that explains nothing whatever in the cases under consideration, for in them the discharge was effected spontaneously, and such spontaneous high rupture and discharge is, at least, very unlikely to occur, and has never been, so far as I know, shown to have

occurred. Its occurrence may be admitted to be possible. I have already stated my views as to the explanation of partial evacuation of the uterus and repeated discharges, on a basis that appears to me better than a mere admission of possibility of high rupture and valvular action.

In the subject which has been discussed in this paper the great question for decision is, Why does not labour come on? Why do conditions which generally induce labour fail to do so in these rare cases? The same question forces itself on us in the remarkable conditions called missed abortion, missed miscarriage, missed labour. In our present state of utter ignorance as to the cause of the coming on of natural labour, it is not to be wondered at that we cannot tell the cause of its failing to come on. But the two subjects may well be studied together, for it is highly probable that he who discovers the cause of natural labour coming on will also be at the same time able to explain why, in the rare abnormal cases to which attention has been here drawn, labour does not come on.

Numerous lines of inquiry have been followed with a view to discovering the cause of the coming on of labour. To me there appears at present no more likely way of achieving this grand result than the circuitous one of endeavouring to discover the cause of labour not coming on. In physiology we desiderate the cause of natural labour. In pathology

we desiderate the cause of labour occasionally not supervening soon after discharge of the liquor amnii ; occasionally not supervening soon after the death and discharge of the foetus in the early months, not supervening even when the secundines are already putrid ; occasionally not supervening soon after the death of the foetus, or after absorption of the liquor amnii in advanced pregnancy ; occasionally not supervening, although the uterus is distended by a hydatigenous ovum, which is forcing it to grow and expand at a rate of excessive and almost incredible abnormal quickness.

CHAPTER III.

THE CURVES OF THE DEVELOPED GENITAL PASSAGE.¹

THE observer of the current literature of midwifery finds nothing more characteristic of it than the number of papers on the mechanism of natural parturition. These papers indicate, for the most part, an enlightened zeal, for they are engaged with a most important branch of this mechanism—namely, the mode of action of the force of labour upon the foetus and upon the passage, and the explanation thereby obtained of the changes which take place in these as natural labour advances.

For these inquiries great additional value would accrue were the amount of power exerted by the combined forces of parturition well known; but they can be carried on to a great degree of advancement even while the amount of power exerted by the machine is unknown or at least unsettled.

Some of these inquiries as to the action of the force of labour upon the foetus and passage are very easily solved, and have been long in this condition. But the most, and by far the most important, are

¹ Read to the Royal Society of Edinburgh on February 19, 1872.

questions only recently raised, and of which it may be said that few are familiar to the profession even as questions, and still fewer can be regarded as settled. Such inquiries form the natural sequel to the most recent developments of our knowledge of natural parturition. These have been chiefly engaged in describing *how* the foetus and the passage actually behave during the process, while the new inquiries are destined to explain *why* they so behave. These new inquiries will introduce us far more deeply into the subject of the mechanism of labour than those which have preceded them. They are specially difficult because of the varying conditions of the force of labour and of the correlated parts—the foetus and the passage. The former has the relations of its parts extensively changed while the process of labour proceeds, and the latter is only produced at the time by what is called the development of parts as the foetus advances.

The subject to which I wish at present to direct attention is the curves of the genital passage and their influence on the phenomena of parturition.

I.

The first curve to which I direct attention is said to be at the brim of the pelvis, and to have its convexity directed downwards and forwards. I do not admit that this curve exists in natural or ordinary circumstances; but it is of the utmost importance

to decide the point, because, without doing so, we cannot possibly determine the primary direction of the driving force of labour. Hitherto and now the axis of the uterus has been and is generally regarded as coincident with the axis of the brim of the pelvis, and to indicate the direction of the resultant of the forces of parturition. But an elaborate attempt has been recently made by Schatz and Schultze—especially by the former of these authors—to demonstrate that the axis of the uterus at rest and in action is inclined to the axis of the brim of the pelvis at a small angle opening forwards and upwards, and of about ten degrees. I have just said that the axis of the uterus has been generally considered to indicate the primary direction of the driving power; but it is evident that this can only be the case if a variety of conditions be satisfied. Of these the following are probably principal:—The assistant driving force, which is auxiliary to the proper uterine force, must be also directed in the axis of the brim of the pelvis, being supposed to be uniformly applied to the uterus by the circumjacent viscera and parts, acting like a fluid, exerting pressure equally in all directions; the uterus must be distended with a fluid which is copious enough to prevent any part of the walls being specially pressed upon or indented by the foetus, or it must have its tendency to become hemispheroidal superiorly unrestrained. Now, Schatz, in addition to giving the proper uterine driving force

an inclination to the axis of the brim by ascribing to the uterine axis such an inclination, still further increases the inclination of the whole driving force by describing the special direction of the auxiliary bearing-down driving force as still more inclined than the direction of the uterine axis. The resultant of the combined or whole driving forces will, of course, according to Schatz, have a direction somewhere intermediate between that of the uterine and that of the auxiliary driving forces.

Smellie's authority is much relied upon in support of the existence of this curve. In his plates he gives the uterus this inclination to the axis of the brim of the pelvis, both in natural cases and in cases of deformity ; but this is not satisfactory evidence as to what he believed, for it is probable that in preparing his plates he did not pay particular attention to the point. Those of them to which reference is here made (as xii. and xiv.) are not, in the proper sense, drawings or pictures, but mere plans, and might very well have been arranged as they are, merely because in other respects the works looked well. Dr. Barnes, in his recent work on obstetric operations, while adhering to the generally entertained view as to the coincidence of the axis of the uterus and of the brim of the pelvis, implies by his descriptions and drawings a belief that in most, if not all, cases of antero-posterior contraction of the brim of the pelvis, the uterine axis is inclined to the axis of the contracted brim, as Schatz be-

lieves it to be in cases generally. This is not the place for any full criticism of what Barnes very aptly calls the curve of the false promontory, because I confine myself to ordinary or natural conditions. I shall merely say that this important and practically valuable doctrine of Barnes regarding the curve of the false promontory is made too general. It can be true and applicable only where the posterior uterine obliquity is present, and it is not demonstrated, nor is it probable, that this always is so in cases of deformity.

It is extremely desirable that means should be devised for ascertaining the direction of the resultant of the combined forces of parturition, and especially of the axis of the uterus in action. The means adopted by Schatz, with this object in view, are not satisfactory; they merely go the length of showing how carefully he entered upon the question. But it may be permitted me to state reasons which tend to establish the ordinary opinion, and to discountenance that of Schatz.

If the uterine axis is inclined to the brim of the pelvis posteriorly to its axis, we should expect to find the child's head at the commencement of labour, while yet freely movable above the brim, to be in a position which has never, so far as I know, been ascribed to it in natural cases. Smellie, in his Plate xii., gives this position consistently, but not truly. He could not avoid doing so unless he represented the child at rest as having a left lateral flexion of the head; which would be ridiculous. His mode of drawing the uterus

with this posterior obliquity created an exigency for him, which he could get over only by what must be regarded as misplacement of the head. One error thus led him into another. The erroneous posterior uterine obliquity forced him to represent the left side as presenting in the very commencement of labour, in an ordinary case of first cranial position, with the occiput looking to the left. I do not see how the difficulty—Smellie's yielding to which gave rise to error—can be avoided, except by assuming that the ordinary view as to the axis of the pregnant uterus is correct.

At the same point where Smellie stumbled, Naegele also fell into error, but in a nearly opposite direction. In his classical essay on the mechanism of birth, describing the first position of the foetal head, he represents it as presenting at the brim of the pelvis, which it has not yet fully entered, more obliquely than when it has entered it, or as having at the earliest stage its perpendicular axis more inclined anteriorly to the axis of the brim ; and in this way he accounts for his allegation that the right ear can generally be felt at this time without difficulty behind the pubic bone.¹ Here a remark may be made similar to that applied to Smellie's drawing : namely, that the head could not be so placed unless the uterus had an anterior obliquity—an obliquity opposite in direction to that figured by Smellie, and described by Schatz ; an

¹ See the work of H. F. Naegele, *Die Lehre vom Mechanismus der Geburt*, S. 12. Mainz, 1838.

obliquity quite incompatible with Naegele's own descriptions in his work on the female pelvis,¹—or unless the child maintained an unnatural and undescribed left lateral flexion of its head.

The now generally entertained views, that the axis of the uterus coincides with the axis of the brim of the pelvis, and that the foetal head presents at the brim directly,² have at least the merit of evading such obvious and adverse criticism as the figure of Smellie and the expressed opinions of Schultze, Schatz, and of Naegele, are liable to be subjected to.

The great authority of Naegele was long sufficient to give currency to his statement that the head of the foetus, as it passed through the brim of the pelvis, had its vertical axis in a position of anterior obliquity to the plane of the brim—an obliquity which is appropriately designated the Naegele obliquity, in order to distinguish it from other obliquities at the same situation. The great argument against this view, and the only one having a final character, is that it is not an accurate description of what takes place; but, in addition, it has been argued against it that it is impossible to find a mechanism to account for it. Stoltz's attempt to explain its occurrence by mere lateral flexibility of the neck of the child is insufficient, because it affords no explanation why the lateral flexion is towards the posterior shoulder; but the

¹ F. C. Naegele, *Das weibliche Becken*. Carlsruhe, 1825.

² See chap. xi. of this work.

now alleged posterior obliquity of the uterus, as regards the axis of the brim, affords a solution which Naegele did not foresee when he described this obliquity as present and increasing with the increasing height of the head in or above the true pelvis. If, adopting the kind of nomenclature introduced by Barnes, we describe a curve of the natural promontory produced at the brim of the pelvis by the posterior obliquity of the uterus, then this curve, representing a deflection of the axis to the extent of about ten degrees, can be easily made to account theoretically for the alleged Naegele obliquity during the first half of the passage of the child's head through the ligamentous pelvis. For if we suppose, with Schatz, that the whole power of labour acts in an oblique line nearly corresponding to that of the axis of the uterus, or inclined still more posteriorly, then there will always be a tendency of the anterior half of the head, or of that which is nearer the concavity of the curvature of the passage, to descend first, and so produce the Naegele obliquity, if there be uniform resistance to the advance of all parts of the head. But, as the occurrence of Naegele's obliquity is now very generally denied, any mechanism which accounts for it derives little or no support of its own accuracy from the circumstance of its doing so.

Still another difficulty in the way of admitting the presence of this curve of the natural promontory as the natural or ordinary condition is worthy of

consideration. It is justly held that in natural labour the advance of the head through the brim of the pelvis is impeded only by friction and imperfect dilatation, or dilatability of the soft parts; but, if this curve of the natural promontory exists, a new and considerable difficulty is introduced, namely, the difference between driving a body through a curved and a straight passage—a new difficulty which it appears to me unreasonable to admit. And this is not all; for this addition of difficulty is not overcome and passed when the child's head has traversed the curve, but lasts during most of the process of the birth of the child. If this curve exists, the axis of the genital passage, regarded in the antero-posterior vertical plane, has the shape of a Roman S; its first or upper curve, the curve of the natural promontory, having its concavity looking backwards; its second, and universally recognised curve, having its concavity looking forwards. I believe we are nearer the truth when adopting the view at present generally entertained, that, in the antero-posterior vertical plane, the genital passage has ordinarily only one curve, having the concavity of its axis looking forwards.

Direct therapeutical bearings of this matter are evident and important, both in natural and morbid parturition. Certain attitudes of the body, by increasing or diminishing the flexion of the iliac beams upon the sacrum (a movement which I have else-

where described as nutation of the sacrum),¹ may alter not only the dimensions of certain parts of the genital passage, but also the relations of the axis of the pelvic brim to the axis of the uterus, or to the direction of the resultant of the forces of labour. In an elaborate paper,² Schultze has attempted to show that similar results may be produced by flexion and extension of the spine. This author assumes that the lower lumbar vertebræ govern the uterine axis, and that the latter is normally inclined posteriorly to the plane of the pelvic brim. He therefore recommends that, when difficulty arises at the brim, the spine should be flexed so as to bring the axes of the uterus and of the brim, if possible, into coincidence; and if we admit his assumptions, there can be no doubt as to the justice of his conclusion. For practical application, however, the proper treatment may be stated in such a way as to offend no theory as to axes of brim or of uterus, or so as to stand good, whatever view is held on these points. When, before labour, or while the fœtal head is still mobile above the brim, it is placed with its sagittal suture not traversing the centre of the brim, but lying anterior to it (as Smellie figures), then it will, during early labour, be pressed with a loss of force against the pubes, not directly into the brim. It will then be worth while to try

¹ *Researches in Obstetrics*, p. 148. See also chap. x. of this book.

² *Jenaische Zeitschrift für Medicin und Naturwissenschaft*, Band iii., S. 272.

whether flexion of the spine, by putting the woman into the attitude assumed in stooping forward, will correct the direction of the head (which I consider an unnatural direction). If it corrects it, the sagittal suture will be observed to leave the neighbourhood of the pubes, and approach or reach the middle of the plane of the brim. Again, if the uterine axis, or the resultant of the forces of labour, has this posterior obliquity to the axis of the brim, then, in the earliest part of its course through the ligamentous pelvis, the foetal head may be expected to show the Naegele obliquity—that is, its half lying in the anterior half of the pelvis will be lower than that in the posterior as regards the plane of the pelvic brim, being pushed down with greater force; and it will be well worth while to try whether or not flexion of the spine will correct this direction of the head (which I consider an unnatural direction).

The existence of this curve, or of posterior obliquity of the uterus, in cases of a minor degree of contracted flat pelvis, may account for the exaggerated Naegele obliquity which so frequently occurs in an early stage of labour in these cases. For it may be assumed, that in the almost complete absence of friction-resistance from the extended walls of the passage, the half of the foetal head next the symphysis pubis will be pressed down with greatest force against the brim, which will offer the same amount of resistance anteriorly and posteriorly, and the sagittal suture will

consequently be pushed towards the sacrum. The displacement of the sagittal suture in an opposite direction in the same class of cases of pelvic contraction—a comparatively rare occurrence—may be likewise explained by an unnatural slight anterior obliquity of the uterus. The very opposite results have been attributed to anterior and posterior obliquity of the uterus by Scanzoni and Litzmann, who disbelieve in such obliquities from the pelvic axis in ordinary or natural cases ;¹ and Litzmann is willing to admit that posterior uterine obliquity may be the cause of the displacement of the sagittal towards the symphysis pubis, which he describes as a rare event in some cases of natural pelvis. In subsequently discussing the third curve, I shall enter more fully on the increase of force propelling or driving at the concave side of the curved tube, and its efficiency when the frictional resistance is absent or nearly so, and other resistance to progress nearly uniform at all points. If my view is correct, then Litzmann's explanation of his cases of presentation of the posterior parietal bone is incorrect. Instead of assuming a posterior uterine obliquity, he should assume a slight anterior obliquity, other conditions being supposed natural.

II.

The second curvature of the genital passage, which I proceed to describe, is, like the former, situated at

¹ *Archiv f. Gynæk.*, Band ii. 1871, S. 435.

the brim of the pelvis ; but of its frequent existence there can be no doubt whatever. Its presence is indicated by the deflection of the uterus from the mesial line to the right or to the left (the axis of the bladder being deflected to the opposite side), and it is well known to be observed at all times—that is, before, during, and after pregnancy ; but as this paper is concerned only with dynamical matters, this deflection or deviation is interesting only as observed during labour. On the direction of this deflection to right or to left I have no remarks to make, but I may refer the student first to the recent papers on this subject by Winkler¹ and Pfannkuch,² and then to the earlier observations of Spiegelberg³ on this uterine position during labour. For my present purpose it is more important to have some idea of the amount of deflection which occurs. With a view to ascertain it, however imperfectly, I examined a series of cases which I found to present this condition. I did not in all of these cases make out whether or not the deflection persisted during uterine action, but I ascertained that it did so in some of them. I hope to make further observations on this point ; but such an inquiry is not essential to my present purpose, it being sufficient to know that the deviation does generally persist during the so-called erection of the uterus in a pain.

¹ *Jenaische Zeitschrift.* Band iv. S. 522. 1868.

² *Archiv f. Gynæk.* 1872.

³ *Monatsschrift für Geburtskunde.* Band xxix. S. 92. 1867.

I proceeded as follows :— Having the pregnant woman lying flat on her back, I made out the position of the uterus by feeling its outline with my hands. This manipulation shortly induced a pain which made the uterine form more distinct than previously ; and then I could observe the outline, mark the projection of the direction of the axis on the skin, and notice its just incidence on the outline of the fundus. Then I measured off, as on a plane, the angle between the projection of the axis and the vertical line joining the ensiform cartilage and the symphysis pubis. I did not try to have guidance from feeling the uterine angles and the parts attached thereto, as Winkler has done in similar circumstances, because I thought that such guidance would not insure greater approach to accuracy in the measurements I wished to make with a view to purely dynamical considerations.

This angle I found in five cases to be, 8° , 10° , 11° , 14° , and 15° , respectively, or on an average about 10° . The problem now to be solved is to make out from this angle on the surface of the spheroid what is the corresponding deflection of the axis of the spheroid ; and since the angle, as measured low down on the surface of the abdomen, lies in a plane nearly parallel to that in which the axis of the uterus is deflected from the antero-posterior mesial plane, the deflection of the axis may be regarded as nearly identical in amount with the angle measured on the surface.

It is probable that this angle of deviation of the

axis of the uterus from the axis of the brim of the pelvis has important physiological and practical bearings; but as yet little has been made out regarding them. It may explain the more frequent dilatation during pregnancy of the right than of the left ureter, observed in women dying in childbirth, by Stadfeldt, and referred to by Roberts.¹ It has been looked upon as affording some explanation of the alleged comparative frequency of laceration of the cervix on the left side in ordinary labour.² It may explain the greater frequency of laceration of the vagina on the left than on the right side, as pointed out by M'Clintock.³ It may explain the greater frequency of pelvic abscess *post partum* on the left than on the right side, as attested by Grisolle, West,⁴ Scanzoni,⁵ Winckel,⁶ and Munde.⁷ Lever, Scanzoni, Winckel, and Munde, attribute the evil to the pressure of the occiput, which more frequently looks to the left than to the right; and this may be a part of the explanation, for the precedence

¹ *Practical Treatise on Urinary and Renal Diseases*, 1865, p. 415.

² *Edinburgh Medical Journal*, June 1871, p. 1061.

³ *Dublin Quarterly Journal of Medical Science*. May 1866.

⁴ See the author's *Perimetritis and Parametritis*, p. 158.

⁵ *Lehrbuch der Krankheiten der weiblichen Sexualorgane*. Band ii. S. 14.

⁶ *Pathologie des Wochenbettes*. S. 182.

⁷ *American Journal of Obstetrics*, February 1874, p. 687. For some interesting facts, and for the opinion of Guéneau de Mussy, see Lucas Championnière, *Lymphatiques utérins et lymphangite utérine*, p. 44. The data of Sutugin (*Klin. Bericht*. Von Bidder u. Sutugin, St. Petersburg, 1874, S. 165) do not support the opinion on this subject given in the text.

of the occiput and its greater nearness to the line of the spinal column may increase its baneful influence by pressure ; but it is probable that the baneful influence depends chiefly on the lateral obliquity of the uterus, which, being most frequently right, then, causes greater pressure to be on the left side of the pelvis during labour, whether the occiput or forehead be adjacent to that side. A like explanation may be given of the greater frequency of phlegmasia dolens on the left side—a frequency which has also, in my opinion, been too much attributed to the greater frequency of left occipito-anterior positions,¹ instead of to the obliquity of the uterus, and the existence of this second curve. But the most interesting application of it is to assist in accounting for the production of face cases.² It has been shown how, under certain conditions, and supposing a right lateral deviation of the uterus, the part of the head on the left side of the brim—that is, the seat of the concavity of the curvature—will have a greater tendency to descend—that is, will be more powerfully pushed downwards than the part on the right side of the brim. Of this there can be no doubt ; and the probability of this being a true theory or explanation of face cases is highly increased by remarking the apt manner in which other things known in regard to face-presentations adapt themselves to it.

¹ Trousseau. *Lectures on Clinical Medicine*. English Transl. Vol. v. p. 291.

² Chap. xiv. of this book.

Another ingenious dynamical theory of face-presentation has been started by Schatz. He states it as follows :—“Where the uterus alone is in action, or when there is also acting uniform resistance around by the walls of the pelvis, a cranial presentation always occurs, if the occipital foramen of the foetal head at the time of the first more important shortening of the long axis of the uterus lies backwards from this towards the back of the foetus, but a face-presentation if it deviates forwards from this towards the breast side of the foetus. With the co-operation of non-uniform resistance by the walls of the pelvis, cranial presentation is produced if the occurring posture or negative distance of the great occipital foramen towards the back of the foetus from the long axis of the uterus, multiplied into the positive or negative difference of resistance by the walls of the pelvis, is greater on the posterior side of the foetus than the product of the same factors on the breast side. In the opposite circumstances, face-presentation is produced.”¹ To all this ingenious theorising there can be no objection, if the conditions are assumed. But the two chief premises are merely assumed ; they are not shown to occur ; they are not shown to be more likely to occur in face-presentation cases than in others. Under these circumstances, I submit that there can be no hesitation in preferring the formerly named theory of face cases, where the

¹ *Der Geburtsmechanismus der Kopflagen*, S. 72.

corresponding assumptions or premises are not mere assumptions, but well-known facts. I refer to the occasional lateral deviation of the uterus, the occasional dolichocephalous condition of the head, and the greater liability of cases of the second or right occipital position to be transformed into face cases than of the first or left occipital position.

III.

The last curve of the developed genital passage which falls to be considered is the most extensive and the best known. It is the great curve in the antero-posterior vertical plane, which begins about the middle of the third bone of the sacrum, and extends through the outlet of the ligamentous pelvis to the outlet from the soft parts. Its length may be greatly diminished by rupture of the perineum, and still more if the sphincter ani is torn through. It forms a curve whose amount of bending varies from about 60° to about 150° .

In connection with this curve fall to be studied the synclitic and allied movements of the foetal head during its progress, to which Kueneke has recently directed attention, and of which the first has been so carefully discussed elsewhere¹ that it is unnecessary to re-enter upon it here. It also leads to the explanation of the greatest frequency of lacerations in the posterior walls of the uterus and vagina.

¹ See *Edinburgh Medical Journal*, June 1870; and the *American Journal of the Medical Sciences*, October 1870, etc.

In connection with this curve have also to be studied the development of the lower part of the genital passage—the greater development posteriorly where the force is particularly or more strongly applied, than anteriorly where there is little more than counter-pressure, or pressure against a fixed wall, and that chiefly during the temporary abeyance of the power of parturition. There is to be noted also, in connection with this curve, the inevitable tendency of the force of labour, not merely to distend the perineum, but also to rupture it centrally, to force the presenting part through it—a tendency the study of which, apart from other considerations, leaves no possible doubt as to the expediency of the practice of supporting the perineum: a practice which can be demonstrated to favour the maintenance of its entirety.

A novel practice, founded upon what I regard as a misapprehension of the conditions of this curvature, has been recently much dwelt upon by Professor Schultze of Jena.¹ The practice has for its object to facilitate and promote the advance of the child after its head has reached the floor of the pelvis. It is proposed to effect this by extension of the spine, with a view to which a hard pillow is placed beneath the loins as the woman lies on her back. The extension of the spine he believes to increase the posterior

¹ See *Jenaische Zeitschrift für Medicin*, etc., Bd. iii., 1867; and *Lehrbuch der Hebammenkunst*, 1870.

obliquity of the axis of the uterus, and therefore of the force of labour as exerted in this part. By the change supposed to be thus effected in the direction of the axis of the uterus, the axis of the force of labour is brought more nearly to the direction of the axis of the outlet of the pelvis, whereby there is supposed to be produced a diminution of the otherwise necessary loss of power arising from the change of direction of the passage at this part. Schultze alleges that he has found this extension of the spine to be useful in practice. If this utility is confirmed and ascertained, nothing of course can be said against it. But for the enforcement of his recommendation of this practice, it is evident that he trusts chiefly to theoretical arguments; and therefore I proceed to examine them, and believe I shall show that they are fallacious. Before doing so, it is worth while to point out that the attitude recommended by Schultze is a very unnatural one, and that a woman straining in labour, advanced to the stage at present under consideration, naturally assumes an attitude nearly opposite to that implied by extension of the spine—an attitude of some degree of flexion—an attitude which, in consequence of the relaxed state of the sacro-sciatic ligaments, may be accompanied by some degree of enlargement of the outlet by the posterior nutation of the apex of the sacrum.

To Schultze's theory of the facilitation of the latter part of the second stage of labour by extension of the

spine several objections may be made. Firstly, it is inconsistent with his views as to the facilitation of the entry of the foetal head into the brim of the pelvis by flexion of the spine. That view is based upon the assumption that the child's head enters the brim of the pelvis so as pretty nearly to occupy it and have a nearly vertical axis in the axis of the brim. If this be true of the foetal head at the brim, it will be true of it during its course *mutatis mutandis*, and it will be true of that part of the body which occupies the brim when the child's head is pressing on the perineum. It will be impossible, therefore, by any change of the axis of the uterus to bring the line of the labour-force to bear upon the perineum in the direction of a straight line, as Schultze represents it. x Second, the upper cylindrical solid portion of the ligamentous pelvis, having a length of at least one inch and a half, has a well-determined axis, with which must correspond the axis of any body fully occupying it, if the body is of uniform consistence—conditions with which the foetus nearly complies. If this be the case, the direction of the force of labour will follow the same axis, and no change of its direction above the brim of the pelvis, however produced, can have any effect upon its direction in any part below the brim of the pelvis. Third, Schultze forgets that his practice is intended to produce or increase posterior obliquity of the axis of the uterus to the brim, to increase the supposed curve

of the natural promontory ; and that every additional degree of that curve necessarily produces additional loss of power. The more, then, he extends the spine, he will diminish the power of labour available at the outlet of the pelvis, instead of increasing it, as he expects. Fourth, if Schultze's views, as illustrated by his diagrams, are correct,¹ a dangerous amount and direction of force would be brought to bear upon the perineum—a structure whose integrity is already sufficiently imperilled by a force whose direction is gradually changed as the foetus passes through the lower half of the ligamentous pelvis.

Before concluding the consideration of the great curve of the genital passage in the antero-posterior vertical mesial plane, it is necessary to point out an important difficulty introduced into its study by the change in the condition of the ovum when passing through it, as compared with the ordinary condition of the ovum when passing the pelvic brim. Hitherto I have spoken on the assumption that the ordinary view of the action of the power of labour holds good at all points of the course of the child. This view is that the power is uniformly applied by the concave surface of the approximately hemi-spheroidal uterine body to the uniform surface of the approximately hemi-spheroidal end of the ovum, in a direction corresponding to the axis of the uterus and of the first part of the developed genital passage. Now, this view is

¹ *Lehrbuch der Hebammenkunst*, Fig. 53.

probably nearly correct so long as the membranes are unruptured, or while no special part of the fœtus impinges on the uterine body so as to injure its approximately hemi-spheroidal form, and provided no part of the fœtus impinges on the passage so as to cause special friction or obstruction at the part impinging. But, while the great antero-posterior vertical curvature of the genital passage is being permeated, this view is no longer tenable, although even then it may, in a confessedly inexact way, be advantageously kept in mind, if other more exact conditions are not stated. While this curve is being described, the membranes are generally ruptured, and the waters more or less completely discharged, and consequently the fœtus is in a variety of places impinging on and changing the form of the propelling uterus, and meeting with frictional obstruction in the passage at special points more than at others. These changes introduce an amount of complication of the problem which damages greatly the value of such considerations as I have above adduced, and I see no means at present of overcoming the difficulty of arriving at exactness, though there is probably no insuperable difficulty in the matter. Another element of confusion is introduced in the want of uniformity which exists in the composition of the fœtus as a mechanical body. It is especially to be noted that it contains a longitudinally placed elastic beam of connected vertebræ, which lies nearer

the surface of the mass at one side than at the other.

Although the foetus is provided with a vertebral column, which considerably differentiates its condition from that of anything having freely movable constituents, yet I believe that it must be regarded as in the main a peculiarly modified viscous mass. When propelled, its parts are all squeezed and compressed ; and, if the head is born, fluids are seen to be forced out of the mouth and nostrils. A part more movable than the rest may be propelled alone, or more quickly than the other parts, as is frequently observed in the case of the cord, or of an arm and hand, and this under various circumstances, and especially frequently after the head alone is born. The rare expulsion of the placenta before the child sometimes illustrates this same condition.

The ovum or foetus, in its passage through the developed genital canal, is subjected, in various circumstances, to various rotations on some more or less longitudinally directed axis. It is also subject, in various circumstances, to various revolutions or sinuous deflections, in which its long axis moves through portions of curves which are measured by corresponding angles. On these curves and their influence I have made a few remarks, while feeling deeply their imperfection and the need of much further observation and research.

The student who has followed the argument in

this chapter will have observed the resort to inferences when direct observations would have been preferable. This remark applies to every subject discussed in it; and, while it is to be greatly regretted that such is the case, it is at the same time not to be forgotten that no method of making direct and exact observations has hitherto been discovered. The adoption of the homalographic method is surrounded with difficulties, not only in the method itself, but also in the procuring of subjects on which to use it; and while results obtained by it would be of greatest interest and importance, it is evident that they would not be complete or sufficient, for they can never be other than observations on parts in the repose of death, not in the turgescence and action of life. The recent homalographic achievements of Braune of Leipsic cannot be too highly praised. They are as yet too few to be justly made a basis of general statements, but, so far as they go, they confirm the views stated in this chapter. Until very recently all our knowledge of the force of labour was on a like imperfect footing; but already ingenuity has suggested a means of basing this subject on exact observations, and Schatz¹ has availed himself of these means, and greatly assisted us to arrive at results which we regard as probably the most important hitherto achieved in obstetric science. Till some ingenuity has succeeded

¹ See his description of his tokodynamometer: *Archiv f. Gynæk.*, 1872.

in devising means of making like observations to settle the points discussed in this chapter, we must be content to do our best to reach the truth by reasoning on what we do know more or less exactly. And it should be remembered that by this method we may reach the greatest assurance, if not certainty. A boy playing with his dissected puzzle-map may be certain that a county is rightly placed if it fits exactly into an entire hole formed of the conterminous boundaries of surrounding counties, especially if it also fits in nowhere else. So a theory which suits itself to all, or is in opposition to none, of numerous known conterminous conditions, may be, provisionally at least, assumed to be correct; and such assumption of correctness will vary in satisfactoriness with the number and testing character of the conditions so humoured by the theory.

CHAPTER IV.

THE POWER EXERTED IN ORDINARY LABOURS.¹

THE dynamics of natural labour have been the field of very little successful study or investigation. I purpose, first, to show what amount of pressure per square inch is sustained by the ovum in the easiest class of natural labours, and thence to estimate the propelling power exerted in such cases.

It is well known that natural births are ever and anon occurring, in which the ovum is expelled whole, the membranes containing the liquor amnii continuing entire. Into this category many more cases would enter, were it not a generally-followed rule for the attendant to rupture the bag should it advance entire as far as the external parts. Again, as Dr. Poppel² has pointed out, the attentive observer of a series of easy natural labours has no difficulty in arriving at the conclusion that in not a few cases the same force which ruptures the bag of membranes is able to complete the delivery, as, in other cases, it actually does.

In all such instances the strength of the membranes to resist impending rupture measures the force

¹ Read to the Royal Society of Edinburgh, April 29, 1867.

² *Monatsschr. für Geb.* 1863.

exerted in the process of parturition. When the bag is produced without laceration, its strength exceeds the power of the labour. When the bag is ruptured at a very advanced stage of labour, as not rarely happens, its strength exceeds the power of labour exerted up till the time of its rupture. When the bag is ruptured by pains, which, without probably increasing in strength, rapidly and easily terminate the process, then the power of labour is only a little greater than the estimate, founded on the strength of the membranes, would indicate.

The strength of the membranes is thus shown to give us a means of ascertaining the power of labour in the easiest class of natural cases.

It might be suggested that, in cases of persistent membranes, they were specially and unnaturally strong. My own experience lends no support to such a notion. Besides, so far as I know, no obstetrician has used the only means of verifying such a supposition—means such as are exemplified in the experiments to be hereafter related. Obstetricians have judged of the strength of membranes to resist a bursting force by their united thickness, or other less definite qualities, which form no criterion. It is not uncommon to read of the bag being strengthened by decidua; and that such thickening may be a source of strength is a common opinion; but as the decidua is far weaker and less extensible than the other membranes, the opinion is merely a natural

delusion. In like manner, it is common to hear tenuity of the membranes spoken of as if this quality necessarily indicated weakness, an opinion which also is erroneous.¹

Experiments at once show that thickness of the membranes is no indication of strength. They also at once show that, for the special purposes of this paper, the amniotic membrane, being the strongest, alone requires to be observed. Long before the amnion is burst, the decidua and chorion have generally given way, and ceased to support the persistent amniotic membrane. The decidual membrane generally gives way first, under a bursting pressure applied to all three membranes. It sometimes does so in experiments with a sound as of a gentle fillip. Occasionally it bursts simultaneously with the chorion; and occasionally, but rarely, all three membranes burst at once. The decidua has been found, in my experiments, to burst at a tension of .35 lb. per linear inch, corresponding under the circumstances supposed to exist in actual labour to a forward pressure of nearly 5 lbs., thus exhibiting an amount of strength quite unexpected.

As a general statement, it may be said that the chorion behaves like the decidua. It is of more uniform strength than the decidual membrane, and is only a little stronger, the average tensile strength

¹ For some remarks on this subject see Hohl. *Lehrbuch der Geburtshülfe*. 1855. S. 751.

being .62 lbs. per linear inch, corresponding to a propelling power in labour of nearly 9 lbs. [In taking

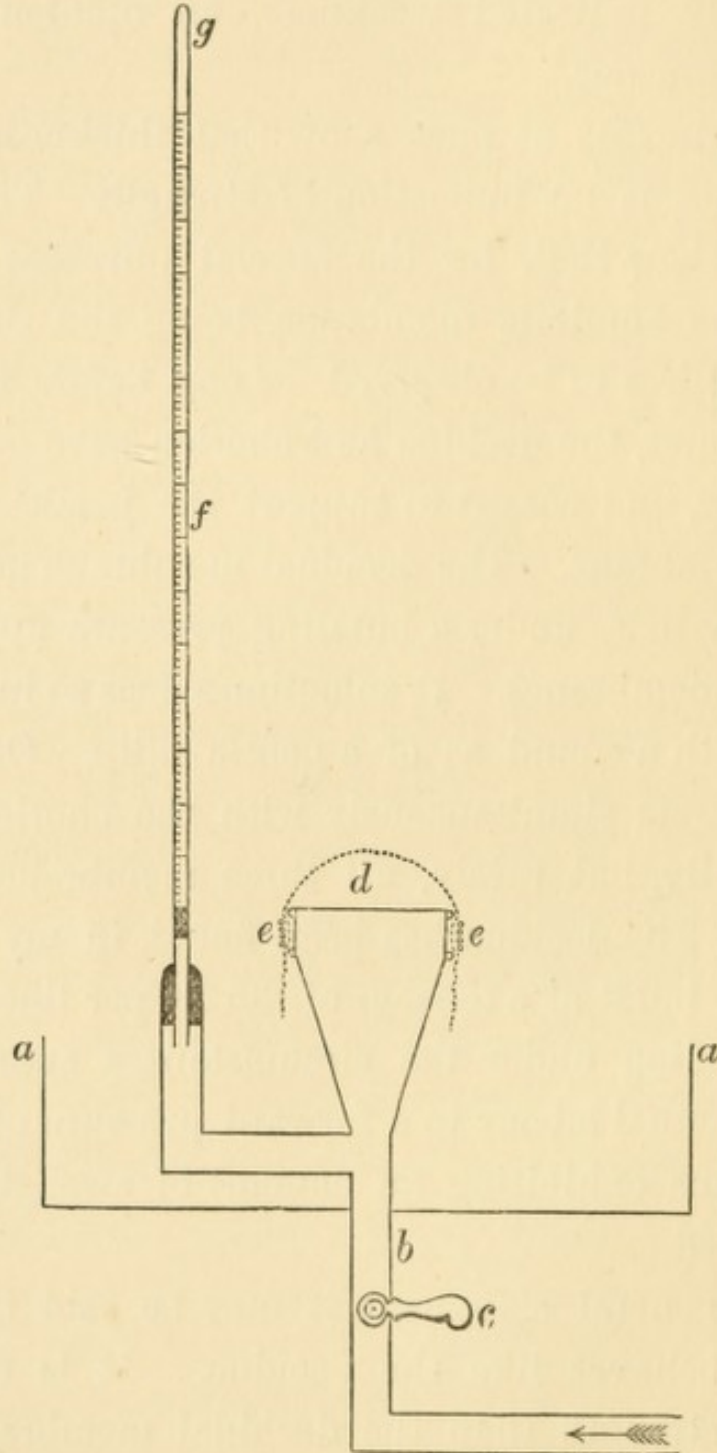


Fig. 1.

these averages, experiment 25 is omitted, because its exceptional value indicates almost certainly a mistake.]

The strength of the foetal membranes lies in the innermost sac, in the amniotic membrane, which appears the thinnest and most delicate of all. To try the strength of it, as well as of the others, I made numerous experiments in the manner to be described.—They were all performed in the laboratory, and with the apparatus of Professor Tait, to whose knowledge and skill I am indebted for their value and accuracy. The apparatus used was connected with a pipe *b* in the bottom of an open cistern *aa* (Fig. 1). Into this pipe water,¹ under high pressure, of which there was a convenient supply, could be admitted gradually by a cock *c*. The apparatus expanded upwards from the pipe to its mouth *d*. In one apparatus used this mouth had an external diameter of 3·35 inches, in the other it had an external diameter of only 2·25 inches. Over the mouth of the apparatus the membranes experimented on were placed, and tied on by a waxed hempen cord, around a broad rim *ee*, immediately beneath the mouth. That the apparatus acted in a fair and satisfactory manner was evident, from the observation that, in almost all the trials, the membrane tested did not burst where it touched the instrument, but in an arc of a circle crossing over the bulged-out membrane; or rarely, in a starlike manner. Connected, by a hollow arm, with the apparatus was a

¹ Water is preferable in these experiments to air, because, when it is employed, there is less violent action at the bursting of the membrane.

vertical glass tube *g*, with scale *f* of inches and tenths of inches. This tube contained a long column of air, confined in it by a short column of mercury. The rise of the column of mercury, compressing the air in the tube, indicated the degree of pressure applied to the internal surface of the membrane fixed over the mouth *d* of the conical vessel. Besides my own supply, I was kindly provided with fresh membranes by Dr. Linton and Mr. Vacher of the Royal Maternity Hospital.

The following table gives, in a categorical form, a narration of each of 100 experiments, as well as the chief calculations founded upon the data obtained from them. The first column gives the number of the trial. The second column gives the number of the set of membranes tested; and it will be seen that, generally, several experiments were made with the same membranes. The third gives the length of continuance of labour till the time when the membranes were ruptured. The fourth column gives the duration of the first stage of labour. The fifth gives the duration of the second stage of labour. The sixth column contains the state of the os uteri at the time of the rupture of the membranes. The seventh states the stage of labour in which the bag of waters was broken. The eighth, ninth, and tenth columns show how many of the three membranes were tested simultaneously. The eleventh, twelfth, and thirteenth columns show what membranes gave way in each experiment. The four-

teenth column states the radius of the circular mouth of the apparatus to which the membranes were tied. The fifteenth gives the barometric pressure at the time of each trial; and it will be observed that the pressure occasionally required a correction which demands explanation. The column of mercury in the apparatus was generally very short, and no correction for its weight was required, the experiments not pretending to an extreme nicety; but occasionally (in the cases noted in the column of remarks) the column of mercury was too long to be neglected, and a correction was made for its length. The sixteenth column gives the length of the column of air enclosed in the vertical tube above the mercury. The seventeenth gives the contraction of this column of air, by the pressure of water which burst the membranes, acting on the short column of mercury. The eighteenth column gives the height of the membrane as it bulged above the mouth of the apparatus, expanded by the water-pressure. The nineteenth gives the effective pressure of the water, at the moment of bursting of the membrane, in inches of mercury. The twentieth gives the diameter of the sphere, of which the membrane when bursting approximately formed a portion. The twenty-first column gives the pressure per square inch of the membrane at the time of the bursting of the membrane, or at the time of the experiment's failing from some cause, such as the slipping of the membrane. The twenty-second column gives the pressure on a

circular surface of 2.25 inches radius, or equal to the assumed dimensions of the lumen of the passage through which the child is expelled. The twenty-third column gives the tensile strength of the membrane, or, in other words, the weight which a band of it, an inch broad, would bear without giving way.

TABLE OF EXPERIMENTS,

WITH THE

DEDUCTIONS THEREFROM.

TABLE OF EXPERIMENTS, WITH

1.	2.	3.	4.	5.	6.	7.	8. 9. 10.			11. 12. 13.			14.	15.
							Membranes Tested.			Membranes Burst.				
							Amnion.	Chorion.	Decidua.	Amnion.	Chorion.	Decidua.		
		<i>h. m.</i>	<i>h. m.</i>	<i>h. m.</i>	<i>inch.</i>							<i>a</i>	<i>b</i>	
1	1						X	X	X	X	X	X	1.675	29.5
2	"						X	X	X	X	X		"	"
3	"						X	X		X	X		"	"
4	"						X			X			"	"
5	2						X	X	X			X	"	"
6	"						X	X					"	"
7	"						X	X		X	X		"	"
8	"						X						"	"
9	"						X	X	X			X	"	"
10	"						X	X					"	"
11	"						X	X		X	X		"	"
12	"						X						"	"
13	3						X	X	X	X	X	X	"	"
14	"						X	X	X			X	"	"
15	"						X	X					"	"
16	"						X						"	"
17	"						X						"	"
18	"						X						"	"
19	4						X	X	X	X	X	X	"	"
20	"						X	X	X			X	"	"
21	"						X	X			X		"	"
22	"						X						"	"
23	"							X			X		"	"
24	5	28 35	28 30	0 25		2d	X	X		X	X		"	"
25	"	"	"	"		"	X	X			X		"	"
26	"	"	"	"		"	X			X			"	"

THE DEDUCTIONS THEREFROM.

16.	17.	18.	19.	20.	21.	22.	23.	REMARKS.
Length of Column of Air.	Contraction of Column of Air.	Bulge of Membrane at time of Bursting.	Effective Pressure at Bursting in inches of Mercury.	Diameter of Sphere of Membrane at Bursting.	Pressure on Square Inch.	Pressure on a Circular Surface of 2.25 inches radius.	Tensile strength of Membrane.	
	λ	h	$\frac{b\lambda}{l-\lambda}$	$\frac{a^2}{h+h}$	p	$1.73 \frac{b\lambda}{l-\lambda} \left(h + \frac{a^2}{h} \right)$	$\frac{T}{h} \left(h + \frac{a^2}{h} \right)$	
19.75	2.		3.32		1.63			
"	2.		3.32		1.63			
"	1.		1.57		.77			
"	3.25	1.5	.58	3.37	2.85	33.87	2.41	
"	.5		.766		.37			Membrane taken close to placenta.
"	1.5		2.42		1.19			Membrane slipped out.
"	.75		1.16		.57			
"	1.25		1.99		.97			Membrane slipped out.
"	1.		1.57		.77			
"	1.5		2.42		1.19			Membrane slipped out.
"	1.25		1.99		.97			
"	2.		3.32		1.63			Membrane slipped out.
20.	1.25		1.96		.96			Membrane taken close to placenta.
"	.75		1.15		.56			
"	1.2		1.88		.92			
"	.6		.912		.45			
"	.4		.602		.29			
"	.6		.912		.45			
"	1.1		1.72		.84			
"	.5		.756		.37			
"	.5		.756		.37			
"	.8		1.23		.60			A considerable leak in the membrane.
"	.2		.298		1.46			
16.5	2.5	.75	4.98	4.49	2.46	37.58	2.75	Barometer corrected by 1.6 inch.
"	2.	.75	3.78	4.49	1.85	29.36	2.09 $\frac{1}{2}$	Barometer corrected by 1.6 inch.
"	2.25	1.	4.40	3.80	2.16	28.96	2.06	Barometer corrected by 1.6 inch.

1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.
27	5	28 35	28 30	0 25		2d	x	x		x	x		1.675	29.5
28	"	"	"	"		"	x			x			"	"
29	6	6 0	4 55	1 10		2d	x	x			x		"	"
30	"	"	"	"		"	x			x			"	"
31	"	"	"	"		"	x			x			"	"
32	7	6 0				2d		x	x		x	x	"	"
33	"	"				"		x	x		x	x	"	"
34	8	5 20	6 25	3 15	1½	1st		x	x		x	x	"	"
35	"	"	"	"	"	"	x			x			"	"
36	"	"	"	"	"	"	x			x			"	"
37	9	7 10	7 0	1 30		2d	x			x			"	"
38	10	6 15	6 5	0 20		2d	x	x	x	x	x	x	"	"
39	11	6 15	4 10	7 15		2d		x	x		x	x	"	29.6
40	"	"	"	"		"		x	x		x	x	"	"
41	"	"	"	"		"		x	x		x	x	"	"
42	12	21 0	24 0	A few minutes	2½	1st	x			x			"	29.45
43	"	"	"	"	"	"	x			x			"	"
44	"	"	"	"	"	"	x			x			"	"
45	13	13 0	12 45	0 45		2d		x	x			x	"	"
46	"	"	"	"		"		x			x		"	"
47	14	11 0	10 15	0 30?		2d	x			x			"	"
48	"	"	"	"		"	x						"	"
49	"	"	"	"		"	x			x			"	"
50	"	"	"	"		"	x			x			"	"
51	"	"	"	"		"	x						"	"
52	"	"	"	"		"	x						"	"
53	15			7 30		1st	x	x					"	28.8
54	"			"		"	x	x			x		"	"
55	"			"		"	x						"	"
56	"			"		"	x			x			"	"
57	16	6 30				2d	x			x			"	29.0
58	"	"				"	x			x			"	"
59	17	3 30	9 30	1 0	½	1st	x			x			"	"
60	"	"	"	"	"	"		x	x		x	x	"	"
61	18			"		"	x						"	"
62	"			"		"	x			x			"	"
63	"			"		"	x			x			"	"
64	19		6 0	1 0		2d	x			x			"	29.2
65	"		"	"		"	x			x			"	"
66	"		"	"		"	x			x			1.125	"
67	"		"	"		"	x			x			"	"

16.	17.	18.	19.	20.	21.	22.	23.	REMARKS.
16.5	2.25	.9	4.40	4.02	2.16	30.76	2.17	Barometer corrected by 1.6 inch. A leak in the membrane.
16.25	2.5	1.	5.07	3.80	2.48	33.37	2.37	Barometer corrected by 1.6 inch.
"	1.5	.75	2.83	4.49	1.39	15.06	1.07	Barometer corrected by 1.6 inch.
"	1.	.75	1.83	4.49	.90	14.21	1.01	Barometer corrected by 1.6 inch.
16.5	2.25	.75	4.40	4.49	2.16	31.18	2.43	Barometer corrected by 1.6 inch.
16.4	.6	.75	1.06	4.49	.52	8.23	.58	Barometer corrected by 1.6 inch. Birth 30 min. after rupture.
16.3	.5	1.	.882	3.80	.43	5.81	.41	Barometer corrected by 1.6 inch.
16.5	.4	.75	.693	4.49	.34	5.38	.38	Barometer corrected by 1.6 inch.
"	.9	1.	1.61	3.80	.79	10.60	.75	Barometer corrected by 1.6 inch.
"	.9	1.25	1.61	3.49	.79	9.73	.69	Barometer corrected by 1.6 inch.
16.2	.6	1.	1.07	3.80	.52	7.04	.50	Barometer corrected by 1.6 inch.
"	.3	.75	.526	4.49	.26	4.08	.29	Barometer corrected by 1.6 inch.
20.	.75	.75	1.15	4.49	.56	8.93	.63	
"	.75	.75	1.15	4.49	.56	8.93	.63	
"	.4	.25	.604	11.47	.30	11.99	.85	
20.5	1.8	1.	2.83	3.80	1.39	18.63	1.32	
"	.9	.75	1.35	4.49	.66	10.48	.74	
"	1.3	.8	1.99	4.31	.97	14.83	1.05	
"	.5	1	.736	3.80	.36	4.84	.35	
"	.45		.661		.32			
"	.7	1.	1.04	3.80	.56	6.85	.49	
"	.8	1.5	1.19	3.37	.58	6.94	.49	Membrane slipped out.
"	2.	2.	3.18	3.40	1.56	18.72	1.33	
20.75	1.5	1.5	3.29	3.37	1.12	13.35	.95	
20.5	1.5	1.5	2.32	3.37	1.14	13.52	.96	Membrane slipped out.
"	2.	2.	3.18	3.40	1.56	18.72	1.33	Membrane slipped out.
21.	1.5	1.	2.21	3.80	1.08	14.55	1.03	Membrane found to have been injured.
"	1.25	1.	1.82	3.80	.89	11.98	.85	
"	2.25	2.	3.45	3.40	1.69	20.31	1.44	
"	1.25	1.	1.82	3.80	.89	11.98	.85	
"	2.7	1.1	4.28	3.65	2.10	27.03	1.92	Child born 30 min. after rupture.
"	2.5	1.	3.92	3.80	1.92	25.80	1.83	
21.2	.5	.5	.700	6.11	.34	7.40	.53	
"	.3	.5	.416	6.11	2.04	4.40	.31	
20.4	1.1	.8	1.65	4.31	.81	12.29	.87	Membrane slipped out.
"	1.25	1.	1.89	3.80	.93	12.44	.88	
20.5	2.3	1.5	3.66	3.37	1.79	21.34	1.52	
19.4	1.5	1.	2.45	3.80	1.20	16.13	1.15	
"	1.6	1.5	2.62	3.37	1.28	15.27	1.09	
22.3	2.	.75	2.87	2.44	1.41	12.10	.86	
"	1.7	.5	2.41	3.03	1.18	12.64	.90	An ill-conducted experiment.

1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.
68	19	3 30	6 0	1 0		2d	x						1.125	29.2
69	"		"	"		"	x			x			"	"
70	"		"	"		"	x			x			"	"
71	"		"	"		"	x			x			"	"
72	"		"	"		"		x	x		x	x	"	"
73	20	9 15	8 20	0 55		3d		x	x		x	x	"	"
74	21		9 0	3 0		"	x			x			"	"
75	"		"	"		"	x			x			"	"
76	22	1 30	2 0	1 0		2d	x			x			"	29.8
77	"	"	"	"		"	x			x			"	"
78	"	"	"	"		"	x			x			"	"
79	"	"	"	"		"	x			x			"	"
80	23		Long	1 0		"	x			x			"	"
81	"		"	"		"	x			x			"	"
82	"		"	"		"	x			x			"	"
83	"		"	"		"	x			x			1.675	"
84	24	32 30	30 0	4 0		2d	x			x			1.125	"
85	"	"	"	"		"	x			x			"	"
86	"	"	"	"		"	x			x			"	"
87	"	"	"	"		"	x			x			"	"
88	"	"	"	"		"	x			x			1.675	"
89	25					"	x	x		x	x		1.125	29.84
90	"					"	x	x		x			"	"
91	"					"		x			x		"	"
92	"					"	x			x			"	"
93	"					"	x			x			1.67	"
94	"					"	x			x			"	"
95	"					"	x			x			"	"
96	"					"	x						"	"
97	"					"	x			x			"	"
98	26	10 10	8 30	2 0		2d	x			x			1.125	"
99	"	"	"	"		"	x			x			"	"
100	"	"	"	"		"	x			x			"	"

16.	17.	18.	19.	20.	21.	22.	23.	REMARKS.
22·3	2·3	1·1	3·36	2·25	1·65	13·08	·93	
"	1·9	·75	2·72	2·44	1·33	15·68	1·11	
"	2·5	·75	3·69	2·44	1·81	15·56	1·11	Burst by air contained between the membrane and water.
"	1·7	·55	2·41	2·85	1·18	11·88	·84	Burst by air contained between the membrane and water.
"	·9	·5	1·23	3·03	·60	6·45	·46	
"	1·2	·6	1·66	2·71	·81	7·78	·51	Membranes not ruptured till after birth of head.
22·2	1·2	·6	1·67	2·71	·82	7·83	·56	Membranes ruptured with first pain of labour.
"	1·7	·8	2·42	2·38	1·19	9·97	·71	
22·	2·	·5	2·98	3·03	1·46	15·62	1·11	A large leak in the membrane.
"	2·4	·65	3·65	2·60	1·79	16·40	1·17	A small leak in the membrane.
"	3·2	·75	5·07	2·44	2·48	21·37	1·52	
"	2·85	·6	4·43	2·71	2·17	20·76	1·48	
"	2·3	·7	3·48	2·51	1·70	15·10	1·07	
20·	1·8	·6	2·91	2·71	1·43	13·64	·97	
"	2·1	·5	3·49	3·03	1·71	18·30	1·31	A small leak in the membrane.
19·3	1·5	1·05	2·51	3·72	1·23	16·16	1·15	
19·8	1·	·6	1·58	2·71	·77	7·40	·53	Membranes ruptured in labour by attendant.
19·5	1·4	·7	2·30	2·51	1·13	9·98	·71	Membranes ruptured in labour by attendant.
"	1·7	·75	2·84	2·44	1·39	11·97	·85	Membranes ruptured in labour by attendant.
"	·9	·55	1·44	2·85	·71	7·10	·50	Membranes ruptured in labour by attendant.
19·3	1·4	·85	2·33	4·15	1·14	16·73	1·19	Membranes ruptured in labour by attendant.
22·	3·4	·65	5·36	2·60	2·63	24·08	1·71	Barometer corrected by ·5 inch.
"	3·6	·8	5·74	2·38	2·81	23·65	1·68	Barometer corrected by ·5 inch.
"	1·8	·9	2·61	2·31	1·28	10·41	·74	Barometer corrected by ·5 inch.
"	3·9	·75	6·32	2·44	3·10	26·64	1·89	Barometer corrected by ·5 inch.
19·2	1·25	·8	2·08	4·31	1·02	15·50	1·10	A leak in the membrane.
19·	1·3	·8	2·19	4·31	1·07	16·32	1·16	
"	1·45	·85	2·46	4·15	1·20	17·66	1·26	
"	2·	1·5	3·51	3·37	1·72	20·46	1·45	Membrane slipped out.
"	1·7	1·	2·93	3·80	1·44	19·29	1·37	Same piece as used in last experiment.
22·	1·5	·6	2·15	2·71	1·05	10·07	·72	Barometer corrected by ·5 inch.
"	1·6	·7	2·30	2·51	1·13	9·98	·71	Barometer corrected by ·5 inch.
"	1·8	·6	2·61	2·71	1·28	12·23	·87	Barometer corrected by ·5 inch.

Professor Tait has supplied the following formulæ, from which the columns of the tables are computed :—

Let b be the height of the barometer, corrected for the short column of mercury in the gauge ;

l the length of the air-column before pressure is applied ;

λ the contraction of the column when the membrane bursts.

Then, since the weight of a cubic inch of mercury, at ordinary temperature, is about 0.49 lbs., we have, for the difference of pressures on opposite sides of the membrane when it bursts, the expression

$$p = 0.49 b \left(\frac{l}{l - \lambda} - 1 \right) = 0.49 \frac{b\lambda}{l - \lambda} \quad (1).$$

in pounds per square inch. No sensible correction is required for the length of the water-column, when the mercury in the gauge and the membrane were not exactly at the same level.

If T be the force in pounds weight which will just snap a band of the membrane an inch broad, ρ the radius of curvature when the membrane bursts, we have, by a known theorem, the membrane being supposed to form approximately a portion of a sphere,

$$\frac{2T}{\rho} = p \quad (2).$$

To find ρ , we remark that the external semi-

diameter of the apparatus a is the radius of the base of a spherical segment, whose height h is measured ; and geometry gives at once the equation

$$2\varrho = h + \frac{a^2}{h} \quad . \quad . \quad . \quad (3).$$

Hence, the tensile strength of the membrane is

$$T = 0.123 \frac{b\lambda}{l-\lambda} \left(h + \frac{a^2}{h} \right) \quad . \quad . \quad (4).$$

If we assume that the membrane is usually burst, by natural processes, when a portion of it forms a hemisphere of 2.25 inches radius, the requisite pressure in pounds per square inch will be, by (2) and (4)

$$\frac{0.245}{2.25} \frac{b\lambda}{l-\lambda} \left(h + \frac{a^2}{h} \right) \quad . \quad . \quad (5).$$

and the effective pressure on a circular surface of 2.25 inches radius will then be

$$\pi (2.25)^2 \frac{0.245}{2.25} \frac{b\lambda}{l-\lambda} \left(h + \frac{a^2}{h} \right) = 1.73 \frac{b\lambda}{l-\lambda} \left(h + \frac{a^2}{h} \right) \quad (6).$$

In making such experiments, a small given error in the estimate of the depth of the approximately spherical segment will be of least consequence when the membrane bursts in a nearly hemispherical form, for by (3)

$$2\delta\varrho = \delta h \left(1 - \frac{a^2}{h^2} \right)$$

and the error in the estimated radius vanishes, if $h = a$. Hence, also, the assumption that, in nature, the rupture takes place when the protruded portion of the membrane is hemispherical, gives a *minimum* value of the whole extruding force.

For the purposes of this paper the greatest value of the Table lies in the twenty-second column, which gives the power of the labour at the time of the rupture of the membranes and evacuation of the liquor amnii, on the supposition that the lumen of the passage opened up was circular, and of $4\frac{1}{2}$ inches in diameter, and that the bulge was hemispherical at bursting. The first striking observation to be made, is the great variation in the strength of the bag of membranes. The force required to rupture the weakest amnion showed that the power of the labour was at least 4.08 lbs. ; that for the strongest, a power of 37.58 lbs. ; and the average power indicated by the experiments on the amnion was 16.73 lbs. The average tensile strength was 1.19 lbs. Next, it is to be remarked, that in the cases whose membranes were tried, the power of labour almost certainly exceeded the power required to burst the bag, for it is not probable that a particularly weak small portion, unlike the rest of the membrane, was ruptured in the labours.

In cases 5, 6, 10, 14, 16, 26, the labour did not last above half-an-hour after the rupture of the membranes ; and the greatest power indicated experiment-

ally by rupturing the membranes was in each case respectively 37·58 lbs., 31·18, 4·08, 18·72, 27·03, 12·23.

In case 22 it was particularly observed that, so far as I could judge, the pain rupturing the bag was stronger than any that followed; it may therefore be supposed that the power terminating labour little exceeded 21·37 lbs., the greatest power indicated by the experiments as rupturing the membranes of that patient's ovum.

The same contractile force of the uterus at different periods of labour, or, to be more exact, at different dimensions of the uterus, will produce greater internal pressure, and consequently greater expulsive force, as the uterus is smaller (*vide* equation (2), p. 78); and, the amount of muscular contraction being supposed to be the same, there may be no sign to the attendant or patient of the increase of power. This may be restated in a manner more pertinent to the actual facts. It is a common belief that uterine pains increase in strength after the evacuation of the liquor amnii. Whether this be true or not, as commonly believed, I do not here consider. But it is certain that, if the uterine contractions remain of the same force after, as they had before, the partial evacuation of the liquor amnii, the power of the labour, or the extruding force, will be increased, as curvature of the contracting organ is increased. On the other hand, the application of the same principle

shows that when the curvature of the extruded portion of the membranes is greatest, the difficulty of rupturing them is also greatest. This occurs when the extruded portion is hemispherical; and it is on this supposition that the numbers in column 22 are calculated.

It was only after conceiving the means above described for arriving at the conclusions of this paper, and after the plan of the apparatus had been made by Professor Tait, that I fell in with an interesting and valuable paper by Dr. J. Poppel of Munich—"Ueber die Resistenz der Eihäute, ein Beitrag zur Mechanik der Geburt," contained in the first part of the twenty-second volume of the *Monatsschrift für Geburtskunde* for 1863. This paper anticipates, to a very great degree, the plans and results here related. But it may be pointed out that Dr. Poppel has neglected to note some conditions of the experiment, which cannot be omitted without damaging materially the accuracy and value of the trials; especially he has always supposed the membrane to burst, in the experiments, when in a hemispherical form, which is certainly an error, and one whose tendency is always to make the strength of the membrane too little [*vide* equation (2)]. He has attached some weight to the part of the amnion tested, considering that greater strength would accompany proximity to the placenta;¹ but my experiments did not confirm this opinion.

¹ This opinion seems to be also held by Hohl, *loc. cit.*

Dr. Poppel's apparatus may be sufficiently though not fully described as follows :—The membrane to be tried he ingeniously fixed over one or other of two glass vessels, of the diameter of five centimetres or two inches, and of ten centimetres or four inches, respectively. The glass vessels were reagent glasses, from which the bottoms were taken off. The affixed membranes represented the bottoms of the reagent glasses. Into the corks of the glasses a long glass tube was passed. Through this tube mercury was poured into the bottle till it filled it and mounted into the tube. Its height in the tube at the time of the bursting of the membranes was carefully noted, because from it was estimated the pressure that burst the membrane. In adding the mercury fitfully, Dr. Poppel erroneously supposed that he imitated the pains of labour, a point, it appears to me, of no importance ; and besides, his idea was manifestly erroneous, for each succeeding pain is not an addition to a force previously in action—it may even be weaker than its predecessor. In every natural case it is an entirely new force, rising in strength from zero, or from the level of the constant intrauterine pressure to its acme, and again gradually fading to zero. Dr. Poppel made allowance for the weight of mercury contained in the reagent glass, over and above what was in the vertical glass tube ; but he neglected the important element of the degree of bulging of the membrane, or radius of its curvature at time of

bursting, with a view to arriving at the diameter of the globe, of which it formed a section at the time of rupture. With this he connects also a statement, that the bulging of the membranes through the mouth of the womb rarely exceeds a hemispherical form, which, though perhaps nearly true, is misleading, if held to be true in regard to the class of cases of persistent membranes specially studied in this paper.

The average strength of the amnion found by Poppel was, keeping an aperture of 2.25 inches in radius in view, 19.21 lbs.; in my experiments it was 16.73 lbs.

Poppel experimented on the membranes in seven cases in which they burst "with the birth." The following table gives the strength of the membranes in these cases, according to Poppel's method of calculating, and the same changed into lbs., as well as increased proportionally from what appertains to a radius of 5 centimetres to what appertains to a radius of $2\frac{1}{4}$ inches, the dimensions used in our experiments :—

No.	Belastung bei 10 Centimeter Durchmesser Kilogramm.	Pressure for Diameter of 2.25 inches in lbs.
	Kilogrammes.	lbs.
1	9.876	27.232
3	2.346	6.469
12	2.134	5.884
13	7.608	20.979
22	4.709	12.985
23	9.461	26.088
28	7.001	19.305

This table gives us, in seven cases, a figure of strength nearly equalling the whole power of labour in these cases. If, in any of the cases, the membranes had persisted after the birth, then the figure in the last column would have certainly exceeded the whole propelling power of labour at any moment during the whole of the labour. Speaking of them, Poppel remarks, that "if we reflect that the table expresses only the minimum of power for the easiest labours, the figures appear to be quite trustworthy, even though they exhibit great variations. It may therefore be assumed that in a very easy labour a power, varying from 4 to 19 lbs., presses the head through the pelvis." As Dr. Poppel gives the passage transmitting the head a diameter of 4 inches, and as I prefer regarding it as nearer $4\frac{1}{2}$, so I, using meantime Poppel's experiments and calculations, make the power exerted in an easy labour vary from about 6 lbs. to about 27 lbs., instead of from 4 to 19. I shall not meantime attempt to show whether Poppel's assumed 4-inch diameter or my assumed $4\frac{1}{2}$ -inch diameter is the more likely to be nearest the truth, because it would lead me into a class of questions remote from the subject-matter of this paper.

If we observe that in Poppel's table of experiments and in mine the power shown to be sufficient to terminate an easy labour was often far exceeded in the course of other labours, we may enunciate the almost certain conclusion that a great mass of easy,

and not merely of the easiest, labours is terminated by a power little in excess of that required to rupture the bag of membranes. The strongest membrane found in the experiments indicated, by the pressure required to burst it, an extruding force of $37\frac{1}{2}$ lbs. We may therefore, I think, safely venture to assert, as a highly probable conclusion, that the great majority of labours are completed by a propelling force not exceeding 40 lbs.

If we regard the figure of 4 lbs. given by Poppel as equal to the power exerted in the easiest labour he has observed, or the corresponding figure of 6 lbs. according to my calculations, and keep in mind that the average weight of the adult fœtus exceeds either of these weights, we are led to the conclusion that in the easiest labours almost no resistance is encountered by the child; that it glides into the world propelled by the smallest force capable of doing so; that, with the mother in a favourable position, the weight of the child is enough to bring it into the world—a result which many clinical facts at least appear to confirm.

Having thus given Poppel's and my own estimate of the force exerted in natural parturition of the easiest kind, I can at present offer nothing positive from which to calculate the strength of labour in the general run of cases. My belief is, that in ordinary labours the power exerted is not in general much above the lower limit; but other accoucheurs may see reason to entertain different opinions.

CHAPTER V.

THE GREATEST POWER OF LABOUR EXERTED IN DIFFICULT CASES.

THE higher limit of the power exerted in natural parturition has been variously estimated. There is an easy and obvious method of arriving at it. Cases are frequently occurring in which labour is artificially terminated by forceps, in circumstances which leave no doubt that, under delay, they would have come with difficulty to a spontaneous conclusion. The power exerted by the forceps in such cases can be measured. Such measurements are not to be at once taken as the power of labour necessary to finish such cases ; but when all of the various sources of error are considered and included, they are of much value. The chief of such sources of error are the neglect of the assistance that may be afforded to the operator by the natural expulsive efforts, and the including of such forces, exerted by the forceps, as may be unnecessary for carrying on the process ; for example, prematurely applied force, or force applied so as to advance the birth too hastily, or force lost by being used in a wrong direction. For the making of observations of this kind by the forceps, special instru-

ments have been invented by Kristeller, Joulin, and others.

But forceps cases do not afford the only evidence available as to the higher limit. Experiments can be made on the dead subject which can be very well relied upon, as reproducing correctly the difficulty encountered in the living, and the power required to overcome it. Such experiments have been made by Joulin,¹ and when suitably arranged, give us the power exerted in cases which the most powerful parturient efforts might bring to a spontaneous termination; and, it may be added, would involve the mother's life in great risk.

Speaking of these experiments, Joulin makes the following remarks:²—"Spontaneous delivery has been sometimes observed in circumstances almost identical. It appears to me, therefore, possible to admit that the figure of 50 kilogrammes (about a hundredweight) of force represents very nearly the maximum of the contractile power of the uterus; for it is necessary to take into account the accessory contingent furnished by the abdominal muscles, which in these instances was wanting. But as this force has not a direct action, it is probable that its actual product scarcely rises above a few kilogrammes." On

¹ *Mémoires de l'Académie Impériale de Médecine*. Tome xxvii. p. 90, etc. See also his *Mémoire sur l'Emploi de la Force en Obstétrique*. *Archives générales de Médecine*: numéros Février et Mars 1867. See also chap. ix. of this work.

² *Traité Complet des Accouchements*, p. 447.

this quotation from Joulin I shall make two remarks : first, that in labour the accessory contingent furnished by the abdominal muscles appears to me to have an action nearly, if not quite, as direct as that of the uterus itself ; and second, that I know of no means of satisfactorily estimating its value.¹ In my experiments and descriptions I treat of the powers of labour, that is, of all the powers exerted in combination. Yet there can be no doubt that the paramount power is that produced by uterine contractions.

Having had extensive and varied experience in the use of the forceps in difficult labours, and having also made some rough experiments with the dynamometer, to ascertain the power I have applied by the instrument, I regard M. Joulin's estimate of a hundredweight as the maximum force of the parturient function as too high. I do not deny that, in very rare cases, such a force may possibly be produced ; but I am sure that it is nearer the truth to estimate the maximum expulsive power of labour (including with the uterine contractions the assistant expulsive efforts) as not exceeding 80 lbs.

At present I can divine no method of arriving at an estimate of the expulsive power of labours generally except the following ; and I must guard

¹ The method adopted by Haughton (see his work on the *Principles of Animal Mechanics*) cannot be regarded as solving the difficulty, and has not gained the confidence of physiologists.

myself from being supposed to recommend its use, in the meantime at least. A fine tube, filled with water and of resisting material, may be introduced into the small pool of liquor amnii which remains after the rupture of the membranes, filling up the spaces otherwise vacant on the anterior aspect of the foetus. This tube should be provided with an aperture at its uterine end ; it should be curved, so that when introduced it may lie easily in the pelvis, occupying the least possible space, so that no unnecessary resistance be offered to the advance of the foetus ; its wall should taper to either side, a cross section of it having a long pointed fusiform outline, in order that its presence may not produce on either side of it a channel for the running off of the pool of liquor amnii ; lastly, its external end should be in communication with a column of mercury in a vertical tube, enclosing a column of air under only ordinary barometrical pressure. During the pains the rise of the mercury in the tube may be measured, and calculations from these measurements might be made, identical with those already given in the former chapter of this part. By this means, if successfully applied, the force of any labour may be exactly known. And it is scarcely necessary even to suggest how immeasurably valuable to the accoucheur such an estimate would be, substituting, as it would, an experimentally accurate statement of great importance for the vague notions at present relied on, even when

the wisest and most experienced practitioner lends his counsel.¹

There can be no doubt as to the great practical importance of the inquiry entered upon in these chapters. Although it is, as yet, far from completed, there is enough demonstrated to enable Dr. Slop, if he have an opportunity, to cast ridicule on the father of Tristram Shandy, who, founding on the statements of Lithopædus Senonensis, asserts that the force of a woman's efforts is, in strong labour pains, equal, upon an average, to the weight of 470 lbs. avoirdupois, acting perpendicularly upon the head of the child!!

It would be interesting to know the source,² if any, whence the Rev. Laurence Sterne drew his information as to the power of labour in days when little attention was paid to this subject. But it is more important to look forward and anticipate the advantages which a handy available dynamometer of parturition would afford to the practitioner. They spring up to the thoughtful mind so readily as scarcely to call for statement—when complaints are severe, or cries for relief urgent, to have a means of judging what is really the power of the labour; when a case

¹ Kehrer describes a similar plan (*Beiträge zur vergleich. und experim. Geburtskunde*, II. Heft., S. 119), and Schatz has actually made a tokodynamometer and used it. (*Archiv f. Gynæk.*, 1872.)

² The source of the names he uses is well known, for the work of Trincavellius and the letter of Albosius regarding the Lithopædion Senonense are found in Caspar Bauhin's second volume of the *Gynæciorum*.

is protracted, to have a means of estimating the pressure exerted on the child and maternal passages ; when, in any case, instrumental interference is under consideration, to have a means of judging if the error is in the force exerted or in the resistance, etc. etc. That some means of thus increasing the beneficent power of the accoucheur will be discovered, I confidently anticipate.

CHAPTER VI.

ON THE POWER OF THE UTERUS TO RESIST A BURSTING PRESSURE.

RUPTURE of the uterus, the accident in nature which the following experiments most nearly imitate, is so interesting and important, and as yet so imperfectly studied, that anything tending to throw light upon it is valuable.¹

Before recording the experiments, it is necessary to state that they were conducted with nearly the same machinery, and on the same principles, as those already described as made upon the bag of membranes. As the power used was found to be great, the apparatus was sometimes immersed in water, to avoid the disagreeable little accidents that might have arisen to the bystanders from the bursting in air.²

¹ See some remarks in a succeeding chapter (xviii.), "On the changes undergone by the cervix uteri during labour."

² The remarks of Schatz are at least uncalled for. The error he points out I never made. Immersion was not used in the experiments hereafter recorded. Besides, the error introduced might easily be made inappreciable by using very small depth of immersion. Schatz's (*Archiv f. Gynæk.*, 1872. S. 64) further remarks on my etiology of the position of the fœtus are not only quite out of place, but quite erroneous, as I could easily show, were this the place to do so.

In the experiments made with air, it was curious to observe the permeability of all the unruptured tissues to this fluid. This, of course, did not vitiate the experiments, but it was unexpected to observe its effervescing, as it were, through the peritoneum covering the part experimented on.

The great, the enormous, strength of the uterus also attracts particular attention. Most of the experiments, in one sense—indeed, all—failed from deficiency of power in the apparatus used. And, though Professor Tait is now provided with an apparatus of power enough, I have unfortunately been unable to procure material for further experiments. The uterus used in our trials was kindly sent me by Dr. Miller of Dundee. It was entire, and contained an entire fully-developed ovum. When I got it, and during most of the experiments, it was fresh, but in some of them it was found that parts had begun to decompose. Its structure was microscopically examined by Professor Turner, who found it to be healthy, some of the fibres only of the muscular tissue having undergone fatty degeneration, and that only very partially, even in the individual fibres showing fat granules distinctly.

Although the experiments are very defective, they yet afford some important results. One of them is the immense superiority of the power of the uterus to resist a bursting pressure, to that believed to be exerted by it in its moments of greatest effort. In other words, the experiments show that Nature has

provided in the healthy uterus an apparatus possessed of far greater capabilities, in a mechanical point of view, than are ever called into exercise. Were the uterus destined to do far harder work than it ever attempts, it needs no increase of strength of materials. From this it follows, as a necessary conclusion, that it never is spontaneously ruptured when it is healthy. For the explanation of the occurrence of spontaneous rupture, it is necessary to invoke the softening and weakening influence of inflammation, or some thinning or other morbid change.

I am indebted to Dr. G. W. Balfour for pointing out to me the opinion of Casper in regard to the increase of strength in dead structures, and that this condition of dead tissues may vitiate my experiments. I shall give this opinion of Casper in the words of Dr. Balfour's translation:—"In § 33, and its illustrative cases, I have shown how often, in cases of sudden death from injuries—when death has occurred from internal and not from external causes—there is often no trace of injury visible externally which could betray the nature of the case. This circumstance, as well as the desire to investigate how far it was possible for a criminal to mask the actual cause of death, and obscure the case by producing injuries on the body of the deceased, just as murderers often attempt to conceal their deed by burning the body; as well as to discover what relation the resistance of the dead organ bore to that of the living one, led to the experi-

mental production of injuries upon dead bodies. I have had uncommonly numerous opportunities of instituting these experiments, and I still continue to repeat them every academical session. Similar experiments have not previously been made anywhere on so great a scale—except in the case of experiments on burning, to which I shall refer under the head of ‘Death from Burning’—and they have been attended by the most astonishing results. It is extremely difficult to break up the organic cohesion of dead organs. In saying this, of course I do not refer to stabs or cuts through the skin and muscles. Our experiments, in respect to mechanical injuries, were confined to fractures of the bones, ruptures of the internal organs, and injuries (alterations) of the cuticular surface.” After giving his experiments on fractures of the bones, he continues: “We have only made a few experiments upon the rupture of internal organs, because no important practical result was to be expected from them. The most violent blows, with beams and the like, upon the regions of the liver and spleen have not been attended with the slightest result.”¹

It is to be remarked, that even admitting to the full the force of Casper’s numerous experiments, we are left with only Casper’s individual opinion. For we have no statement, nor any good grounds for forming a reliable opinion, as to the force required to

¹ *A Handbook of the Practice of Forensic Medicine.* Sydenham Society’s Translation, vol. i. p. 45.

produce injuries on the living. And I must here take the liberty of simply stating that Casper has not convinced me of the truth of his views. I hold, indeed, in the meantime, that it is far from being proved. That dead tissues should have more strength than living ones is at least highly improbable after all that Casper has said. I have indeed supposed that to some tissues vitality might impart force.¹ I do not know if any experiments have been devised to show the comparative strength of muscles in action and muscles at rest, but I do regard it as a subject worthy of investigation. It seems possible that the contraction of a muscle might increase its cohesion, might augment for the time its tensile strength.

The last point in connection with the following experiments to which I shall call attention is the rupture, crack-like with irregular edges, of the peritoneum. The records of the details of the experiments show that this membrane repeatedly gave way, while the muscular part of the wall remained entire. This shows that it possesses a less degree of extensibility than the muscular layer, and may perhaps throw some light on those interesting and not extremely rare cases of crack-like fissuring or rupture of the peritoneum observed in some autopsies after

¹ The tensile strength of a muscle may be increased by contraction, bringing all its fibres into play to offer a united resistance to the disrupting force, as Sir W. Thomson has suggested.

parturition.¹ It forms also a curious contrast, pressure and rapid distension easily rupturing a structure which is capable of growing or distending slowly to any extent, as is observed in pregnancy and ovarian dropsy.

¹ See Hennig. *Der Katarrh der inneren weiblichen Geschlechtstheile*, 1862, S. 10.

TABLE OF EXPERIMENTS,

WITH THE

DEDUCTIONS THEREFROM.

TABLE OF EXPERIMENTS, WITH

No. of Experiment.	Radius of Apparatus.	Barometric Pressure.	Length of Column of Air.	Contraction of Column of Air.	Greatest Bulge of Uterine Wall.	Greatest Effective Pressure, in inches of Mercury.	Diameter of Sphere of Uterine Wall at greatest Distension.	Pressure on Square Inch.
	a	b	l	λ	h	$\frac{b\lambda}{l-\lambda}$	$\frac{a^2}{h+\frac{b\lambda}{l-\lambda}}$	$\frac{b\lambda}{.49(l-\lambda)}$
1	1.125	29.3	20.	7.	...	15.78	...	7.73
2	1.125	29.3	14.	8.	...	39.06	...	19.14
3	1.125	29.3	14.	6.5	...	25.39	...	12.44
4	1.125	29.3	12.	6.5	.75	34.63	2.44	16.97
5	1.675	29.3	20.5	12.5	.75	45.78	4.49	22.43
6	1.85	29.3	10.75	5.75	1.25	33.69	3.91	16.51
7	1.85	29.3	9.25	5.	8.	34.47	4.95	16.89
8	1.85	29.3	8.25	5.25	1.	51.27	4.32	25.12
9	1.25	29.3	8.	4.5	.75	37.67	2.83	18.46
10	1.25	29.3	8.	4.	.75	29.30	2.83	14.36

THE DEDUCTIONS THEREFROM.

Tensile Strength of Uterine Wall.	
$\cdot 123 \frac{b\lambda}{l-\lambda} \left(h + \frac{a^2}{h} \right)$	REMARKS.
...	Air, forced by a pump, was used in this experiment and in the two following. In all three it came freely through the uterus by innumerable apertures. The part tested was taken from the upper part of the posterior wall of the uterus. The placenta was attached to the anterior wall. The part experimented on was not burst. It slipped off the apparatus.
...	The same part used as in last experiment. Like the last, the experiment was imperfect. The uterus was not burst. The peritoneum gave way, forming a long fissure.
...	The same part used as in last experiment. The india-rubber pipe of the apparatus gave way.
10·39	Water-pressure was used in this and the succeeding experiments. The part experimented on slipped.
25·28	The part experimented on slipped. It was taken from anterior wall, low down.
16·20	In this experiment only the internal half of the texture slipped. The uterus was thus not burst through its whole thickness.
20·99	The part is from anterior wall, not placental. The part burst was thin and softened by decomposition.
27·24	The part used was placental. The peritoneum alone gave way. It presented numerous elongated, irregular, cracks.
13·11	The part used was placental. Peritoneum cracked and partly peeled off.
10·20	The part used was the same as last. Numerous unsuccessful efforts were subsequently made to burst this by using the pump, but they all failed. The india-rubber junction-pipe gave way.

CHAPTER VII.

ON THE EFFICIENT POWERS OF PARTURITION.¹

THERE can be no doubt that, among the numerous matters at present occupying the attention of obstetricians, none is more important than this. So evident is the truth of this statement that one cannot but wonder why attempts to arrive at the truth have been, so far as we know, delayed till the present day. It is long since excellent researches of an analogous kind in regard to the force of the circulation of the blood, the power of the ventricles of the heart, were published ; yet such researches do not seem naturally so attractive, nor do they give promise of so valuable practical results as those into the power of labour.

It is well known that the first, and I believe the best, results in this inquiry have been obtained by careful deduction from experiments on the tensile strength of the amniotic membrane. The researches referred to were made quite independently, and published soon after one another by Poppel, of Munich, and by Tait and myself conjointly. Studying this subject, I thought of some other modes of reaching

¹ Communicated to the Royal Society of Edinburgh, 6th February 1871.

conclusions, such as by observations on the caput succedaneum. Means might be taken to find the force required to raise this swelling in different degrees of thickness. Such an investigation would, no doubt, lead to similar valuable results ; but the plan has never been employed. Again, observations might be made to ascertain the force required to rupture the fourchette or the perineum, and thus a fact might be got which would be of service in this inquiry. It is well known to accoucheurs that these parts sometimes offer a successful resistance to all the powers of labour. This resistance, if its force be ascertained, is of course a measure of the power employed ; at least it would afford a valuable result as to the higher limit of the power. Like statements might be made regarding the laceration of the margin of the cervix uteri as a test of the power exerted at the completion of the first stage of labour. Many methods were available, but none were, till very recently, worked out.

Recently Dr. Braxton Hicks has suggested¹ another method of reaching the determination of the uterine power, namely, finding the strength of the vaginal and other attachments of the uterus, on which, of course, the uterus must pull. In connection with this it would be interesting to study cases of spontaneous delivery after these attachments were nearly entirely torn through.

¹ *British Medical Journal*, August 16, 1873, p. 186.

It is probable that many intelligent and thoughtful accoucheurs had some rough ideas as to the amount of power exerted in parturition. They could not fail, in attending on ordinary labours, to observe the amount of exertion by hand and arm required to keep back the head when too rapidly advancing over a delicate perineum. This power is, under certain conditions, a measure of the force of the labour ; but I am not aware that any one has hitherto made the simple dynamometrical experiments proper to decide the amount of force so exerted by the accoucheur.

The problem may be more exactly stated as follows :—If in an unobstructed and powerful labour, the accoucheur, by the directly opposing pressure of his hand on the foetal head, arrests its progress for one or several pains, he has in the pressure by his hand a force which, added to the small amount required to effect parturition, exceeds all the combined powers of labour in this case. He may then estimate, by dynamometrical experiment, what was the force he used or what force he is capable of applying in the way in which he actually applied it to arrest the progress of labour. This experiment may be varied in different ways, and I shall mention one. Let us suppose a case of rigid vulva, the perineal resistance being overcome, and the head retroceding during the interval between powerful bearing-down pains. Now, it is well known that in such a case a little manual pressure from above may be enough to push the head

down again upon the perineum or to resist its retrocession, or that the first and painless part of the next pain will make the head that has retroceded again bulge out the perineum before it is forced by the powerful acme of the contractions against the resisting vulva. If, then, the practitioner opposes the advance of the head, even so far as to bulge out the perineum, he must have a nearly exact measure of the force which the labour could bring to bear against the vulvar obstacle. The conditions given for this experiment are introduced in order to secure that friction may be reduced to a trivial amount.

In such experiments or practice, what force does the accoucheur exert? I have a hand well accustomed to such work, and I find, by actual trial with an accurate dynamometer, 50 lbs. to be about the highest power I can use, situated as I am at the bedside in attendance on a case. I have ample reason, then, in such experience, to believe that very few of the most powerful labours exert a force of 50 lbs.; that an ordinary strong labour is easily arrested by a smaller force than 50 lbs.; that the great majority of labours are accomplished by repeated impulses, whose highest power probably never exceeds 25 lbs. I may add that, in the great mass of short forceps deliveries, the force required from the accoucheur, even when he delivers the head unaided by the natural efforts, seldom reaches 50 lbs.¹ These statements are, to a

¹ On this subject see a subsequent chapter "On the tensile strength of the fresh adult foetus" (chap. ix.)

great extent, either arbitrary or dependent on my skill as an observer ; yet I feel very confident of their accuracy.

Again, the intelligent practitioner who has observed a case of difficult labour, finished either by the long forceps or by podalic extraction, could not but form some rough idea of the force he used and compare it with the force which the labour exerted in its nugatory struggles. The force which the accoucheur thus exerted would not be certainly the equivalent of what the labour must have put forth in order to produce a spontaneous termination. It would, no doubt, in most cases surpass the force which the mother must have exerted to produce the spontaneous birth. But it would be, nevertheless, a valuable measurement indicating a force which in such a case the labour failed to produce. Joulin and I have made dynamometrical experiments to make use of such measurements in estimating the highest power of labour.

Another method of advancing our knowledge of this subject has been followed by the Rev. Professor Haughton. This gentleman does not, as his predecessors, examine the effects produced by the powers of labour, and thus gets results having a very distinct positive value. He follows a plan which may be justifiable, yet which is difficult and dangerous. He takes an almost opposite method to that used by me. He measures the bulk and the extent of the involun-

tary and voluntary muscles employed in the function, and from these data he arrives at conclusions which he, in one particular, corroborates by a simple experiment. The results arrived at are statements of the powers of the parts, which are true if his methods are true. Even if his methods are correct, the results are not actual values but possible values or statements of what may be, not of what has been.¹

These results are very different from those of Poppel, Tait, and myself, and it is one of the objects of this paper to inquire into their value. In doing this I shall not discuss the method, but merely examine the results, by the aid of any obstetrical light which I can throw upon them.

Before proceeding to this inquiry it is to be remarked that Haughton arrives by his method at new results which the methods of previous observers did not afford the means of reaching. There are, as is universally known, two great forces employed in labour—the uterine contractions and the involuntary and voluntary bearing down. The former of these forces is peculiar to the parturient female ; the latter, as Haughton truly observes, is not peculiar to parturition, but is “available to expel feces, urine, or a foetus.” Haughton’s plan is, to examine the uterus, measure it, and through this arrive at a conclusion as to its power ; then to examine the muscles which

¹ See the criticism on his methods in the notices of his book given in the *Athenæum* and *Journal of Anatomy and Physiology*.

co-operate to produce bearing down, measure them, and through this arrive at a conclusion as to their power. The addition of the two results will, of course, give the power of labour. As I have already said, this is a dangerous and difficult plan to follow, and this is because there is room for error at every step.

The conclusions which Poppel and Tait and myself enunciated regarding the power of natural parturition stand on a completely different, and, it appears to me, far more secure, footing. There can, indeed, be scarcely any important objection raised regarding them. The strength of the foetal membranes is ascertained by experiment. Certain facts are well known regarding the rupture of the membranes generally, and regarding their rupture in the labours in which the membranes experimented on were produced. These two sets of data, when put together, lead, by a process of reasoning which it would be tedious here to recapitulate, to conclusions regarding the lower limit of the power of natural labour, and regarding the power of labour generally, which cannot, so far as I can see, be cavilled at. It is evident that this method tests only the whole or the combined powers of labour. It can afford no hint as to the comparative value of the two forces which combine to produce the power which is to be measured.

Of the results given in Professor Haughton's paper, those which appear to me to be both new and im-

portant, are three. I shall first state them, and then proceed to their examination one by one.

I.—The first conclusion is, that “the uterine muscles are capable of rupturing the membranes in every case, and possess, in general, nearly three times the amount of force requisite for this purpose.” . . . “It would be a waste of power (adds Haughton) to endow the uterus with more force than I have shown it to possess, for it is not necessary that the uterus should complete the second stage of labour, as the abdominal muscles are available for this purpose; so that by using them, and not giving the uterus more force than is absolutely necessary for the first stage of labour, an admirable economy of muscular power is effected. . . . “The extreme force of uterine contraction produces a pressure of 3·402 lbs. per square inch, which is equivalent to a pressure of 54·106 lbs. acting upon a circle of $4\frac{1}{2}$ inches in diameter, which is assumed as the average area of the pelvic canal.”

II.—The second of Professor Haughton’s new and important conclusions is that the action of the voluntary abdominal muscles “constitutes the chief part of the force employed in difficult labours.” . . . “The amount of available additional force given out by the abdominal muscles admits of calculation, and will be found much greater than the force produced by the involuntary contractions of the womb itself.”

III.—The third conclusion is, that, “on an emergency, somewhat more than a quarter of a ton

pressure can be brought to bear upon a refractory child that refuses to come into the world in the usual manner." . . . "Adding together the combined forces of the voluntary and involuntary muscles we find—

“ Involuntary muscles	,	. . .	54·10 lbs.
“ Voluntary muscles	,	. . .	523·65 lbs.
			—
“ Total	,	. . .	577·75 lbs. av.”

I.—The first of Professor Haughton's conclusions on which I comment is to this effect, that the unaided uterine muscle can exert in labour a force of 54 lbs. ; that this force is employed in dilating the cervix and rupturing the membranes, and that it can or does effect little more.

Now, it appears to me that Haughton limits far too much the use of the power of the uterus. I have no doubt that the uterine efforts not only dilate the cervix and rupture the membranes in most cases, but also do in most cases perform the chief part of the work required to bring forth the child. Although I do not coincide with Haughton in his reflections on the economy of muscular power, I shall not discuss the point therein raised. Yet I cannot avoid saying that, in the present instance, his own statements invalidate his reflections, for he asserts that the uterine muscle has three times the amount of muscular power required to do the work demanded of it. In endowing the uterus with this great power, Haughton,

in my opinion, furnishes conclusive evidence against his own view as to the use of the contractions of the uterus. For I am sure that the great mass of births, even in difficult labours, excluding only the most difficult, is effected and completed by a force less than what Haughton ascribes to the uterine muscle alone, unaided by the voluntary muscles. I am satisfied that the whole combined powers of labour seldom reach above 50 lbs., while Haughton gives the uterus alone a power of 54 lbs.

I do not say that Haughton is wrong in supposing that the uterus can exert a force of 54 lbs. On the contrary, I have no reason to doubt it. But I am sure that, while easy labours require for their whole work a force scarcely exceeding the weight of the child, only a very few difficult labours require for their whole work a force exceeding 50 lbs.

Every accoucheur knows to some degree of exactness the force which is required to restrain the forward movement of the child when there is no special resistance to its advance. This power I have measured approximately by dynamometrical experiments, and I find it to be at the most 50 lbs.—a power less than what is ascribed by Haughton to the unaided uterus. In other words, the uterus and voluntary muscles combined, stimulated to violent effort by insuperable temporary resistance, exert a force greater than is required to complete the labour; yet this force is generally much less than 50 lbs., and possibly never exceeds it.

It is well known to accoucheurs that the great resistance to the progress of the child in the second stage of labour is what is called in obstetrics the perineum. The power of this part I do not know, and guessing is a bad proceeding in a scientific paper. Yet I may venture to say that no perineum would long resist a force of 50 lbs., repeatedly applied—a force less than Haughton ascribes to the uterine muscle.

II.—Haughton's second conclusion is that the chief force in parturition is furnished by the voluntary muscles. The available power of these is (he says) 523 lbs., while that of the uterus is 54. The whole amount of expulsive force of the voluntary muscles is, he says, not usually employed to assist the uterus in completing the second stage of labour; but this does not contradict the conclusion we have ascribed to him. The conclusion is, indeed, for Professor Haughton, inevitable; for every accoucheur knows that the bearing-down efforts, whatever be their actual measured power, are very strong—perhaps as strong as possible, quite frequently in ordinary labours. Besides, Haughton himself expounds his meaning in the following words:—"It is plainly necessary that the first stage in the expulsion of the foetus should not be entrusted to a voluntary muscle, and hence an involuntary muscle is gradually provided, which takes the initiative and commences the process of parturition, the completion of which is

then accomplished by the aid of voluntary muscles, to the employment of which, at this stage, no moral objection can be raised. It is also necessary (if the Contriver be allwise, or if the principle of least action in nature be true) that the involuntary muscle, so produced, should not possess more or less force than is requisite for its purpose. The uterine muscle does not grow to meet a growing resistance (as happens frequently in other cases), and its precise degree of strength cannot be produced by a tentative process; for in healthy gestation the uterine muscle never tries its force against the membranes it is called upon to rupture until the actual period of parturition has arrived."

The view propounded in these words has great authority on its side, besides that of the quoted writer,¹ for the point therein raised as to the relative powers and uses of the uterine and auxiliary forces of parturition is one that has been much discussed and for a long time. The great Haller, indeed, held opinions which are in accordance with Haughton's view. This renowned physiologist discarded the opinion common in his day, and now almost universally entertained, that the uterus is the main source of the power exerted in every stage of parturition.

Haughton gives no reason for discrediting the general opinion of obstetricians, relying apparently

¹ A similar opinion is entertained by Schatz. *Geburtsmechanismus*, S. 28.

on his conclusions alone regarding the comparative power of the two forces—that of the uterine muscle and that of the assistant voluntary muscles. No doubt he makes some observations, intended to be corroborative, as to the economy of force and other so-called laws of nature; but such reflections cannot be regarded otherwise than as premature by those who, like myself, do not adopt this writer's hypotheses, upon whose verity their justice depends.

In the course of his concise view of this question in his work on Physiology, Haller twice takes care to express his doubts as to the truth of his own opinions, and he ends by appealing to anatomists for light upon the subject. This appeal is, at least, ingenuous, for his argument against the ordinary opinion rests greatly upon the uterine fibres, their direction, and the direction of the force evolved by them; and as Haller's notions on this anatomical point were very imperfect, and his mechanical ideas equally so, we need attach no weight to this part of his argument. Besides this, however, he has really nothing deserving the name of good evidence on his side. He thinks the effects produced by the expulsive pains greater than the power of the uterus, but this is evidently mere begging the question. So also is his dependence for aid in his argument on a picture of the great struggles of the voluntary muscles.

Authors generally do, as I have said, entertain an opinion opposed to that of Haller and Haughton.

They are too numerous to name, and none of them merits special mention ; for, so far as I know, no one has distinguished himself by the novelty or elaborateness of his arguments in support of the ordinary view that the uterus is the chief agent in the whole process of parturition, and that the voluntary muscles, whether stimulated by volition or by reflex excitement, are in a secondary position, aiding the uterus indeed, but not supplying the chief force. There is no positive value in an argument of appeal to authority, yet it is evident that the amount of authority against him made Haller hesitate to enunciate his own views ; and when we consider the number, the intelligence, and the acute attention of the obstetricians who form a majority, scarcely differing from the whole body, in favour of one view, we cannot but be weightily impressed in its favour.

I must point out that some of the arguments made by obstetric authors to do regular service in defence of their view are very weak or quite vain, and I may cite examples. Cases of parturition, completed when the uterus is prolapsed and is said to derive no assistance from bearing-down efforts, are cited. But such cases prove almost nothing, even supposing they are correctly described, for there is in such cases absence of the ordinary difficulties of labour, which consist in the propulsion of the child through the pelvis. Cases of expulsion of the child after death of the mother are quoted. But, so far as I have perused

them, they are given with a deficiency of circumstantial data such as to invalidate them altogether. Indeed it is, in some of them, not even shown that the uterus acted at all; while in all there is the assumption that the difficulty of birth after death is as great as before it. The like objections may be made to examples of labour in asphyxia, narcotism, and syncope. It has been also asserted that narcotism by chloroform affords evidence that the uterus is the chief agent in parturition. But I must assert the incorrectness of this argument; and I cannot understand why Haughton should call attention to the influence of this agent, for any argument from it is valid, so far as it goes, only against his views. I have, in a large experience, never seen chloroform inhalation destroy the action of the voluntary muscles. I believe it generally weakens their action; and it is well known that, at the worst, it only weakens the powers of labour. It is not known whether it weakens the uterine action or the action of the voluntary muscles in the greater degree. If it does, as is alleged, when given profusely, destroy the action of the voluntary muscles, it certainly seldom completely arrests the progress of labour. Lastly, cases of labour in paraplegic women are cited in favour of the ordinary opinion. But I fear they do not even appear to favour it; and with a view to the present question, they cannot be held as settling anything, seeing we do not know what influence

paraplegia may exert on the uterus itself. Besides, the cases are insufficient in every way.

The arguments on which I place chief reliance are the following :—

1. The great power of the uterus felt by the hand of the accoucheur, as in the operation of turning, long after the rupture of the membranes.

2. The great and sufficient power of the uterus, observed in cases where the action of the voluntary muscles is weak or restrained.

3. The regulating influence of purely uterine pains on the progress of the second stage of labour.

4. The supremely important demand for, and presence of, powerful uterine action after the expulsion of the child.

5. The arrest of the progress of labour by inertia of the uterus. The argument from this appears to me unanswerable, for the condition often occurs when there is certainly only very slight resistance to the progress of the child—when the mother evidently desires the completion of labour, and bears down violently with this object in view.

6. In cases of uterine inertia, such as are above described, the practitioner may find, by pulling with the forceps from below, or by pushing with the hands from above, in the absence of all parturient effort, whether of the uterus or of the voluntary muscles, that a very small force, say not exceeding the weight of the child, is sufficient to finish a labour upon

whose progress violent bearing-down efforts have had no effect.

7. The circumstance that, were the voluntary muscles the chief agents, expulsion of the child would be in great part a voluntary act, which it certainly is not.

8. The asserted completeness of the function of parturition in animals in which the assistant bearing-down efforts are annihilated by opening the abdomen ; the process being carried on by their uterine and vaginal muscles, which are weak when compared with those of women.

Baudelocque and Velpeau¹ relate cases which appear to show that woman has very rarely voluntary power over the progress of parturition, for a time. Such cases offer no difficulty when regarded with a view to the present question. They are explicable in more ways than one ; and an illustrative statement is, for my present purpose, quite sufficient. Every experienced accoucheur has seen cases where voluntary increase of bearing down has sufficed to expedite or even precipitate labours which, if the women had been left in a sleepy, lethargic condition, might have been protracted for an indefinite length of time.

There can be no doubt that the uterus is a very powerful agent in expelling the foetus from its cavity into the world ; that it is not the sole agent, and

¹ *Traité complet de l'art des accouchements.* Ed. Bruxelles, p. 227.

that it is assisted by the action of the voluntary muscles. Though I have not proved absolutely that the uterus is the chief agent in the performance of this function, yet I have no doubt that it is so ; and I think that the arguments which I have adduced give this belief of the profession the highest degree of probability. The belief does not imply that the aid afforded by the voluntary muscles is inconsiderable or unimportant. It only renders it quite incredible, that while the power of the uterus is 54 lbs., that of the voluntary muscles can be 523 lbs.

III.—Haughton's conclusion, on which I wish, last of all, to comment, is, "that, on an emergency, somewhat more than a quarter of a ton pressure can be brought to bear upon a refractory child that refuses to come into the world in the usual manner."

In my work entitled *Researches in Obstetrics*,¹ to which Professor Haughton refers, I have discussed carefully but briefly this point, and announce the conclusion that the comparatively small figure of 80 lbs. gives the highest power of labour ; and I quote Joulin, who estimates it as somewhat above 100 lbs. I do not deny that in exceptional circumstances a few pounds above 80 may be reached, but I feel pretty sure that seldom, in the history of woman, has the figure 80 been attained, whether on an emergency or not. This conclusion is arrived at by experiment and observation—experiments on the

¹ See chapter v. of this work.

force required to pull a child through a contracted brim of pelvis ; observations of the force used to complete a difficult labour, which nature, in its most violent throes, has failed to accomplish.

Every accoucheur will, I suppose, readily admit that in a case of delivery by podalic extraction the surgeon can exert a great deal more force to bring the child into the world than the most energetic labour can. Now, in these circumstances the surgeon can use no force nearly reaching a quarter of a ton. A very much smaller power would rend the luckless body of the child in pieces.¹

Such a power as a quarter of a ton does, in my opinion, represent a strain to which the maternal machinery could not be subjected without instantaneous and utter destruction. To speak of a rigid perineum resisting such a power, or the fourth part of it, would be ridiculous. The possession and use of a considerable portion of such a power would render the forceps and cephalotribe weak and useless instruments ; the mother could bray the child as in a mortar and squeeze it through a pelvis which would, under other circumstances, necessitate Cæsar-ean section. Such a power would, if appropriately applied, not only expel the child, but also lift up the mother, the accoucheur, and the monthly nurse—all at once. It would be dangerous not only to the

¹ See a subsequent chapter (ix.) "On the tensile strength of the fresh adult fœtus."

mother and the child ; it would imperil also the accoucheur. It has been calculated for me that if this force were applied just as the chief resistance to delivery was overcome, the child would be shot out of the vagina at the rate of thirty-six feet per second.¹ The blow it could thus inflict would be equal to the shock produced by the fall of the child from a height of twenty-one feet.

In an early part of this paper I have said that the method of inquiring into the subject which Haughton adopts is both difficult and dangerous ; and I think I have said enough to show that danger has not been avoided. There must be error in Professor Haughton's calculation of the power produced by the action of the voluntary muscles, or there must be error in judging of the application of this power to the accomplishment of the function ; or there must be error in both. I shall not attempt to show where the error lies, but its occurrence does not astonish me ; for any one who has studied the difficult subject of the retentive power of the abdomen will recognise the difficulty of reaching conclusions as to the power of labour by Haughton's method. It is highly probable

¹ In making this calculation the child is taken as 7 lbs., the pressure as 580 lbs. ; and it is supposed to be exerted through a space of three inches—measurements which are fair statements of the case. It is further supposed that the friction is negligible when compared with the forward pressure. This is certainly the case if the forward pressure is nearly as much as is stated by Professor Haughton as possible.

that the power of the voluntary muscles is dissipated—perhaps in compressing intestinal gases, perhaps in consequence of being misdirected.

Whatever may be the real source of error, it is highly desirable to find it out, in order that we may, by more accurate proceedings, arrive at the true results which Haughton hoped to reach.

CHAPTER VIII.

ON THE CHIEF DIRECTIONS AND EXTENTS OF UTERINE SHRINKING ; SPECIALLY AT THE TIME OF THE COMPLETE EXPULSION OF THE CONTENTS OF THE GRAVID UTERUS.¹

So far as I know, no notice has hitherto been taken of this subject. Much has been written, no doubt the result of careful anatomical investigation, on the direction of the uterine muscular bundles, and I suppose that any student who has thought of the subject of this paper has more or less identified in his mind the chief directions of the bundles of uterine fibres with the chief directions of uterine shrinking. The researches as to the arrangement of the muscular uterine bundles are very imperfect and unsatisfactory ; but, supposing them to be perfect and satisfactory, it is evident, on a little reflection, that observation of the lines in which they mostly run at different parts can form no just basis for conclusions as to the chief directions of uterine shrinking. The directions of the bundles of uterine fibres may indicate the direction of uterine action or contraction,

¹ Read before the Obstetrical Society, Edinburgh, April 8, 1874.

but they cannot give any indication of the amount of shrinking or the result effected, which is the subject of this paper. Other researches as to the line of direction of the resultant of all the parturient forces may occur to the mind ; but it is, as yet at least, impossible to connect these in any way with the directions of the bundles of uterine fibres, or of uterine shrinking.

The extents and directions of the shrinking of the whole anterior wall of the uterus, of the fundus, or of the posterior wall, may be found out by comparing the various dimensions of the surfaces of these parts after delivery, and their estimated, or perhaps really ascertained, dimensions before delivery. These parts have limits more or less exactly ascertainable, from which the two sets of measurements can be made.

The parts or surfaces just referred to are extensive, and within their limits there may be various smaller subdivisional parts, each of which undergoes shrinking which may be variously conditioned as to extent and direction. The observations to which I am about to direct attention furnish evidence in regard to the shrinking of the various smaller parts of the uterus, and indicate a method of reaching valuable results.

The method consists in observing the rugæ, ridges or wrinkles ; and the sulci, furrows or grooves, occasionally found on the peritoneal surface of the

contracted uterus. To this may be added observation of the cracks or fissures occasionally observed in the same membrane. The most superficial study of these furrows, or cracks, or fissures, is sufficient to convince the student of their value as indications of the extents and directions of uterine shrinking at the parts where they are present.

The peritoneum is not, in ordinary circumstances, an inelastic membrane. It shrinks, by elasticity, co-ordinately with the subjacent uterine muscular tissue to which it is attached, and consequently, on the recently emptied uterus no rugæ or sulci are, in ordinary circumstances, observed. This peritoneal elasticity probably furnishes a part of the constant pressure inside the replete uterus.

In certain circumstances the peritoneum has not elasticity sufficient to ensure its contraction co-ordinately with the subjacent muscular layer, and, when the uterus is evacuated, is thrown into rugæ and sulci. Under what circumstances the peritoneum is deficient in elasticity I do not know. That inflammation has sometimes an influence diminishing or destroying elasticity is rendered probable to me by the histories of some of the cases in which it was fissured, and by the exquisiteness of the furrows, cusped in a cross-section, which were seen in a case of undoubted peritonitis, to be hereafter fully stated. It is important, however, with reference to this case, to remark that the layer of moderately dense coagu-

lated lymph which renders the peritonitis indisputable, itself, on account of softness and friability, altogether inelastic, may yet, by its adhesion to the peritoneum, have, in a slight degree, prevented the elasticity of the serous membrane from fully showing itself.

While it is evident that these wrinkles and furrows give evidence as to the extent and direction of uterine shrinking, we must be careful in drawing conclusions from them, as yet at least when the number of observations is small. The occurrence is exceptional. The deficiency in elasticity may be only partial, or only in certain directions, or in certain directions more than in others, or it may be confined to certain portions of the membrane.

There are different kinds of markings on the surface of the uterus. The grooves, or furrows, or sulci, may be cusped in a cross section, as in Fig. 2, *a* and

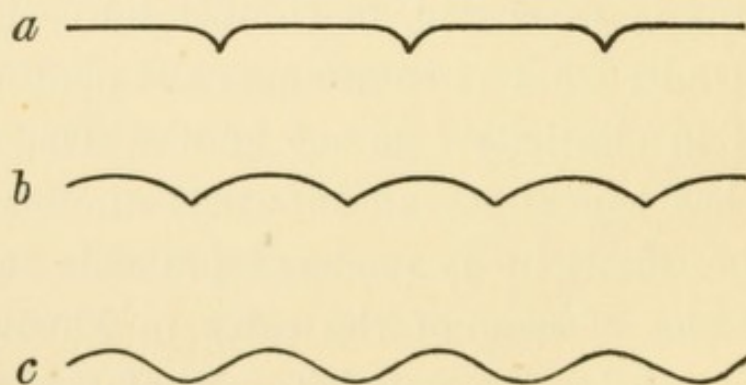


Fig. 2.

b; or they may in a cross section show a sinuous outline, as in Fig. 2, *c*. Then there may be no ridges or rugæ between the grooves, as in Fig. 2, *a*; or there

may be intervening ridges or rugæ as in Fig. 2, *b* and *c*. Besides the great rugæ and furrows there are observed minor secondary rugæ and sulci, running in various directions, but, for the most part, transversely to the great rugæ and furrows. In connection with all

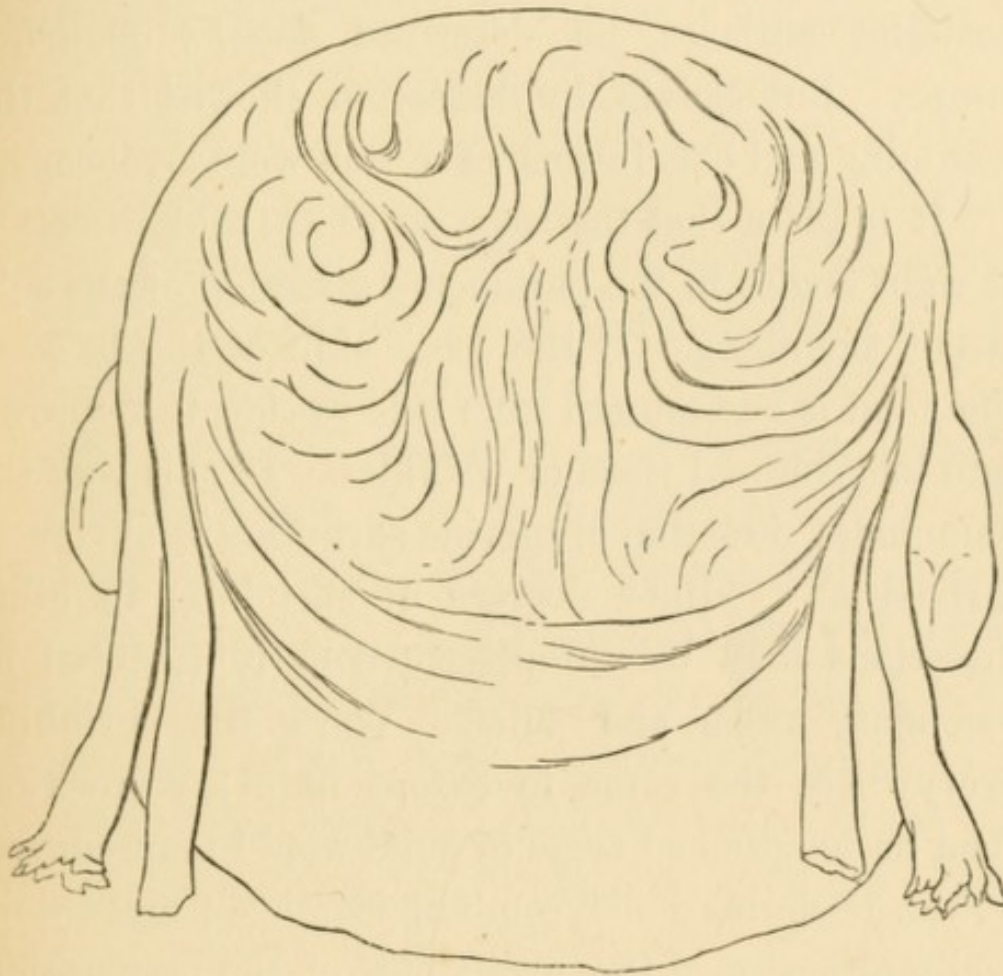


Fig. 3.

kinds of rugæ and sulci, it will be necessary to study their number, and height, or depth, in a limited space, as well as their direction. The degree or amount of shrinking of subjacent structures may be measured by the number of the rugæ, or by their height, or the depth of the furrows.

Peritoneal wrinkles have been held by J. Sue¹ as giving sufficient evidence, by their direction, of the same direction of the subjacent uterine bundles. Founding on such evidence, and finding in one example two concentric sets of rugæ side by side, near the fundus uteri (see Fig. 3), on the anterior and posterior surfaces, he described *muscles quadrijumeaux*. But Boivin, by dissection, showed that the reasoning and conclusions of J. Sue were erroneous.

It is plain that the rugæ will have their direction at right angles to the paramount direction of the subjacent shrinking. If the loose skin of the back of the hand is compressed from side to side, it is thrown into longitudinal rugæ or wrinkles. In this way the paramount direction of uterine shrinking is obviously arrived at. Minor degrees of shrinking in other directions may be unrepresented or represented by secondary rugæ and sulci. Were the shrinking everywhere the same in extent in every direction, then no lengthened rugæ or sulci would be produced, but an irregular honeycomb appearance; and this is really observed in the upper part of the preparation, partially depicted in Fig. 4.

The observations which I shall presently describe give indications as to uterine shrinking at or near

¹ My only authority for this statement is the text descriptive of Planche iv. of Madame Boivin's Atlas for her *Traité pratique des maladies de l'uterus*. The wrinkles in Boivin's case are shown in Fig. 3.

the end of the third stage of labour. The conditions of uterine shrinking at this time may be quite different from those in earlier parts of the process of parturition. On this latter subject some inquiries have been made, and I may refer to some by myself, stated in subsequent chapters of this volume, on "The separation of the placenta when it is prævia," and on the "Causes of unavoidable hemorrhage."¹

The following are a few particulars of the case of miscarriage, from which is derived the drawing given as Figure 4. M. T., æt. 34, unmarried, arrived at about the end of the sixth month of pregnancy, was believed to be using drugs to procure abortion. No positive evidence, however, was at any time obtained to show that any drugs or interference was actually resorted to with this object. On the 10th January she was seized with great abdominal pain and tenderness. On the 12th I was called in consultation, and found her suffering from acute peritonitis; the large pregnant uterus being especially tender. On the 16th January she unexpectedly miscarried, labour appearing to be very rapid. The uterus, after delivery, could only be indistinctly made out by Dr. Underhill and Mr. Beatson, who then attended her, abdominal tension being very great. There was very little blood lost. On the 19th January she died, little more than two and a half days after delivery.

The *post-mortem* examination revealed universal

¹ See pp. 338 and 374.

peritonitis, the abdomen containing a large quantity of fetid purulent fluid. When the abdomen was opened, the uterus, nearly as shown in the figure, could be seen. To the middle of the upper part of its anterior surface, at a part shown in the figure, was attached a tightly stretched band about $1\frac{1}{2}$ inch broad, formed of the great omentum.

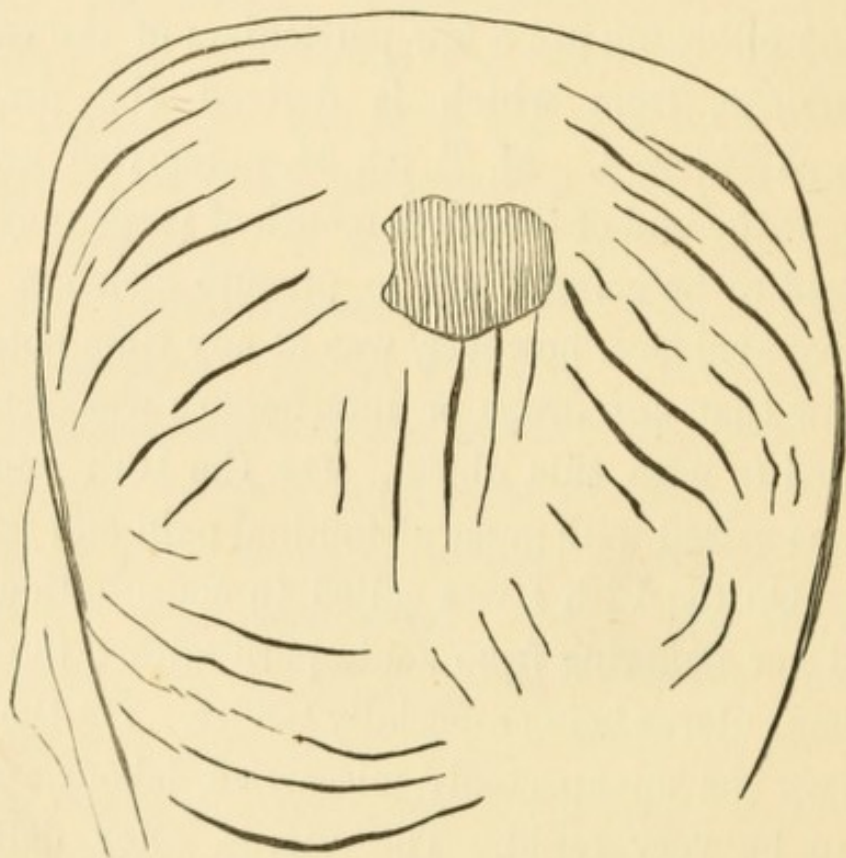


Fig. 4.

The rugæ seen in the figure are scattered over the whole anterior surface of the body of the uterus and the anterior half of the fundus. The bit of peritoneum exposed by separating the adherent omentum shows no wrinkles. The posterior wall of the uterus has no adherent layer of lymph and no rugæ. The characters

of the chief sulci and rugæ need no description, being easily made out in the woodcut, wherein are expressed their position, relations, and number and size. On and near the fundus the rugæ and sulci had a sinuous outline in a cross section, elsewhere there were no marked rugæ, but only sulci. The uterus, as depicted, actually measures about 5 inches, both vertically and transversely.

Figure 4 shows shrinking to have taken place in directions quite in accordance with what might *a priori* have been expected. Near the centre of the anterior surface of the body of the uterus the shrink-

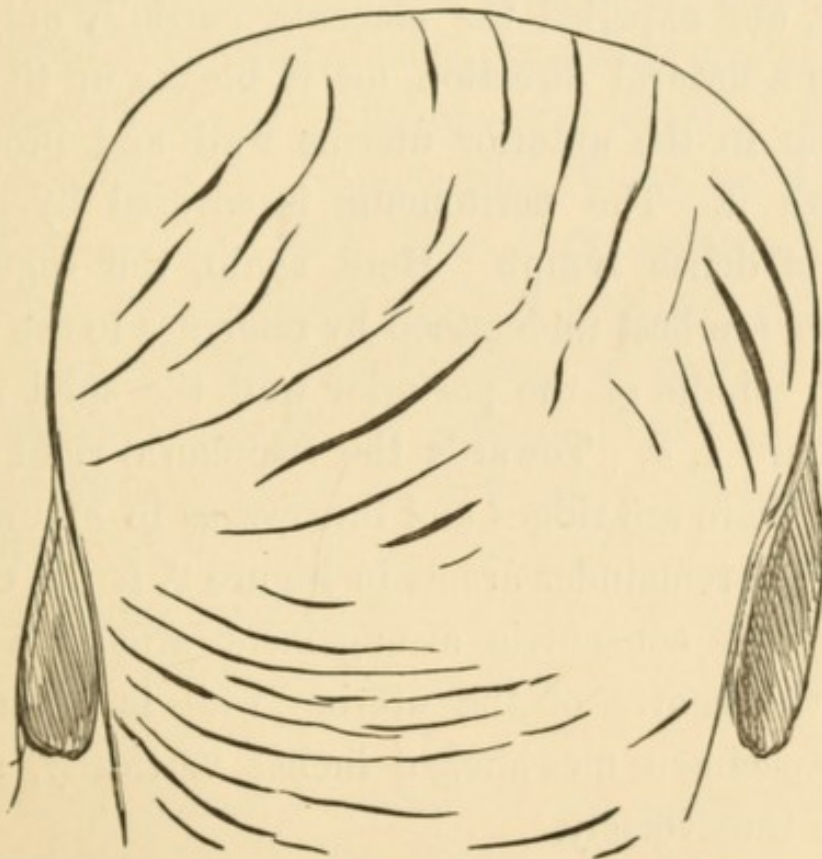


Fig. 5.

ing has been mainly transverse, and around this space it has taken place somewhat concentrically, so far as

the upper half of the body of the uterus is concerned. In the lower half of the body of the uterus the shrinking has been mainly in a vertical direction, especially on the right side.

The lines of Figure 5 are remarkable, and it must be observed that the case is altogether extraordinary. The drawing shows the posterior surface of the uterus of a woman who died of enteritis, and on whom Cæsarean section was performed immediately after death. The patient was not under my care or inspection. The full term of pregnancy was nearly reached. After the operation the uterus contracted firmly, and expelled the placenta partially only, and not in a natural direction, for it blocks up the long incision in the anterior uterine wall and protrudes through it. The peritoneum is covered by a thin layer of dense lymph. Here, again, the rugæ and furrows are best understood by reference to the figure. In the middle of the posterior wall the sulci are as in Figure 1, *a*. Towards the (woman's) right upper corner there are ridges and furrows as in Figure 2, *c*, while the remainder are as in Figure 2, *b*. A certain amount of concentric arrangement around a point near the centre of the uterine wall is observable. This specimen measures 5 inches vertically, and 5 inches transversely.

CHAPTER IX.

ON THE TENSILE STRENGTH OF THE FRESH ADULT FETUS.¹

THERE are almost innumerable laboratory experiments demanded in the present state of obstetrical science ; for, while many important points in the mechanism of natural and of unnatural delivery may be admirably illustrated or explained by them, others can in no other way be made out or settled. To give a list of those which at once occur to the mind would be tedious, and, to me at least, very tantalising, for I have been able to make very few, and this deficiency arises, in great part, from the want of material. Yet, those acquainted with the recent progress of obstetrics can easily recall to mind some of the most valuable of our recent steps in advance which have been attained through following that experimental method, the dawn of whose usefulness is as yet scarcely recognised by obstetricians.

The experiments which I am now about to describe were made with a view to arrive at correct ideas as to the force available in delivering a child

¹ Read to the Obstetric Section of the British Medical Association at Norwich, August 12, 1874.

footling, as, for example, in the operation of podalic extraction, now so extensively recommended in the conduct of certain cases of contracted pelvis. I knew that, even among our best recent authors, there prevailed gross error on this point, the available force being absurdly overrated, and the force that could be exerted in the alternative forceps operation almost as absurdly underrated. It would be easy, but it is unnecessary, to cite strong asseverations to this erroneous effect from the most recent authors, widely reputed as our best. Now the point is not of second but of first rate importance in the question, indeed a fundamental matter. It is therefore well worthy of careful discussion, and fortunately it is easily settled in a quite conclusive manner. I have assumed, what I believe to be true, though it is not proved, that the strength of a fresh dead foetus is the same as that of a living one.¹

I took a fresh newly-born child, and, after passing its body through an aperture so cut in hard wood as to represent the brim of a contracted pelvis, or after fixing its head below the bi-parietal diameter between two parallel bars, I applied above an ankle an apparatus consisting essentially of a hook whereby to suspend the weights, which were to be gradually increased till they produced disseverment of the body of the child.

As is well known, the disseverment in actual

¹ On this subject see some remarks in chap. vi. p. 96.

practice takes place, as it did in all my experiments, in the neck. The disseverment was therefore always decapitation.

The force effecting decapitation is in these experiments equal to the combined weight of the child beneath the line of disseverment, of the apparatus attached above the ankle, and of the special weights applied. But great nicety was not attempted, the increments of weight attached being made by several pounds at a time.

It was desirable to limit the time during which in each trial the weight was allowed to operate, and the term of a half-minute was adopted, the weight not being allowed to act longer than that time in one trial. A limit of this kind was necessary to bring the experiments into conditions somewhat analogous to the pains or force of labour and the efforts of the accoucheur in a difficult case in actual practice.

The decapitating force thus discovered is the measure of the tensile strength of the fœtus ; and of course in podalic extraction, whether by the feet or by the Prague seizure, no greater force effective in delivering the head can possibly be applied, for the materials give way when the weight reaches the decapitating amount.

In actual practice a greater dragging force may be exerted by the accoucheur and applied to the child. But such force, so far as it exceeds the decapitating amount, does not directly help the

delivery of the head. It is either usefully spent in overcoming friction of some part below the line of disseverment, or uselessly or even banefully spent, because misdirected. In actual practice, however, when the head is fixed in the brim, the decapitating force must be very nearly an exact highest limit of the force that can be exerted by the accoucheur.

These remarks are made without regard to the life of the child, and with a view merely to the strength of the materials. The experiments to be narrated will show that the life of the child is compromised before the limit of the tensile strength of the fœtus is reached. This is easily explained by the destruction of the spinal column, which occurs under a force considerably less than the decapitating force ; and this circumstance must be kept in mind when podalic extraction is compared with forceps extraction ; for, in making the comparison, it is supposed that the life of the child is not compromised. Neither operation would be selected as the best if the securing a living child were hopeless.

Experiment I.—A fresh adult female fœtus, weighing 5 pounds 6 ounces, and measuring eighteen inches in length. The spinal column gave way under 90 pounds. Decapitation took place under a weight of 118 pounds. The disseverment occurred between the fifth and sixth cervical vertebræ.

Experiment II.—A fresh adult female fœtus, weighing 7 pounds 7 ounces, and measuring twenty

inches in length. The spinal column gave way under a weight of 120 pounds. Decapitation took place under 141 pounds. The disseverment occurred between the sixth and seventh cervical vertebræ.

Experiment III.—A fresh adult female foetus, weighing 8 pounds 15 ounces, and measuring twenty-three inches in length. The spinal column gave way under a weight of 122 pounds. Decapitation took place under 136 pounds. The disseverment was between the fourth and fifth cervical vertebræ.

Experiment IV.—A fresh adult male foetus, weighing 5 pounds 12 ounces, and measuring twenty-one inches in length. The spinal column gave way under a weight of 91 pounds. Decapitation occurred under a weight of the same amount. The disseverment was between the fourth and fifth cervical vertebræ.

Experiment V.—This experiment is not to be included with the former, as it was performed not on a newly-born child, but on one a fortnight old. It was fresh and weighed 7 pounds 5 ounces, and measured twenty-two inches in length. The spinal column gave way under a weight of 147 pounds. Decapitation took place under a weight of 163 pounds. The disseverment was between the third and fourth cervical vertebræ.

The dissevering or decapitating force in these experiments was 118, 141, 136, and 91 pounds respectively, or, on an average, about 120 pounds. This is probably far from being what most obstet-

ricians would regard as a great force ; and it may be said with truth that, if we are to avoid premature decapitation, the force used should not reach this amount, should not, indeed, exceed a hundredweight. Such a force, if used only for a short time, if it cause no great contusion, and especially if it do not break the continuity of the mucous surface, may be safely used in repeated efforts in delivery. On this point there is much inexactness from our ignorance of how much and how long the soft parts will endure compression without great resulting evil to the mother.

The podalic dragging force of a hundredweight is considerably greater than any ever known to be exerted in propulsive efforts in natural parturition, and so also is the force of 100 pounds, which is about the limit of what is available without destroying the life of the child. Estimates of the force of labour pains have, as is well known, been published, which make it much above a hundredweight, but such estimates have been shown to be unreliable in themselves, and in the methods by which they have been reached. It is a part of the theory of podalic extraction in contracted pelvis that the force exerted then by the accoucheur exceeds that of the best labour pains, and it is interesting to observe that the experiments confirm at least this part of the theory. At the same time it is necessary to realise that the excess of force in favour of podalic extraction is far from "infinite," and is truly very limited.

I am sure that most accoucheurs, not being familiar with dynamometrical experiments, will regard each his own available muscular power as being far more than he can dare to use in podalic extraction, that is, far more than a hundredweight. No doubt he is right in so far as that he can easily lift a hundredweight, perhaps even two ; but that is a very different and a very easy matter compared with applying a dragging force of a hundredweight in a special direction. Probably no ordinary man can stoop over a bed and pull in a direction across the front of his body, or in what would now be called a coronal direction, with a force of a hundredweight ; and this is what many attempt, and erroneously fancy they can do. Numerous failures arise from practitioners erroneously estimating the amount of extractive or dragging force they are exerting on the foetus, since it is common to confuse the amount of force exerted with the amount made efficient as a dragging force ; and it is the direct dragging force that may be allowed to rise to about a hundredweight. If a correct judgment is formed of the great difficulty of using a dragging force of a hundredweight, except when the accoucheur is favourably placed, it will be easily understood how important it is to have the patient in a convenient position, for the practitioner must wield his great force skilfully, and gently and easily, if he is to do his work in the best way.

In the foregoing experiments it will be observed that the cervical part of the vertebral column invariably gave way, and the dissevered vertebrae became widely separated from one another before the whole neck yielded and decapitation was effected. While the vertebral column yielded when the weight or force was about 105 lbs., a further addition of about 15 lbs., forming a total of 120 lbs., was required to produce decapitation. From this, it is evident that at first almost the entire dragging force is exerted through the vertebral column; that the tensile strength of all the tissues of the neck is not simultaneously and equally tried, and that the vertebral column is weaker than the combined soft parts. The vertebral column yields with a snap or jerk, and immediately a marked elongation of the foetal body occurs. Of both of these events the accoucheur can scarcely fail to be aware, even if he did not expect them, and this information is important practical direction to him if he wishes, as he almost certainly does, to avoid decapitation. The yielding of the column of bones does not restrict the force he can exert, for he can, generally, not always, apply more, to the extent of about 15 lbs., without producing decapitation. But the events referred to form a distinct warning to him as to how near he has reached the amount of force available by dragging the foetal body. He cannot, when they have occurred, pull with greatly increased force, even for half-a-minute at a time,

without increasing the imminent risk of decapitation.

Another point is clearly shown by these experiments, namely, the sufficiency of a single limb for dragging, so far as mere strength of materials is concerned; and this determination has many evident practical bearings. In the trials narrated we uniformly attached the apparatus and its connected weights to one leg, just above the ankle. The parts were protected by a piece of leather wrapped around them from the cutting sort of edge presented by the cord used to tie on the apparatus. One leg always proved stronger than the whole neck. It is interesting to remark, with reference to the leg, that, just as in the neck, the bony column was weaker than the other combined soft parts, for, during the dragging, various cracks were heard and subsequently found to have been produced by separation of femoral or other epiphyses. One limb always proved more than sufficient for the neck. Whatever advantages may or may not arise from using the Prague seizure, one thing is sure, that, with a view to having merely sufficient strength of materials, it is not required; one leg is, in this point of view, sufficient. It is plain that this is not exactly proved by the experiments, for in them the weight acting on the neck exceeded that acting on the ankle by the greater part of the weight of the body; but the ankle will not have this advantage except when the body is dragged

downwards vertically, or nearly so, which is not done in practice.

I have spoken incidentally of the comparison between the power that may be exerted by dragging the child in podalic extraction, without compromising its life, and the power that may be exerted by dragging the child by the forceps. While the limit of the former power is easily ascertained, and has been fully stated as about 100 lbs., it is no part of my present object to determine the higher limit of the latter, and it is a far more difficult matter. But there are various ways of showing that the latter is considerably higher than the former, which, as I have already said, is not the universal or even the general belief. I have made many experiments in connection with this subject, but I shall not here enter on them, satisfying myself with giving some easily appreciated evidence.

An accoucheur can decapitate a child by the efforts of podalic extraction; in other words, his available power overcomes the whole tensile strength of the foetus. Few men pulling in an advantageous manner find much difficulty in applying a dragging force of 120 lbs.; but it is quite possible to apply a forceps to a foetal head, at the brim of an actual pelvis, or of one artificially constructed for experimental purposes, so as to defy the utmost efforts of the most powerful accoucheur. These iron hands can be made virtually of any strength, and the

grasped spheroidal fœtal head is firm enough to resist such quasi-cephalotriptic compressing or otherwise destroying force as they exert in maintaining their hold against a dragging force greater than can be used in pódalic extraction, because greater than is sufficient to rend in twain the fœtal body.

CHAPTER X.

ON THE PELVIC ARTICULATIONS IN PARTURITION.¹

IN the lower animals we find beautiful examples of the changes taking place in the pelvic joints in the end of pregnancy. And the intimate nature of these changes is most satisfactorily studied in these animals, because in them it is comparatively easy to procure specimens of the altered tissues at any period, and still more so, because in many of them the changes are to an extent far exceeding what is ever found in the human female. In illustration, I may at present cite the changed condition of the pelvis of the guinea-pig and of the cow. In the former there takes place, at the time of parturition, a very considerable separation of the pubic bones ; the ligamentous tissue stretching, in this small quadruped, to the extent of about an inch. This enlargement of the pelvic circle, by separation of the pubic bones, necessarily implies great relaxation of the tissues on the anterior or inferior part of the sacro-iliac joint, and freedom of motion in it. After parturition the pubic bones again become closely united.

In the cow, the changes in the pelvic joints differ in some important respects from those just described as occurring in the guinea-pig. In the latter, it has

¹ See *Dublin Quarterly Journal of Medical Science*, August 1854.

been stated that the most notable change is the elongation of the ligaments of the symphysis pubis (nature in this way foreshadowing the operation of symphyseotomy), and the separation of these bones giving rise to corresponding motions in the iliac bones, analogous to the abduction of the limbs. In the cow, on the other hand, these movements are completely absent. The symphysis pubis is consolidated by bony union, and thus incapable of distraction, and consequently the abduction of the iliac bones is impossible. But, nevertheless, the changes in the cow's pelvis are of great importance. They have been described by Professor Barlow of the Veterinary College,¹ and I have demonstrated them upon Mr. Barlow's preparation to the members of the Edinburgh Obstetrical Society. They consist in an increased development of the large sacro-sciatic ligaments, which, from being of moderate thickness and in a state of tension in the non-pregnant cow, become much increased not only in thickness but also in length, and are thus made slack and yielding. The tension of these ligaments tends to fix the sacrum and consolidate it with the ilia, and their relaxation leaves it freer to move. Further to facilitate this motion, the sacro-iliac joints, which in the non-pregnant cow are described by Mr. Barlow as secured by a material closely resembling intervertebral substance, now have the opposing bony surfaces smooth and

¹ *Monthly Journal of Medical Science*, January 1854, p. 83.

lubricated, and the surrounding fibrous ligaments relaxed. By these changes the ilia become extensively movable upon the sacrum (or *vice versâ*) in an antero-posterior direction, the motions being analogous to those of flexion and extension in the limbs. The final result of these changes and motions is to enlarge the genital passages in this animal.¹

It has hitherto been customary to regard the articulations of the pelvis in man as virtually immovable, and to describe cases where motion evidently takes place in the female, at the time of parturition, as morbid in their character.² But Mr. Zaglas³ has pointed out that in man there is distinct motion of the ossa innominata in an antero-posterior direction, or upon an imaginary line passing transversely through the second sacral vertebra from one side to the other. In other words, the sacrum may be described as having a nutatory motion upon this imaginary transverse axis, the promontory of the sacrum advancing downwards and forwards, while its apex

¹ See Kehrer, *Beiträge zur vergl. und exper. Geburtskunde*, Heft. II. S. 127.

² For authority see Wood, art. "Pelvis," Todd's *Cyclopædia of Anat. and Phys. Suppl.* p. 14. See also Baudelocque, *System of Midwifery*, Heath's Transl. vol. i. p. 33, who refers to Louis, "Dissertation sur l'Ecartement des Os du Bassin," *Mémoires de l'Acad. Roy. de Chir.* tome iv. See also Lachapelle, *Pratique des Accouchements*, tome iv. p. 85. For an example of an old author denying mobility even in labour, see the works of Realdus Columbus, quoted in Siebold's *Geschichte der Geb.* ii. Band. S. 47.

³ *Monthly Journal of Medical Science* for September 1851, p. 289.

moves in a contrary direction, and *vice versâ*. In the downward motion of the promontory, which in the non-pregnant is to the extent of about a line,¹ the brim of the pelvis is diminished to the same extent in its conjugate diameter, while the corresponding upward motion of the apex of the bone, to the extent of about two lines, puts the sacro-sciatic ligaments on the stretch, and enlarges the dimensions of the outlet. By observations on the living and on the dead subject, Mr. Zaglas has shown that in the erect position the sacral promontory is not in the position of greatest projection into the brim of the pelvis, but the reverse, and consequently that the apex is in its forward position diminishing the outlet, and relaxing the sacro-sciatic ligaments. When the body is bent forward, on the other hand, the base of the sacrum is protruded into the brim, the apex is tilted upwards, the sacro-sciatic ligaments put on the stretch, and the outlet of the pelvis consequently enlarged. These movements take place ordinarily in both man and woman, in defæcation, etc., but in her they are of greatest interest and importance in the function of parturition. Before entering on this part of the subject, I shall first point out some peculiarities in the pelvic articulations in woman, and describe the changes taking place in them in the end of pregnancy.

The three large pelvic articulations present the

¹ Schultze estimates them at about two millimetres, *Jenaische Zeitschrift*, 1867, S. 289.

following important peculiarities in regard to their mode of union :—Each articulating surface of bone presents two distinct parts ; the one comparatively smooth, covered with cartilage, and only partially united to its neighbour ; the other rough, for the attachment of very strong and numerous bands of fibrous and fibro-cartilaginous tissue, firmly uniting it to the corresponding surface of its neighbour. The former surfaces form the anterior and inferior parts of the sacro-iliac joints, and in the skeleton are known as the auricular surfaces. In the symphysis pubis these surfaces form the superior and posterior parts of the joint. Interposed between the investing cartilages at these parts is a synovial bursa. It is on these surfaces that the articular motion is most free, the ulterior advantages of which, in the physiology of the erect position, etc., this is not the place to demonstrate.¹

Mr. Zaglas points out that on the os innominatum we may divide the entire articular surfaces into four parts. The two antero-inferior of these correspond to what is called the auricular surface, and are inclined to one another at an angle which looks outwards, and forms a ridge inwards. The two posterior

¹ In a specimen I exhibited to the Obstetrical Society there is a double synovial bag in the symphysis pubis. This joint was removed from a virgin about eighteen years of age. Luschka gives a similar observation. For a fuller exposition of these anatomical points see the *Traité des Accouchements* of P. A. Dubois ; also Mr. Zaglas's "Observations on the Symphysis Pubis," in *Monthly Journal* for November 1851, p. 489.

superior surfaces (which are separated by a large mass of fibrous ligamentous tissue from the corresponding surface of the sacrum) are inclined to one another at a similar angle; and there is accordingly a crooked ridge running between the four surfaces in a direction from the spine to the symphysis. Taking the four surfaces, however, in another relation, it may be seen that the two posterior superior surfaces are separated from the two anterior inferior by a groove (running nearly parallel with the axis of the sacrum), and are inclined to them at an angle which looks inwards. The consequence of this arrangement is, that while some motion is permitted, any tendency to displacement is entirely obviated, so long as the innominate bones are retained in a due degree of proximity by the pressure on the acetabula, and by their ligaments.¹ But, besides these just remarks on the general arrangement of the whole joint, it is important to notice another striking peculiarity always observed, and frequently in a very marked degree. This consists in the existence upon the posterior articular surface of the ilium of a bony prominence of irregular outlines, but frequently assuming the form of a solid projecting angle. This projection is found to correspond to a distinct cavity on the opposed surface of the sacrum, which is, in some of its functions, analogous to a cotyloid cavity. For upon these parts (which are generally on a level with the

¹ See Report of the Physiological Society, *loc. cit.*

upper part of the second bone of the sacrum) the motions of the ilia must take place ; and whilst they will offer no absolute resistance to the motions of the ilia upon the sacrum, analogous to flexion and extension in the limbs, they will, like the general arrangement of the entire articular surfaces of the articulation, tend to prevent the slipping upwards or downwards of the one bone upon the other, motions which would necessarily prejudice the security of the erect position.

In the latter half of pregnancy the soft tissues contributing to form the pelvic joints are invariably, or almost invariably, found softened as if by serous infiltration ; and the joints are consequently relaxed. Most anatomists and obstetricians,¹ who have paid attention to this subject, agree in this statement. The softening of these tissues is generally accompanied by their increase in thickness, a change which will in itself have, as a necessary consequence, the separation of the bones, and the enlargement of the pelvic circle.² And I have no doubt that this favourable circumstance, together with others connected with the motions of the joints to be presently discussed, forms an important part of the explanation of some cases of delivery, by a simpler operative pro-

¹ See Burns's *Principles of Midwifery*, p. 8 ; Velpeau, *Traité des Accouchements*, Bruxelles, p. 122 ; also Moreau, *Traité des Accouchements*, tom. i. p. 40.

² See Luschka. *Virchow's Archiv*. Bd. vii. 1854.

cedure than may have been predicted to be necessary. Indeed the experiments of MM. Giraud et Ansiaux¹ seemed to them to show that in contracted pelvis this change in the joints takes place to a greater extent than in well-formed pelves. In some cases the thickening of the tissues goes on to quite an extraordinary extent. Boyer states that in one case he found the sacro-iliac joint separated to the extent of half-an-inch; Chaussier found the symphysis pubis separated still more in an easy labour, and Madame Boivin asserts that she sometimes found the pubic bones separated to the extent even of an inch. In some cases, as in those of Smellie, Diemerbroek, and Denman, the separation appears to have taken place chiefly during the course of a difficult labour.

But although there can be no doubt as to the thickening and softening of the tissues forming the pelvic joints, there is great difference of opinion as to their capability of motion. In this country, indeed, most authors seem to think that motion in these joints, in pregnancy, is always to be considered the result of a morbid process. This opinion is, without doubt, erroneous, although there are observed, not unfrequently, cases where the natural relaxation of these joints increases to such an extent as to interfere with the function of progression.²

¹ Jacquemier, *Manuel des Accouchements*. Tom. ii. p. 476.

² Some extraordinary observations have been made by obstetri-

The observations which I have already made upon the movements in the pelvic joints in the non-pregnant, set aside at once all the arguments adduced to show that there is naturally no such mobility in pregnancy, and that when movements are observed in these joints a morbid condition exists. Founding upon what has just been stated as to the condition of the ligaments of the pelvic articulations in the latter part of pregnancy, we can, without difficulty, assert that at that time the pelvic bones enjoy freer and more extensive movements than at other times. In very numerous cases, scattered through obstetric literature, where these joints have been examined after delivery, authors have described the mobility of these articulations ; sometimes, indeed, as being to a very great extent. In addition, cases are not very unfrequent where these movements, either from their freedom and extent, or from their causing pain, attract the attention of the patient and physician. They are then sometimes easily perceived on making the proper examinations.

The movements which occur may be described as follows in regard to certain motions of these bones in difficult labours, and under the influence of the pressure of the presenting part. For instance, Madame Lachapelle mentions a case where one ilium became dislocated forwards upon the sacrum, so as to enlarge the oblique diameter of the pelvis, through which the head was passing. Others have described a similar dislocation of both bones simultaneously. A case is recorded by Breit. See Luschka, *loc. cit.* See also Smellie, *Collection of Cases and Observations in Midwifery*, vol. ii. p. 4 ; and Hyrtl. *Topograph. Anat.*, Bd. ii. S. 14.

consisting in the elevation and depression of the symphysis pubis, the ilia moving upon the sacrum ; or if the sacrum be regarded as the moving bone, it describes a nutatory motion upon an imaginary transverse line passing through the second bone. By the elevation of the symphysis pubis (or nodding forwards

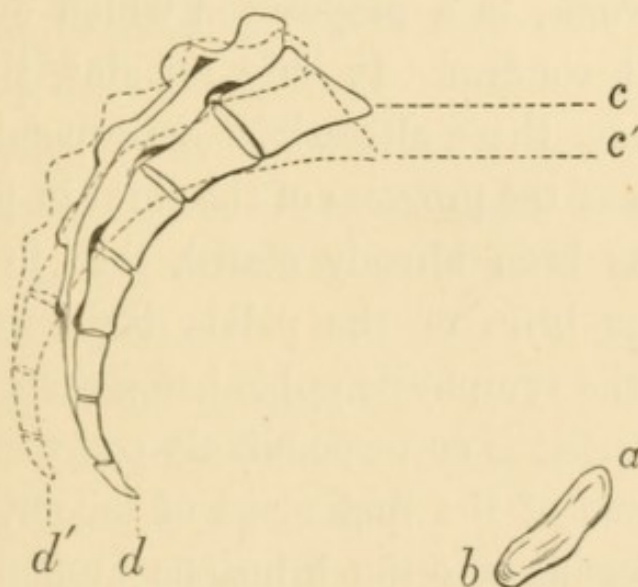


Fig. 6.

of the promontory), the angle of inclination of the pelvis is lessened, and the conjugate diameter of the brim of the pelvis is diminished to the extent of one or even two lines (Fig. 6) ; the corresponding diameter also of the outlet is increased, probably about twice as much. This different ratio of the effects of the motion upon the brim and outlet results from the

Fig. 6. Showing the nutatory motion of the sacrum in parturition. *ab* is the symphysis pubis which is supposed to be fixed. *cd* is the sacrum in its ordinary position. *c'd'* is the sacrum at its extreme of nutation, the promontory being advanced forwards, and the apex correspondingly projected backwards, so as to enlarge the outlet.

fact of the centre of motion being much nearer the promontory than the apex of the bone. The promontory, therefore, will describe an arc of a smaller circle than the apex.¹

That the alteration of the dimensions of the brim and outlet by these movements is not insignificant, but the reverse, is a proposition which every obstetrician will confirm. It only remains, then, to be observed how these alterations correspond with the phenomena of the progress of the child in parturition. Now it has been already stated, that in the erect position the brim of the pelvis is in its enlarged condition, the symphysis pubis being then depressed, while the outlet is correspondingly contracted. Now in the course of the first stage of labour, while the head is pressing into the brim, the human female is generally standing, sitting, or lying on her back, or in an easy position. But as soon as the head has descended into the pelvis and impinged upon the sensitive vagina, then forcing efforts accompany the pains. These forcing efforts consist, in great part, of powerful contractions of the anterior abdominal muscles, the effect of which, especially the action of the two recti muscles, will be to tilt up the symphysis pubis, thus throwing the promontory forwards, contracting the

¹ This motion of nutation of the sacrum is not described by Luschka, *Virchow's Archiv*, Bd. vii. 1854, nor by Schwegel, *Monatsschrift für Geburtsk.* 1859. For a diagrammatic representation of this motion, see Wood, article "Pelvis," Todd's *Cyclopaedia of Anat. and Phys.* supplementary volume, p. 145.

brim, and enlarging the outlet, and diminishing the angle of inclination of the pelvis. To all these changes the position usually assumed by the female in the second stage of labour will contribute. For it has already been stated that the simple bending of the body forwards has for its effect the tilting upwards of the apex of the sacrum and enlarging of the outlet. And it is a curious fact, that a woman in her forcing pains, in the second stage, is found to draw up her legs, and bend her body forwards, thus inducing changes in her pelvis which facilitate the advance of the child in that stage.¹

The motions of the pelvic bones, which we have been detailing, agree exactly with those which take place in the cow in parturition. In that animal the first effect of each pain is to elevate the tail, and thus enlarge the outlet for the escape of the calf.

The mechanism we have just been describing in the human female is analogous to that which we have previously shown to occur in the pregnant and parturient cow. The changes which occur at the time of labour in the guinea-pig find their analogues in the altered conditions of the symphysis pubis in the human female. But in her they are only to a small degree comparatively. It is important, however, to

¹ Another motion is described by Laborie, namely, separation of the ischiatic tuberosities. But it appears to me to demand further inquiry, before being accepted as true. See *Gazette Hebdomadaire de Med.*, No. 34, 1862.

remember, that in this joint the thickening of the ligaments is generally more apparent than in the sacro-iliac joints. The distension of the pubic ligaments will be easier in the recumbent than in the erect attitude, which last implies strong compression of the tissues of the joint by the pubic bones. Moreover, the separation of the thighs, which is habitually practised at the latter part of a labour, will favour any possible slight separation of the pubic bones, especially if the internal femoral muscles are in a state of contraction while the thighs are apart. Indeed, the study of the whole subject illustrates beautifully how nature leads the human female, in the act of childbirth, to assume positions and make exertions which are necessary for perfecting the mechanism of the process.

There is another source of information as to the state of the pelvic joints, especially the sacro-iliac, namely, the results of section of the symphysis pubis, an operation which appears to me to have been prematurely abandoned. In the performance of this operation upon the living female in order to aid in delivery, it has been found that the pubic bones can be separated to the extent of from one and a half to two or even three inches, without any damage to the sacro-iliac joint, or with the result of merely lacerating the capsular fibres of the anterior part of the articulation. In many of the cases the pubic bones, after the division of the symphysis, seemed to part from one another with resiliency, as if their union

counteracted some force tending to separate them. This phenomenon admits of two explanations : either by attributing it to the weight of the limbs acting upon the acetabula, or by ascribing it to the elasticity of the great mass of elastic fibrous tissue in the posterior and upper parts of the sacro-iliac articulation, which is not resisted by the different mode of union in the anterior and lower part of the joint as already described. In the pelvis of the male and non-pregnant female, when this operation is performed after death, it is found that a separation of from one, in some cases, to even two inches can be effected without injury. These facts illustrate the relaxation of the sacro-iliac joints at the end of pregnancy. They are, however, perhaps of more importance in regard to symphyseotomy, as showing the amount of separation that may be produced without injury. But the dread of injury is probably, in the main, a groundless fancy ; for we know that to destroy the sacro-iliac joint, after section of the symphysis pubis, it is necessary to use great force, a circumstance which is, in itself, the best guardian of the safety of the joint.

The operation of symphyseotomy, as reintroduced to the profession in 1768 by MM. Sigault and Le Roy, is one which has, with justice, been condemned. But the jealousy of the Academy of Surgery, which discountenanced M. Sigault's operation at the first, led the members, after the subsidence of the excitement produced by its first and only occasional suc-

cesses, to repeat their condemnations of it, and prevented its obtaining a fair consideration. It yet remains to be seen whether the operation, as more broadly proposed, long before Sigault, by Severin Pineau,¹ may not be one which is destined to have a place among the operations of practical midwifery, devoted to saving the life of the unborn child. In this country, the operation received, after its proposal by Sigault and Le Roy, the high sanction of W. Hunter and Denman, so far as its own peculiarities were concerned. But they, at the same time, showed that it could be of very little, if any, service in the cases for which it was proposed, namely, those of extreme pelvic distortion where Cæsarean section would otherwise be required. In this condition matters have been allowed to rest. British obstetric authors have loaded the operation itself with calumnies which are quite unfounded, and raised difficulties about it which are sufficient to deter a superficial inquirer from its consideration.

There is every reason to believe that the operation in itself is one of slight danger at the time, or even ulteriorly, if compared with the dreadful results of craniotomy and Cæsarean section. For the latter operation it can very seldom be a substitute. But it remains to be seen whether the former—namely, craniotomy—may not in some cases be superseded by

¹ Baudelocque's *System of Midwifery*, Heath's Transl. vol. iii. p. 238.

it. There is every reason to think that the operation would be much less dangerous to the mother than craniotomy, even with the allowance of great freedom in the selection of cases ; and it would give a chance of saving the child, whose life is necessarily compromised by that proceeding. Moreover, the operation might probably be simplified by adapting to it the subcutaneous method, and other improvements.

I conclude these remarks with the following quotation from the most esteemed author in British obstetrics, whose name and influence have contributed greatly to the neglect into which the operation has fallen :—

“It is proved,” he says, “in the first place, that some enlargement of the capacity of the pelvis is actually obtained by dividing the symphysis of the ossa pubis.

“Secondly, that the evils which have followed this operation have been very much occasioned by its being performed unskilfully, or by injudicious endeavours to increase that enlargement of the capacity of the pelvis beyond the degree which naturally follows the division of the symphysis.

“Thirdly, that many women who have undergone this operation have recovered ; though of those who recovered, many suffered very serious complaints for a long time, or for the remainder of their lives.

“Fourthly, that some children were born living when this operation was performed.

“We may, therefore, presume to say that if a case could be so precisely marked that there should only be a deficiency of just so much space as would be supplied by the simple division of the symphysis, the operation might in that particular case be considered.¹

“We may also say, that this operation is not so certainly fatal to those women on whom it may be performed as the Cæsarean operation; nor so certainly destructive of children as that of lessening the head.

“We may, then, be allowed to suppose a case, and such a one is more than possible, in which a person of very high rank, the life of whose child might be of the greatest public importance, could not be delivered without the destruction of the child, or her child be preserved but by the Cæsarean operation at the expense or great hazard of her life; and that she, through human frailty, might refuse to submit to the Cæsarean operation, yet the great interests and policy of the nation might forbid the destruction of the child. Of course both the mother and child would be inevitably lost. Should such a case occur, which,

¹ “The confidence of M. Sigault in this new method was so diminished in the latter periods, that he declined performing it when the pelvis had not at least two inches and a half in the small diameter of its entrance.”—Baudelocque’s *System of Midwifery*, Heath’s Translation, vol. iii. p. 242. For Baudelocque’s and Lachapelle’s views of the utility of the operation, see Lachapelle’s *Pratique des Accouchements*, tome iii. pp. 428, 430, etc.

as I said before, is more than possible, then the section of the symphysis of the ossa pubis might be proposed and performed, as it would in some measure meet both these interests ; being less horrid to the woman than the Cæsarean operation, and, instead of adding to the danger, give some chance of preserving the life of the child."

This testimonial from the eminent and sagacious Denman is the more extraordinary, as he is an author who joins strongly in the cry against the operation, and expressly says, in regard to the above passage quoted from his own work on Midwifery, that he does not "mean to insinuate a wish or advance an argument in favour of this operation, in the cases for which it was originally proposed, or any other which can be imagined."

The last paragraph of the passage just quoted gives in few words a general notion of the cases to which this operation may yet be adapted. But it must be remembered that, in our day, a section of this class of cases has already been provided with a suitable treatment in the operation of premature labour ; an operation, however, whose use is not inconsistent with the simultaneous use of symphyseotomy.

CHAPTER XI.

THE OBLIQUITY OR LATERAL FLEXION OF THE FŒTAL HEAD.¹

THE object of this paper is to show that the obliquity, or lateral obliquity, of the fœtal head when passing through the brim of the pelvis, described by Nægele, by some of his predecessors, and by his followers down to the latest authors, does not exist in natural parturition ; and that obliquity, or lateral obliquity, of the fœtal head when passing through the outlet of the pelvis, not described by Nægele and his followers, does occur in natural parturition.

This word, obliquity, and the alternative expression, lateral obliquity, are terms requiring, for most readers, some explanation ; their meaning certainly not lying on the surface. The words ordinarily used to express the state, considered as a fœtal attitude, seem to me far more appropriate, and scarcely susceptible of being misunderstood—viz. lateral flexion—a term which Stoltz has applied to it.

I have for many years ceased to teach in my class-room the doctrine of Nægele on this subject. At the same time I need scarcely assure my professional brethren that it is with the greatest diffidence

¹ See *Edinburgh Medical Journal*, August and September 1861.

that I venture to adduce views opposed to those of Nægele, of Dubois, and of subsequent authors too numerous to mention. It is for the profession to decide whether truth lies with them or with me.

But I am not alone and unsupported in the views I entertain. Many authors, indeed, enter at length into descriptions of, and arguments for, Nægele's opinions ; but I know of none who has taken up the opposite side with any fulness. At the same time, I have, since writing my first paper on this subject, found, on reference to the works of Velpeau,¹ Cazeaux,² and of Dr. R. U. West,³ that these gentlemen have reached conclusions similar to my own regarding the direct entrance of the foetal head into the pelvis.

Since my views on this subject were first published in 1861, there has appeared a treatise on the mechanism of parturition by Dr. W. Leishman, which contains what appears to me to be far the best exposure of the errors of Nægele, and the best defence of the views which I entertain. Dr. Leishman has almost justly characterised my statement as wrong, when I said in the place referred to that "obliquity, or lateral obliquity, of the foetal head when passing through the outlet of the pelvis, not described by Nægele and his followers, does occur in natural par-

¹ *Traité Complet de l'Art des Acc.* Bruxelles, 1835, p. 250.

² *Traité de l'Art des Accouch.* 1858, p. 423.

³ *Glasgow Medical Journal*, 1857, p. 304.

turition." To make my statement quite correct, I should have inserted the words "as such" into the above quotation, making it "not described as such by Nægele and his followers." With this correction I reassert my former statement. I knew, indeed, that Nægele's essay cannot be understood without supposing he believed there was lateral flexion at the outlet, in a direction opposite to what is supposed by him to occur at the brim; and Dr. Leishman, even, admits that in the part of Nægele's essay referring to this point he is unfortunately a little vague in his details—a fault, indeed, which is scarcely to be found elsewhere in his admirable essay.¹ It appears to me that I had been, so far, misled by desire to state the point antithetically.

Hubert seems to hold an opinion midway between Nægele and that here supported. "The head," he says,² "is slightly inclined in such a manner that the anterior parietal bone is found a little lower than the other, not only as regards the horizon, but also as regards the plane of the superior strait of the bony pelvis." . . . "The two opinions (Nægele's and Cazeaux's) appear to us both admissible in given cases; a head of little bulk, a large pelvis, an inferior segment of the uterus well hollowed out, a uterus well placed in the axis of the upper strait, permit the

¹ *An Essay, Historical and Critical, on the Mechanism of Parturition*, p. 81.

² *Mém. de l'Acad. Roy. de Méd. de Belgique*, 1857, p. 118.

vault of the cranium to present itself perpendicularly to the entrance of the pelvic canal ; whilst a pelvis not very spacious, a narrow resisting inferior segment of the uterus, a bulky head, a prominent belly, will force the cranium to become inclined, and to offer to the plane of the upper strait the subparieto-superparietal diameter, instead of the biparietal properly so called."

Nægele's teaching is to be found in his original paper, "Über den Mechanismus der Geburt," published in 1819 in *Meckel's Archiv für die Physiologie*. I shall quote from Dr. Rigby's translation of it, published in London ten years afterwards. Before doing so, I must premise that the quotations refer to the first or commonest position of the head in labour, and that in the whole of this paper I shall suppose this position to be understood. The doctrines apply to the other positions as well as to the first, and if proved or disproved for the one, stand or fall for the others.

"At the entrance of the pelvis (says Nægele), the head does not take a perpendicular, but a perfectly oblique direction, so that the part which lies lowest or deepest is neither the vertex nor the sagittal suture, but the right parietal bone. The sagittal suture is much nearer to the promontorium of the sacrum than to the os pubis, and divides the os uteri, which projects backwards, and generally somewhat to the left, across into two very unequal segments."—
(P. 13.)

“The higher the head is . . . the more oblique is its direction; from which reason the right ear can generally be felt behind the pubes without difficulty, which would not be the case if the head had a perpendicular direction.”—(P. 16.)

“On account of the oblique position of the head, the greatest width of the cranium (from one tuber parietale to the other), as well as that of its basis, can never, during its passage, coincide with the diameters of the pelvic entrance.”—(P. 19.)

In further illustration of this doctrine, I shall quote the account of it in Tyler Smith's *Manual*, a late British systematic work on midwifery:—

“The right side of the cranium is considerably lower than the left, so that the most depending part of the cranial surface is the protuberance of the right parietal bone. This lateral depression is called the obliquity of the head.”—(P. 268.)

“The movement of the fœtal head upon its occipito-frontal axis. . . . This movement causes one side of the fœtal head to become lower than the other during the whole progress of labour, after the head has entered the brim, constituting the obliquity of the head.”—(P. 274.)

If it be recollected that the axis of the child's¹ body, the axis of the uterus, and the axis of the brim of the pelvis, are represented by the same line in the

¹ For a discussion of the views of Schultze and Schatz as to the non-identity of these axes, see chapter iii., p. 39, of this book.

normal or standard condition, or very nearly so, then it is easy to see that this obliquity implies lateral flexion of the child's head, or the approximation of its left ear to its left shoulder. In respect of this lateral flexion, the child's attitude is thus represented as being changed from that maintained in utero before labour. The position of the child's head, as described in this changed attitude, is oblique; that is, the vertical axis of its head is said not to impinge upon the plane of the brim at right angles to it, but obliquely, forming an acute angle opening forwards and pointing backwards.

I do not believe that, in normal or standard conditions, any such lateral flexion or obliquity exists, but that Nægele and his followers are in error in this particular, and that the head enters the brim without any lateral flexion, and directly—that is, with its vertical axis at right angles to the plane of the brim.

I. The first and chief reason for denying the existence of obliquity of the fœtal head at the brim of the pelvis is, that it is not observed. I have been in the habit of carefully making out the position of the fœtal head in the brim of the pelvis at the beginning of labour; and although I have observed that it varies to a slight extent in different cases, I have satisfied myself that it enters the brim, very generally, directly and not obliquely.

The obliquity which is described has never been

seen. It is only a supposed result of the observation of many particulars in many cases ; and the observations are of a kind demanding great information and carefulness. The liability to be misled by preconceived theories is very great ; and I cannot help thinking that the obliquity under discussion is the fruit of a mind powerfully impressed with the important part that obliquity undoubtedly plays in other departments of the mechanism of parturition.

I have not found the sagittal suture nearer the promontory of the sacrum than to the symphysis pubis. I have not found more of the right parietal bone approachable by the examining finger than would be so if the head entered the brim without obliquity. I have not found the parietal protuberance lying near the centre of the brim of the pelvis, nor approaching to it. I have not found the right ear of the child easily reached, while the vertex of the child was at or near the brim of the pelvis.

It is needless to pursue a career of assertions opposed to the statements of the believers in the obliquity. I shall proceed to evidence corroborative of the position I maintain. But before doing so, I shall simply mention that in cases in which, for various reasons, I have introduced my hand, and felt the whole head as it lies in the brim, I have not found the obliquity under discussion. This is valuable evidence, especially in cases where there is

no deformity of the head or structures in the neighbourhood.

Leishman, in his work already referred to, has added another valuable evidence in favour of the belief that the sagittal suture of the head entering the brim of the pelvis is midway between the promontory and the pubes (p. 70). "For my part," says he, "I have left no means untried by which this might be tested. On introducing an instrument which is well known to surgeons as Professor Buchanan's rectangular staff for lithotomy, I have been able to place the angle on the second bone of the coccyx, inclining the short limb until it coincided, as nearly as I could guess, with the axis of the brim, when it never failed to guide me, if properly placed, to the sagittal suture, or some point very near it, on either side. I have even attempted a crucial experiment by measuring, by means of a flexible scale, the distance from the sagittal suture to the promontory of the sacrum on the one hand, and the pubes on the other; and although, for obvious reasons, the results were not so accurate as to warrant of themselves any definite conclusion, they certainly tended to confirm my belief."

Küneke has on this point the following remarks corroborative of my views,¹ whose intended force, however, in some subsidiary points, I do not admit. "Duncan (says he) investigates with the whole

¹ *Monatsschrift für Geburtsk.* Bd. xxvii. 1866. S. 30.

hand, and Leishman uses, for measuring, Professor Buchanan's rectangular staff for lithotomy. I hold both methods as at least superfluous, and am almost always satisfied with the method of examination which Michaelis has taught us for measuring the diagonal conjugate—namely, by means of the simultaneously introduced middle and index finger. And here, if the fingers are only placed right in the axis of the pelvis, it becomes evident that the aspect of the sagittal suture as stated by Nægele is only apparent, and that in reality it stands at equal distance from the promontory and the symphysis."

II. The descriptions of the oblique position of the head given by Nægele and others are more truly applicable to its direct than to its supposed oblique position. This will be best shown by an examination of Nægele's remarks bearing on this matter.

"*The finger,*" says he, "*which is introduced in the direction of the central or middle line of the pelvic cavity, and brought in contact with the head, will touch the right parietal bone in the vicinity of its tuber.*" It is to be remarked, *firstly*, that the finger is not introduced in the central line of the pelvic cavity, but only in the direction of it; *secondly*, that it is not made evident that the head is to be touched or approached in the direction of the axis of that part of the pelvis where the head is lying—that is, in the axis of the brim. The finger may approach the head

in the direction of the axis of the brim, but it is scarcely long enough to do so in the axis of the brim, or as nearly in it as, in the circumstances, can be guessed. The consideration of these points shows Nægele's statement to be so vague as to be without much value in this discussion. But it will be evident to all that the quotation cited is perfectly true, on the supposition that the head enters the brim directly, not obliquely.

To bear satisfactorily upon this question, Nægele's statement should have been to the effect, that the finger approaching the child's head in the axis of the brim, or where the imaginary axis of the brim passes through the surface of the vertex, touches the tuber of the parietal bone or its vicinity. Now, while I admit that the finger introduced in the direction of the axis of the brim, as may be done in vaginal examinations, touches the cranium near the tuber parietale, I assert that, on the other hand, the finger, carefully introduced in the axis of the brim, touches the cranium in or near the sagittal suture, and at a point in its length varying according to the degree of flexion of the head. This is what Leishman effects by using Professor Buchanan's rectangular staff.

“ At the entrance of the pelvis (says Nægele) the head does not take a perpendicular, but a perfectly oblique direction, so that the part which lies lowest or deepest is neither the vertex nor the sagittal suture, but

the right parietal bone." Now, it is evident that, at the entrance of the pelvis, the head does not take a perpendicular, but a perfectly oblique direction. It does so because the whole child is lying obliquely ; and to enter the brim of the pelvis directly—that is, in the direction of the axis of the brim—it must take a perfectly oblique direction. If it took a perpendicular direction—that is, a direction at right angles to the horizon—it would necessarily enter the pelvis with obliquity ; but it takes an oblique direction in order to enter the opposed oblique brim of the pelvis directly, that is, in the direction of its own axis and of the axis of the brim.

Further, as Nægele says, the part which lies lowest or deepest is neither the vertex nor the sagittal suture, but the right parietal bone. All this is a necessary consequence of the direct entrance of the head. No doubt it may also be a consequence of the oblique descent of the head, but its occurrence is no proof of the obliquity. The direction of the head entering the brim is nearly that of a line striking the horizon at an angle of 30 degrees. This is a very considerable obliquity to the horizon, but is perpendicular to the brim of the pelvis, which is inclined to the horizon at an angle of 60 degrees. To enter the brim of the pelvis obliquely, the child's head must advance horizontally, or in a line of direction striking the horizon at an angle less than 30 degrees. Nægele does not say that the part which lies lowest

or deepest in the brim is the right parietal bone. If he intended to say that, I am at issue with him; and appeal to observation in support of my assertion, that both parietal bones enter and pass the brim simultaneously, both being in the plane of the brim at the same time.

“*The sagittal suture (says Nægele) is much nearer to the promontorium of the sacrum than to the os pubis, and divides the os uteri, which projects backwards, and generally somewhat to the left, across into two very unequal segments.*” The position of the sagittal suture in regard to the promontory of the sacrum cannot be discovered by an examining finger, the parts being too distant to be reached in that way. When the hand is introduced into the vagina to feel the whole relations of the parts before the foetal head has passed the brim, the sagittal suture is not found nearer the promontorium than to the pubic symphysis. I am thus, at this point, quite at variance with Nægele, and I may venture to point out the cause of his error. It is my opinion that it has arisen from not making the observations relied upon, while the foetal head was at the brim of the pelvis, and then only; for, after the head has passed the brim and entered the pelvic cavity, the sagittal suture is generally found nearer to the sacrum than to the pubes; and this is not very unfrequently observed even before the os uteri is much dilated, or the labour has been long continued. This approximation

of the sagittal suture to the sacrum arises from the descent of the head in the axis of the brim, which coincides with the axis of nearly the whole upper half of the bony pelvis. This axis, when prolonged, strikes the sacrum at or near its point. The foetal head has a tendency to advance in this axis, and does so till it is arrested by the posterior wall of the pelvis. While it is passing between the symphysis pubis and the two upper bones of the sacrum, it has the sagittal suture equidistant between them: but afterwards, and until the head begins to advance more or less forwards, the sagittal suture approaches to the sacrum, as it descends in an axis which leads it in that direction.

The last part of the quotation just given from Nægele has very little value, for two reasons—viz. because the situation of the os uteri is far from being fixed or invariable, and because no observations have been made even as to what is its most ordinary position with minute exactness. Nægele himself mentions a frequent deviation to the left, which has not received much notice from subsequent authors. While the head is at the brim of the pelvis, it is difficult to examine satisfactorily the relations of the sagittal suture to the moderately dilated os uteri; and the greater facility of reaching and examining the right and anterior than the left and posterior parts of the head naturally leads, and I believe has led, to exaggeration of the extent of the anterior half over

the posterior half of the part corresponding to the circle of the os uteri. But it is out of place to pursue this particular point further, because its settlement would prove nothing, and lead only to the further question of the exact position of the os uteri in early labour, for the ascertainment of which we have no satisfactory data.

“The higher the head is (says Nægele) the nearer its long diameter corresponds to the lateral diameter of the pelvis, and the more oblique is its direction; from which reason the right ear can generally be felt behind the pubis without difficulty, which would not be the case if the head had a perpendicular direction.”

In regard to the increase of obliquity according to the height of the head, I have not one word to say, except that it is a mere statement on Nægele's part, unaccompanied by any corroborative evidence. As I do not believe in the obliquity at all, I can find no place for this refinement.

Nægele's assertion regarding the right ear is quite as much in accordance with the theory of the direct entrance, or entrance in a perpendicular to the brim, as with his own theory of obliquity. I must confess myself completely at a loss, however, as to the full bearings of his argument; for I know well that under no circumstances is the right ear felt behind the pubes without difficulty, and that its position when felt, and the forcing of the finger between the pubes and the head in order to reach it, indicate the direct, not the

oblique, entrance of the fœtal head into the brim of the pelvis.

It is worth while here to remark that Stoltz, who supports the doctrine of Nægele, takes care to guard himself from being held as agreeing with the Heidelberg professor in his statement that the higher the head the more oblique is its direction. He says,—
 “Je n’ai jamais trouvé, malgré toute mon attention, que la tête fût d’autant plus inclinée qu’elle était plus haute ; encore moins ai-je jamais pu sentir l’oreille du fœtus aussi longtemps que la tête était au-dessus du détroit supérieur.”¹

III. The third reason for rejecting the theory of obliquity at the brim of the pelvis is based upon a careful study of the production of the caput succedaneum, and of the relations of this swelling to the presentation,—a subject in regard to which much has been assumed without evidence, or in defiance of it.

“Under certain circumstances (says Nægele), a swelling of the integuments of the head frequently forms soon after the os uteri has begun to dilate. . . . This swelling is situated upon the right parietal bone, close to its upper edge, and equally distant from both angles : a small piece sometimes extends over the sagittal suture unto the other parietal bone ; its circumference depends upon the degree of dilatation which the os uteri

¹ *Considérations sur quelques points relatifs à l’Art des Accouchements*, p. 20.

had attained." This statement of facts by Nægele may be added to, but cannot, I believe, be controverted. The caput succedaneum of the first stage of labour can be felt and seen to be as described in the passage. The statements in the quotation immediately preceding this last, and in others, are in quite a different position, involving points not of easy observation, and in regard to which there are manifest inducements to error. But, while I agree entirely with Nægele's statements in this last passage, I would make two additions thereto, of which only the second has an important bearing on the subject of this memoir.

First, It is a condition necessary for the formation of a true caput succedaneum, that the liquor amnii be evacuated, or that it be in such minute quantity as to have no hydrodynamical properties. *Secondly,* While the extent of the caput succedaneum of the first stage may be limited to the right parietal bone, it is generally so limited only when it is not well developed. When it is well developed, it is found to extend over the upper part of the left, as well as of the right parietal bone ; but its greatest thickness is, as a rule, always in the portion overlying the right parietal bone. Nægele himself mentions the extension of the swelling over the left parietal bone as an occasional occurrence of which he gives no explanation.

Before further advancing, it is necessary to inquire what evidence is derivable from the caput succedaneum. In answer, it is certain that it only in-

dicates what was the unsupported part of the head,—in the present instance, what part lay over the os uteri. And as the position of the os uteri is uncertain, and denotes nothing exact topographically, so the position of the caput succedaneum will denote nothing exact topographically, or relative to the position of the foetal head in the brim of the pelvis. For a fuller development of this subject I beg to refer the reader to a following chapter.

But, before leaving this point, I think it advisable to show that, supposing, as Nægele seems to do, that the os uteri occupies exactly the centre of the brim (except slight deviation to the left), and that the caput succedaneum formed on the part of the head lying over it marks the part lying in the centre or axis of the brim, the indications afforded by this swelling are not truly read off; and that, if truly read off, they indicate direct, not oblique, entrance of the head into the brim.

The caput succedaneum of the first stage of labour is often formed after the head has passed the brim of the pelvis, and is lodged in the upper half of the cavity of the bony pelvis. Were we to be cautious and exact in reasoning, all such swellings should be excluded from the argument, for evident reasons. It is only those formed at the plane of the brim, or very near it, that can, under any circumstances, afford assistance in settling this question. Under the actual deficiency of exact data, we must be content with

stating principles. Now, it is evident that the direction of the caput succedaneum of the first stage will be that of least resistance—that is, the direction of the axis of the undilated vagina ; in other words, the caput will be thickest where the head is least supported, and may, in other parts within the circle of the os uteri, be so inconsiderable as not to attract notice. Further, and for the same reason, the centre of the caput succedaneum, or the centre of the os uteri, will not correspond with the thickest portion of the swelling, but in this case be behind it, or nearer the left parietal bone. The oblique direction downwards and forwards of the vagina will lead the caput in that direction ; and the support given by the posterior wall of the vagina to the posterior half of the space enclosed in the circle of the os uteri will cause thickness of the swelling over the right, and comparative thinness over the left parietal bone, and displacement of the thickest portion of it forwards in the pelvis—that is, in the direction of the right parietal and away from the left parietal bone.

IV. A very cogent argument against the existence of this lateral flexion of the child's head, or obliquity to the plane of the brim, at the commencement of labour, is derived from the impossibility of finding a mechanism to account for it.

If the membranes are still entire, and there is present any considerable quantity of liquor amnii, and

if the axis of the child and uterus are parallel to or identical with the axis of the brim, all which conditions are usually found, it is impossible to conceive any cause of the obliquity but a spontaneous lateral flexion of the child's head ; and I daresay no obstetrician will support so extraordinary a doctrine as that the child should, without any discoverable cause, and I may add without any desirable object, bend its head towards its left shoulder as it begins to pass the brim.

If the liquor amnii has been evacuated before the fœtal head has entered the brim, or if the liquor amnii be very scanty in amount, then forces produced by the pains, or the lower parts of the womb and adjacent structures, may be imagined to act directly upon the child, and cause the obliquity. But although they may be imagined, they only rarely, I believe, exist.¹ If the uterus, for example, became, during pains, more nearly horizontal than it ordinarily is, or quite horizontal, then this grave anterior obliquity of the uterus would probably cause the head to present indirectly or obliquely at the brim. But the opposite of this is observed in nature. The uterus, during a pain, becomes, as it were, erect, and to a certain extent corrects any anterior obliquity it may have during relaxation, becoming, when in contraction, nearly perpendicular to the brim of the pelvis,—that is, occupying its axis. It is certain then, that no anterior uterine obliquity is observed, which might account for the

¹ See p. 48.

obliquity of the head at the brim. And it is necessary to remark that the obliquity at the brim, if supposed to be produced by anterior obliquity of the uterus, would not be accompanied by lateral flexion of the head as a change in the foetal attitude. The flexion of the foetal head which is so often observed in the early part of labour is easily accounted for by the circumstance that the fulcrum of the head—the spinal column—is nearer the occiput than the sinciput; and all forces acting equally on the various portions of the vertex, act with advantage on the sinciput, as it is at the end of a longer lever than the occiput. But in the case of the sides of the head, the right against the left tuber parietale, no such inequality is observed.

In short, no mechanism has been devised to account for the phenomenon, and it is a vain pursuit to seek it, at least on my part, as I deny the existence of what is to be accounted for.

I must not omit, however, to state that Stoltz has suggested an explanation of the occurrence of lateral flexion. But to me it is so unsatisfactory as not to lead me to modify the statements I have made. His words are—

“Mais quand les contractions de la matrice sont fortes et qu’elles agissent puissamment sur le corps qu’elle renferme, quand la tête s’engage dans l’entrée du bassin, alors son inclinaison devient plus grande; la suture bipariétale s’éloigne peu à peu du centre du

détroit, et se retire en arrière et en haut ; la bosse pariétale droit se découvre presque entièrement, principalement à l'endroit où elle correspond à l'angle postérieure et supérieure de cet os, et cette flexion latérale de la tête ne fait qu'augmenter jusqu'au moment où elle va franchir le détroit supérieur. Voici comment je m'explique cette disposition. Au commencement du premier temps du travail les contractions n'agissent pas encore fortement sur le fœtus ; il peut conserver sa position droite ; mais quand une fois la tête s'engage et éprouve une grande résistance, le col, qui est la partie la plus faible du tronc, cède ; les contractions n'agissent plus d'après une ligne droite, qui, du centre de l'extrémité qui se trouve au fond de l'utérus, irait à peu près au milieu du sommet, ce qui incline la tête de côté, et fait qu'on n'atteint plus alors qu'avec plus ou moins de difficulté la suture sagittale qui est dirigée en arrière. C'est alors que, située obliquement par rapport au détroit du bassin, elle présente la pariétale plus bas que la suture sagittale elle-même." ¹

In the first part of the third chapter of this book (p. 43), I have suggested an explanation of the occasional occurrence of Nægele's obliquity, founded on the occasional existence of a posterior uterine obliquity of slight degree, such as Schatz and Schultze allege to exist. But as this posterior obliquity of

¹ *Considérations sur quelques points relatifs à l'Art des Accouchements*, p. 20.

the active uterus does not, in my opinion, exist in ordinary circumstances, it cannot be held as accounting for the doctrine of Nægele. It is easy also to understand how this Nægele obliquity might exist in the very beginning of labour if there were great and morbid anterior obliquity of the inactive uterus.

V. Assuming that the foetal head enters the pelvic brim obliquely, Nægele claims for this condition a mechanical advantage over the direct entrance. "*On account,*" says he, "*of the oblique position of the head, the greatest width of the cranium (from one tuber parietale to the other), as well as that of its basis, can never, during its passage, coincide with the diameters of the pelvic entrance.*"—(P. 19.)

It is necessary, *in limine*, to state that discussion on this point, and conclusions in regard to it, can lend no aid to the settlement of the question under consideration here. It is, indeed, quite a work of supererogation to consider at all the advantages presented by an oblique or direct entrance of the head into the pelvic brim, until the previous question be settled, whether the entrance be direct or oblique.

The position apparently offering the greatest mechanical advantages is not always adopted by nature. Mechanical difficulties seem in various points to be sought, instead of mechanical advantages. The whole process of labour is, indeed, beset with difficulties, one of whose objects is, without doubt, to prevent its too

easy and rapid accomplishment. One example, germane to the subject of this article, I may adduce from the entrance of the foetal head into the pelvic brim. It is well known that its entrance, considerably flexed, has a great mechanical advantage over its entrance slightly flexed or not flexed at all; yet, in spite of this mechanical advantage on the side of the greater flexion of the head, we find that it generally passes the brim slightly flexed or not flexed at all.

Nægele places the mechanical advantages of the supposed obliquity entirely in the dimensions presented to the plane of the brim by the transverse diameters of the cranium and of its basis. In including the basis in his statements, he is decidedly wrong. He would, indeed, appear to forget that the foetus has a neck, the addition of which to the basis, even when the head is laterally flexed, makes the direct entrance of the basis mechanically the most advantageous, and that so evidently that it is really needless to do more than assert it.

There is no doubt that if the foetal head passes the brim directly, the greatest biparietal diameter (from one tuber parietale to the other) passes it, and that, if the head enters the brim with obliquity, a smaller biparietal diameter supplants the greatest,—that is, in the first position, a diameter measured from below the right tuber to above the left. But Nægele seems, for the moment, to have forgotten that the long diameter of the foetal head is not in the

transverse diameter of the brim, but in an oblique diameter of this part, and that consequently (as well as for other reasons), the diameters of the foetal head which he wishes to be compared are not the diameters it is necessary to compare, for they do not pass the smallest diameter of the brim. If mechanical advantage in the way of dimensions is to be of service in the mechanism, it must meet the difficulty,—that is, the gain in diminution of dimension must be in the part traversing the conjugate, or small diameter of the brim ; and this is not true of the gain spoken of by Nægele. But, in truth, no gain is desiderated in any natural case, and when the comparison of the oblique and direct diameters of the part of the head traversing the conjugate diameter, as suggested by Nægele, is made so as to be true to the mechanical conditions, it is found that no appreciable gain is got from obliquity. This comparison is too difficult to give in words so as to be useful to a reader. It is necessary to institute it with the foetal head and calipers in the hands in order to verify it.

There are, however, mechanical conditions of the laterally flexed head, or of the head presenting one parietal bone to the brim, which, if such a position were assumed, would lead to great and perhaps insurmountable difficulties in a labour. For, if the vertex was by any arrangement displaced from its position at the brim, as Nægele describes, it would tend always to be more and more displaced, till an

ear, or even a shoulder, descended. And if the canal of the uterus were rigid and contracted enough to resist such unnatural dislocation, the uterine efforts would be directed along the body of the child to its head, at a great disadvantage. Other mechanical evils might be suggested; but it is in vain to raise difficulties, which, if the object of my memoir is gained, are all chimeras.¹

¹ For some historical details, see Gerdy, *Archives Générales de Médecine*, 1832. Tome xxviii.

CHAPTER XII.

OBLIQUITY OR LATERAL FLEXION AT THE OUTLET.¹

WHILE I hold it proved that the child's head passes through the pelvic brim directly, I have, before leaving the subject, to point out that, after it has passed through the brim and upper half of the ligamentous pelvis, it does advance obliquely in its subsequent progress,—that is, the head of the child impinges on the planes of the parts of the pelvis through which it is passing, or on planes at right angles to the axes of the lower parts of the pelvis, not directly, but indirectly or obliquely. A mesial part of the fœtus does not first touch these planes, but a lateral point.

Leishman has pointed out that Nægele somewhat vaguely describes this lateral flexion. Stoltz, who adopts Nægele's view as to lateral flexion at the brim, describes it unmistakably. He says—

“Cette manière de se dégager du détroit inférieur, ressemble infiniment à celle suivant laquelle la tête s'engage et franchit l'entrée du bassin. Elle présente d'abord une des extrémités du diamètre transverse et une du diamètre antéro-postérieure, la bosse pariétale

¹ *Edinburgh Medical Journal*, September 1861.

et l'occiput, et seulement quand ces deux sont sortis, les deux autres s'engagent et sortent à leur tour : c'est la manière la plus favorable possible. Il est pourtant juste de dire que quelquefois il se fait un véritable mouvement de rotation, telle qu'on l'a décrit ; mais ce sont des exceptions, qui n'ont lieu que sous l'influence de causes particulières ; qu'il serait trop de détailler ici." ¹

In the first half of the head's course through the ligamentous pelvis, a point in or near the sagittal suture is the presenting point. There the caput succedaneum is formed. During this part of its course the head advances in the axis of the brim, which almost exactly corresponds with the axis of the upper half of the ligamentous pelvis. During all this time, if the head is not covered by the cervix uteri, the right parietal bone is the part first and easiest felt ; and the further it advances, the more is this the case. As it advances, and passes the first bone of the sacrum, the sagittal suture approaches nearer to the sacrum, or rather to its lower portions, and becomes more distant from the symphysis pubis. When the biparietal diameter of the head has reached the lower boundary of the upper half of the pelvis, it is arrested in its direct progress. The vertex impinges on the posterior wall of the pelvis, and in its further advance, the head, as a whole, must change the direction of its

¹ *Considérations sur quelques points relatifs à l'Art des Accouchements*, p. 25.

course. This change, of course, is too abrupt for the parts of the head to follow it perfectly. Moreover, there is no room in the pelvis for such a degree of lateral flexion as this would imply,—that is, as would be necessary to maintain the head presenting directly or synclitically to the plane of the pelvis through which it is now passing. While advancing at this point of its progress, the presenting part, therefore, is changed. It soon becomes the upper and posterior part of the right parietal bone, instead of, as before, a point in the mesial line of the head. With this point advancing in the axis of the pelvis, it is evident that the sagittal suture or mesial parts are far removed from it, and consequently that the head is passing through the lower half of the pelvis, the outlet, over the perineum, and through the vulva, more or less obliquely, and not directly. In accordance with this obliquity, the child's head is flexed laterally, or, to be more exact, flexed obliquely,—that is, bent not directly over the right shoulder, but in a direction midway between extension and direct lateral flexion. As it approaches the orifice of the vulva, and rotates so as to bring the occiput nearer to the pubes than it was in the earlier parts of its progress, this flexion gradually approaches nearer to extension; but it does not become direct extension, almost always maintaining an obliquity,—that is, a direction between extension and flexion.

Under two sets of circumstances, not observed in

ordinary labours, the presentation of the foetal head may be direct from the beginning to the end of the process. In the rare cases where the head enters the brim and passes through the whole pelvis with its long axis in the antero-posterior vertical plane of the passage, the head will offer itself not obliquely, but directly, in its whole course. The presentation, indeed, will only shift backwards upon the child's head as it descends, maintaining always a position in the mesial line. Again, it is possible that the foetal head may descend directly with its long axis in the transverse vertical plane of the pelvis, till it makes a complete quarter of a circle rotation, bringing it into a direct antero-posterior position. Such cases are not subjected to the ordinary laws of the mechanism of parturition.

It is to be remarked, then, that in the second half of its progress the head does not present directly, but obliquely, and that it is born with this obliquity. But this last obliquity is unlike the lateral obliquity at the brim, in being quite in accordance with Nægele's statement of the phenomena, though he omits to name it lateral obliquity; the obliquity which he names and describes in this portion of labour being the position of the long axis of the head in the right oblique diameter of the pelvis, not the oblique presentation of the head to the plane of the outlet, or other portions of the pelvis through which it passes in the latter parts of its course. Further, not only

is there observed this obliquity to the planes of the pelvis, or absence of synclitism, but there is a change in the attitude of the foetus simultaneously produced. The head is at first, in this second part of its course, laterally flexed to the right with a backward obliquity; and when passing the vulva this is slightly changed, the condition being one of extension, with a lateral obliquity to the right shoulder.

This obliquity of the child's head to the planes of the lower parts of the pelvic passages is not only observed, but is easily explained. In its descent, the head, if of its ordinary size, must follow the direction of the curved axis of the pelvis. It is possible to imagine the presentation continuing direct while the other parts of the mechanism remain unchanged; but there is no room in the pelvis for the great right lateral flexion of the head that would be necessary to maintain the presentation direct, and the mechanism does not demand it. A certain amount of lateral flexion is made, and this diminishes the obliquity. This moderate lateral flexion is not produced by spontaneous foetal motion, but by the powers of labour urging the child through a canal which at this part is rigid and contracted enough to force the soft foetus to adapt itself to its graduated curvature.

CHAPTER XIII.

ON THE SYNCLITIC MOTION OF THE FŒTAL HEAD.¹

IN an ingenious work recently produced at Berlin, by Dr. W. L. Kueneke, and entitled, "Die vier Factoren der Geburt. Grundzüge einer Physik der Geburt," several alleged phenomena in the mechanism of labour are for the first time described. Of these, the synclitic movement of the foetal head in its course through the true pelvis appears to me to be one of the most important. It is alleged to be followed by a quite opposite kind of movement of the head as it begins to distend the perineum, called the enclitic movement. It is my object at present to investigate this so-called synclitic movement, that it may be confirmed and accepted, if found to be true; and that it may be rejected, if found to be false, and the older views confirmed, elucidated, and explained. All this is particularly necessary, because the character of the progress of the foetal head in the true pelvis, whether synclitic or oblique, has never attracted the attention which it deserves. In the sequel, indeed, I hope to satisfy the student that Kueneke himself, while fully alive to the importance of the subject, has omitted to

¹ Read to the Obstetrical Society, April 27, 1870.

bring to bear upon its discussion one of the chief agencies influencing it, and some collateral results throwing light upon it.

In order to explain, first of all, what is meant by the synclitic movement of the foetal head in its progress through the pelvic cavity, I go back to refer to some points in its progress through the first part of the same bony canal. It is well known¹ that Nægele described the foetal head as entering the brim of the pelvis obliquely as regards the plane of the brim,—that is, with its vertical axis not in the direction of the axis of the brim; with the plane of a horizontal section of the head not in the plane of the brim, but with the edge of this imaginary horizontal plane which looks anteriorly advancing first; with a lateral obliquity, in a position involving left lateral flexion of the foetal head. Adapting Kueneke's nomenclature to this part of the progress of the foetal head, we might say that Nægele described the foetal head as entering the pelvic brim, not synclitically, but obliquely or antisynclitically. Most recent authors, including Kueneke, who have carefully studied this point, have differed from Nægele, and describe the foetal head as entering the brim synclitically, not

¹ In the whole of this discussion, I shall assume that the advancing foetal head is in the first position, the occiput directed towards the mother's left. It is scarcely necessary to add, that the kind of obliquity here considered is what Kueneke aptly designates the Nægele obliquity. The Roederer obliquity and the Solayres obliquity are not the subjects of this paper.

obliquely, with its horizontal plane in the plane of the brim, the plane of its vertex synclitic with the plane of the brim. We shall hold it as settled, that in the first part of its course through the true pelvis, the foetal head does advance synclitically.¹

Very few authors have paid special attention to the exact condition of the foetal head in this respect in its course through the pelvic excavation. Those who have described it are, with the exception of Kueneke, so far as I know, of opinion that its progress is oblique, at least in the latter part of its course. Kueneke asserts that it is synclitic, and he has found an able supporter of his doctrine in Hodge² of Philadelphia. It is generally held that when the foetal head begins to leave the direction of the axis of the brim — begins to advance forwards — it advances obliquely, not with a point in the mesial line leading, but with a point in a parietal bone leading. Kueneke's doctrine implies that a mesial point always, in this part of its course, leads, a point in the sagittal suture.

If we define as the presenting point³ that point on the surface of the child's head through which the axis of the developed pelvic canal passes ; then,

¹ Schatz, in his *Geburtsmechanismus*, and Schultze, in the *Jena-ische Zeitschrift*, 1867, S. 274, support the doctrine of Nægele. So also does Hubert, *Cours d'Accouchements*, p. 417.

² *American Journal of the Medical Sciences*, October 1870.

³ See p. 240 of this work.

according to Kueneker's view, the presenting point is always in the mesial line, always in the sagittal suture, and must be so. According to most authors, the presenting point is, at least after the head begins to advance forwards, situated on the anterior parietal bone. If, indeed, the head becomes rotated so completely as to bring the sagittal suture into a direct antero-posterior position, then the presenting point will be, as in the passage of the brim, in the mesial line, in the sagittal suture, but only then. Here is a great difference between Kueneker and other authors. Kueneker describes the course of the head through the true pelvis as being synclitic; authors generally describe it as being oblique in the latter half of this progress.

The difference between Kueneker and others may be further elucidated by stating it as follows:—Kueneker's synclitic motion of the fœtal head keeps the point of intersection of the antero-posterior and transverse diameters of that plane of the true pelvis through which the fœtal head is passing always in the sagittal suture. Other authors hold that the intersection of these diameters is in the anterior parietal bone, in the latter part of this course through the pelvic excavation.

“Let us,” says Kueneker,¹ “regard this canal of the pelvic cavity, curved as it is towards the symphysis, as divided by any chosen number of trans-

¹ *Vier Factoren der Geburt*, S. 36.

verse-sectional planes, which the head has to go through, one after another, then, as these planes diverge in the sacral direction, the head makes in every succeeding plane a turning movement, whereby it is kept parallel to each plane as it reaches it, so that at length arrived at the last plane, close to the plane of the outlet, it stands parallel to this also." This synclitic movement is said by Kueneker to have the symphysis for its fulcrum, the transverse diameter of the head representing a single-armed lever moving on the symphysis as a fulcrum through an arc of 30 degrees, a figure which is said to be the measure of the difference between the inclination of the brim or inlet, and that of the outlet as defined by Kueneker, or of a line running in a sagittal direction through the top of the pubic arch and the point of the sacrum (not of the coccyx).

"To attribute," says Kueneker, p. 38, "to the symphysis the importance of a point of support for the revolving lever, is, in my judgment, perfectly well founded; but withal, the mechanism of the synclitic movement is still by no means sufficiently elucidated. For there is still reserved the question, how at last the symphysis comes to be in a position to offer a resistance to the driving power; a question which has as yet not been once proposed, far less answered. I shall attempt to make out a solution of this not very simple mechanism.

"We know that the driving power falls at a right

angle on the plane of the inlet, that its axis bisects its sagittal diameter, and passing through the cavity of the pelvis permeates the coccyx, and is inclined to the horizon at an angle of 30 degrees. Now, the matter would be very simple, if it could be shown that the symphysis was so situated as to converge towards this unchangeable direction of the driving power. If, to show this, there be drawn at the symphysis a line parallel to this direction, then there is at length found to be no convergence, not even parallelism, but, on the contrary, a divergence to about 10 degrees, of the symphysis, which is inclined at an angle of 40 degrees. Here, then, can therefore be no talk of resistance of the symphysis, and consequently none of synclitic movement. Much rather would this one-sided treatment of the relations establish the persistence of the orthophoric movement all through the pelvic cavity, and justify those who adhere to the theory of the obliquity of the cranium in the pelvic cavity.

“As the symphysis does not, without some further condition, offer the expected resistance, we must look about for something else which may be fitted to afford to the symphysis the postulated peculiarity. This I find in the posterior pelvic wall. If we draw at the posterior wall of the pelvis, from the promontory onwards, a parallel to the direction of the driving power, it is found to coincide with the anterior surface of the two upper bones of the sacrum, passes

diagonally through the third vertebra, and then runs behind the sacrum. There is thus from above to this point a lessening degree of friction between the advancing head and the two pelvic walls implicated, which compresses the skull in the direction of its transverse diameters ; but as yet no one-sided resistance, which might produce a revolving. Evidently rather the friction begins first at the third sacral vertebra, and increases from hence downwards more and more. This friction is, nevertheless, still not in a position to produce a distinct resistance, which in case of its being in excess would act as a fulcrum ; but the inclined planes of the excavated vertebræ exert, as the cranium glides over them, a pressure against the symphysis. This result has already been expressed by Roederer in these words : ‘ *Vis sane composita oritur, ex dolorum impetu, qui in directione axeos pelvis agit, et plani inclinati et mobilis (sc. posterioris vaginæ parietis), cujus directio ad anteriora vergit, renixu.*’ Here, now, there would be produced a resistance, even supposing the symphysis formed a convexity corresponding to the sacral excavation. So much the more must this resistance come into play when the direction of the symphysis, in spite of its divergence from the direction of the driving power, has a not insignificant convergence with the direction of the lower part of the sacrum ; through which last, as has been already shown, the direction of progress is in a corresponding

manner inclined forwards ; and this resistance, which, at the same time, is not strong enough to arrest the advance, forms the fulcrum round which, under a constantly diminishing progression from above downwards, the implicated transverse section of the cranium revolves as a single-armed lever, and so keeps in parallelism with each successive transverse-sectional plane of the pelvis into which it is brought in consequence of its advance. This is the synclitic movement."

I have thus given Kueneke's theory in a nearly literal translation of his own words. To what I have quoted, Kueneke adds very little further. Only, I may add that he expresses his belief that, through the whole of the pelvic cavity, the head advances synclitically.

Before proceeding to discuss the statements and theory of Kueneke, I shall give a brief account of what I (in common, I believe, with many obstetricians) hold to be the facts in regard to this point in the position of the head as it passes through the cavity of the pelvis, from the brim to the outlet.

The foetal cranium enters the brim or inlet directly or synclitically, that is, without lateral obliquity or lateral flexion. The presenting point is in the sagittal suture. The transverse and antero-posterior diameters of the brim intersect in the sagittal suture. Regarding only the matter now in question, the foetal head advances, in unchanged relations, in the direction of

the axis of the brim till it is arrested by the sacrum and the floor of the pelvis. In this way it often advances unchanged even till the vertex is nearly pressing on the coccyx. Up to this point its progress is direct, unchanged, because it has met with no special resistance; the vertical axis of the head is still parallel to the axis of the brim. So long as the head was passing through the upper half of the pelvic cavity, the plane of the vertex was synclitic with the successive parallel planes of that part of the pelvis. But there was no new mechanism to produce this synclitism. The head simply advanced in unchanged position thus far. As I have already said, it generally advances still farther in the same direction, and till it meets the resistance of the lower half of the sacrum and floor of the pelvis, maintaining its original position towards the axis of the brim; the axis of the brim still, when continued downwards, traversing the sagittal suture. But as soon as it so advances beyond the middle of the pelvis, beyond the transverse sectional plane which passes through the middle of the third sacral vertebra, it ceases to be synclitic with the succeeding planes, and it never regains completely its original synclitism such as it had in the upper half of the pelvis. The axis of the second half of the pelvic cavity is never again (unless, indeed, the head so completely rotates as to have its sagittal suture lying exactly in the antero-posterior pelvic diameter) found traversing the sagittal suture, but traverses a point on

the surface of the anterior parietal bone. The antero-posterior and transverse diameters of the transverse-sectional planes of the lower half of the pelvis never intersect in the sagittal suture.

In making these statements of what I believe to be the facts in nature, I do not assert that there is no tendency or progress towards synclitism in the second half of the pelvic cavity. There is certainly such tendency, and I believe there is also, as a result of the tendency, an approach to the synclitic state. But the synclitic condition is never reached.

I shall now proceed to give the evidence upon which my statements as to the absence of synclitism in the second part of the progress of the foetal head through the pelvic cavity (and, indeed, in its still further progress also) are founded. The great peculiarity of Kueneker's description of this matter lies in his assertion and attempted demonstration that there is a movement of the foetal head keeping it synclitic as it advances. I believe it is not synclitic in the second part of its course.

I shall arrange the evidence I have to adduce under five different heads :—

First. I shall appeal to observation, and in this, of course, Kueneker will join me. I may, however, describe a common condition, and assert that its truth will be generally recognised, and that it is quite inconsistent with Kueneker's views.

When, during an ordinary labour—say, for example, a first labour, in which there is plenty of time for careful observation—a digital examination is made when the vertex has reached the plane of the outlet of the ligamentous pelvis or of the pelvic excavation, or nearly reached it; when, in addition, as often happens, the case is favourable for the purpose, from the cranium having not yet begun, or only slightly performed, its regular rotatory movement on its vertical axis, it is found that the sagittal suture lying transversely, or more or less in the right oblique pelvic diameter, is far behind or posterior to the transverse diameter of the plane which the vertex or upper half of the foetal head is occupying. The antero-posterior and transverse diameters of the plane occupied by the head do not intersect in the sagittal suture. The sagittal suture is intersected, or nearly so, by the axis of the brim prolonged downwards; it can be felt to be just above the coccyx. The anterior parietal eminence can be felt in the upper part of the pubic arch, and nearly in the same pelvic plane as the sagittal suture. The sagittal suture is far behind the transverse diameter of this part of the pelvis. The axis of this part of the pelvis traverses the anterior parietal bone, upon which, therefore, lies the presenting point. The head is not placed synclitic with the transverse-sectional plane of this lower part of the pelvic cavity. The anterior side of the head is in advance of the posterior.

I choose the position of the head above described for two reasons, which render it suitable for testing and for disproving Kueneker's views. For at this stage of progress we still have the coccyx and other bony landmarks to indicate whereabouts the axis of the developed genital canal lies. At a more advanced stage these landmarks are absent, and the ascertainment of the position of the axis is rendered more difficult, on account of the head in its progress developing, and at the same time occupying, the passage, and thus introducing difficulty. At this stage of progress, also, there is often very little rotation effected, and the occurrence of rotation renders it difficult to judge regarding synclitism. Continued synclitism is conceivable during and after rotation, but Kueneker does not say it then exists.

Before leaving this part of the subject, I must make Kueneker's own testimony available against his synclitic doctrine. Kueneker describes very clearly the position of the sagittal suture after the head has undergone its rotation. This description he ends as follows:—"If we trace the sagittal diameter of the outlet upon the head as it lies in this position, its course will be, from the top of the pubic arch, about two centimètres distant from the sagittal point of the small fontanelle, starting from the right limb of the occipital suture, coming gradually to convergence with the sagittal suture near the forehead, intersecting it at its ending in the great fontanelle, passing

over this last to the front of the left frontal bone, and on to its tuber, and further somewhat laterally to the left orbit." Now, this description by Kueneker is the description of a head which has certainly rotated, but which has not become synclitic. Kueneker does not notice this circumstance, and, of course, does not account for it. Were the head synclitic before rotation, it should be synclitic during it. But while the head is not synclitic, it is exactly situated as my views would lead one to expect. It is advancing with the anterior or right parietal bone, leading; with the posterior or left parietal bone, on a higher transverse-sectional plane of the pelvis than that occupied by corresponding parts of the right. Before it rotated it was not synclitic, and Kueneker's descriptions are erroneous. After it has rotated, it still is not synclitic or in the analogous state of its new position, and this accords with Kueneker's description just given, but is subversive of Kueneker's views;¹ for the axis of the genital passage traverses the right parietal bone. The horizontal plane of the child's head is not synclitic, for the whole sagittal suture is to the left of the mesial line. The axis of the pelvis does not pass through the sagittal suture, but through the parietal bone on the right side of the sagittal suture, just as it did before rotation commenced. It is thus evident that Kueneker has himself described the head

¹ Kueneker's enclitic movement is supposed not as yet to have commenced.

in a position which is inconsistent with his own synclitic doctrine.

Second. Kueneke describes an enclitic movement of the foetal head succeeding to the synclitic movement. While I do not admit the accuracy of Kueneke's description of this enclitic movement, so far as it is novel, yet there is no doubt that it is, as Kueneke says, truly antisynclitic. It is opposed to the whole synclitic theory. The enclitic movement is observed after the rotation of the head, and consists essentially in a flexion movement. The synclitic movement involves comparatively rapid descent of the parts of the head lying near the posterior wall of the genital canal. The enclitic movement implies retardation or lagging behind of the parts situated posteriorly. Kueneke will, of course, reply to our statements, and to a great extent truly, that synclitism and enclitism are different conditions not justly to be compared. To this we reply, that if enclitic movement takes place, synclitic movement is thereby rendered improbable; that the same circumstances which produce the enclitic or antisynclitic state after rotation of the head prevent the synclitic state before the rotation. According to our view, the so-called enclitic state is merely a continuation and increase of the antisynclitic condition which previously existed.

Third. I have already, near the commencement of this paper, given, in a translation of a passage from Kueneke's work, that author's own description of the

mechanism by which the so-called synclitic movement is supposed to be produced. To that description I object on two grounds, namely, that it is scientifically incorrect, and that it omits the chief agency that Kueneke should have invoked to support his own doctrine.

Kueneke's description of the supposed mechanism of synclitism is scientifically incorrect ; for it assumes that the reaction is greater than the action. Kueneke says the posterior wall of the pelvic passage pressing on the adjacent left side of the head fixes, or nearly fixes, the remote right side of the head upon the symphysis, so that this right extremity of the transverse cranial diameter becomes the nearly fixed end of a lever represented by this transverse diameter. Now, supposing no other force acting, these pressures on both ends of this transverse diameter of the head must be equal and opposite, and in such a case there will be no tendency of the one extremity of this diameter to be more retarded by friction than the other. But there is another force acting, namely, the propelling power, whose direction is such as inevitably to make the pressure on the posterior wall or on the left extremity of the transverse cranial diameter, and therefore the friction there, much greater than on the anterior pelvic wall.

But Kueneke's statement of the conditions and results is farther incorrect. If the head, arrived in the lower half of the pelvic cavity, is pressed forwards

against the pubes, it is also pressed downwards by the driving or propelling power, and the resultant will not be a pressure against the symphysis leading to fixation by friction of the part of the foetal head adjacent to the symphysis, but a tendency of the whole head to advance in the direction of the axis of the pelvis, or nearly so. Though we make this statement in order to show the error of Kueneke, we wish it to be understood that we do not adopt the mechanism implied in our exposition as the best way of reaching a true view of the advance of the head.

That there arises from the mechanism a direct tendency to the production of the synclitic condition, there can be no doubt. This is demonstrated in the following manner:—In the lower half of the pelvic cavity, the genital canal begins to be curved forwards in the antero-posterior vertical plane. In consequence of this, the posterior part of the passage or the concavity of the canal is always nearer the line of the propelling force than the anterior part or convexity of the canal. The posterior parts of a body traversing the canal under these conditions will be acted on with more force than the anterior. Though Kueneke's alleged mechanism does not exist, there will necessarily be a tendency to the synclitic movement of Kueneke.

But there are two elements of this mechanism which prevent the synclitic movement. One of these is the circumstance that the foetus is not a simple

plastic mass, subject, without disturbance, to the conditions stated in the preceding paragraph. This plastic mass is furnished with a column of bones passing through it, which forms at least a main vehicle of the propelling pressure. So long as the foetal head lies transversely, the statement of the preceding paragraph will be applicable, if there be no modifying circumstances. But the foetal head generally has a Solayres diagonal position in this situation, and then the column of bones conveys the chief part of the force to the half of the head nearer the anterior wall of the genital canal, and the synclitic tendency is so far obviated. The increase of pressure on the half of the foetal head nearest the pubes, in consequence of the Solayres obliquity, will not be great; but there is another reason why the synclitic movement does not take place.

The posterior half of the genital canal is the half where there is by far the greatest resistance to the progress of the head. It would not be very misleading to assert that, in that part of the course of the foetal head, where synclitic movement is said to take place, the resistance is almost entirely in the posterior or concave side of the genital canal. In the middle, anteriorly, no structure has to be developed. There is only a minimum of resistance from friction. On either side, departing from the middle of the anterior wall of the passage, the resistance increases till the posterior wall is reached. There the development of

the passage is greatest, and this development is the source of the resistance.

“On the other hand, however,” says Schroeder,¹ “the resistances which the head experiences at the posterior and anterior pelvic wall are very different. At the entrance of the head into the small pelvis, the anterior and posterior pelvic walls run nearly parallel; the pelvis here may be regarded as a uniform cylinder, into which the head is driven. It is evident that, through such a cylinder, the head must pass in the same position in which it enters it; and this is what the head does so long as the pelvis answers to this description. But as soon as the head has completely entered it, the dissimilarity of the anterior and posterior pelvic walls makes itself remarked. At the posterior pelvic wall, the resistance which the head experiences increases; at the anterior it diminishes.” It is this increasing resistance of the posterior pelvic wall which prevents the synclitic movement of Kueneke, just as the same resistance produces or maintains the subsequent alleged flexion-movement, or enclitic condition, as Kueneke designates it.

Fourth. There are some of the changes in the shape of the child's head—the result of the forces to which it is, during labour, subjected—which throw light on the questions discussed in this paper. Those to which I shall specially refer are the overlapping of one parietal bone over the other, and the shears,

¹ *Schwangerschaft Geburt und Wochenbett*, S. 45.

which have recently been described by Stadtfeld, Welker, and Dohrn—especially by the last-named author.

It might be expected that I should, with this view, refer to the caput succedaneum, but a little reflection will show that it can give no aid. The caput succedaneum¹ can only show what part of the foetal head was, during labour, not subjected to uterine pressure; it cannot show what was the presenting point, or where, on the surface of the head, the antero-posterior and transverse diameters of the passage intersected. The caput succedaneum shows what part of the foetal head presented to or affronted the lumen of the as yet undeveloped part of the genital passage. If the axis of the undeveloped part of the genital passage (as at the brim of the pelvis) nearly corresponds with the axis of the pelvis or fully-developed genital canal, then the caput succedaneum will be at the presenting point. But if, as is the case in the second half of the progress of the foetal head through the pelvis, the axis of the as yet undeveloped part of the genital passage does not correspond with the axis of the fully developed pelvic canal, then the caput succedaneum will have no relation to the presenting point, or that part of the surface of the head in the axis of the pelvis; but will show where was the axis of the undeveloped part of the passage.

The equitation or overlapping of one parietal bone

¹ See p. 233 of this book.

by another is a subject which has, as yet, been insufficiently studied. In particular, sufficient observations have not, so far as I am aware, been made as to the comparative frequency with which the right overlaps the left, and *vice versa*. But it might, I think, be expected that, during labour, the left or posterior should overlap the right or anterior, if Kueneker's synclitic movement took place, for then the left or posterior would be pushed most downwards in the pelvis; while a compressing force acting laterally on the head would cause equitation: the left would overlap the right. But I believe it is generally the reverse: the right or anterior rides over the left or posterior. The overlapping of the left or posterior by the right or anterior is, however, just what would be expected in labour under the conditions which I have described, and attempted to prove to be the ordinary or natural ones.¹

Fifth. The last evidence of the directions of the pressures to which the foetal head is subjected is the

¹ "For ordinary," says Schroeder, *op. cit.* S. 103, "the parietal bone lying posteriorly is shoved under the anterior; but numerous exceptions to this occur even in the quite normal course of labour; especially in the first position of the head, the right parietal bone is not rarely shoved under the left; but, truly, in a normal pelvis there most frequently takes place no evident overlapping. In the pelvis, contracted in the conjugate diameter, with or without a less degree of general contraction, the anterior parietal bone is at least as frequently shoved under the posterior; while, in the equably uniformly contracted pelvis, the posterior parietal bone appears to be almost invariably overlapped by the other."

shearing¹ (*verschiebung*), described by Dohrn.² I shall give the account of it in Dohrn's own words, merely premising that we do not entirely assent to all that he says :—

“It is,” says he, “a known fact, that the skull is usually placed obliquely in the pelvis in such a way that the side of the head lying anteriorly is situated more deeply than that turned posteriorly. It is also known that the side of the head directed towards the back is usually flattened, that directed forwards more strongly curved out. Both are consequences of the more powerful pressure which the head undergoes from the posterior wall of the pelvis.

“It is also this more powerful pressure of the posterior wall of the pelvis which produces the lateral shear of the cranium.

“If you look from above on a child's head which has been born in a cranial presentation, shortly after birth you will almost always find that it is oblique. This depends partly on the position of the caput succedaneum being mostly on a side ; partly, however, it depends on a lateral shear of the two halves of the skull. This last you can easily make out, if you remark the position of the tubera parietalia. You will then find that the one stands much farther forwards than the other.

“The pressure of the posterior pelvic wall brings

¹ Thomson and Tait's *Natural Philosophy*, § 171.

² *Monatsschrift f. Geburtsk.*, Bd. xxiv., S. 418. 1864.

about this shear in the following way:—If I push downwards against the promontorium a fœtal cranium whose occiput lies deep down, its side directed to the promontory comes to be shoved forwards towards the coronal suture.

“If, on the other hand, I place the sinciput deep down, and now shove the cranium downwards on the promontory, then its side directed backwards towards the promontory is shoved towards the occiput. If I let the vertex advance first, and place the great and little fontanelle at an equal height, then there comes to take place a shear corresponding to the deep position of the sinciput, and always to the greater extent the more the head is inclined towards the forehead, the more directly from above you look at it, the less it appears to be direct.¹

“This is the general law which regulates the degree and direction of the lateral shear. It is self-evident that it will be influenced by the energy of the pains, the size of the child’s head, and the structure of the cranial bones; it is sufficient for me to point these out.

“Let us inquire now how far the law is confirmed by experience. I have for this purpose measured very many children’s heads, and have noted carefully the appearances in the last forty which have come before me in normal pelves. During the first hours

¹ The translator is confessedly at a loss as to the meaning of this sentence.

after birth, I placed a small cyrtometer horizontally around the greatest periphery of the skull, noticed at which links of the chain the frontal suture, the tubera parietalia, the occipital protuberance stood, and marked out the form of the cyrtometer on a paper divided into squares. In this way I found among the forty cases a lateral shear thirty-eight times ; and it always happened that, where the half of the head turned towards the promontory had undergone a shear, pushing it forwards, there was during labour deep position of the occiput ; where the reverse was the case, deep position of the sinciput was noted.

“The extent of this lateral shear in the first position of the cranium was on an average 5·3 millimètres ; in the second position, where the occiput is originally more frequently directed backwards, and in consequence the pressure of the posterior wall comes more strongly into play, on an average 6·6 millimètres. At this point I must remark, that the different position of the tubera parietalia does not depend simply and alone on a shear of a parietal bone *in toto*, but to a small extent must be referred also to a change of place of the tuber in the bone itself.

“In connection with the frequency and the degree of the lateral shear, it may appear striking that it has not earlier attracted notice. This may well arise from the circumstance that observers have allowed themselves to be deceived by the caput succedaneum, and

particularly that the head is seldom carefully examined soon after birth. This last is necessary, if you wish to make out the lateral shear, for it quickly disappears, often already some hours after birth, and much earlier than the flattening of one half of the head in the vertical direction, which is often observed for a long time. The pressure of the brain, the elasticity of the bones and soft parts, as well as the tension of the muscles, co-operate to produce this disappearance.

“The importance which I attach to my observation is not merely scientific, but also diagnostic. If we examine a child’s head shortly after birth, with reference to the mentioned influences, it will be possible for us to arrive at a retrospective conclusion as to its position during labour. The flattened side of the head is almost always that which was turned towards the promontory. If it is pushed forwards, then deep position of the occiput occurred; if it is pushed backwards, then there was deep position of the forehead. In this way it will be possible to control the results of examinations made during labour, and, in cases in which the labour was not personally observed, to form retrospectively a judgment as to the position of the head.”

Before concluding, we must make a few remarks on Dohrn’s valuable observations; and we must first ask our readers not to hold us as agreeing with Dohrn in ascribing to the promontory the place in the pro-

duction of shear in ordinary cases which he gives it.¹ For us, Dohrn here is in a manifest error. Instead of the word *promontory*, we would place the posterior wall of the lower pelvis. In a natural labour the head is subjected to no kind of powerful pressure from the promontory. It is first subjected to such influences when it has passed through the upper half of the true pelvis.

Dohrn's description involves two shears, one vertical, the other lateral, which may be regarded as components of one shear.

The vertical implies the flattening of the posterior parietal bone and the bulging of the anterior. It is the result of the resistance offered by the posterior wall of the pelvis, which, as we have before seen, prevents the synclitic movement. The adjacent parietal is flattened, and the anterior parietal, bearing the mass of the caput succedaneum, is more than naturally bulged outwards.

The lateral shear, to which alone Dohrn attaches the name (*verschiebung*), is merely that component of the whole shear which depends upon the Solayres obliquity of the head, and the rotation effected while the shear is in course of production.

Both shears at once disprove the existence of the

¹ Views somewhat similar to Dohrn's are entertained by Ols-hausen in his valuable paper, No. 8 of Volkmann's *Sammlung*, *Über die nachträgliche Diagnose des Geburtsverlaufes aus den Veränderungen am Schädel des neugeborenen Kindes*.

synclitic movement, and help to explain why it does not take place.

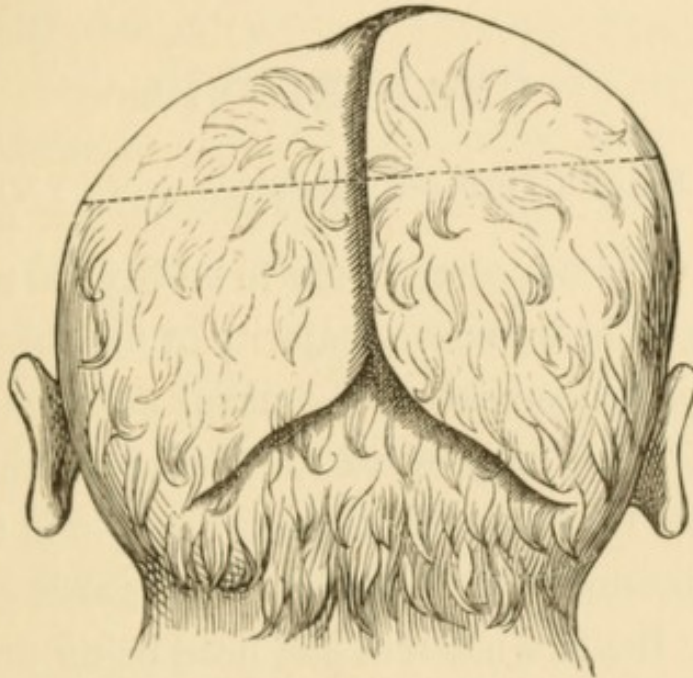


Fig. 7.

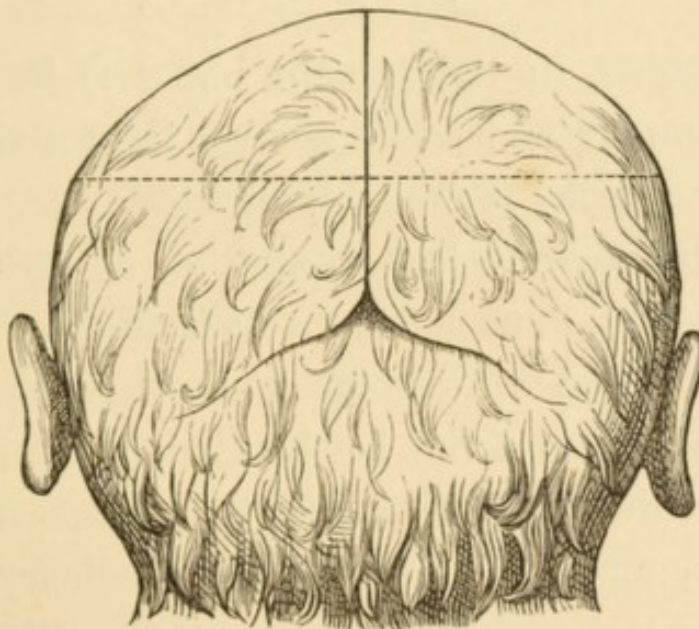


Fig. 8.

Figure 7, copied from one given by Fankhauser (*Die Schädelform nach Hinterhauptslage*, Bern. 1872), represents very well the shearing of the head, as it may be seen in an ordinary moderately difficult labour that would not necessarily be called morbid, although there was some delay at the outlet of the pelvis, and the pains were powerful. Fig. 8 shows the symmetrical natural head. Fig. 7, the same head after alteration by the shears.

CHAPTER XIV.

ON THE PRODUCTION OF PRESENTATION OF THE FACE.¹

IT has always appeared to me to be beyond reasonable doubt, that the immense majority of face cases are the result of derangement of the usual mechanism of the first part of the progress of the foetal head through the pelvis ; or, that presentation of the face is the result of a displacement of the vertex backwards as regards the child, the extension of the head being produced at or near the brim of the pelvis by the propelling powers of labour.

My object in the present paper is to show the mechanism by which this change from a vertex to a face presentation is probably effected.

As has already been hinted at, all cases of face presentation do not demand this kind of explanation. Instances are recorded, for example, where the extreme extension of the head, existing in an ordinary face case during labour, has been observed before labour commenced. To this extraordinary kind of face presentation, and to some others allied to it, I make now no reference whatever, confining myself to the mechanism of the production of face presentation in ordinary circumstances.

¹ Read to the Obstetrical Society, April 13, 1870.

Quite recently, a remarkably interesting and valuable contribution has been made to our knowledge of face cases by the pamphlet of Dr. Hecker of Munich, entitled *Ueber die Schædelform bei Gesichtslagen*, published at Berlin in 1869. In this work Hecker clearly shows that a great majority of children born with the face presenting have the dolichocephalous form; the height of the skull is small, it is prominent posteriorly, and has narrow and but slightly bulging parietal bones. These conditions of the cranium he demonstrates, so far as his collection of measurements justifies him, to be not the result of the labour, but to be original and permanent states of the heads of children born in this peculiar manner.¹

This discovery would naturally lead to the supposition that the face presentation is a consequence of the dolichocephalous condition. Hecker does not expressly say that it does, but the direction of his thoughts may be fairly guessed at by regarding the importance which he attaches to the comparative increase of length of the posterior cranial lever-arm, and the nicety with which he has investigated this point. Hecker has omitted to mention another point in the dolichocephalous structure, which, though dependent on the increase of length of the posterior cranial lever-arm, is not identical with it—namely, the projection of the occiput; a condition well indicated

¹ See also Fasbender. *Beiträge zur Geb.* Berlin. I. Band, S. 108, etc.

by the obtuseness of the mastoid angle or angle formed by the junction of the squamous and lambdoidal sutures.

The production of face cases, as I am about to describe it, does not depend upon the increased comparative length of the posterior cranial lever-arm, nor on the projection of the occiput, but depends upon a quite different mechanism. This I shall presently describe. But I wish here to point out the place and power of the dolichocephalous form, as introducing a condition in which the mechanism alluded to will operate at extraordinary advantage, and therefore will operate with extraordinary likelihood of producing the transformation. The transforming mechanism will produce the transformation with greater certainty and ease if the child is dolichocephalous than if its head is of ordinary construction.

The mere dolichocephalous condition explains nothing, for even in the dolichocephalous the comparatively long posterior cranial lever-arm is shorter than the anterior cranial lever-arm.¹

In ordinary labour, with the vertex presenting, whether the occiput looks to the mother's right or to the mother's left, transformation into a face case is

¹ In Hecker's eight cases of delivery under face presentation, in which he made careful measurements of the anterior and posterior cranial lever-arms, the posterior was shorter in every case than the anterior. The average difference between the two was, in foetal heads generally, 1 centimetre. In the eight face case heads, it was .4, .1, .2, .4, .6, 1, 1, 1.7.—*Ueber die Schaedelform, etc.*, S. 46.

prevented by the greater length of the anterior cranial lever-arm. Strong pressure equably exerted on the upper surface of the cranium, as it will be if the longitudinal axis of the uterus is parallel or nearly so to the axis of the pelvic brim, will lead to flexion, not extension of the foetal head. In other words, if there be uniform resistance to the advance of all parts of the cranium, as there will be in the condition of the uterus above named, the occiput will advance first, if the resistance is sufficient to produce flexion of the head. If, in the same circumstances, the posterior cranial lever-arm were the longer, then extension of the head and forehead presentation would result, the first step towards face presentation, which would subsequently be produced.

The above statements may be held as true, because the relative length of the cranial lever-arms is the only disturbing element of what would otherwise secure uniform progress in unchanged conditions. In that part of the pelvis where transformation of the vertex into a face case takes place, the axis of the passages is one and unbent, except by the usual slight lateral obliquity of the uterus to the right. To the bending or curvature of the passage produced by this right lateral uterine obliquity we shall afterwards draw particular attention. The curvature of the genital passage which begins posteriorly about the middle of the third sacral vertebra, a curvature in the antero-posterior vertical plane, is an important

curvature in the mechanism of parturition ; but it is not important in the subject now under discussion, for the simple reason that the transformation of a vertex into a face case is ensured before the cranium affronts the commencement of this antero-posterior curvature in the vertical plane. Were this great curvature not so situated, its influence would be great. It is very great in the mechanism of the descent of the head through it under a pressure with a uniform direction. But this has no relation to presentation of the face, which, no one can doubt, is determined before this part of the genital passage is reached.

Although the great antero-posterior vertical curvature of the genital passage does not interfere in the production of face cases, there may be interference from another curvature, which is produced by the well-known usual right lateral obliquity of the uterus. This very frequent obliquity produces a curve of the genital passage at or near the brim, whose mesial line is concave to the right. This curvature I have found to persist during labour when its influence is exerted in producing face cases.

To show how frequent is the deflection of the uterus to the right, I shall quote some figures from the work of MM. Dubois and Pajot referring to this point :—“Of 100 pregnant women,” say they, “taken without selection, and examined at the Hospice de la Maternité, at the commencement of the ninth month of pregnancy, 76 presented a very evident right lateral

obliquity ; 4 a left lateral obliquity ; 20 an anterior obliquity without appreciable lateral inclination.”¹

Now, in pushing a plastic body, such as a foetus, through a curved tube, such as the pelvis, and supposing that the resistance is equal at all parts of the tube, that part will be pushed with greatest advantage which is nearest to the line of the propelling force, and it will advance first. In the example referred to, the parts nearest the concave side of the passage will advance first.

We now proceed to apply this general statement to the subject under discussion.

In the great majority of cases of right lateral obliquity of the uterus, the occiput of the foetus occupies the concave side of the passage of the genital canal at or near the pelvic brim. If the head meets with much resistance, the occiput will tend to advance first, from the comparative shortness of the posterior cranial lever-arm, and from its position nearer the concavity of the passage, and therefore nearer the line of the propelling force, which must incline towards the left in consequence of the deflection of the uterus. The vertex presentation will be maintained.

¹ *Traité Complet de l'Art des Accouchements*, tome i. livr. 2me, p. 381. See also the Observations of Spiegelberg, *Monatsschrift für Geb.* Band. xxix. 1867, S. 92 ; and of Winkler, *Jenaische Zeitschrift*, 1868, S. 522. The results of Winkler do not well agree with the others. For the degree of obliquity, see chap. iii., p. 50.

In a certain proportion of cases, nearly 1 in $3\frac{1}{2}$, the foetal occiput looks to the right side of the passage, to the convexity of the canal, and the forehead to the left side or concavity of the canal. The propelling power, *per se*, will therefore tend to make the forehead descend, and bring on a forehead presentation in anticipation of a fully-formed face case. This tendency to push down the forehead instead of the occiput will be, in the majority of cases, counteracted by the agency of the more efficient resistance by the soft parts to its advance in consequence of the greater comparative length of the anterior cranial lever-arm. But, in a case of exaggerated right lateral obliquity, this resistance may be overcome, and the forehead may descend. In every case in which the dolichocephalous form exists, the resistance from the comparative length of the anterior cranial lever-arm will be diminished, or, in some cases, even almost annihilated, and the forehead be invited to descend first, the head becoming extended.

Now it cannot fail to strike the intelligent student, as being more than a coincidence, that while right lateral uterine obliquity is the ordinary one, face cases are comparatively much more frequent in the second position—that is, with the occiput looking to the right or the convexity of the canal, before the transformation; and that the children are generally dolichocephalous. In other words, the conditions which can be shown, *a priori*, to be likely to lead to

the transformation of a vertex into a face case are exactly the conditions which are found, not indeed invariably, but to prevail.

“The numbers,” says Professor Hecker, “which I have previously given for the proportion of the first to the second face position, 22 : 21, are now changed, so that now 28 first and 21 second positions are noted; the proportion of the latter to the former, therefore, is expressed as 1 : 1·3, and if I compare with this my former calculations in reference to first and second vertex positions, which yielded a proportion of 1 : 2·3, then I must justify Winckel when he judges from his statistics, ‘that the second position of the face in proportion to the first occurs oftener than the second position of the cranium to the first; that, therefore, *ceteris paribus*, a face position is easier produced when the back of the child lies to the right than when it lies to the left.’”—(S. 15.)

It appears to me, then, to be highly probable that the chief cause of face cases is obliquity of the uterus in any direction, ensuring a curvature of the genital canal at the brim of the pelvis; that this cause operates when the forehead of the child is placed near the concavity of the curved canal, or nearest the line of the propelling force; and that the dolichocephalous form will greatly favour the transformation under these circumstances of a vertex into a face case.

There is a very remarkable circumstance, which the above proposed theory of face cases may explain.

I find that Hecker and other foreign authors ascribe to face cases a frequency far in excess of what is, I believe, observed in this country. Hecker says there is a face case in every 168. In Collins's Dublin Hospital experience, there was only 1 in 497. Now, this remarkable difference¹ may be explained by differences in the position of the women during labour in various countries. The almost invariable decubitus of British women on the left side may, by diminishing the frequent right lateral uterine obliquity, prevent the transformation of many vertex into face cases that would otherwise be so changed. I am not sufficiently informed to contrast this position of British women with the dorsal decubitus or other position of German women in like circumstances. But it appears to me to be a subject worthy of attention.

Before altogether quitting the subject, I wish to point out that Baudelocque probably entertained views closely similar to those I have advanced. I was aware of the existence in his works of the passage regarding the production of face cases which I am about to quote, but it was not till after I had published the above given theory that I was struck with

¹ "Thus," says Churchill (*Theory and Practice of Midwifery*, 4th ed., p. 409), "in British practice, out of 141,259 cases, there were 567 face presentations, or 1 in 249; among the French, 50,141 cases, and 189 face presentations, or about 1 in 265½; and among the Germans, 69,417 cases, and 411 face presentations, or about 1 in 169½; the whole giving 1167 face presentations in 260,817 cases, or about 1 in 223½ cases."

its accuracy, so far as it goes. Speaking of cases in which "the chin quits the breast, and the head turns backward as soon as it begins to engage," Baudelocque says,¹ "M. Levret, in one of his observations, gives us the most exact picture of this accidental bad position of the head; but, according to that author, it proceeds from no other cause than the oblique and lateral situation of the child's body, which he regards as the cause least known of all those which render labour difficult. 'All the obstacles,' says he, 'which obstruct the exit of the child in the present instance, arise from the situation of the body exclusively; it is the shoulders stopped at the superior strait, which hinder the head from descending, whether Nature endeavours to expel it, or we attempt to extract it with the forceps, or otherwise.'² De la Motte and Smellie have not explained themselves so clearly concerning this disagreeable position of the head; but they have better pointed out the true indication which it offers.

"This position is the effect of the direction of the expulsive forces of the uterus, and of the manner in which they act on the head. We hardly ever observe the obliquity of the uterus, which is the determining cause of it, not to be on the same side to which the occiput answers. The direction of the expulsive

¹ *System of Midwifery*. Heath's translation. Vol. ii. p. 193.

² See *La seconde Observ. de M. Levret, sur les Causes et les Accidents de plusieurs Accouchements Laborieux*. 4me edit. p. 4.

forces in other cases is constantly such that they act on the head so as to make the occipital extremity descend; but in this, the direction of those same forces traverses the head obliquely from the base to the vertex, and from the occiput to the forehead, a little before the centre of motion, and obliges it to turn backward in proportion as it advances. M. Levret attributed this effect to the lateral situation of the placenta, and to that of the child's trunk in the uterus, only because he thought that the lateral obliquity of that viscus always proceeded from the insertion of the placenta on one of its sides."

Before the study of face cases led me to the above-mentioned results, I, and others, used to think and speak of hitching or obstruction of the occiput on some part of the brim of the pelvis, leading to its complete arrestment, and the subsequent extension of the head; the forehead, and then the face descending. This hitching, if it ever takes place, will be evidently favoured by the projection of the occiput in the dolichocephalous form. But such ideas were admittedly very vague and unsatisfactory, for such hitching can scarcely even be conceived except in a case of deformed pelvis, and it is only a small proportion of face cases that actually occurs in complication of labour with deformity of the brim of the pelvis. Though the hitching of the occiput is a very rude and utterly insufficient explanation of the majority of cases, it may possibly occur in some of the face cases

which are produced where there is deformity of the brim.

In the cases hitherto discussed in this paper the forehead presents before the face comes to present frankly. The vertex first presents, and under the influence of labour it is displaced and replaced by the forehead, which latter again is displaced and replaced by the face. But there is a class of cases in which the vertex first presents, and under the influence of labour is displaced and replaced by the forehead, or, at least, by the anterior part of the vertex, and this latter is again displaced and replaced by the occipital part of the vertex. These cases are seen chiefly when the pelvis is flat or antero-posteriorly contracted, the contraction being chiefly in the conjugate diameter. Now, it appears to me that the mechanism of the pushing down of the forehead is in these cases quite different from that of face cases in healthy pelvis; and this, even although the pushing down of the forehead may, and does, in contracted pelvis, occasionally prove the commencement of a full face case. Of the mechanism of these changes in the presentation, Schröder¹ gives an account and a theory, with both of which I cannot coincide. He describes the impediment as being nearer the occiput than the forehead. He describes the occiput as meeting with greater resistance than the forehead. He describes the increased resistance to the occiput as a sufficient

¹ *Manual of Midwifery* (Engl. Transl.), p. 246.

reason for the descent of the forehead, although it is at the end of the longer lever. He describes the extension of the head as making the anterior foetal lever arm the shorter, whereas the extension is not to a sufficient extent to produce this change in relative length. To his further details of this mechanism objections may also be made.

While I cannot adopt the theory of Schroeder, I do not bring forward my own with great assurance. It is as follows. In a simple contracted pelvis the impediment is affronted by the bitemporal diameter, or nearly so. The occipito-frontal diameter of the head is divided into two lever-arms in such cases by the bitemporal diameter, and of these the anterior is the shorter. The forces of labour do not act exclusively through the spinal column, and this will be the case especially when there is great resistance and the foetus is powerfully compressed. I have elsewhere¹ tried to show that the foetus may in some degree be regarded as a viscous mass. The forces of labour will press every point of the occipito-frontal diameter. No doubt the vertebral column will bring an increase of pressure on the posterior or occipital lever-arm; but both lever-arms are powerfully pressed. To both the resistance is justly supposed to be equal. The result, viz. the descent of the forehead, shows that the same resistance at the ends of both lever-arms has acted more powerfully on the posterior. Here the

¹ Chap. iii. p. 60.

lever-arm is to be measured not from the occipital foramen but from the bitemporal diameter, and the posterior lever-arm is the longer. In natural labour the lever-arms are measured from the centre of motion, the occipito-atlantoid articulation and the anterior is the longer, and consequently the occiput descends. Here it is otherwise. But even in cases of contracted pelvis a mechanism identical with that of natural labour comes into play, when the impeding contracted conjugate is passed; and then, as is well known, the occiput descends, the bitemporal diameter being no longer the centre or fulcrum on which the occipito-frontal lever moves; but the occipito-atlantoid articulation as in the early part of labour in a natural pelvis.

CHAPTER XV.¹

ON THE CAPUT SUCCEDANEUM, THE PRESENTATION, AND
THEIR RELATIONS IN CASES WHERE THE HEAD COMES
FIRST.

I HAVE found so much indefiniteness or error in the subject matter of this chapter, that I have thought it necessary to attempt to give some precision and clearness to the terms "Presentation" and "Caput Succedaneum," and to show some of the relations which the one bears to the other. I hope to convince my fellow-students of the necessity of adopting the views I now submit ; and I have no doubt of their at least agreeing with me in feeling the need there is of exact definition of terms in such discussions as those concerning the mechanism of parturition. If I do nothing more than explain my own views, I shall have the advantage of starting from them as standpoints in future writings, and of more easily carrying with me the intelligence, if not the convictions, of my readers.

It is apparent that, before describing the relations of one thing to another, it is necessary to define clearly what these things are—in the present case,

¹ See *Edinburgh Medical Journal*, July 1861.

what the caput succedaneum is, and what the presenting part is.

With a view only to the present discussion, it is not needful to describe fully the nature and origin of the caput succedaneum. It is defined sufficiently as a swelling which may be produced during labour in the scalp of the fœtus. When it occurs, it is not formed indiscriminately upon any part of the cranium of the child, but upon those parts which are little supported or altogether unsupported by the structures forming the maternal passages. In the course of labour, after the evacuation of the liquor amnii, the child is during pains subjected to strong pressure from the parturient forces, and equally strong counter-pressure from the resisting maternal passages. Every part of the child is subjected to these forces, except that adjacent to the as yet undilated passage through which the child is being urged. This last suffers in some parts no counteracting pressure, and in some it may have only a modified degree of it, and upon all such parts the swelling may be formed.

Schwartz,* Litzmann,† and Olshausen‡ allege that a slight caput succedaneum may be formed before the discharge of the waters, but I think the evidence they adduce, and the explanations they give of the admittedly rare occurrence, are very unsatisfactory. For instance, Schwartz says he could perceive it in

* *Die vorzeitigen Athembewegungen*, S. 289.

† Volkmann's *Sammlung*, No. 23, S. 187. ‡ *Ibid.* No. 8, S. 61.

cases of contracted pelvis, while the head was still movable above the brim. Further, Schwartz and Litzmann seem to attribute it to pressure on the veins, which I regard as an unsatisfactory and insufficient account of its causation in any case. All of them seem to imply the independence, at least in some cases, of its production upon the strength of pains, an independence which I cannot admit in any case. Before the rupture of the membranes, and discharge of the waters, the head may undergo shearing or other alteration in shape, since it may be pressed upon at that time. Before rupture of the membranes, there may exist œdema of the scalp, and this occurrence may be favoured by the dependent position of the head, but this œdema will be distinguishable by its extent, and by its characters on being laid open, from a true caput succedaneum. After all, it is quite conceivable that a true caput succedaneum may be formed before the discharge of the waters, although its occurrence is, for me, not yet demonstrated. It is conceivable in the nearly complete absence of the waters, when there is no fluid forming a connection between that before the head and that in the uterine cavity; and in a case where the occipito-frontal, or occipito-bregmatic circumference of the child is so closely embraced by the adjacent structures as to prevent any connection or action of the water in the uterine cavity on that below the presenting part.

A caput succedaneum may be formed on the presenting part of the child, whatever that part may be. In ordinary labours the presenting part varies, being a space around the presenting point of various shape and extent. Whatever may be its shape or extent, after discharge of the waters, there may the caput succedaneum be formed. Of course it will be formed, in highest development, when there is most delay and difficulty, and, notwithstanding what some authors (as Litzmann) assert, it will certainly be developed in proportion to the delay and difficulty; yet the actual amount may not be exactly the same in different cases, though the delay and difficulty may be nearly alike. There are three principal or frequent places of delay and difficulty, and at these the three most characteristic kinds of caput succedaneum are formed: *first*, before the complete development of the cervix uteri, giving the caput succedaneum of the first stage; *second*, at the outlet of the bony pelvis, giving the caput succedaneum, generally called that of the second stage; and *third*, at the orifice of the vulva, giving that of the termination of the second stage. The last two may be fused into one another; but the last, though often to be seen, is seldom carefully observed. When in a characteristic form it is elongated and pear-shaped, and generally lies obliquely on the sinciput, extending from the upper and posterior part of the presenting parietal bone, and tapering over the squama of the occiput towards the nape of the neck.

The caput succedaneum is very incompletely defined unless its direction is observed, as well as the influence of this direction in modifying its shape,—in other words, in giving it different degrees of thickness in different portions of the area upon which it is formed. Now it is evident that this direction will be the direction of least resistance. It will not necessarily be the direction of the propelling forces, nor the direction in which the child will subsequently advance, but it will necessarily be the direction of least resistance to it at the time of its formation. The caput succedaneum of the second stage of labour is a good example of its direction not being that of the propelling force. The caput succedaneum of the first stage of labour, with the head in the brim of the pelvis, is often a good example, though not so striking as the former, of its not adopting the direction in which the child will make its next advance.

If we keep in view the area upon which the caput succedaneum is formed, and the direction in which it projects, it will be easy to understand that its mass and thickness will be greatest in the line of projection. Obstetric authors evidently assume tacitly that the thickest or most prominent part of the swelling corresponds to the centre of the area upon which it has been formed, and this is a mistake leading to further erroneous conclusions. No doubt, if the direction of least resistance be a line at right angles to the centre of the unsupported area, then the thickest portion of

the caput succedaneum will indicate the centre of the unsupported area. But if the direction of least resistance be a line at right angles to the unsupported surface of the child's head, but not passing through its centre ; or, if it strikes this surface at an acute angle (as generally occurs in the history of the caput succedaneum of the first stage of labour), then the greatest thickness of the swelling may be very far from corresponding to the centre of the unsupported area on which it is formed (that is, in the case of the caput succedaneum of the first stage, the centre of the os uteri).

The term presentation is used by authors to signify very different and ill-defined things. With some it means the whole part of the head that can be reached by the finger. By others it is used to imply the part of the head first reached by the examining finger. By others again it is employed simply to mean the part coming first. For any of these meanings I would suggest the use of the term "presenting part."

In Dr. Tyler Smith's *Manual* a passage occurs which indicates a feeling of need of definiteness on this point :—"It is necessary," he says, "that what is meant by the presenting part of the foetal head should be clearly defined. Hitherto a good deal of confusion has prevailed upon this subject. Is it the part found most prominent within the ring formed by the soft parts of the parturient canal in the different stages of labour—namely, the os uteri, the vagina, and the

ostium vaginae? Is it the part of the head found lowest in the pelvis during the progress of labour? Or is it the part first met with on introducing the finger into the pelvis in the direction of its axis? It will be found that all these points of view are mixed up together in some of the best and most recent works on obstetrics, with the effect of causing considerable uncertainty. For instance, the right tuber parietale is very commonly said to be the presenting part in the first and fourth positions, as the head passes through the brim and upper part of the pelvis, because it is the lowest point met with on introducing the finger into the vagina. It is, however, more frequently—indeed, almost invariably—felt through the anterior wall of the cervix, and not within the ring of the os uteri, unless after the full dilatation of the latter. I would suggest that it would be best to define the presenting part in every kind of cranial position or presentation, as that portion of the foetal head felt most prominently within the circle of the os uteri, the vagina, and the ostium vaginae, in the successive stages of labour.”—(P. 282.)

I regret that I cannot adopt Dr. Smith's definition, simply because it is not more definite than its predecessors. For we have no explanation of what is meant by the most prominent part; and if we had a definition, we could not ascertain it with any certainty; and further, were it ascertained with certainty in any case, it would be a part having no important mechani-

cal relations to anything ; or, in other words, no fixed relations to the passages in the mechanism of parturition. The definition I submit in this paper is at least certain, and fixed, and important. Although it may be in any case impossible to make it out exactly, it is always to be made out approximately, and is the centre of the advancing surface of the child. It is, at least, capable of being always understood, if not marked on the actual surface of the child.¹

It will be apparent, on reflection, to all obstetricians, that this vagueness can no longer be allowed to rest upon the useful and important word presentation. At present it is generally used to mean the part first reached by the finger examining per vagi-

¹ On this subject Leishman has the following reflections, which I introduce with the remark that it appears to me that it is our duty to seek scientific accuracy, to seek good definitions, quite as much when we cannot in practice adhere to them as when we can. "As regards the word Presentation," says he (*Mechanism of Parturition*, p. 55), "the difficulties are even greater, and the confusion among various writers more embarrassing. If we imagine a series of planes radiating from a common centre in front of the pubes, and passing to the posterior part of the pelvic wall, the upper of these being the plane of the brim, the lowest that of the outlet, the parabolic curve which passes through the centre of them is manifestly the axis of the pelvis. Now, if we are forced to name a *point* as the presentation, we might, with some show of propriety, adopt that definition which limits the presentation to that point on the surface of the child's head through which the axis of the pelvis passes.—(Dr. Matthews Duncan, *Edinburgh Medical Journal*, 1861.) But the objection to this is to be found in the fact, that it is practically impossible to determine this with anything approaching accuracy, and

nam, and passed in the direction of the axis of the pelvis. This is the meaning of it in Nægele's writings; and this definition, if modified so as to be exact and incapable of being misunderstood, will, I believe, be found satisfactory. It might be wished to reduce the "presentation" to a point instead of a part. But, at present at least, in view of the mobility of the head in the pelvis, and on the neck, and considering the difficulty of arriving at nice precision, the presentation, while regarded as a point, may be spoken of in practical discussions as a part. The only definition, having sufficient exactness, which I can suggest, necessitates the adoption of a point, and it is that point on the surface of the child's head, or

therefore we must adopt, for our information and guidance in practice, some more simple, if less accurate, idea of the term. This being then—so to speak—beyond the pale of scientific accuracy, every one has been left to attach his own interpretation to the phrase, some regarding it as the part lowest in the pelvic cavity; some as the part which the finger first touches when introduced in the axis of the outlet; and others as the part which is lowest with reference to the plane of that part of the cavity in which the head is situated. Of these the last is the most correct. Dr. Tyler Smith, who seems to have recognised both the difficulty and importance of clearly understanding what a presentation is, suggests 'that it would be best to define the presenting part, in every kind of cranial position or presentation, as that portion of the foetal head felt most prominently within the circle of the os uteri, the vagina, and the ostium vaginae, in the successive stages of labour.' This definition is so far faulty in overlooking the relation which the head bears to the bony pelvis, but it is nevertheless the most correct and intelligible of any that I have seen."

other part advancing first, through which the axis of the pelvis passes. It is evident that this corresponds to the part first touched by the finger, if passed in the axis of that part of the pelvis in which the presenting part is lying. But if the presenting part is high up, no finger can do this; and wherever the presenting part may be, no accoucheur can, groping with his finger, fix very exactly the position of the imaginary line forming the axis of the pelvis. The direction of the axis may be more easily found than the axis itself.

It must be remarked that, while the presenting point is frequently the lowest in the pelvis, it is not necessarily so; that while it is frequently in the part first reached by the examining finger, it is not necessarily so; that while, in the first stage of labour, it frequently corresponds to the area encircled by the os uteri, it is not necessarily in that area; and that while in the course of the second stage of labour it frequently corresponds to the lumen of the vagina, it does not necessarily do so, and generally actually does not do so. In all of these cases it is for the same reason that the presenting point and the various parts named are not always identically placed, or so that the presenting point corresponds to the lumen of the passage. The reason is, that the various parts named are occasionally not in the axis of the part of the pelvis occupied by the advancing part.

The definition just given of the term presentation has the very great advantage of adapting itself to the fundamental writings of Nægele on the mechanism of parturition. These writings will, indeed, be found easier understood if the definition above given be assumed as that which Nægele would have given had he addressed himself specially to the point. It is to be regretted that he did not; for, so doing, he would have probably more carefully considered the value of the caput succedaneum as an index of presentation, and thus avoided some inaccuracies, if not errors, which this deficiency has led to in his admirable work.¹

In order to facilitate the comprehension of what has now to be said regarding the relations of presentation to caput succedaneum, it will be well for

¹ The following passage from Dr. Churchill's Manual is cited in illustration of the present method of treating this subject. In the opinions expressed by Churchill he is supported by Nægele, Hohl, Scanzoni, Cazeaux, and Braun, as well as by British authors.

Speaking of the diagnosis of cranial presentations, Dr. Churchill says (p. 196, Edition 1860)—“We possess an unfailing test of the correctness of our diagnosis in the tumour of the scalp, or ‘caput succedaneum,’ as it has been called. It is formed by the pressure of the head against the openings through which it has to pass—*i.e.* first against the circle of the os uteri, and secondly against the circumference of the vaginal orifice; and it always forms on the lowest or presenting part, so that the primary tumour indicates the part of the head which presented at the os uteri, and the primary and second together, that which occupied the lower orifice.” It is evident that in every respect these statements are too vague.

the reader to suppose that the head is presenting in the usual way. The general principle as to their having no absolute or necessary connection with one another has already been stated. To apply this principle, and show its influence at different times, still remains.

In the first stage of labour, before the os uteri is much dilated, the caput succedaneum, if present, always corresponds to the os. Wheresoever the os uteri may lie, there is the caput succedaneum. The caput succedaneum may or may not correspond to the presentation or presenting point. It will always correspond with it, if the axis of the part of the pelvis in which the os lies passes through the centre of the os. When the os is at or near the brim of the pelvis there is no doubt that, under standard conditions, this is the case ; and consequently the caput succedaneum of the first stage frequently coincides with the presenting part at that time. In the first stage of labour, before the os uteri is fully dilated, and while it is at or near the brim, the direction of the caput succedaneum corresponding with the direction of the vagina will be oblique ; that is, it will not strike the head at right angles to the presenting part, but at an acute angle opening towards the anterior parts of the mother. This corresponds pretty nearly to the direction of the index finger, as ordinarily employed in vaginal examination. As a result of this oblique direction, the caput succedaneum will be

much thicker over the anterior portions of the presenting part than over the posterior,—that is, over the portion near the anterior lip of the cervix than over that adjacent to the posterior. And this is a matter of common observation.

In the course of the second stage, after the complete dilatation of the os, and while the presenting part descends more or less in the axis of the superior strait, the relations of the caput succedaneum and presentation will approximate to those described as existing between them in the first stage of labour. In the course of the second stage, after the head has begun to advance less or more in a direction forwards, the presenting point will not be in the centre of the caput succedaneum, but nearer its anterior margin than its posterior ; and the same will be true of the caput succedaneum often formed while a projecting portion of scalp is encircled by the rigid vaginal orifice. In such cases, as I have said, the caput tapers towards the nape of the neck. It is real caput succedaneum ; yet I have seen this tapering portion described by high authority as the result of gravitation or diffusion of the fluid in the œdematous parts after birth ! The position of the presenting point, anteriorly, on the child's surface, to the centre of the caput succedaneum, is easily accounted for by the development of the genital passage taking place chiefly posteriorly. The axis of the developed genital passage—passing through the presenting point—runs

not in the centre of the lumen of the undeveloped passage where the centre of the caput succedaneum is, but near to or in the anterior margin of the yet undeveloped posterior wall of the genital canal.

CHAPTER XVI.

THE EXPULSION OF THE PLACENTA.¹

IN this short chapter, I use the comprehensive word "mechanism" in the limited sense and misapplied manner in which it has hitherto been generally employed in obstetric literature ; that is, as implying mere relative position at different stages of progress.

The present subject has not, I believe, attracted sufficient attention, else, I am sure, the little addition to its history that I propose to make would, long ere this time, have been contributed. No one will dare to say it is unimportant ; for there is no truth in nature, which, however insignificant it may appear, has not even now, or may not have in future, bearings upon practical rules which may be of value to the obstetrician.

A study of our copious obstetric literature satisfies me that the point I propose to describe in the mechanism of labour is generally misunderstood. To illustrate the erroneousness of widely-entertained and generally-written views, I might refer to many authors, and to woodcuts and plates. Among the former, I select only the excellent and respectable

¹ Read to the Obstetrical Society, March 22, 1871.

Ramsbotham, who says,¹ "The placenta passes through the vagina inverted, so that its foetal surface becomes external." Among the latter, I select the most recent, and the most elaborate and largest—one of the wall-plates² of Professor Schultze of Jena, from which the Plates 1 and 2, here given, are copied on a reduced scale as to size.

Every one knows that the membranes are expelled inverted or flapped over upon themselves; and the same view is held regarding the placenta. It is to this last part's condition during expulsion that I am now directing attention.

The wall-plate of Schultze is an admirable representation of the expulsion of the placenta as it occurs in the first two modes described by Baudelocque; and I may add that this author's descriptions have been repeatedly copied. Baudelocque, drawing chiefly on his imagination for his supposed facts, says³—
"Sometimes this separation begins at the centre of the placenta, and sometimes at some point of the circumference, which produces different phenomena. In the former case, the middle of the placenta being pushed forward, it forms a bag behind which fills

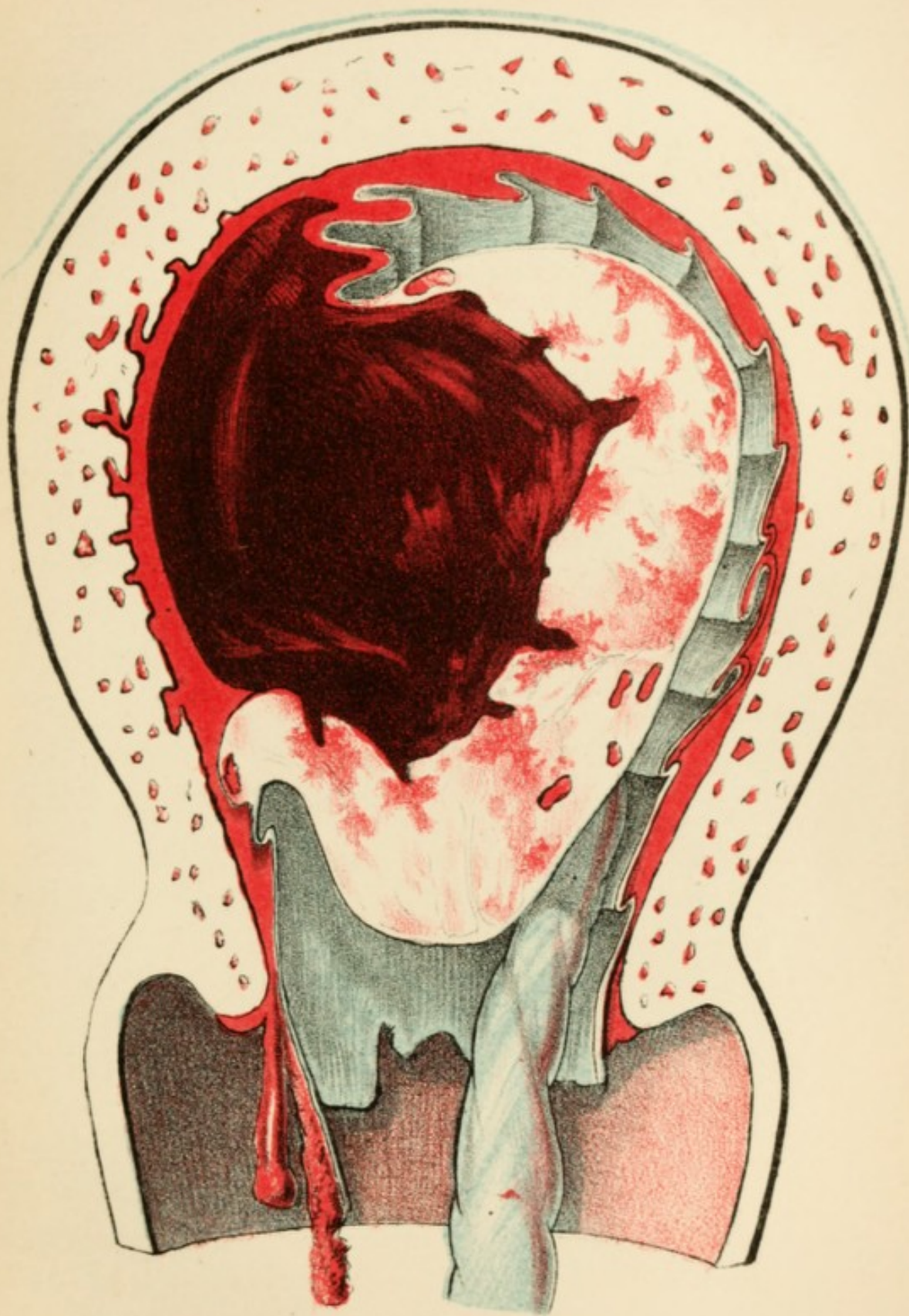
¹ *Obstetric Medicine and Surgery*, 5th edition, p. 126.

² *Wandtafeln zur Schwangerschafts- und Geburtskunde*—an excellent and useful work. In the second volume of *Hubert's Cours d'Accouchements* are given several figures with relative description of this subject. The whole is a remarkable example of description based on imagination not on observation.

³ *System of Midwifery*, Heath's Translation, vol. ii. p. 4.

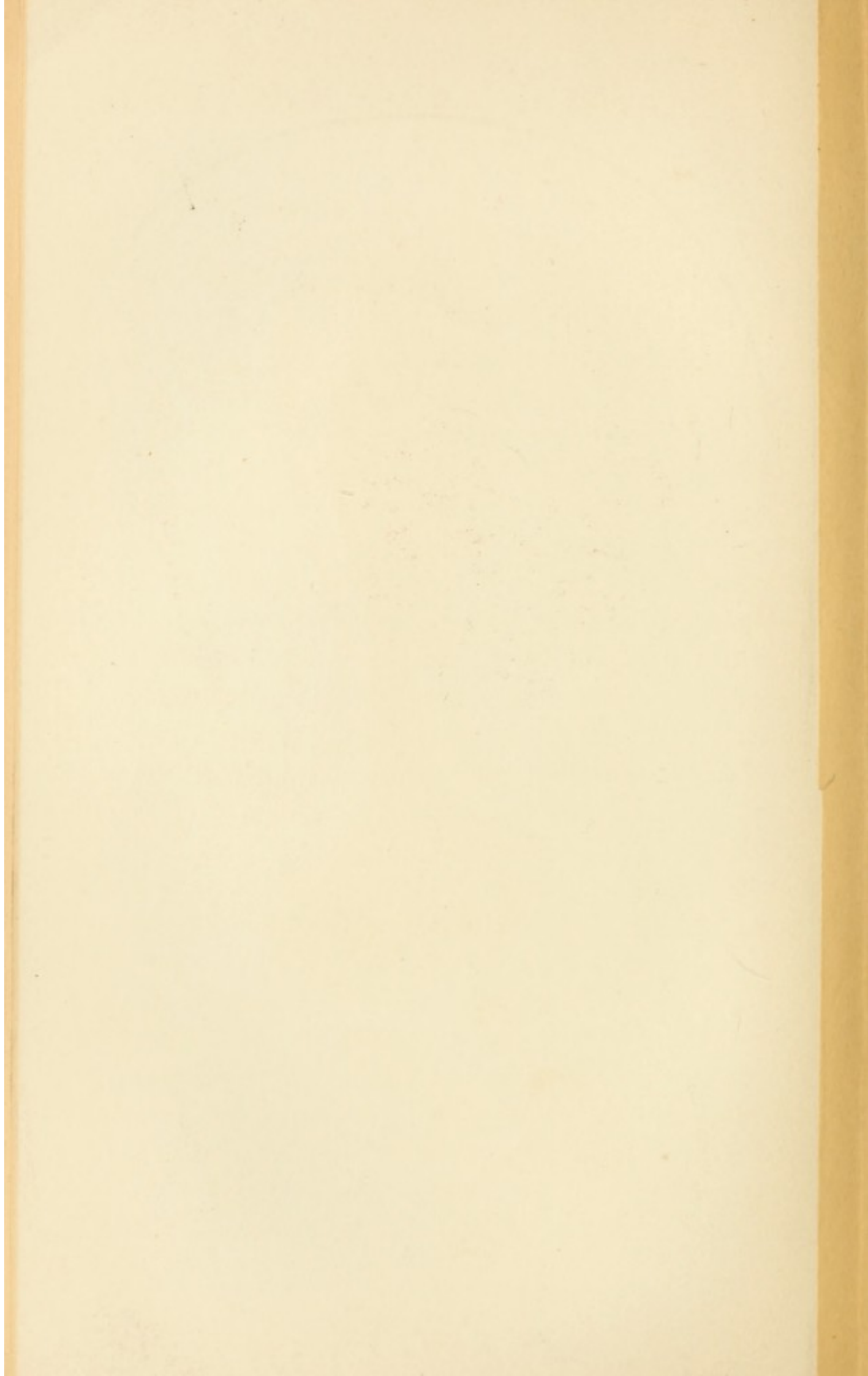
with blood, and it presents that side to the touch which is covered with the membranes and vessels. The placenta forms nearly a similar bag, and presents in the same manner, when it begins to separate from the uterus at that part of its edge which is furthest from the orifice. But things go on very differently when the separation begins at its lower part, especially if it be in the neighbourhood of the orifice. In this latter case the placenta rolls itself up in the form of a cylinder, and according to the length of the uterus, so as to present its anfractuous surface to the touch; and its exit is always preceded by a little fluid blood." Schultze¹ goes a little farther than Baudelocque, and actually describes the accumulated hæmorrhage from the uterine sinuses as co-operating

¹ *Wandtafeln*. Tafel xvi. fig. 3, Demonstration der Wehen. Schultze's words are:—"Schon die Wehen, welche das Kind vollends austrieben, verkleinerten die Gebärmutter so bedeutend, dass der Mutterkuchen zum grossen Theil von ihrer Wand abgetrennt wurde. Die Nachwehen und der Bluterguss, der aus den zerrissenen Blutadern der Gebärmutterwand reichlich erfolgt, drängen den Mutterkuchen vollständig von der Gebärmutterwand ab und in die jetzt lehre Eihöhle hinein." A similar mechanism is described by Spaeth. Speaking of the pains, he says:—"Unter ihrer Einwirkung wird der Uterus allmählig kleiner, seine Innenfläche zieht sich über die entsprechende Fläche der Placenta zusammen und vollendet die Lostrennung der letzteren, welche schon in der vorigen Geburtsperiode begonnen und das abfliessende Nachwasser blutig gefärbt hat, gewöhnlich derart, dass die Placenta zuerst in ihrer Mitte gelöst mit der Fötalfläche durch den Muttermund heraus gewölbt und endlich vollends in die Scheide herunter getrieben wird."—*Compendium der Geburtskunde*, S. 85.



SEPARATION & EXPULSION OF PLACENTA,
after Schultze.

Inamnet. 1



to push down the already almost completely detached placenta and complete its separation,—a view so utterly unsupported by observation or argument, and so unlikely, that I shall not say anything more regarding it. (*See* Plates 1 and 2.)

Now, the erroneous belief that the placenta generally descends presenting its foetal surface, seems to me to have arisen from observers not keeping in mind the very great frequency with which the natural mechanism of delivery of this cake is interfered with. I may say that it is unfortunately the rule to interfere with this part of the natural mechanism of delivery. Such interference, generally carried out as it is by pulling the cord, produces an unnatural mechanism—inversion of the placenta, as Ramsbotham calls it; and this unnatural mechanism, this inversion of the placenta, comes to be described as the natural conduct of the delivery. The placenta, says Rigby,¹ “descends into the vagina inverted, *i.e.* with its foetal or amniotic surface turned outwards. Whether or not,” he adds, “this is produced by pulling on the cord, is perhaps a question.”

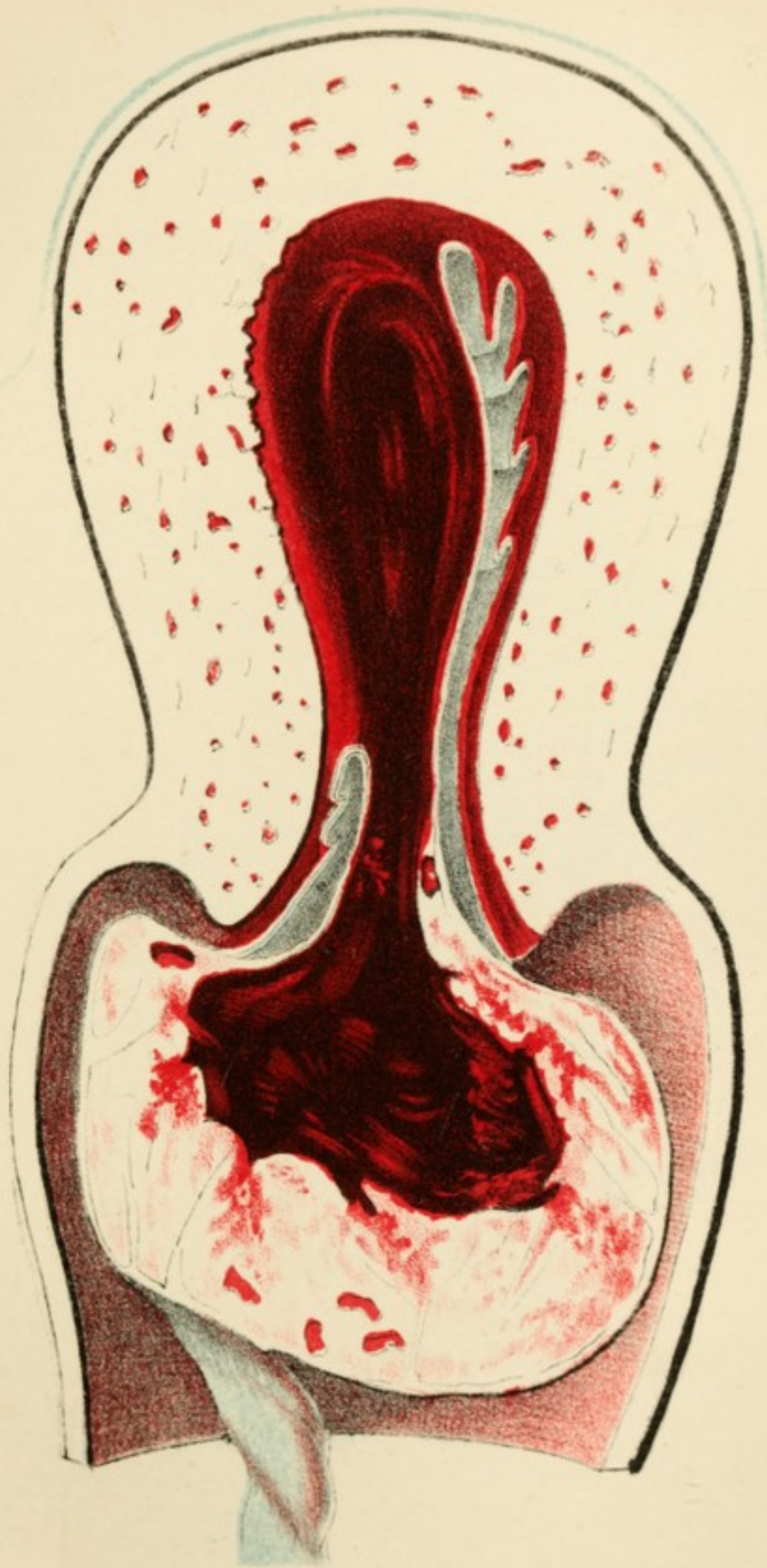
To find out the natural mechanism of the expulsion of this cake, it is only necessary to watch the process as nature conducts it; that is, in cases in which the practitioner does not try to modify it in any way. This any one can easily do, by wounding or otherwise marking the part presenting at the

¹ *System of Midwifery*, p. 103.

mouth of the womb, and then after its birth examining the placenta to find where the wound is ; or the observer may pinch the part first presenting at the vaginal orifice, and retain hold of it till the whole is born, and then find what is the part so pinched.

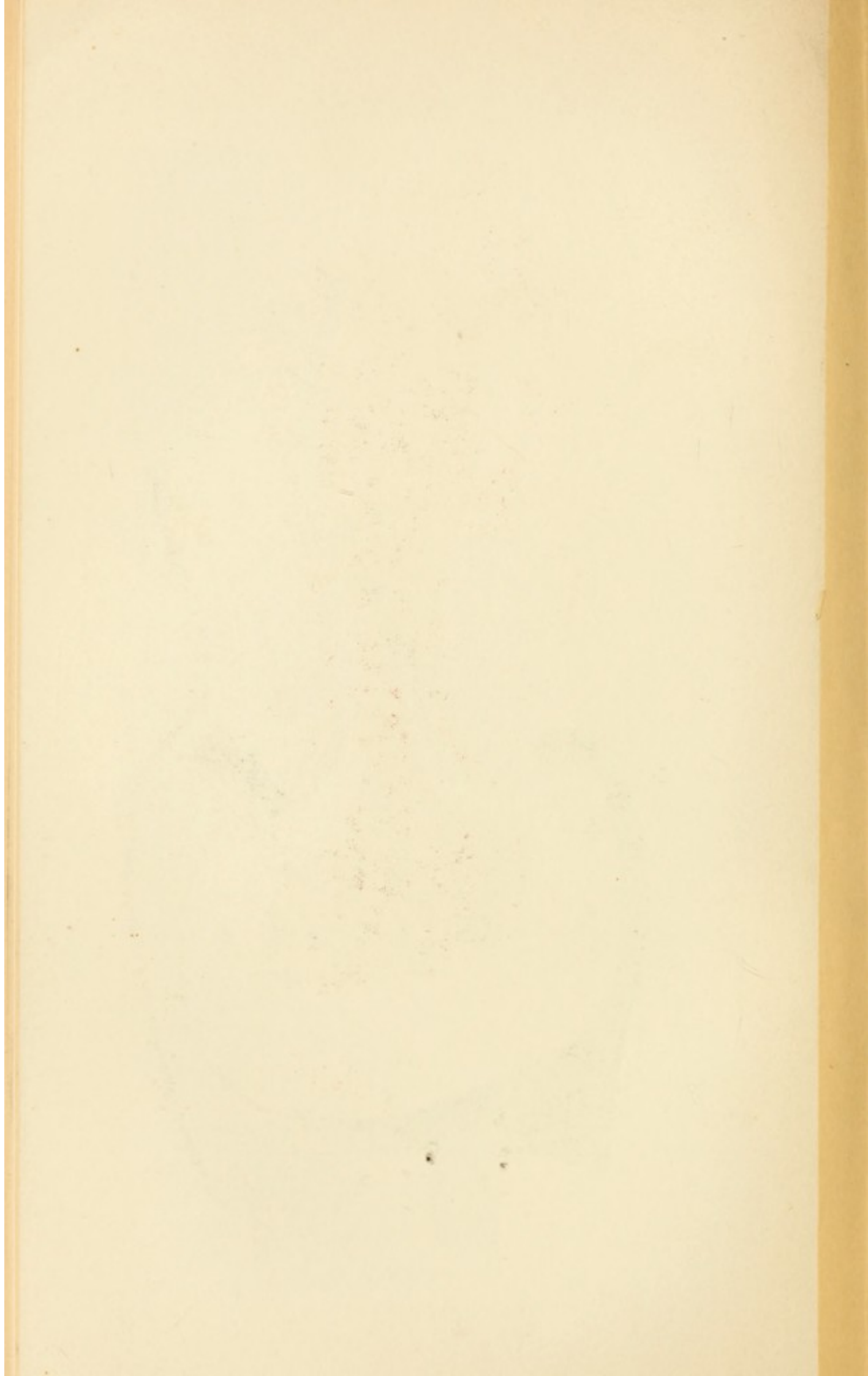
In this way it is easily discovered that the part of the placenta presenting at the os uteri, and subsequently at the os vaginae, is not the foetal or amniotic surface, but the edge of the placenta, or a point very near the edge. When it is not exactly the edge, the placenta is not inverted or folded upon itself, there is only a little of the lower marginal part of the cake transversely folded up, as I have depicted in the third plate ; it is still really the edge that presents, only thickened a little by being folded on itself ; and I think this folding occurs chiefly in placentæ which are thin at the part folded. This folding is manifestly caused by the pulling up of the edge by the still adhering membranes ; the resistance of the force required for their separation being greater than the rigidity of the marginal part of the placenta so folded.

My own numerous observations satisfy me that the inversion of the placenta, or its folding upon itself transversely to the passage, or the presentation of its foetal surface, as authors describe, and as Schultze and others depict, is a very rare occurrence, —so rare as to debar describers from calling it *a* natural, and still more from calling it *the* natural, mechanism. The placenta is folded upon itself dur-



EXPULSION OF PLACENTA,
after Schultze.

*Incisus 2 - Placenta is
inverted*



ing the process, as I have attempted to depict in my drawing ; but the folds are according to the length of the passage, not transverse to it, as inversion or presentation of the foetal surface implies. (See Plate 3.)

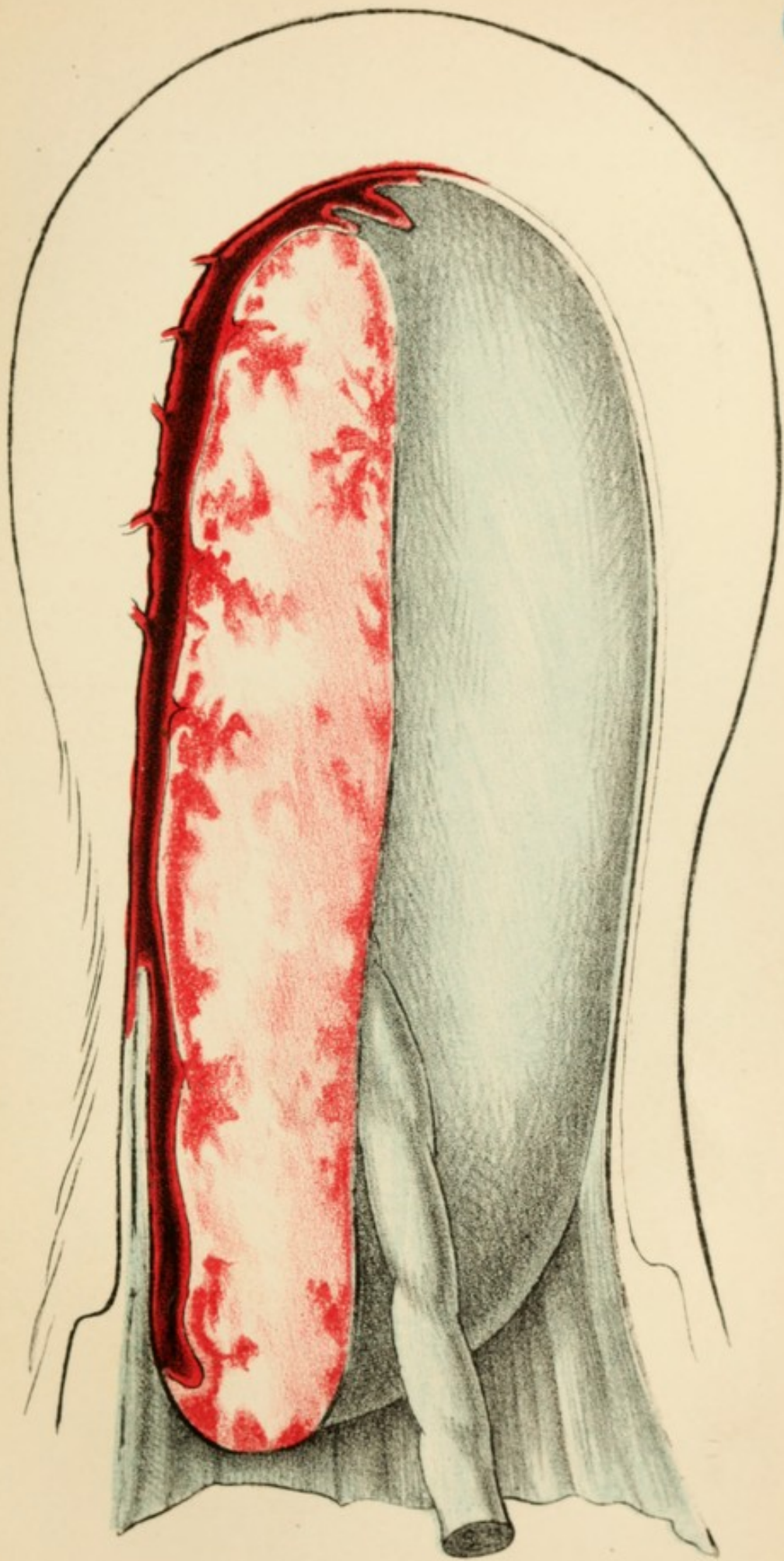
These remarks are strikingly corroborated by the careful remarks of Von Ritgen,¹ and by the memoir of Dr. Lemser on the Physiological Separation of the Placenta. These gentlemen's observations appear to me to be inconsistent with the views of Baudelocque and Schultze as to the separation of the placenta, and they are the best observations on the point which we possess. But it is more interesting, with a view to the present discussion, to remark that Ritgen and Lemser describe the border or edge of the placenta appearing in the os uteri as the ordinary result of separation and propulsion of the cake.

The advantages of the natural mechanism, as I have described it, are obvious. It is true, that after the passage of the bulky child, there is no such necessity for a mechanism of the delivery of the comparatively small placenta as there is for the passage of the child's head. There is ample room and verge enough for the placenta passing in any way. But the natural mechanism claims respect as the *natural* mechanism, and, moreover, it presents obvious advantages over any other mechanism. Just as the child's head passes through the pelvis so as to dilate the passages as little as may be, or in the manner de-

¹ *Monatsschr. f. Geb.*, Band VI. 1855, S. 273.

manding least expenditure of force, so also does the placenta. It comes edgeways. If it came inverted, or transversely doubled up, or folded into a cup shape, we should have a body passing that required at least twice as much space as is required if it passes edgeways, and only longitudinally folded. But this is not the only advantage of the natural mechanism.

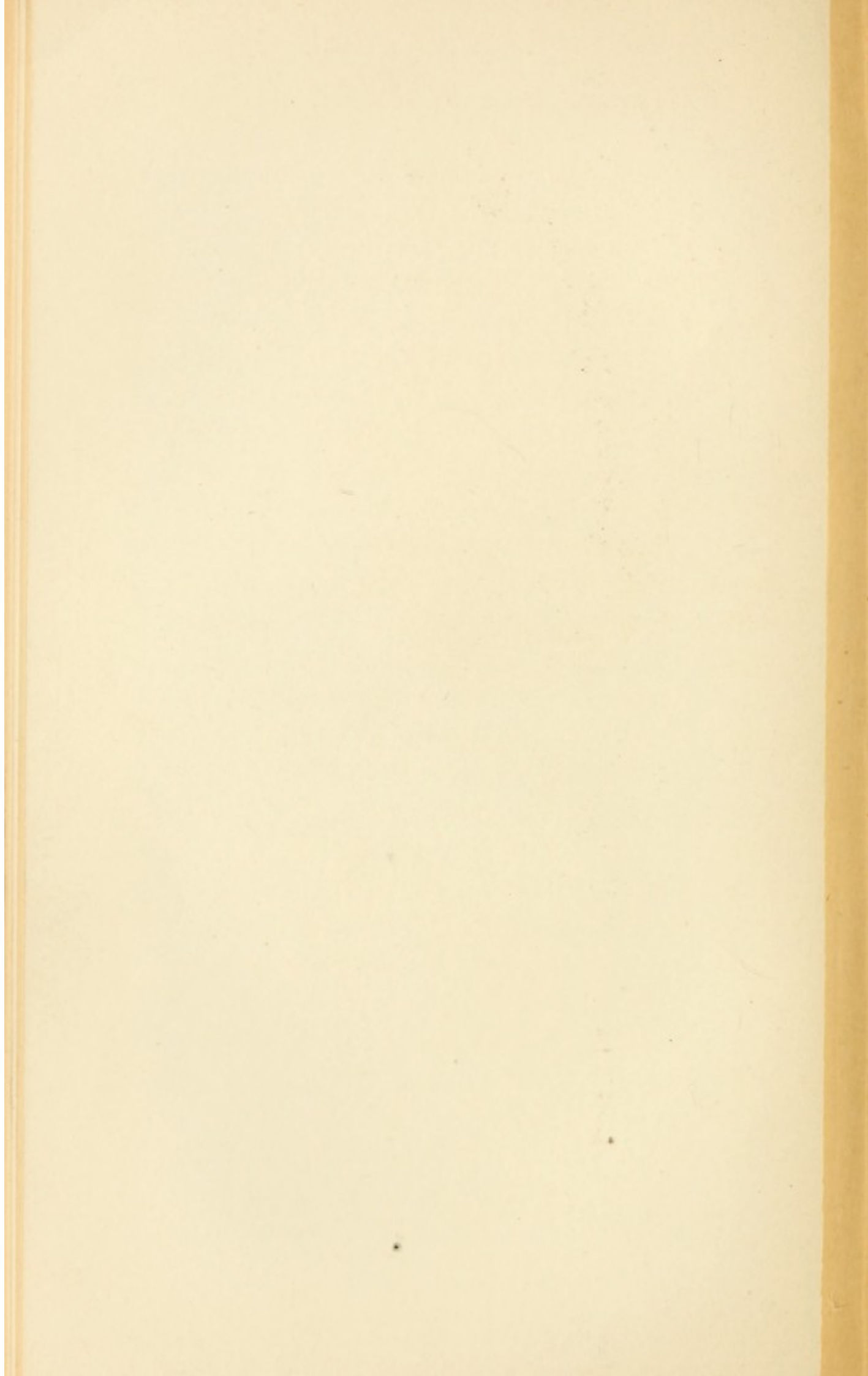
If the placenta is expelled as Baudelocque describes, and as Schultze depicts, then a loss deserving the name of a hæmorrhage is almost as necessary as it is certainly a generally described accompaniment of the process of the expulsion of the placenta. For the placenta has a certain amount of rigidity, and its folding on itself and the forcing of it into a cup-like shape cannot be effected without a hollow space being offered for the reception of blood, or indeed without a certain force being exerted to produce the folding and a vacuum, which force will also tend to draw blood into the said hollow from the open uterine sinuses which were in apposition to the part folded. Baudelocque's descriptions and Schultze's drawings, while they do not give what is natural, yet do indicate a mechanism of which they justly make considerable hæmorrhage a necessary or nearly necessary part. It would be easy to show great, if not insuperable, difficulties in the way of accepting Baudelocque's description of this mechanism. It is enough for me to assert that it does not exist as a fact, save as a rare exception to the ordinary process. I say no more,



SEPARATION & PARTIAL EXPULSION OF PLACENTA,

Matthews Duncan.

Correct. Not Inverted



because I have no intention of entering here on the subject of hæmorrhage during the detachment and expulsion of the placenta.

According to Baudelocque and Schultze, the folding is always on the uterine surface. But this is far from being the case. According to Lemser,¹ it is more frequent than folding upon the foetal surface. In my drawing I have represented the placenta, with a view to pictorial facilities, as folded upon its foetal surface.

If the placenta comes edgewise, its uterine surface glides along the surface of the uterus ; its foldings, parallel to the length of the maternal passages, are well squeezed together, and little space is offered for the reception of blood flowing from uterine sinuses. The uterine wall keeps close to the folded placenta. The uterus contracts, forces the placenta downwards, and at last its body is nearly globular and empty. There is no hæmorrhage worthy of the name. Hæmorrhage, when it does occur, is not demonstrated to take place according to the description of Baudelocque or the plate of Schultze ; and I believe these gentlemen do not give the correct account of it. Authors too frequently, I may say almost invariably, describe too great an amount of hæmorrhage as part of this natural process. I admit that the frequency of some hæmorrhage is a strong argument

¹ *Die physiologische Lösung des Mutterkuchens.* Giessen, 1865. S. 17.

in favour of this proceeding. But I believe that interference, which, though common, is frequently injudicious, is occasionally the cause of this hæmorrhage, which is, therefore, in such circumstances, unjustly laid to the account of the natural mechanism. It is far from uncommon to observe labours in which there is no hæmorrhage, in which not an ounce of blood is lost during delivery, there being only enough to smear the uterine surface of the placenta with a very thin layer. This absence of hæmorrhage I regard as the natural state, and in this I suppose all obstetricians will join me, at least if I introduce the element of desirableness as an indication of naturalness. Such absence of hæmorrhage depends on the adoption of what I describe as the natural mechanism. The presence of hæmorrhage is a part of the erroneously described natural mechanism, and to me this presence is one proof of the erroneousness of the description.

But although the mechanism of Baudelocque and the picture of Schultze do not give the natural process, they indicate a state of matters which is frequently observed after the separation of the placenta. Schultze's second drawing is an admirable representation of what takes place frequently, perhaps generally, when any considerable force is used to deliver the placenta by traction of the cord. Then, indeed, truly, the placenta is inverted, and its edge puckered up purse-like. The insertion of the cord comes first,

as is so frequently represented in woodcuts. The placenta is transversely bent on itself, and puckered up; hæmorrhage flows to fill up the partial vacuum which is thus produced. The inverted mass forms a firm plug, closely filling the vagina. Traction on this plug is exactly like traction on the piston of a pump. If hæmorrhage does not naturally take place to fill up the void which tends to be formed beyond the placenta, then it is powerfully attracted and induced by the piston-like action of the placenta pulled by the cord. The interior of the uterus, already scarified by the separation of the placenta, requires but this pulling at the cord to be effectively cupped.

From all this there follows the very valuable corollary, that in practice the third stage of labour should be left to nature, and that, when interference is required, the natural mechanism of the birth of the placenta should be as closely imitated as circumstances admit.

I shall conclude with a quotation from Cazeaux,¹ which shows that this practical view has not been altogether neglected—" 'When the placenta is partially engaged in the orifice (of the womb) by a portion of its periphery, this plan,' says M. Guillemot, 'ought to be somewhat modified; for in this presentation, the root of the umbilical cord, instead of corresponding to the cervix, is higher up in the uterine cavity; and hence, if the

¹ *Theoretical and Practical Treatise on Midwifery.* Bullock's Translation, p. 385.

operator resorts to traction, the centre of the placenta will have a tendency to enter the orifice, and thus add its bulk to the disk already engaged there. Such a disposition sometimes constitutes an obstacle to the further delivery of this mass ; but it is surmounted by making some moderate tractions, not on the cord itself, but rather on the part previously engaged, by applying two fingers on its surfaces.' We have," adds Cazeaux, "had numerous opportunities of testing the practical utility of M. Guillemot's advice." Swayne,¹ also, has noted the advisability of hooking down the edge of the placenta when extracting it, by pulling on the cord.

¹ *Obstetric Aphorisms.* 2d. edit. p. 26.

CHAPTER XVII.¹

ON THE SIZE OF APERTURE NECESSARY FOR THE PASSAGE OF THE PLACENTA AND FOR THE PASSAGE OF THE ACCOUCHEUR'S HAND.

At a late meeting of the Obstetrical Society I raised the question of the dimensions of an aperture such as the os uteri, when just capable of transmitting the accoucheur's hand, and its dimensions when just or earliest capable of transmitting the placenta. These questions had evident bearings upon the methods of treatment of placenta prævia, which was the subject under discussion; and, as their solution was not known to myself or to the members present, I resolved to attempt to settle them.

The first of these two questions which I shall consider is the smallest aperture capable of transmitting a placenta uninjured. It is evident that the aperture will be circular, or nearly so, and in the experiments I used a circular aperture. It is also evident that, with a view to exactness in the experiments, and with a view also to the practical utility of them, the placenta should be brought through uninjured. Indeed, injury or laceration would make little difference in the results, unless the organ were greatly torn;

¹ Read to the Obstetrical Society, February 25, 1874.

for, supposing the organ to be circular and of equal thickness in all its parts, laceration would not facilitate transmission through a small aperture, unless the laceration was such as to remove from the aperture a part of placenta which would otherwise be there while the thickest part or centre of the placenta was passing.

Mr. A. C. Harris, one of the resident physicians of the Royal Maternity Hospital, kindly undertook to perform for me the following experiments, and it is the results which he obtained that I now give.

TABLE OF EXPERIMENTS.

No. of Experiments.	Weight of Placenta.	Diameter of Placenta.	Thickness of Placenta.	Diameter in Inches of Aperture of Transmission.					
				1 $\frac{1}{4}$	1 $\frac{1}{2}$	1 $\frac{3}{4}$	2	2 $\frac{1}{4}$	2 $\frac{1}{2}$
1	1 lb. 1 oz.	7 inch.	$\frac{3}{4}$ inch.	...	<i>e</i>	<i>d</i>	<i>a</i>
2	1 ,, 5 ,,	6 $\frac{1}{2}$,,	1 ,,	<i>d</i>	<i>d</i>	<i>a</i>
3	1 ,, 4 ,,	7 ,,	$\frac{3}{4}$,,	...	<i>e</i>	<i>c</i>	<i>b</i>
4	1 ,, 1 $\frac{1}{4}$,,	6 $\frac{1}{4}$,,	$\frac{3}{4}$,,	...	<i>e</i>	<i>c</i>	<i>a</i>
5	1 ,, 2 $\frac{1}{2}$,,	6 $\frac{1}{2}$,,	1 ,,	<i>c</i>	<i>b</i>	<i>b</i>	<i>a</i>
6	1 ,, 2 ,,	6 $\frac{1}{2}$,,	$\frac{1}{2}$,,	<i>c</i>	<i>b</i>	<i>b</i>	<i>a</i>
7	1 ,, 5 ,,	8 ,,	$\frac{1}{2}$,,	<i>d</i>	<i>b</i>	<i>b</i>	<i>a</i>
8	1 ,, 6 ,,	6 $\frac{1}{4}$,,	1 $\frac{1}{8}$,,	<i>d</i>	<i>b</i>	<i>a</i>
9	1 ,, 4 ,,	7 ,,	1 ,,	<i>d</i>	<i>c</i>	<i>b</i>	<i>a</i>
10	1 ,, 4 ,,	6 $\frac{1}{2}$,,	1 ,,	<i>d</i>	<i>c</i>	<i>a</i>
11	1 ,, 6 $\frac{1}{4}$,,	7 ,,	1 ,,	<i>d</i>	<i>c</i>	<i>b</i>	<i>a</i>
12	1 ,, 1 $\frac{1}{2}$,,	7 ,,	$\frac{3}{4}$,,	<i>c</i>	<i>b</i>	<i>b</i>	<i>a</i>
13	1 ,, 6 ,,	7 ,,	$\frac{1}{2}$,,	<i>c</i>	<i>b</i>	<i>b</i>	<i>a</i>
14	14 ,,	6 ,,	$\frac{1}{4}$,,	<i>e</i>	<i>c</i>	<i>b</i>	<i>a</i>

In this Table—*a*=Quite easily. Its own weight sufficient to pass it.

b=Easily. Placenta requiring careful folding.

c=With slight difficulty. Requiring folding and slight pushing.

d=Difficulty. Requiring folding, pushing, and pulling.

e=Very difficult. Requiring, in addition, time and nice manipulation.

The Table shows that an aperture of fully two inches in diameter is required for the transmission of the uninjured mature placenta. Of course, in a much lacerated state, it may be brought through an aperture of smaller dimensions.

I do not remember any statement of such results as I have just given with reference to the placenta, nor do I know of any similar statement regarding the transmission of the hand. But this latter point has been described, for practical purposes, in a valuable practical way, by many authors, and of this kind of description I shall give examples. It is evident that all of them are purely practical statements, and do not in the least solve the important question which is raised in this short chapter.

Burns (*Principles of Midwifery*, etc., 10th edition, p. 370) says—"We may safely deliver whenever the hand can be introduced without much force." Collins (*Practical Treatise on Midwifery*, p. 94) says—"The earliest moment that the mouth of the womb will, by gentle efforts, permit the introduction of the hand." Rigby (*System of Midwifery*, p. 262) says—"When once the os uteri is sufficiently dilated to admit the hand." Ramsbotham (*Obstetric Medicine and Surgery*, 5th edition, p. 369) says—"When it (the os uteri) has acquired the diameter of half-a-crown ($1\frac{1}{4}$ in. diam.), or a crown ($1\frac{1}{2}$ in. diam.), it will generally suffer itself to be dilated to such an extent as will admit the hand without injury to its

structure (p. 370). As soon as the mouth of the womb is sufficiently open to admit the thumb and the four fingers as far as their second joint, we may expect that it will offer but a slight impediment to the passage of the whole hand." Murphy (*Lectures on the Principles and Practice of Midwifery*, 2d edition, p. 457) says—"If the os uteri be one-half, or even one-third, dilated, you may deliver."

All of these statements, and I might quote more of them, merely imply that you should pass your hand if you can, or if you dare. The terms of caution have no definite meaning, and are of little value, being interpreted in a different way by different minds. Now, what I desire to make out is the answer to the question—How large must the os uteri be to transmit the accoucheur's hand? or, to what extent must it be expanded for this purpose? The authors just quoted deal with and attempt to describe that stage of dilatation at which farther dilatation to the necessary amount for passing the hand may be expected. Their determinations are almost worthless, as already said; for it is very difficult to estimate the size of the os uteri, and more depends on its softness and dilatability than on its size. I have delivered, passing the hand and turning, when the os at the commencement of the operation had undergone no active dilatation. The operation was easy, and without any injury to the cervix that I could ascertain, it being soft and dilatable.

The os, supposed to be circular, must have a diameter of from $2\frac{1}{2}$ to 3 inches in order to transmit the hand of the accoucheur. My own hand can be forced through a round aperture of $2\frac{1}{2}$ inches in diameter.

In conclusion, I offer a single remark, having a practical bearing, that almost naturally follows the above measurements.

The placenta can be got away without disruption through an os of 2 inches in diameter or a little more, and the hand can be passed through an os of $2\frac{1}{2}$ inches in diameter or a little more. The placenta is soft and lacerable, and can exert little force in distending the os. The hand is hard and firm, and can be used to dilate with considerable force. The os uteri is, in cases of placenta prævia, rarely rigid and undilatable. It may, therefore, be safely asserted that, in the very great majority of cases of placenta prævia, the hand may be passed into the uterus, if the placenta can be extracted from it in a satisfactory manner without disruption.

This is not the place to enter on evident farther applications of this result, in unfavourably criticising the treatment of placenta prævia by separation and removal of the organ.

CHAPTER XVIII.¹

ON THE CHANGES UNDERGONE BY THE CERVIX UTERI DURING LABOUR.

MODERN inquiries² have shown that, during pregnancy, the cervix uteri is greatly thickened, softened, and often slightly elongated. Indeed, it undergoes hypertrophy or enlargement in every dimension. Up till the very end of pregnancy—that is, till the supervention of active labour—it is, usually, and remarkably in cases of placenta prævia, a part quite separate from the body of the uterus, its cavity forming no part of the cavity containing the ovum (as is also the case in at least several of the lower animals), but separated from it by an easily-found constriction or boundary. In many cases its cavity is gradually obliterated from above downwards, and taken up to contribute to the formation of the lower part of the cavity containing the ovum, by a process of silent or

¹ Read to the Obstetrical Society, April 23, 1873.

² See the Author's *Researches in Obstetrics*, pp. 243 and 249. See also Siebold, *Geschichte der Geb.*, II. Band, S. 717; and Kehrer, *Beiträge zur vergleich. u. exper. Geb.*, II. Heft, S. 124-126.

painless labour, going on for hours or days before the supervention of active painful labour, as Millot and Kehrer have excellently described. This is shown by a circumstance of common occurrence, namely, the discovery, on a first digital vaginal examination, at the beginning of painful labour, that the os uteri does not lead into a cervical cavity, but is merely a perforation or hole in the lower segment of the wall of the grand uterine cavity.

The condition of the cervix uteri shortly after labour is also known.¹ At this time it is in a state contrasting remarkably with that of the body of the uterus, being relaxed, thin, and elongated. Labour, which, when completed, leaves the body of the uterus shortened, thickened, and firmed or hardened, leaves the neck elongated, thinned, and softened. This elongation quickly diminishes after delivery. It is often seen to be about three inches, and Madame Boivin² describes it as sometimes found five to six inches long immediately after labour is over.

From this contrast of the conditions of the cervix uteri before and after labour, its state during labour might be safely guessed or argued. But, while we shall avoid dependence on guessing, and rely on verification by actual observation, it may be useful to point out the erroneous notions hitherto entertained on this matter.

¹ See chap. xix.

² See Guillemot, *Archives Gén. de Médecine*, 1833, p. 199.

The highly-esteemed Roederer laid it down, that, during the latter half of pregnancy, the cervix was gradually widened out or expanded from above downwards, so as to form the lower part of the uterine cavity containing the ovum ; but he gives, so far as I know, no anatomical evidence in favour of this view, which, however, he succeeded in impressing on the general obstetrical mind from his own time down to our days. Looking over his icones, we cannot, I think, be far wrong in fixing on one of his figures, Tabula VII., fig. 1, as that to which he would have directed us for at least seeming anatomical proof of his view as to the development of the cervix during the latter half of pregnancy. This figure shows the lower part of a gravid uterus laid open ; and the cervix, easily recognised by the arbor vitæ, is evidently expanded so as to form part of the ovum-containing cavity. But Roederer, having no anticipation of such an anatomico-physiological question as we are here discussing, could not be expected to foresee the criticism to which it is now subjected, and which brings his specimen into a position of hostility to his view. The criticism is, that the dissection was not that of a simply pregnant uterus, but of a uterus which had been engaged for about twelve hours in labour, and from which the liquor amnii had already been discharged. Roederer's figure shows the condition of the cervix after some hours of labour, and before the internal os is much

expanded, or an early stage of the change effected by labour upon the cervix, not the condition of the cervix before labour has commenced.

Madame Boivin, following Roederer, and arguing, I suppose, in like manner and on the same grounds—for I am not aware that she anywhere refers to actual confirmatory dissections—describes¹ all the lower portion of the uterus, the small end of its ovoid, as being, at the end of pregnancy, formed entirely at the expense of the cavity of the neck, and the internal orifice of the neck as no longer an orifice, from its having been for a long time in this way developed. At the upper part of the developed neck (she says), where the orifice had previously been, the circumference of the cavity is nearly thirteen inches, and this part is (she adds) now five inches above the external orifice. While I am not, as I have said, aware of any dissection, whether of Boivin or of any other anatomist, which confirms these statements, I may be allowed to guess that she reasoned somewhat as Roederer did, using her actual observations of what was at the end of labour, by a transference, to describe what she thought was before labour commenced. For it is at least very remarkably corroborative of this view, to note that Boivin, in an actual dissection of a woman dying immediately on being confined, to which I have already referred, as given on the authority of Guillemot, observed the same condition

¹ *Mémorial de l'Art des Accouch.*, p. 90.

and measurements which she elsewhere gives, without any actual confirmatory dissection, as those existing before labour begins. Guillemot's words may be translated as follows:—"According to Madame Boivin, the uterine neck in this state of flaccidity presents sometimes five or six inches of length and four or five inches of diameter, as she has several times seen, notably in a woman who died immediately after being confined."

The cervix uteri suffers elongation under various circumstances. Sometimes it is elongated and hypertrophied, as in certain cases of uterine fibroid. Sometimes it is elongated without any or with very little hypertrophy, as in many cases of uterine prolapse and procidentia, where the change should be called tensile elongation rather than hypertrophic elongation. In labour the elongation is different from either of these two kinds. There is no time for hypertrophy: the elongation is accompanied by thinning: it is also accompanied by dilatation. The elongation and dilatation are both tensile, and go on simultaneously.

The conditions of the cervix during labour have been well described by Kehrer.¹ They are also beautifully illustrated in the supplementary part of the Topographic-Anatomical Atlas of Dr. Braune of Leipsic. There, a specimen is delineated in which the uterus is still distended by the body of the foetus while its

¹ *Loc. cit.*, Heft II. S. 126.

head is in the pelvic excavation. In this case, while the cavity of the body of the uterus has its walls thickened, and is reduced in length to nearly a half of its original size, or to a little more than $6\frac{1}{2}$ inches, the cervix is, in contrast, thinned, dilated, and elongated to more than twice its length before dilatation began, or to above four inches.

Labour, then, simultaneously elongates, dilates, and thins the walls of the cervix.¹

The body of the uterus is active, and pushes the presenting part of the ovum against the lumen of the cervix. The cervix is mainly passive; and, while it is dilated, is also elongated and thinned. The body of the uterus, in its regular contractions, acts upon the cervix and on the vagina equably and at every part, pulling upon them and pushing the foetus into and through them, somewhat as the arms pull on the leg of a boot while the foot is being pushed into and through it. In accordance with this condition of matters, the vagina, when lacerated by the uterine efforts, as it is not very unfrequently, is torn transversely to the direction of its axis; while the direction of the force, propelling the foetus through a curved passage, leads to the posterior part of the vagina being the ordinary seat of the laceration, and also to the tear generally implicating the left more than the right half of the vagina. I am not prepared to speak from a suffi-

¹ For some remarks on this elongation, see Lott, *Archiv für Gynækologie*, 1873, V. Band, 1 Heft, S. 201.

cient induction of facts as to the characters of similarly produced lacerations of the uterine cervix ; but, arguing deductively, I have no hesitation in expressing my expectation that, when duly investigated, they will be found, in the respects referred to, to be in the same category with vaginal lacerations.¹

Contrasted with these are the more irregular ruptures of the body of the uterus which is in action. These are not lacerations or tears so much as burstings

¹ In M'Clintock's classical essay "On Laceration of the Vagina in the Course of Labour" (*Dublin Quarterly Journal of Medical Science*, May 1866), there occurs the following paragraph :—"Lacerations of the vagina—the upper part I mean—are not so frequent a consequence of parturition as are ruptures of the uterus. Let me here make an observation. Collins, and many other observers, when describing the seat of a laceration, employ the expression 'at the junction of the cervix and vagina,' or some phrase of like import. Now, all such cases—their number is but very small—I include under the heading of vaginal laceration, not merely because their claims to being considered uterine or vaginal are equally balanced, but because, in some important particulars, they more resemble lacerations of the vagina than of the fundus or body of the womb."

In an ingenious and valuable paper by Braxton Hicks (*Lancet*, 23d January 1869, p. 119) are to be found many important remarks on the subject discussed in the closing part of this paper, and from it I quote the following, which is pertinent to this special point :—"Now, this transverse rupture of the vagina is not so rare as is generally supposed. . . . Sometimes the rent is at the point of junction of the womb and vagina, sometimes just below, sometimes just within the cervix ; but they all owe their origin to the same force."

or explosions ; and, no doubt, the laws regulating their position and direction will, in course of time, be clearly made out. The body of the uterus in action bursts or explodes ; the cervix and vagina, being passive, are torn or lacerated.

CHAPTER XIX.¹

INCREASED LENGTH OF THE CERVIX UTERI AFTER LABOUR.

ALTHOUGH the progress of our knowledge of the cervix uteri in pregnancy, labour, and the puerperal state has been very tardy,² yet there is evidence that correct ideas had, in some quarters, existed long ago. On the subject of this chapter, a quotation, to be given, will show that Levret, Douglas, and particularly Guillemot and Boivin, knew the true conditions, at least from individual cases. The knowledge of the ordinary states of this part is a very recent acquisition.

In order to illustrate the subject, I may describe a uterus for which I am indebted to Professor Turner. It is from a woman very recently delivered, and Professor Turner kindly showed it to me as a remarkable example of the greatest possible distinct-

¹ Read to the Medico-Chirurgical Society, January 20, 1869.

² The conclusions of this chapter on the subject of the cervix after labour are the subject of an intended refutation by Isaac E. Taylor, M.D., New York. *American Journal of Obstetrics*, May 1874.

ness, even to the naked eye, of the mucous membrane covering the internal surface of the recently evacuated uterine body, and continuous with that of the cervix. The whole uterus is $7\frac{1}{2}$ inches long. The cervix, as measured by the length of the arbor vitæ from above downward, is $2\frac{3}{8}$ inches long; a much greater length of cervix than this part attains under any other healthy condition, except during labour. In other cases I have observed it 3 inches long.

In the *Edinburgh Medical Journal* for September 1863, I called attention to this condition of the cervix in the following words:—"The conditions of the cervix during labour, and shortly after delivery, especially its elongation from above downwards, do not naturally come to be considered in this paper (on the cervix uteri in pregnancy). But they call for the attention of obstetricians, with a view to the completion of the history of this interesting part."

Since this passage was written, the subject has attracted the attention of Eduard Martin and of P. Müller.

In the work of Eduard Martin, entitled *Die Neigungen und Beugungen der Gebärmutter nach vorn und hinten*, published at Berlin in 1866, some details on this subject are casually given without remark. He describes the uteri of seven recently delivered women, and gives the measurements of the cervical canals. The points of these seven cases,

bearing on the subject under discussion, are given in the following table :—¹

No. of Case.	Length of Cervical Canal.	Time of Death after Labour.
1	3 inches.	1 hour.
2	2 $\frac{1}{4}$ inches.	2 $\frac{1}{2}$ hours.
3	3 $\frac{1}{2}$ inches.	1 hour.
4	3 inches.	48 hours.
5	2 inches 6 lines.	80 hours.
6	2 inches 6 lines.	12 hours.
7	2 inches $\frac{1}{2}$ line.	1 $\frac{1}{2}$ hour.

P. Müller gives (S. 126) a single measurement, from a case of death on the fourth day after delivery, with placenta prævia, where the cervix was two inches long. It is to be found in his elaborate paper, entitled *Untersuchungen über die Verkürzung der Vaginalportion in den letzten Monaten der Gravidität*,² published in 1868.

The cervix uteri, elongated as it is after delivery, contrasts, in relaxation and thinness, with the cervix uteri elongated in cases of fibrous tumour, when it is generally tough and thick.

It is worth while to remind obstetricians, who may be studying the question of the site of hour-glass contraction *post partum*, that a deceitful feeling of hour-glass contraction may be given to the examining finger by the change, which Martin has well

¹ More measurements are given in a subsequent edition of Martin's work.

² *Separatabdruck aus Scanzoni's Beiträge zur Geb., etc.* Band V. Heft 2.

pointed out, from thinness and relaxation of the cervix to thickness and comparative hardness of the body of the uterus; and that further deception as to the site of contraction may arise from not keeping in mind the point considered in this paper, namely, the increased length of the cervix. I can easily understand an observer, ignorant of these two points, boldly yet erroneously asserting the existence of hour-glass contraction in the body of the uterus, a condition whose occurrence some authors have denied.

Speaking of hour-glass contraction, Guillemot¹ makes the following statement:—"Levret has described with exactness the changes which take place in the neck of the womb, after the expulsion of the child. 'If you carry (says he) the hand into the womb, you discover in the vagina its neck so disfigured that it resembles the remaining portion of a truncated great intestine, and at the bottom of it is found, at one or two inches in depth, a kind of constriction which is the internal orifice puckered and almost entirely closed.' According to Madame Boivin, the uterine neck, in this state of flaccidity, presents sometimes five or six inches in length by four or five inches in diameter, as she has several times seen, especially in a woman who died immediately after delivery. In this example, the uterus and the internal orifice were perfectly contracted, while the

¹ *Archives Générales de Médecine.* 2 Serie. Tome 2. 1833. P. 198.

neck, remaining in inertia, presented the volume and extent of the body of the uterus. Douglas adopts the same ideas in the formation of the hour-glass; the structure which divides the uterus, at the union of its neck and its body, determines two cavities, of the which the superior comprises the body and fundus, and the inferior the neck and the vagina; the one and the other have an equal capacity. Baudelocque had long ago published some of the views which we have enunciated. Madame Boivin has remarked, with reason, that one has often taken the contraction of the internal orifice for the irregular contraction of the body of the uterus. These errors have been committed by men justly celebrated; one may account for these mistakes by the difficulties experienced in well defining the place of the contraction in the midst of the changes of the body and of the neck of the womb. It even appears that the attention of practitioners is always exclusively directed to the means to be used to triumph over the accident, and that they were content to regard the constriction as taking place in the body of the uterus, because it occupied the middle of the organ. This is, at least, the reflection which arises on reading the observations. But in applying to these occurrences the remarks drawn from anatomical examination of the womb at the time of parturition, one will be led to conclude that the middle part of the viscus corresponds to its internal orifice."

It is probably at the union of the cervix and body of the uterus, that the partial uterine inversion, the inversion of the cervix, takes place, which I have figured and described¹ as not of rare occurrence.

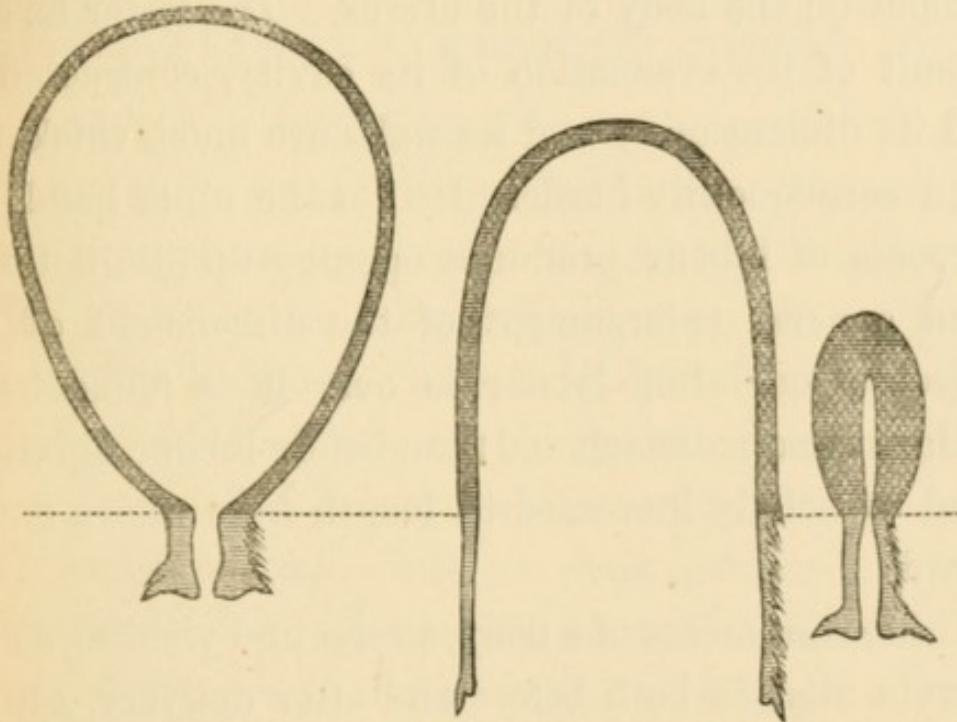


Fig. 9.

Fig. 10.

Fig. 11.

I have before me two cases of embedded fibrous tumour of the body of the uterus. In one the diameter of the globular tumour is about $3\frac{3}{4}$ inches, and the length of the hypertrophied arbor vitæ is $1\frac{1}{2}$ inch ; in the other the diameter of the tumour is $2\frac{1}{4}$ inches,

¹ See p. 286.

Description of Woodcut.

In the three figures the dotted line cuts the uteri at the position of the os uteri internum, or the internal os of the cervix.

Fig. 9 is a diagram of the uterus and its cervix in advanced pregnancy.

Fig. 10 is a diagram of the uterus in the second stage of labour.

Fig. 11 is a diagram of the uterus and its cervix shortly after delivery.

and the length of the hypertrophied arbor vitæ is $1\frac{3}{4}$ inch.

I have thus shown the effect of labour on the cervix uteri, and contrasted this with the effect of labour on the body of the uterus. The latter is, as a result of the evacuation of its cavity, contracted in all its dimensions; and its walls are much thickened and consequently hardened. On the other hand, the process of labour produces opening up or dilatation and general enlargement of the dimensions of the cervix; and after labour is over it is found to be thinner and more relaxed than before labour, enlarged, and especially increased in length from above downwards.

Measurements of a body, so soft and yielding as the cervix uteri is, both before and after delivery, cannot pretend to minute exactness, because the measurement may be easily somewhat changed, even by a change of position of the part measured. But there need be no hesitation in asserting that the cervix uteri after labour is, as a general rule, more than twice as long as it is before labour. For comparison with the measurements given in this paper, I may refer to my own post-mortem measurements of the cervix before labour,¹ and to P. Müller's less trustworthy measurements during life by his metrauchenometer.²

It has yet to be ascertained what is the rate of diminution of the cervix after delivery.

¹ See my *Researches in Obstetrics*. ² See his paper already referred to.

CHAPTER XX.

THE PRODUCTION OF INVERTED UTERUS.¹

MY principal object in this paper is to throw light on the subject of the mode of production of inversion of the uterus, and especially to establish the theory of its causation in cases which more than others deserve the name of intussusception, in which the uterus is inverted, not merely spontaneously, as the phrase is, but inverted by its own active efforts.

In pursuing this object, I shall not describe the phenomena of any individual cases of this rare disorder. I have met with instances of the accident, and may, at a more fitting time, record them. They would only encumber me at present, because no individual case has other than very limited bearings on the extensive subject to be discussed. Although no individual case is of much value, yet the whole knowledge which we have of the affection is founded on individual observations, and it is this knowledge which forms the basis of all the reasonings to be now detailed.

In the outset, and to give some previous idea of what is to follow, I here state that much illumination of the subject has arisen to myself from regarding

¹ Read to the Obstetrical Society, 27th March 1867.

retention of the placenta by uterine spasm or stricture, generally called in this country hour-glass contraction of the uterus, as the affection most closely allied to inversion of the uterus produced by uterine action. Holding that hour-glass contraction is a disorder nearly identical in its first stage with the first stage of spontaneous active uterine inversion, I shall, I hope, be able to make these two conditions throw light one upon another, so as to establish the true theory of both.

It will be useful here to define the terms qualifying inversion which I propose to use. By passive and active, I imply passivity or activity of the body of the uterus, not passivity and activity generally; and uterine activity necessarily implies muscular uterine action. By spontaneous and artificial, I indicate the absence or presence of forces external to the patient. Spontaneity implies that forces in the organism of the patient effected the inversion. Artificiality implies that the production of the inversion was partly or wholly the result of some kind of mechanical interference by the accoucheur, or even by the patient herself.

I shall not enter into historical details regarding this subject. Many authors, British and foreign, have made advances towards what I regard as the true theory of uterine inversion. Paralysis, or non-contraction, or weakened action of parts of the uterus, with contraction or spasm of other parts, has been,

by many authors, pointed out as contributing to an understanding of the phenomena of encysted placenta, or hour-glass contraction. And there we have the germ of the theory I now propose. Although my view of this subject is more systematic than that of any other author whom I know, yet the essential points of it are given in the admirable essay of Crosse,¹ who refers to Deleurye, Leroux, St. Amand, Dugès, and Velpeau, with reference to the importance of partial paralysis.

Levret, speaking of one of his drawings, says—“It gives an idea, though superficial, of what sometimes happens when the placenta is encysted instead of separated from the uterus after the birth of the child, which is generally a consequence of inertia of the part of the uterus on which the placenta happens to be implanted.”² In another place, describing a case of encystment, he speaks of the contracted condition of the uterus “except in the site of the attachment of the placenta, which was persisting in a state of inertia, and had become the formal and occasional cause of the uterine cell which imprisoned, so to speak, the placenta. . . . This was also the cause of the close adhesion of the placenta to the portion of the uterus on which it happened to be implanted.” . . .³

¹ *On Inversio Uteri*, p. 123.

² Fig. 15, vol. ii.—*L'Art des Accouch.* 1761, p. 294.

³ *Observations sur les causes et les accidens de plusieurs acc. labor.*
Suite des observations. Obs. xxvii. p. 132.

“The neck of the uterus,” says Lachapelle, “is often inert, although the fundus is contracted; sometimes the reverse happens, and it is then that the placenta, enclosed in the uterus, appears to be encysted in it.”¹

Hegar, in his work² on retention of the placenta, makes the following statements:—“According to Seiler, the cause of the incarceration is the activity of the uterus in those parts to which the placenta is not attached, along with inaction of the seat of its insertion. . . . According to Desormeaux, the enclosure of the placenta arises from this, that the part to which it adheres does not make progress in its contractions equal to that of the other parts.” These extracts justify what I have said in a former paragraph. I know no author who has insisted in like manner on the importance of the same conditions in the mechanism of the production of inverted uterus.

In recent times, there has been an increasing amount of support given by authors to the old opinion that inversion is frequently produced spontaneously by morbid uterine action. Formerly, the chief place in the causation of the disease was generally given to mechanical interference, and generally injudicious or ill-executed interference by the accoucheur. This change, whether well founded

¹ *Pratique des Accouchements*. Tome ii. p. 378.

² *Die Pathologie und Therapie der Placentarretention*, etc. Berlin, 1862. S. 30.

or not, is a step towards what I regard as the true theory of spontaneous active inversion. Yet I shall, subsequently, show that it has led the most recent authors into erroneous speculations as to the nature of the phenomena.

I may here say that I am inclined to think that the above-named change in opinion has probably gone too far, as I believe that pulling the cord or otherwise removing the placenta, and other mechanical interferences, whether well or ill done, may, in the condition of general uterine relaxation, often be the only assignable cause of inversion; and in the condition of irregular uterine action, it may be a part of the cause, leading to depression of the placental insertion into the uterine cavity, changing the case from one of uterine spasm, with encystment of the placenta or hour-glass contraction, into one of commencing inversion, producing thus the first stage of complete inversion. To this view, as to the change of opinion, some facts lend support, such as the frequency of the affection in unskilled hands, and its rarity in such institutions as the Lying-in Hospital of Dublin.¹ But I am not prepared to demonstrate

¹ "No example," say Messrs. Hardy and M'Clintock in their work on *Midwifery and Puerperal Diseases*, p. 223, "of acute *inversio uteri* has ever fallen under our notice, and the accumulated experience of Drs. Clarke, Labatt, Collins, Kennedy, and Johnson, in the hospital, does not furnish a single instance of the occurrence of this accident, though the number of women delivered during their united masterhips amounts to upwards of seventy-one

the truth of my impression, and it forms no part of my present object to do so. I shall only further quote a passage from Denman to show that he believed mechanical interference to be sometimes the cause of a beginning of inversion which was subsequently completed spontaneously or by uterine action, an opinion in which I concur:—"I have," he says, "been assured that in some cases there has been a spontaneous inversion; that the accident happened at least when no force, or none capable of producing the effect, had been used; and then it was imputed to the shortness of the funis, giving the disposition before the birth of the child; or to some untoward action of the uterus. But with this assurance, or explanation, I do not feel quite satisfied, because degrees of force must always be vaguely estimated; though if a disposition to an inversion be first given by the force used in pulling by the funis, it may be completed by the action of the uterus; or, if the least possible degree of inversion were given by the shortened funis, it might certainly be completed by a very slight additional force in pulling by the funis, or by the mere weight of the placenta."¹

Among recent authors, Drs. Radford and Tyler Smith are distinguished for their support of the doctrine. But since the work alluded to was published, a case has occurred to Dr. Denham, a recent Master of the Hospital.

¹ *Introduction to the Practice of Midwifery.* Waller's edition, p. 421.

trine of the efficiency of unaided uterine action. Both collate the affection with irregular uterine action ; Tyler Smith distinctly compares it to hour-glass contraction ; and both speak of irregularity of uterine action as comprising a weakened action in parts of the uterus and strong action in others, or, in Tyler Smith's words, both spasm and inertia. Doing so, had they at the same time kept in mind the view of Levret, Seiler, and Desormeaux, as to the condition of the uterus in cases of incarcerated placenta, a view which they themselves apply in some cases of such incarceration, they would, no doubt, have arrived at the true theory of spontaneous active uterine inversion.

Radford¹ believes that action of the inverted part, with want of contraction of parts below it, is the theory of active uterine inversion. "There are," says Radford, "several varieties of irregular uterine contraction, and in all, some part of the organ must be in a state of weakened action, whilst another is in a highly excited condition. It is not essential to inquire here which of these two states lays the foun-

¹ Saxtorph is said by West to be the originator of Radford's theory; but I have not been able to satisfy myself of this. Saxtorph's words appear to me to be too vague to justify the ascription of the theory to him. See West's *Diseases of Women*, 3d edition, p. 225. Besides, the statements of Desormeaux and P. Dubois (*Dict. de Médecine*, tome xxx. p. 354) seems to transfer the credit to Astruc. Crosse, *On Inversio Uteri*, pp. 124, 127, describes the view of Moser and Leroux in a manner which shows that they also anticipated Radford.

dation of the mischief. The uterus is divided, in hour-glass contraction, into two compartments; in the upper the placenta is uniformly found. A contraction sometimes takes place in the body and cervix, leaving a chamber at the fundus, in which also the placenta is always found. There, again, occurs a contraction in the os uteri, with deficient action in the body and fundus. This condition leads to a retention of the placenta, and in some instances it takes place after the placenta has been removed, giving rise to internal flooding. There are states of the uterus where some portion or portions contract with greater force than the rest. Now, it appears to the writer that inversion is another instance of irregular contraction, in which the fundus acts powerfully, whilst the cervix and os uteri are in a state of atony. It is obvious that if the fundus and body continued their action after the expulsion of the child, before the cervix and os uteri have regained their proper powers, an inversion must take place.”¹

To comment on this opinion of Radford fully would lead to largely anticipating what is better deferred to another part of this chapter. I shall, therefore, now only say, that I cannot conceive on

¹ Essays on various subjects connected with Midwifery: *On Inversion of the Uterus*, p. 16. A somewhat similar view is expressed by Klob. *Pathological Anatomy of the Female Sexual Organs*. Engl. Transl., p. 101.

what grounds Radford can support his view. For the powerful action of the fundus, whilst the cervix and os uteri are in a state of atony, is a natural, ordinary, healthy concourse of conditions. They appear to me to be the conditions which best secure the female against the dreaded accident; and I cannot imagine the production of it while they last. If Radford attaches special virtue to the word "powerful" in his assertion, it would only lead, so far as I can see, to clearer perception of the impossibility of inversion while the conditions last.

The only kind of uterine inversion consistent with, yet not the result of, Radford's conditions of powerful contraction of the fundus, and relaxation below that part, is inversion of the lower part or cervix of the uterus alone. It is not rarely observed after delivery. I have depicted it diagrammatically in Fig. 12. It is, in ordinary circumstances, quite trivial, demanding no treatment, because it soon spontaneously disappears. It is produced under the presence of Radford's conditions for real uterine inversion, or inversion of the fundus, when there is bearing down or any other pressure on the contracted fundus, pushing the uterus downwards through the brim of the pelvis.¹

Tyler Smith believes that, in the production of

¹ For remarks on some peculiar cases of this inversion, see Klob, *loc. cit.*, p. 103, who refers to Lawrence, Virchow, and Tyler Smith. See also Crosse *On Inversio Uteri*, p. 23.

inversion, action of the fundus carries or pushes this part into the uterine cavity, where the introcedent or depressed part is seized by an hour-glass contraction, and propelled through the lower part of the uterus, then in a state of inertia, but soon to be in a

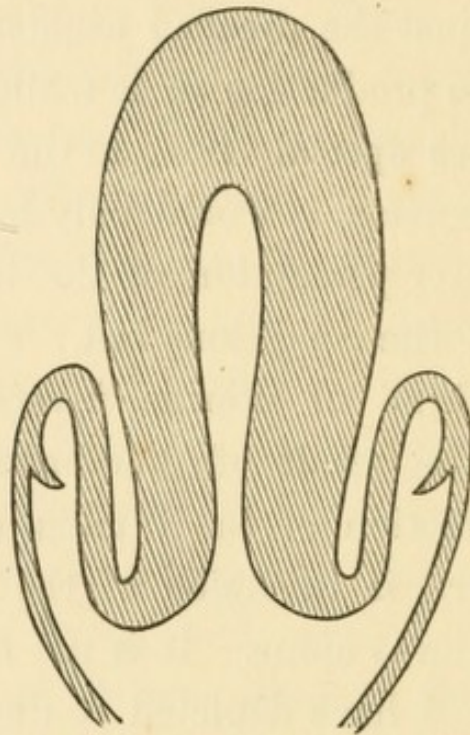


Fig. 12.

state of active contraction. This theory differs but inconsiderably from that of Radford, as Dr. Smith's own words will show. Nevertheless, Dr. Smith's account of the production of a spontaneous active uterine inversion is the best which I have anywhere met with. "I have always," he says, "been of opinion that, in the great majority of cases, the accident happens in consequence of irregular but active contractions of the uterus itself. No doubt, cases may occur in which inversion is produced by

Fig. 12. Diagram of inversion of the lower part or cervix of uterus.

great tension upon the cord, while the placenta is firmly adherent to the fundus, and that all violent traction of the cord, while the placenta is attached to the fundus, is reprehensible. But when inversion is thus produced, there must be a consenting action of the uterus." . . . "All the facts connected with this catastrophe show that it generally depends, not upon a passive, but an active condition of the organ." . . . "It has even been known to occur in the unimpregnated uterus." . . . "The nulliparous organ has been known to invert itself, as the result of spasmodic action, in long-continued menorrhagia, or as the result of a small polypus or fibrous tumour in the cavity of the uterus, or upon its peritoneal surface." . . . "There is, first, cup-like depression of the fundus uteri; coincident with, or immediately following upon, this depression, there is hour-glass contraction of the body or lower portion of the uterus. The annular contraction of the body of the uterus grasps the introcedent fundus as it would a foreign body, and carries it downward for expulsion through the os uteri, the os itself being at this time either in a state of inertia, or actively dilated, just as at the end of the second stage of labour. After the inverted uterus has passed through the dilated os uteri, this part of the organ becomes contracted, preventing re-inversion from taking place." . . . "In simple hour-glass contraction the cavity of the uterus is divided into two parts by the con-

traction of the middle portion of the organ ; but when, owing to irregular action of the fundus, this part of the organ descends into the cavity of the uterus, and the hour-glass contraction then occurs, the fundus uteri is seized by the contracting ring of the uterus, borne down through the os uteri and vagina, and inversion is thus rendered complete.”¹

Before advancing farther, it will be advantageous, with a view to making my own views more clear, to point out some errors in Tyler Smith's explanations. And, first, I draw attention to his assertion that the unimpregnated uterus, not having a polypus or fibrous tumour in its interior, has been known spontaneously to invert itself. I have no doubt Tyler Smith has considered the recorded cases of the kind, to which he can appeal in proof of his statement, but he has not given any reference to them.² In the meantime, I

¹ *Manual of Obstetrics*, pp. 480 and 489. See also *London Obstetrical Transactions*, vol. x. p. 33. Betschler adopts the views of Tyler Smith. *Klinische Beiträge zur Gynækologie*, 1 Heft, S. 23.

² On this point see some apposite remarks by Dr. West, *Diseases of Women*, third edition, p. 223. “Inversion of the uterus,” says he, “the turning of the organ inside out, is an accident clearly impossible in the natural condition of the unimpregnated woman ; it being obviously essential for its occurrence that the organ should have attained a certain size, and that its walls should be comparatively yielding. It is indeed only at an advanced period of pregnancy that these conditions are generally met with, and only during labour that an exciting cause is likely to be superadded capable of producing the misplacement ; but at that time violent

shall only say that I cannot believe that such an inversion ever occurred. The unimpregnated uterus is so thick and hard, and its cavity so small, as to render inversion necessarily slow in its progress, even when a polypus attached to its fundus renders the occurrence of inversion quite intelligible. The polypus, in such a case, is a uterine content, which the uterine walls can seize and expel from the uterine cavity, even though it drags the fundus uteri after it. This slow inversion is an operation, the study of which might have suggested the recently-demonstrated curability of the most chronic inversions. For, if the hard and small-cavities unimpregnated uterus can be inverted, it is reasonable to expect that the similar but displaced parts of a chronic inversion can be replaced

traction at the funis by some unskilled practitioner, before the detachment of the placenta, may mechanically invert the womb, or the organ may, by its own contractions, invert itself, just as the intestine does in cases of intussusception. The late Mr. Crosse of Norwich, in his very elaborate essay on *Inversion of the Uterus*, which unhappily he did not live to complete, states (Part ii. p. 70) that in 350 out of 400 cases of inverted uterus of which he had found mention, the accident occurred as a consequence of parturition; and there can, I think, be no doubt but that the real proportion of cases in which it is traceable to this cause is much higher than 7 to 1. Of the remaining 50 cases, 40 were said to have occurred in connection with the presence of a polypus in the interior of the womb, the accident sometimes taking place spontaneously, in other instances resulting from traction at the out-growth in some attempt to accomplish its removal.

“ Almost all of those rare cases in which the uterus is alleged to have become inverted, independently of either of the above causes,

by means like to those which produced the displacement. The power, which Dr. Tyler Smith and others have shown that we possess, of replacing a chronic inversion, is only not a very great and renowned discovery because of the rarity of the affection to be cured. Similar great results are achieved in various departments of surgery and midwifery by like means—namely, the substitution of gentle and continued effort for more powerful and less continued effort.

If inversion of the otherwise healthy unimpregnated uterus ever takes place, it cannot be in the generally understood way, a part of the fundus being first displaced. It is conceivable, according to the mechanism described and figured in p. 285 ; the in-

are deficient in such details as are needed to substantiate their correctness, and doubt may be reasonably entertained, with reference either to the accuracy of the diagnosis, or else as to the truthfulness of the history related by the patient. (Baudelocque's remarkable case of alleged inversion of the womb in a girl fifteen years old, who suffered from menorrhagia, appears to me to be one in which we may be allowed to entertain some doubt as to the accuracy of the diagnosis ; while nothing can be vaguer than the history of Lisfranc's patient (*Clinique Chirurgicale*, vol. iii. p. 380), whose symptoms are said to have existed five years before she came under his observation.) Enlargement of the uterine cavity, however, associated with some cause capable of exciting contraction of its fibres, may be looked on as the two conditions essential to the inversion of the organ ; and when these two co-exist, as in Dr. Thatcher's case of enlargement of the womb from hydatids (as narrated in Crosse's Essay, part i. p. 57), there the possibility of inversion taking place must be conceded." (See also Colombat, *Maladies des Femmes*, tome i. p. 321.)

version beginning at the cervix, and proceeding upwards as the tissues are relaxed and expanded under a long continuance of pressure from above.

But although spontaneous active inversion of the unimpregnated uterus containing a polypus is comprehensible, and although it may be admitted that spontaneous passive inversion of the healthy unimpregnated uterus is conceivable; yet, to me at least, spontaneous active inversion of the unimpregnated uterus, having no organic disease, is quite inconceivable.¹ For it is necessary to suppose that muscular action of its walls could push some part of them into the uterine cavity, should bend some part inwards, which is impossible. Muscular action of a bulging part may make the line of its fibres straight, may destroy the outward bulging curvature of that part of the uterine wall, but can not go farther and produce a bulging inward. As a bulging inwards, or the first step to inversion, cannot be attained, spontaneous active inversion is impossible. Holding such views, I cannot accept even the evidence of cases, unless of the most unimpeachable character in every respect.

For the same reasons as are resorted to in the last

¹ Barnes's opinion on this point is not to be distinctly made out. He seems to me to imply (*Diseases of Women*, p. 715) that spontaneous active inversion without disease is possible. Yet the case he gives to illustrate it is a case of Hunter's, where the inversion was complicated with a fibrous polypus, a kind of which many examples are well known.

paragraph, spontaneous active uterine inversion after delivery is impossible according to Tyler Smith's theory ; for no action, regular or irregular, can produce introcession of a part of the uterine wall. Before active uterine inversion can be begun, such introcession must take place. Some part of the body of the uterus must be in a position to be seized by the remainder, must become, in a sense, a uterine content, acting just as a polypus does in cases of inversion in connection with this disease. Some other condition than uterine action must therefore be called in.

Four kinds of uterine inversion occur after delivery :—

1. Spontaneous passive uterine inversion.
2. Artificial passive uterine inversion.
3. Spontaneous active uterine inversion.
4. Artificial active uterine inversion.

The only uterine condition essential to the production of all these kinds is paralysis or inertia, or complete inaction. This is the condition of the whole organ at the time of production of the first two kinds. In the last two kinds it is accompanied by uterine activity, and as these cannot co-exist in the same part, the paralysis is partial, and the activity partial. Action of the uterine wall cannot cause introcession of it. Activity of the whole of the uterus, or of its body, renders inversion impossible. Activity of a part of the uterus renders introcession of that part impossible. There must, therefore, be paralysis of the whole or of

a part of the body of the uterus, before inversion can be begun.

Supposing paralysis, then, how is introcession, or the first stage of inversion, produced? In cases of artificial inversion, cases of the second and fourth kinds, foreign force is applied either from above, to push the paralysed uterine wall into the uterine cavity, or from below, to pull it into the cavity. Spontaneous uterine inversion admits of no foreign power being called in, and we must find the desiderated power in the uterus or in the mechanical conditions of the abdomen. We have already shown that the uterus cannot effect it, and must fall back upon the mechanical conditions of the abdomen. Conditions fitted to produce the result are to be found in the ordinary bearing-down effort, or in absence of the retentive power of the cavity,¹ however produced.

In cases of active uterine inversion, cases of the third and fourth kinds, it is necessary that the activity or muscular contraction of the uterus be in a part situated below the paralysed part. For as inversion consists in causing the descent of the fundus of the uterus, this cannot be effected by any uterine efforts, except those made by parts below the fundus.

Paralysis or inertia of the whole uterus is a well-known condition. If this condition is required to

¹ See *Edinburgh Medical Journal*, December 1865, and the author's *Researches in Obstetrics*, p. 409.

explain any kind of inversion, it is at least not necessary to enter upon proof of its occurrence.

Paralysis of a part of the uterus is also well known, for every practitioner must often have felt the lower part of the uterus in this state (already described, p. 285) while the upper parts were firmly contracted. But, for our purpose, it is necessary to show that paralysis or complete inertia of the higher parts of the body of the uterus may occur while the lower parts are in a state of active contraction. Of this occurrence, hour-glass contraction is the most frequent exemplification.

Much discussion has taken place as to what is and what is not real hour-glass contraction. By some the term is applied to contraction of the mass of circular fibres at or near the situation of the internal os of the cervix, while others apply the term to such contractions, higher in the uterine walls, as encyst the placenta in the region adjoining the internal orifice of a Fallopian tube.¹ Most British authors use the term without implying any definite locality of the contraction, provided it is not at the os uteri externum. My experience leads me to join Hélié,² and many other authorities, in asserting the occasional occurrence of the contraction in any of the situations already named.

¹ See the discussion in chap. xviii.

² *Recherches sur la Disposition des Fibres musculaires de l'Uterus*, etc. Paris, 1864, p. 58, etc. etc.

Hour-glass contraction cannot exist unless the parts above the contraction are in a state of inertia, unless there is a local paralysis of the uterus. This paralysis must be perfect, or nearly so, if the hour-glass contraction persists; for were the higher parts of the uterus even in moderate action, the hour-glass contraction would soon be overcome.

Local paralysis of the higher parts of the uterus generally includes within the affected area that part to which the placenta is attached, or affects that part alone. Just as the placenta is generally found attached to the inverted uterus, so is the placenta almost invariably found in the sac above an hour-glass contraction; and the inactivity of the placental insertion is in both cases frequently shown by the persistency of the attachment.¹ In my own experience I have met with exceptions to these conditions, but they are universally admitted to form the rule.² As an example of an exception, I may mention that I have met with a persistent hour-glass contraction of only a narrow uterine band running across the insertion of a morbidly adherent placenta. I was called to this case by Dr. Linton.³

¹ See remarks by Crosse *On Inversio Uteri*, p. 125.

² Radford (see the quotation, p. 284) and others erroneously make the rule invariable.

³ This interesting case is in exact opposition to the generally true statement of Hegar (p. 39)—“Bei totaler Adhäsion ist der Kuchen vollständig in der Höhle enthalten.” For the placenta was everywhere morbidly adherent.

In cases of paralysis of the placental insertion, bleeding will naturally be expected to occur if the placenta be partially or wholly separated. But in the case of hour-glass contraction the bleeding may be prevented by the contraction ; and, as Tyler Smith has said, the same prevention of hæmorrhage may arise, in a case of inversion, from "the os uteri acting as a tourniquet to the uterus," and it is more than probable that this condition soon arrives even in cases of passive inversion.

It is worth while here to introduce the observation that the placental site is believed to differ, not only in liability to paralysis, but in other more intimate conditions, from the rest of the organ. "Contraction," says Madame Boivin,¹ "ought to be less active, less strong in the region where the placenta is inserted than in that which is free." "Merriman," says Klob, "observed in most of the inversions of the uterus which occurred after parturition, a firmer attachment of the placenta to its walls, combined with greater thinness and laxity of the organ, and he looked upon these conditions as the cause of inversion." "Let us remark besides," say MM. Dubois and Pajot,² "a fact to which we shall return : it is, that the fibres of the portion of the uterus where the placenta is found inserted, increase less in volume than the rest, and undergo more considerable modi-

¹ *Memorial de l'Art des Acc.*, p. 97. See also p. 417.

² *Traité complet de l'Art des Accouch.*, p. 419.

fications of their structure. . . . In the internal half of the thickness of the uterine walls, then, occur modifications of structure more considerable. They commence nearly about the end of the second month of pregnancy, and are especially well marked in the site and in the neighbourhood of the placental insertion. These changes consist in the production of molecular, greyish, azotised granulations, soluble in acetic acid, and of some fatty granulations. These last go on increasing in quantity and volume with the periods of pregnancy, and particularly so in the layer immediately contiguous with the *caduca*."

In their descriptions of and reasonings upon cases of hour-glass contraction, of inversion, and of hæmorrhage, many authors have erred in taking a hard part of the uterus for a part necessarily in a state of action. Now, while hardness of uterine wall is in most cases a good and sufficient sign of muscular action in the hardened part, it is not invariably so. For, in cases of hour-glass contraction and of inversion, the hardness may arise from passive tension. The bladder of urine and ovarian cysts frequently illustrate hardness of flaccid bags produced by passive distension. In like manner, a paralysed portion of uterus may be hard from being tightly replete with placenta or other contents retained by stricture; and the fundus of an inverted uterus may be hardened by the circumstances

of its inversion, apart from muscular action of the hard part.¹

In the medical literature of post-partum hæmorrhage, especially of long-continued draining, much interesting evidence of the not very rare occurrence of persisting paralysis of the placental insertion is gradually accumulating.² In his work on pathological anatomy, Rokitansky describes it as a disease of the puerperal state as follows:—"We must," says he, "here mention a very singular circumstance, which may, on account of the consequent danger, become important, and may even be misunderstood in post-mortem examinations; it is paralysis of the placental portion of the uterus, occurring at the same time that the surrounding parts go through the ordinary processes of reduction. It induces a very peculiar appearance. The part which gave attachment to the placenta is forced into the cavity of the uterus by the contraction of the surrounding tissue,

¹ For some remarks bearing on this point, see Ferguson's Introduction (p. xli.) to the Sydenham edition of Gooch's *Most Important Diseases of Women*. See also chap. xxv. p. 421 of this book.

² In the works of an author so old as Mauriceau we find a case of this kind described by him, and based on a post-mortem examination. I quote from Desormeaux and P. Dubois (*Dict. de Médecine*, tome xxx. p. 349),—"Il y a simple dépression, quand le fond de la matrice est un peu déprimé en dedans, comme est le cul d'une fiole de verre, ainsi que le dit Mauriceau, qui a constaté ce déplacement sur le cadavre." See also Hohl, *Lehrbuch der Geburtshülfe*, S. 864, for some remarks on this point. See also Crosse *On Inversio Uteri*, pp. 73-104.

so as to project in the shape of a conical tumour, and a slight indentation is noticed at the corresponding point of the external uterine surface. The close resemblance of the paralysed segment of the uterus to a fibrous polypus, may easily induce a mistake in the diagnosis, and nothing but a minute examination of the tissue can solve the question. The affection always causes hæmorrhage, which lasts for several weeks after childbirth, and proves fatal by the consequent exhaustion. We have met with it twice, once after abortion, and once after parturition at the full period." . . . "Dr. Betschler, during his visit to Vienna in 1840, communicated a similar case to me as having occurred at Breslau; and there can be little doubt that Dr. Burckhardt (vide *Berliner Centralzeitung*, x. 19) speaks of this condition under the title of acute fungus hæmatodes uteri, as of a new and hitherto unknown cause of flooding after childbirth."¹

Referring to Rokitansky's description, we find Kiwisch saying,—“Atony of the uterus is either general, or it affects only a part of this organ; and in the latter condition, the partial paralysis of the placental portion, with sinking inwards of it, appears as a peculiar, and, at the same time, very dangerous

¹ *Sydenham Translation*, vol. ii. p. 304. The physical diagnosis of this condition of the uterus may be effected even many days after delivery, should Schröder's statements be verified. See his *Schwangerschaft, Geburt und Wochenbett*. S. 192.

condition. We have, as yet, had opportunity to observe it in only a single case.”¹

Klob also gives an account of the affection—“Defective contraction,” says he, “of that part of the uterine wall which forms the placental insertion is of extraordinary importance. In such cases, this part of the uterus, having the shape of the uvula, or of a body of a conical or round form, with a neck-like constriction, sinks inwards into the distended uterine cavity, while the other parts of the organ seem tolerably well contracted. At the corresponding part of the external surface of the uterus can be seen a funnel-shaped depression, or at least a more or less considerable inversion of the wall. The inverted tumour-like part filling up the uterine cavity is spongy, bleeding, shows the peculiarities of the placental insertion, and in most cases there are on it some remains of the placenta, or there adhere to it coagula of blood in pretty large lumps.

“Rokitansky points out, that in the origination of such depressions of the paralysed placental insertion, the tuggings, on artificially separating the placenta, might have influence; and I would have this specially attended to, on this account, that I particularly cannot understand wherefore the paralysed portion of the uterine wall always sinks inwards into the uterus, while, at the same time, in consequence of the energetic contraction of the other

¹ *Klinische Vorträge, IVte Auflage, I. Bd. S. 427.*

parts of the walls of the uterus, its cavity is reduced to small dimensions. I take it to be absurd to propose to regard the pressure of the abdominal viscera as the cause of the sinking inwards of the paralysed part. Absolute or relative shortness of the umbilical cord may, in such cases, come into prominence as a frequent cause of the pulling.

“The consequences of this condition are hæmorrhages, which may at last lead on to a fatal termination.

“The frequency of this occurrence in abortion is striking, and Engel’s first case concerns a woman, 34 years of age, who had already gone through seventeen labours successfully, and in her eighteenth pregnancy suffered an abortion in the fourth month. Engel points out the large number of the pregnancies, and their quick succession, as the cause of the paralysis of the placental insertion, and mentions that in the dead body (and how much more in the living) the swelling formed by the projection inwards of this paralysed part might easily be confounded with something else, as a polypus decomposing at its lower end.”¹

I shall now, in conclusion, give a brief account of the four kinds of uterine inversion that may occur.

Spontaneous passive² inversion occurs in cases of

¹ *Pathologische Anatomie der weiblichen Sexualorgane*, S. 264.

² A very good practical account of passive inversion is given by

paralysis, or inertia of the whole uterus; the organ being large, its walls lax, and capable of being inverted by little force. Bearing down produces, in general, collapse and compression of the organ; but it may produce inversion if the depressing force is applied under favourable circumstances, and the inversion will be complete if the bearing down is strong and continued. Should the original condition of inertia persist, the neck not contracting around the inverted organ, then replacement will be at least as easily performed as inversion. It is to this category that I am disposed to refer the cases of inversion post mortem which Bœrner and Klaatsch have recorded.

Artificial passive uterine inversion demands little description. It is the kind of inversion commonly described by the older authors. It differs from the spontaneous passive inversion only in this, that foreign force replaces the bearing down. The foreign force may be applied from above by pushing, or from below by pulling the cord, or manœuvring with the placenta. It would be a more frequent occurrence

Desormeaux and P. Dubois, at p. 350 of the article already referred to. See also Colombat, *Traité complet des Maladies des Femmes*, tome i. p. 316. This kind of inversion has recently been the subject of some interesting papers by Lazzati, Professor of Midwifery in Milan. He has been erroneously described as forestalling the theory of active uterine inversion given in this chapter. See the *Annali Universali di Medicina*, vol. cxci. 1865; also the *British and Foreign Medico-Chirurgical Review*, Chronicle, October 1867.

than it is, were it not the case that the interference which tends to produce it also tends to bring on that general uterine action which prevents it. It is in all respects similar to the former kind. In both kinds of passive inversion hæmorrhage will probably occur, if the placenta is separated, and the conditions of their production persist.

Spontaneous active uterine inversion is the kind which modern authorship is bringing more and more into notice as the most common kind. I have already said that I am disposed to think this tendency is being pushed too far. In this kind, paralysis of the fundus, or of a portion of it, probably of the placental portion, occurs. The state of the retentive power of the abdomen, or positive bearing down, leads to this portion projecting into the uterine cavity. It is seized by the adjacent contracting segments of the uterus, is pushed down and expelled through the os uteri into the vagina, or beyond the vagina. It is difficult of replacement, in consequence of the contraction of the uterus around the inverted parts.¹

Artificial active uterine inversion differs in nothing from the kind last described, except in this, that the inversion of the paralysed portion is effected by pressure from above, or by pulling on the cord, or other interference from below.

It can scarcely be considered out of place to add,

¹ Barnes seems to adopt this kind as the ordinary mechanism. *Diseases of Women*, p. 714.

in conclusion, a few remarks on spontaneous replacement of inverted uterus. It is almost useless to say that spontaneous replacement of the first stage of an inversion, simple depression, often occurs, and is accounted for by contraction of the inverted part, or by alteration of the condition of the retentive power of the abdomen changing an inversion into an elevation of the paralysed portion of the uterus. But spontaneous replacements of completely inverted uteri have been reported by Dailliez¹ and others, and Spiegelberg² and Scanzoni³ have recently published essays of a valuable and comprehensive character on the subject. The possibility of this spontaneous replacement has been denied by Kiwisch, M'Clintock, West, Crosse, and many other esteemed authors.⁴ But I see no reason to doubt its occasional occurrence. Dailliez, Meigs, Spiegelberg, and Schatz, believing spontaneous reduction to be possible, attach much importance to the action of the round ligaments. I am rather disposed to attribute it to an inverted or opposite condition of the mechanism which produces spontaneous passive inversion. Spontaneous active uterine replacement is inconceivable, because the necessary conditions, which may be described generally as the same as those for spontaneous active inversion, cannot be supposed to exist.

¹ *Dict. de Médecine*, tome xxx. p. 359.

² *Archiv f. Gynæk.* V. Band, 1873, S. 118.

³ *Beiträge zur Geb.* V. Band, S. 111.

⁴ See Crosse *On Inversio Uteri*, p. 175 ; and Barnes *On Diseases of Women*.

CHAPTER XXI.¹

ON HÆMORRHAGE DURING PREGNANCY IN CASES OF PLACENTA PRÆVIA.

THIS accident is common, but many cases of placenta prævia, of all kinds, do not present it, for frequently they go on to the full time, or near it, without any hæmorrhage during the pregnancy, and frequently the occurrence of hæmorrhage before the full time is the immediate precursor of miscarriage, or a consequence of its commencement. It is very desirable to know the relative numerical proportions of these different courses of pregnancy with placenta prævia of different kinds—how many have hæmorrhage during the continuance of pregnancy, how many have not, how many end in miscarriage.

The hæmorrhages now to be discussed are well known. They may occur once or oftener in the latter months of pregnancy. They are comparatively more frequent as pregnancy is more advanced. They frequently appear to have a monthly periodicity. They are often to a great amount, and sometimes they prove fatal without labour commencing.

¹ *Edinburgh Medical Journal*, November 1873.

Although it is these well-known hæmorrhages that I propose now to discuss, I desire to state my conviction, based on observations, that it is an error to regard these as the only hæmorrhages that occur during the continuance of pregnancy in cases of placenta prævia. They are not the earliest that occur, as is attested by the circumstance that placenta prævia is not unfrequently observed in abortions.¹

Hæmorrhage in the latter months of pregnancy in cases of placenta prævia may be produced in various ways. So far as I know it has hitherto been described as being produced only by separation of the placenta ; and several theories of the mechanism of this separation—all of them, I believe, erroneous—have been enunciated. For my part, relying partly on my own observations, I believe such hæmorrhage to occur most frequently without any separation of the placenta ; though separation does in some cases undoubtedly occur. In those cases of hæmorrhage from placenta prævia which have come under my own observation, I have not found in the placenta those pathological appearances which must, I believe, follow

¹ See William Hunter's *Anatomy of the Human Gravid Uterus*, Plate xxxiv., Figs. 3 and 4. See also Braun Chiari and Spæth's *Klinik der Geburtsh. u. Gynæk.* S. 181 ; also the Author's *Researches in Obstetrics*, p. 261 ; also Barnes' *Lectures on Obstetric Operations*, 2d edition, p. 412 ; also Hecker's *Klinik der Geburtshunde*, II. Band, S. 170 ; also Braun, *Lehrbuch der Geburtshülfe*, S. 624, who gives references to Wenzel, D'Outrepoint, Busch, Feist, and Thudichum.

partial separation of one or of several months' standing ; and, in the very great majority of cases which I find on record, no mention is made of such pathological conditions of the placenta. These pathological conditions, hereafter to be mentioned, are not such as would easily escape notice, or as would be passed over by observers without description.

There are four ways in which this kind of hæmorrhage may occur :—

1. By rupture of a utero-placental vessel, at or above the internal os uteri.

2. By rupture of a marginal utero-placental sinus within the area of spontaneous premature detachment, when the placenta is inserted, not centrally or covering the internal os, but with a margin at or near the internal os.

3. By partial separation of the placenta, from accidental causes, such as a jerk or fall.

4. By partial separation of the placenta, the consequence of uterine pains producing a small amount of dilatation of the internal os. Such cases may be otherwise described as instances of miscarriage commencing, but arrested at a very early stage.

To the *first* of these modes of hæmorrhage I shall now direct attention ; that, namely, in which there is no separation of the placenta, but the blood flows from an opening in an utero-placental vessel. Hæmorrhage of this kind will take place when the placenta is inserted over the os internum, and when,

as not rarely happens, the internal os uteri is not at all dilated.

This pathological explanation, it will be observed, quite accords with the absence of all premonitory symptoms, the unexpected character of the bleeding, which observers uniformly describe. Were the bleeding the consequence of some disturbing cause producing separation, whether the cause involved ✓ uterine action or not, then it could scarcely be said to occur without any premonition.

When I speak of rupture of a utero-placental vessel, I exclude rupture of the placenta, or rupture extending into the intra-placental maternal blood-spaces. Such ruptures of the placenta do occur, and produce bleeding, not only from the maternal, but also occasionally from the foetal system, some intra-placental branch of an umbilical artery being lacerated and giving issue to blood. It is impossible to conceive such a rupture, with continuance of the pregnancy, as ending otherwise than in pathological changes of the affected cotyledon or cotyledons, which could not escape the notice of a careful observer when the pregnancy came to an end. In the cases now under discussion, no pathological change in any cotyledon is necessarily produced; and, when the placenta comes to be born, none is to be expected except what is of the most recent kind.¹

¹ See the argument of Jüdel. *Archiv f. Gyn.*, VI. Band, S. 461.

When the placenta is exposed to such violent stretching as to lacerate its whole structure, down to the chorion, or even through it, according to reports of some cases, then it will almost certainly be also separated to some extent ; and I shall not refer to this kind of lesion under a separate heading.

So far as I know, there has not yet been made a proper dissection of a case of placenta prævia centralis, but it is much desiderated. In consequence of this anatomical deficiency, we can only guess at the condition of that part of the surface of the placenta which, in cases of this first kind, overlies the internal os of the cervix. For the complete attainment of this desirable object, it will be necessary to secure the body of a woman who has died in advanced pregnancy with placenta prævia, and in whom the connections of the placenta and uterus have been altogether undisturbed ; and it would be best if the woman were destroyed by some disease or accident quite foreign to the genital system. Although the attainment of this is extremely desirable, and for the settlement of some points quite necessary, yet it appears to me probable that a careful anatomical investigation, with the aid of the means of histological research, would discover and identify in the delivered placenta of central, or nearly central, insertion, the spot which, during pregnancy, overlay the internal os of the cervix, and which had no direct connection with uterine wall ; and it may be anticipated that

efforts will be made in this direction. The analogous spot may, indeed, be discovered in the membranes even in ordinary cases of delivery.

In the lower animals which have a diffused placenta nearly co-extensive with the whole surface of the membranes, those pieces of chorion which overlie the uterine openings—that is, the internal os uteri and the uterine openings of the Fallopian tubes—are bare of villi, or non-placental. Examples of this are seen in the Balænoptera and Orca dissected by Professor Turner,¹ and in other animals, as the mare. But it is otherwise in woman. These whales and the mare have a thin, diffuse, and very extensive placenta; and while it is quite possible that their whole vascular chorionic surface may act functionally as placenta, it is certain that the villi have a comparatively simple arrangement, and do not even entirely cover the subjacent chorion. In woman, the placenta is concentrated in one small area, and its great thickness, sometimes increased in cases of central placenta prævia, is composed of an aggregated mass of villi, whose arrangement is so complex as never yet to have been unravelled. In her, there is no bare chorionic spot where the organ is inserted over the Fallopian tubes or internal os uteri. Indeed, as already said, the placental spot at these situations has never, in cases of this first kind, been described. At the Fallopian

¹ *Transactions of the Royal Society of Edinburgh*, session 1870-71, vol. xxvi. p. 479.

tubes its area must be very minute, but at the internal os uteri it must be considerable, covering a surface which is at least of the extent of a square line, and probably much greater.

It is necessary to be very cautious in such a statement as that just made, because we do not know to what extent the attachment of the placenta may restrain or increase the expansion of the internal os. When the placenta is not prævia, the internal os varies greatly in size when near the full time. It is often big enough to transmit two fingers. In a uterus about the sixth month of pregnancy, kindly shown to me by Professor Turner, and in which the mucous plug is still present, and, as he points out, curiously projects into the uterine cavity, the internal os is an oblong opening of a little above half-an-inch in length, the sides being nearly in contact, but separated by the mucous plug. In one of the dissections which afforded Braune material for his homalographic sections, he found the internal os uteri only big enough to transmit the head of a good-sized sound.

This placental area over the os uteri probably strongly resembles the surface of the healthy separated placenta, and consists of decidual structure, the caducous portion of the decidua serotina. It may be called a reflected portion or reflexa of the decidua serotina, for it has a certain analogy with the decidua reflexa. The latter covers the ovum or chorion on

the side facing the uterine cavity, while this new or reflected portion of caducous decidua serotina covers the otherwise bare portion of villous chorion on the side of the ovum facing the openings of the Fallopian tubes, or of the canal of the cervix in the uterine wall. Beneath a thin decidual layer, at this spot, will run a portion or portions of utero-placental sinus, conveying maternal blood from adjacent intra-placental cells or caverns.

Sometimes, in woman, an approach to the bare condition of the chorion over the internal os uteri in the whale has been observed. Instances of this kind are well worthy of study, and they may come to be proved to be what they can at present be said only to appear to be. The evidence in favour of there being a tendency to a bare spot is deficient, because of the rarity of the specimens showing it, because so many are seen which do not show a trace of it, and because they are perhaps susceptible of a different explanation, founded on the supposition that they are portions atrophied in consequence of long past separation. Yet, on the other hand, such thinning of the placenta, or absolute bareness of chorion, may appear to be far less frequent than it really is, in consequence, as Hecker suggests, of the rarity of real centrally inserted placenta at or near the full time. It is only after a careful investigation of a sufficient number of abortions in placenta prævia that the question raised can be settled. Meantime, I may be allowed to give

some extracts in evidence of its occasional occurrence, especially as it has important practical bearings, on which, however, I do not here enter.

D'Outrepont¹ alleged that the portion of placenta in the neighbourhood of the orifice of the uterus or upon it is less thick and less developed than the rest of the mass. Hecker² makes some interesting remarks on this subject, and describes a very good example of it. He also refers to allied, not quite identical, observations by Küneke and Schuchardt, and describes one of his own, wherein there was placenta succenturiata. The smaller lobe in Küneke's case, in Schuchardt's the membranous junction of the two lobes, overlay the internal os uteri. In Hecker's good example the placenta was composed of two not quite equal lobes, separated by a thinned part; and this two-lobed placenta was so attached over the cervix uteri that the thin part, in which, however, there was evident villous placental tissue, was right over the internal os uteri; and through this thin part, when the lobes were torn asunder, the child was born.

Dr. Sirelius, of Helsingfors, has more fully entered

¹ Emmanuel Stein. *Reflexions sur l'implantation de l'arrière-faix sur le col de la matrice*, p. 52.

² *Klinik der Geburtskunde*, Band II. S. 168. The separation into two nearly equal lobes is the natural condition in the tailed monkey of the old world. I have recently seen a fine example of it in the human female. See Turner *On the Placentation of the Sloths*.—*Trans. Roy. Soc. Edinburgh*, vol. xxvii. p. 98.

on this point than any other author whom I know. I have not had access to his work, but only to a partial republication of it in the *Archives Générales de Médecine*.¹ His views are in several respects very different from those everywhere entertained. For example, he evidently believes that in all cases of placental insertion over the lower portion of the uterus there is atrophy of the part overlying the internal os of the cervix, which I cannot but regard as an error. It is well known that most cases of central placenta prævia present no such atrophied portion. Besides, were there such an atrophied portion, there would not only be no bleeding before dilatation of the internal os, but there would be no bleeding during the early part of this dilatation—a circumstance quite inconsistent with the clinical history of the great mass of cases of placenta prævia. The same atrophied portions which Sirelius describes as original ovular changes, not the result of separation, are described by Braun as the result of early separation of these parts of the placenta; and in accordance with this condition Braun describes the absence of bleeding during the early part of the dilatation of the cervix. But I see no reason to doubt that the view of Sirelius may be in some instances correct. His description, founded, he says, on an exact analysis of ten cases, is as follows:—“En récapitulant les faits rapportés cidessus, on voit que le placenta, se dévelop-

¹ Vol. ii. 1861.

pant sur l'orifice de l'utérus, subit des changements de forme importants ; quelquefois, mais rarement, il s'étale en membrane sur presque toute la surface du chorion, ou bien il y a deux placentas entièrement séparés, ou, et c'est ce qui arrive le plus souvent, le placenta est divisé incomplètement en deux par un sillon allant du bord libre jusqu'au milieu. Comme altérations correspondantes, on trouve la membrane inter-utéro-placentaire, incomplètement développée sur les parties situées à l'orifice de l'utérus, et quelquefois remplacée par une couche de tissu connectif, ou par de la caduque en transformation graisseuse, et parsemée de pigment recouvrant une lame mince de substance placentaire où les villosités, transformées en tissu cellulaire, forment des cotylédons aplatis près de l'orifice de l'utérus, sans veine circulaire. Si le placenta ne se développe pas primitivement sur l'orifice, mais sur les parties latérales, un lobe seulement étant en contact avec l'orifice, on n'y observe pas ces lésions remarquables, mais seulement des amas de fibrine et des apoplexies dans les couches superficielles et dans l'intérieur des villosités."—(P. 448.)

But it is common to have hæmorrhage, in cases of central or nearly central insertion, occurring in the latter months of pregnancy, and no pathological alterations of the placenta of a corresponding age. Such hæmorrhage is not the result of separation of the placenta. Now, the only explanation which

accords with all the facts of the case is that it arises from opening or rupture of a utero-placental vessel overlying the internal os uteri, not from separation of any part of the placenta or laceration of its whole structure. These utero-placental vessels are large trunks, with walls of extreme delicacy or thinness, and they occur at every part of the uterine surface of the placenta, and are very large in the intercotyledonary sulci and in the margin of the placenta. If the opening is formed, there is no difficulty in accounting for hæmorrhage, however great. That such a rupture or opening may occur is proved by the consideration of utero-placental apoplexy, or utero-placental hæmorrhage, there being rupture, first of all, of a utero-placental sinus in such cases. The opening may be produced by violence or increased blood-pressure, or it may be the result of some disease of the part.

Such openings are not very rarely seen when the structures are much denser and thicker, as in the bleedings from varicose veins of the leg or of the vulva. Analogous opening of a vein in the broad ligament and hæmorrhage into Douglas's space has been demonstrated by Ollivier d'Angers and others.¹ But the nearest analogy is found in the opening or openings of uterine sinuses observed in a uterine polypus or in the lower part of the surface of a projecting

¹ *De l'Hæmatocèle rétrouterine, etc.* Par le Dr. Auguste Voisin. P. 76. Paris, 1860.

uterine fibroid. Various pathologists have described these openings, and I have myself published cases.¹ These openings have to be made through the wall of the sinus, and a thin decidual layer in the case of placenta prævia, while in the case of a uterine fibroid there have to be perforated, in addition to the wall of the sinus, a muscular layer and a layer of mucous membrane.

It is interesting to remark, that in a uterine fibroid or polypus having such vascular rupture or opening, the hæmorrhage does not persistently or constantly flow, but undergoes long periods of arrestment; and this is the case with the floodings of placenta prævia. In both cases the hæmorrhage may prove rapidly fatal.

Further, it is interesting to observe that, in a uterine fibroid or polypus having such vascular rupture or opening, the hæmorrhage may, in its recurrence, exhibit a monthly periodicity; and this is sometimes the case with the floodings of placenta prævia. †

Of the mode of arrestment of this kind of hæmorrhage I know nothing quite certainly. One may imagine that decidual contractions may have something

¹ See my paper on *Hæmorrhage from Fibrous Tumour of the Uterus*, *Edinburgh Medical Journal* for January 1867. See also *Edinburgh Medical Journal*, April 1869, p. 955; and, in connection with this subject, see Leopold's case of *Cavernous Uterine Fibroid*. *Archiv d. Heilk.*, Band xiv. Heft 5.

to do with it, for Turner¹ believes he can demonstrate a muscular layer in the caducous part of the serotina. But the arrestment is probably the result of a local or general anæmia.

The *second* cause is rupture of a marginal utero-placental vessel. Nowhere in the placenta are these vessels more largely developed than at its very margin, and blood may flow from a rupture without separating any portion of placental structure, the
× blood passing directly between the decidua and the uterus, and escaping through the cervix into the vagina.

This origin of hæmorrhage, during the continuance of pregnancy, in cases of placenta prævia, is not available when the placenta is inserted centrally or nearly so ; but it may give rise to it when the placental margin is attached anywhere between the internal os and that circle of latitude of the lower uterine hemispheroid which forms the limit of the spontaneously detaching area. It may, indeed, be a source in all positions of placental attachment, as Jacquemier² has pointed out ; but it is only when in the position described that it can be properly included under the pathology of placenta prævia. In hæmorrhage from this source there will not be necessarily any dilatation of the internal os uteri, nor

¹ *Journal of Anatomy and Physiology*, 2d Series. No. XI. November 1872, p. 127.

² *Manuel des Accouch.*, tome ii. p. 229.

any subsequent morbid alteration of a cotyledon or cotyledons ; just as in cases of the first source.

Hæmorrhages arising from this source require no special cause to account for them. Like those from the first source, they may be produced by most of those causes which originate accidental hæmorrhage. But the position of the placental insertion will make a woman more liable to their occurrence than in ordinary conditions, from the increased blood-pressure to which the lower part of the uterus is, in many circumstances and frequently, subjected.

The *third* cause of hæmorrhage, during the continuance of pregnancy in placenta prævia, is partial (or even complete) separation from accidental causes, such as a jerk or fall. There can be no reason why a case of placenta prævia should not be liable to the same accidents as cases where the placenta is normally inserted ; and there can be no doubt that hæmorrhages are produced in this way in the abnormal condition.

When separation is produced in this way it is not subject to the laws of spontaneous separation produced by expansion of the internal os of the cervix. Any part may be detached.

When separation arises in this accidental way, the laws of accidental hæmorrhage proper will be applicable to it, always excepting such modifications as arise from the internal os uteri being in or near the separated area. Especially, the part separated

will, if pregnancy continues sufficiently long, undergo well-known pathological changes, and ultimately become atrophied.

The *fourth* cause of hæmorrhage is partial separation of the placenta, the consequence of uterine pains producing a small amount of dilatation of the internal os. In such cases, miscarriage may be said to have commenced, but has been soon arrested, and pregnancy has gone on.¹

X | That the fourth cause of hæmorrhage may occur, no one will, I suppose, be disposed to deny; for all the theories now entertained regarding the cause of these hæmorrhages, erroneous as I am sure they all are, assume that partial separation may take place

¹ It is worth while to note here a possible cause of hæmorrhage to which I have no other opportunity of referring, and which may produce it if the internal os is considerably enlarged during pregnancy with placenta prævia centralis. In such circumstances, a spasm of the internal os may be to such an extent as to produce separation, the spasm of the circular fibres producing detaching expansion. The detaching expansion in this case will be produced by the circular fibres of the lowest part of the body of the uterus, not by the longitudinal uterine fibres, as in ordinary detaching expansion in early labour in cases of placenta prævia. The direction of the expanding force will in this case be the opposite of that in ordinary cases. Burns seems to have entertained notions somewhat similar to the above. He says (*Principles of Midwifery*, 10th edit., p. 342), "Spasmodic action about the cervix uteri must produce a separation of the connecting vessels;" and again (p. 344) he speaks of a condition of the circulation which "induces premature contraction of its fibres, with a consequent separation of the connecting vessels."

and lead to hæmorrhage without the arrestment of the progress of pregnancy. When the placenta is naturally inserted, it may during pregnancy be partially separated by uterine pains or spasm ; hæmorrhage may thus be produced, and yet pregnancy may continue ; and I see no reason why this may not happen when the placenta is prævia.

I believe it is now generally admitted, and I am able myself to attest it, that at all periods of pregnancy, from the earliest onwards, the cervix uteri may be opened up to a greater or less extent without the interruption of pregnancy. Indeed, many cases of threatened abortion in early pregnancy are not sufficiently explained without, in addition, supposing detachment of the lower part of the decidua vera ; so large and so persistent is the hæmorrhage. Although it is of the nature of a digression, I may add, that in a case of threatened abortion, which eventually reached the full time, and which I observed, the conclusion seemed inevitable, that not only was the lower part of the decidua vera separated from the uterine wall, but that also a piece of it was detached from the remainder and expelled. I arrived at the same conclusion in a case of early abortion of one of twins, when a portion nearly as big as the palm of the hand was detached and expelled, while pregnancy with the remaining twin went on.

Separation produced in this way is subject to the laws of spontaneous separation of the placenta, pro-

duced by expansion of the internal os of the cervix. The first part detached will always be that nearest the internal os, and the rest in regular subsequent order according to position.

The parts of placenta separated in this way will undergo the pathological changes which take place in portions of placenta accidentally separated in any position during pregnancy, and which Gendrin and Simpson have described as the result of separation in quite a different manner. They will, first of all, be thrombosed; subsequently decolorisation will take place; and, lastly, thinning and drying, or atrophy, will occur if the pregnancy continues long enough after the separation.

The anatomical source of the hæmorrhage and the arrestment of it, in cases where it is produced by this and the last-described causes, I do not here enter upon, as the discussion of these matters is fully taken up when the subject of hæmorrhage during labour is considered.

Many authors, among whom are Desormeaux and Dubois,¹ Simpson,² Chowne,³ and Murphy,⁴ describe the hæmorrhage in cases of placenta prævia as being a hæmorrhage by retrogression or by regurgi-

¹ *Dictionnaire de Médecine*, 2de edit. tome xix. p. 654.

² *Selected Obstetrical Works*, vol. i. p. 236.

³ *Lancet*, vol. ii. 1847, p. 250.

⁴ *Lectures on the Principles and Practice of Midwifery*, 2d edit. p. 420.

tation. I know of no great hæmorrhage which takes place in this manner, and I know no reason for regarding this kind of hæmorrhage as following such a course. The hæmorrhage, when the result of a rupture of an utero-placental vessel, as described under the first two categories, resembles any venous hæmorrhage, and involves no retrogression or regurgitation. When the hæmorrhage is caused by separation of the placenta, either before or during labour, it is not a hæmorrhage by retrogression. It is more truly described as an oozing on a great, sometimes a vast, scale. The uterine sinuses, or these along with the utero-placental sinuses, form a network of elephantine vessels, which combined constitute a great reservoir into which the uterine arteries pour a copious supply of blood, and from one or from numerous openings in this great reservoir the blood is discharged. When a lake is formed in the course of a river, this lake may be tapped and the water drawn off without any regurgitation or retrogression. In this case the network of the uterine and utero-placental sinuses is the lake in the simile.

The explanation which has, in this paper, been given of the hæmorrhage in placenta prævia during the continuance of pregnancy, I have for many years expounded in my annual course of lectures, and I know of no other which is in accord with the facts of the case. Indeed, I can conceive no other, unless it be possible that a healing reunion and return to

the previous healthy condition can take place after partial separation of the placenta. I see no reason to doubt the possibility of such healing reunion and return to the previous condition of health, except the very important one that, while clinical experience offers no reliable example of it, or even reason to suppose its occurrence, it offers innumerable examples of failure to reunite, with the invariable consequence of pathological changes in the separated portions of the placenta, ending in atrophy if sufficient time elapses. This curious subject has been noticed by Mauriceau, who asserts that the "secundine being once loosened, . . . never joins again to the womb."¹ Gendrin also discusses it in the second volume of his *Traité Philosophique de Médecine Pratique*, and arrives at what must be regarded as sound conclusions. His opinion may be stated as follows:—that, while healing reunion never occurs, or probably never occurs, it is quite possible between the placenta and the disjoined part of the body of the uterus where it had been previously attached, but that it is impossible that it should take place between the separated placenta and the surface of the expanded cervix, with which it might come to lie in close contact; for Gendrin held the old theory of the production of hæmorrhage, during the continuance of pregnancy, by expansion of the cervix from above downwards

¹ *Diseases of Women with Child, etc.* Chamberlen's translation, p. 87.

during the latter half of the continuance of this state. But neither of these authors has remarked that healing reunion, which alone they discuss, does not imply restoration of the circulation in the separated placental cotyledon or cotyledons; and it must be admitted that this constitutes a great additional pathological difficulty in the way of admitting reunion as a means of substantiating other causes of hæmorrhage besides those already described.

Burns¹ believed that while in the early months a separated portion of decidua might be reunited and the breach thus healed, a separated portion of placenta could not be so. Dubois² believed that a reunion might occasionally occur, and this opinion is founded on the case of Noortwyk's wife; but the evidence and arguments already given in this chapter sufficiently account for the absence of appearances in that case, and it is this absence of an atrophied portion of placenta that seems to have been sufficient to convince M. Dubois of the occasional actual occurrence of this extraordinary result.

The old explanation of unavoidable hæmorrhage during pregnancy was based on the erroneous belief that the cervix began, about the middle of pregnancy, to be expanded from above downwards, so that its cavity might contribute to form the lower part of the general uterine cavity, and that this expansion gra-

¹ *Principles of Midwifery*, 10th edit. p. 355.

² *Dictionnaire de Médecine*, 2de edit. tome xix. p. 665.

dually progressed during the latter half of pregnancy. With the destruction of this erroneous belief, the theory of course simultaneously fell ; or rather, it ought to have fallen. But, like many errors, it has a lingering vitality, and so late a writer as Simpson is found still adhering to it. Even had the belief been anatomically verified, the theory founded on it would not have been in accord with the facts of the case. For, were this theory true, then hæmorrhage should regularly or invariably occur in the latter half of pregnancy, which is not the case. Were this theory true, then there should be, in every case of placenta prævia centralis at or near the full time, a
× morbid and probably atrophied portion of placenta where the separation, during the continuance of pregnancy, took place ; which is also not the case. Besides, this theory leaves those bleedings unaccounted for which take place before dilatation of the internal os has begun.

This theory was held by Gendrin, and accordingly he carefully describes the morbid appearances observed in the placenta, giving also an account of how to investigate recent specimens. His words are as follows :¹—“ Les altérations anatomiques que les auteurs ont signalées dans le placenta, lorsqu’il était greffé sur l’orifice interne du col de l’utérus, ne sont point en rapport avec toutes les circonstances que présentent les hémorrhagies qui se rattachent à cette

¹ *Traité Philosophique de Médecine Pratique*, tome ii. p. 216.

disposition anormale. Elles consisteraient toujours, suivant les descriptions qu'ils en ont données, dans un état comme de dessèchement et d'atrophie de la portion du placenta implantée sur l'orifice interne du col de l'utérus, avec la présence sur l'arrière-faix d'une tuméfaction mamelonnée correspondant à l'orifice du col, vers lequel la portion du placenta qui le recouvre se trouverait poussée et engagée au moment du travail de l'accouchement.

“ Les altérations du placenta, dans les cas d'implantation anormale, ne sont ni aussi simples, ni aussi invariables ; elles se modifient de différentes manières qui se trouvent en rapport avec les diverses formes des accidents hémorrhagiques.

“ L'altération la plus simple que présente le placenta, lorsqu'il a été inséré par un segment peu étendu de son bord sur l'orifice utérin, est la suivante : son tissu spongieux est affaissé, comme déprimé et converti en un tissu homogène rougeâtre, dans lequel du sang est immédiatement incorporé, et qui ressemble assez bien pour l'aspect au parenchyme du poumon refoulé dans la gouttière vertébrale après la pleurésie, mais avec une fragilité qui se rapproche de celle de la rate. Le chorion tantôt s'étend sur cette partie du placenta sans présenter de modification apparente, tantôt il est rompu transversalement et détaché à la limite de la portion altérée et des parties saines continues. Cette rupture nous a toujours semblé avoir été produite dans le travail de l'accouche-

ment par l'interposition nécessaire du segment décollé du placenta entre la tête ou le siège du fœtus et le bord du col. Cette interposition produit aussi très probablement, au moins en grand partie, la dépression de la portion du placenta engagée dans le col. Quand le chorion n'a pas abandonné la partie du placenta qui se trouvait décollée, on le trouve dans presque tous les cas rompu à quelques lignes du bord externe. L'altération placentaire que nous venons de décrire est celle qui se rencontre quand l'hémorrhagie a précédé de peu de temps ou accompagné le travail de la parturition.

“ Dans les cas où des hémorrhagies se sont reproduites à différentes fois et par intervalles plus ou moins éloignés, avant l'accouchement et au moment du commencement du travail, le segment du placenta inséré sur le col est plus étendu et les altérations du bord de cet organe qui a été décollé ne sont pas les mêmes sur tous les points ; il faut alors les considérer sur trois zones excentriques l'une à l'autre.

“ Sur la zone la plus voisine du centre de l'organe, le tissu placentaire est condensé et réduit en un tissu homogène dense, comme granuleux, d'une couleur jaune grisâtre, se rompant avec facilité, traversé par des filaments blanchâtres qui arrivent jusqu' à la surface utérine en se ramifiant, et se perdent dans la couche jaunâtre qui ne présente pas dans son épaisseur le moindre vestige de points rouges. Au milieu de ce tissu homogène se rencontrent toujours de petits

caillots d'un rouge noirâtre, ordinairement très nombreux, qui ne s'isolent pas du tissu homogène environnant, et se fondent dans sa substance. Ces petits caillots pénètrent le plus souvent jusque sous le chorion. La surface du placenta, sur ces parties ainsi altérées, présente souvent des taches blanches d'une à deux lignes de diamètre, très nombreuses et plus ou moins confluentes, faisant un léger relief, et ressemblant au premier aspect aux petites plaques que forme la matière tuberculeuse sur la séreuse abdominale après les péritonites tuberculeuses.

“La zone intermédiaire, excentrique à celle qui vient d'être décrite, présente un tissu rougeâtre dans lequel on reconnaît du sang coagulé, infiltré et même incorporé avec le tissu placentaire. Ce tissu est plus mou et beaucoup plus friable que celui du placenta à l'état sain ; son apparence homogène et la continuité de sa trame sont interrompues par des foyers sanguins qui pénètrent à différentes profondeurs de la surface utérine vers la surface fœtale.

“Le chorion, sur les deux zones internes qui viennent d'être décrites, ne nous a jamais offert ni altération de texture, ni déchirure.

“La zone extérieure présente l'état d'altération qui se trouve dans les cas où une petite portion du bord du placenta correspondait à l'orifice du col.

“Il est évident que ces altérations ne diffèrent que par leur ancienneté ; elles se sont produites à des époques diverses. Les plus voisines du centre

du placenta sont les plus anciennes et correspondent aux premières hémorrhagies, et les plus excentriques aux plus nouvelles : la forme et les limites latérales des altérations le démontrent aussi. L'altération la plus voisine du centre est toujours la plus étroite et la plus éloignée la plus large, en sorte que toute la partie malade du placenta a la forme d'une triangle, dont la base est vers le bord de l'organe.

“ Indépendamment de ces altérations l'on trouve sur le placenta des lésions qui ont souvent été produites au moment du travail d'expulsion ; elles consistent en des déchirures remplies d'une plus ou moins grande quantité de sang coagulé, que l'on enlève aisément par une macération de quelques heures. Ces ruptures ne sont point transversales, elles ont l'aspect des ruptures, en quelque sorte étoilées, qu'on produit en serrant dans la main un corps mou et fragile, de manière à rassembler ses parties périphériques autour d'un centre.

“ Les divers degrés d'altération du tissu placentaire, que nous venons de décrire, constituent la première période d'une lésion plus ancienne, que nous avons rencontrée plus fréquemment que toutes les autres sur des arrière-faix expulsés par des femmes qui ont eu à des périodes éloignées de la parturition des hémorrhagies plus ou moins considérables qui ne se sont pas renouvelées. Cette lésion consiste dans la conversion d'une partie du bord du placenta dans toutes son épaisseur, en un tissu homogène, d'une

jaune pâle, tirant sur la teinte grisâtre, n'offrant aucune trace de vaisseaux apparents. La partie du placenta ainsi altérée est constamment au moins de moitié plus mince que les parties saines de l'arrière-faix correspondantes ; elle est lisse et d'épaisseur égale dans toute son étendue ; les bosselures des mamelons n'y sont plus apparentes. Cette partie de l'arrière-faix contient toujours dans son épaisseur des portions rougeâtres qui ne sont point disposées par couches, mais par grumeaux ramollis, dans quelques uns desquels on reconnaît très bien du sang altéré. Nous avons trouvé dans un cas un seul, et une autre fois deux foyers hémorrhagiques très circonscrits et de six à huit lignes de diamètre pénétrant et occupant toute l'épaisseur de cette partie du placenta qui avait été inserée sur l'orifice du col et qui était devenue jaunâtre. Le chorion correspondant était d'un blanc mat légèrement jaune ; il était épaissi comme par l'addition ou l'infiltration d'une matière d'apparence albumineuse, et l'on ne remarquait point en lui d'injection vasculaire anormale."

Simpson, holding the erroneous theory, and closely following Gendrin, gives an account of what he erroneously calls "successive apoplectic infiltrations," and describes the changes which they would undergo in the latter months of pregnancy. "We can occasionally trace (says he) in the placenta, after its expulsion, different parts of it, showing a series and gradation of pathological changes arising from

successive partial detachments.”¹ But his description has not the appearance of being derived from the actual examination of any placenta, and certainly he does not describe any. I have never seen such a specimen, nor do I know of any detailed description of one. That such a condition may occur, produced by the fourth cause which I have described, I see no reason to doubt, but it must be very rare. It is to be hoped that obstetricians will direct their attention to placentas born in cases of placenta prævia, with a view to discovering and describing the desiderated specimen.

It is easy to find apoplectic intra-placental clots, utero-placental layers of clotted blood, and thrombosed cotyledons, of various ages and corresponding degrees of decolorisation, in the same or in different placentæ; but this is not what is wanted. Thrombosed cotyledon or cotyledons, without or with utero-placental blood-clot, or intra-placental apoplectic clot, must be found in the same placenta, and with due concentric arrangement of different degrees of decolorisation, and in such circumstances as dispel doubt as to the cause being successive separations of the placenta around the internal os uteri. This is what is wanted.

When this oldest theory became untenable, another was adopted, based on a closely analogous anatomical supposition—namely, that the lower portion

¹ *Selected Obstetrical and Gynæcological Works.* Edited by J. Watt Black, M.D., etc., p. 223.

of the uterus became, during the latter half of pregnancy, greatly and specially developed, and that an incompatibility between the placenta and this part was produced, and thus separation; because the placenta ceased to grow about the sixth month, or at least long before the great development of the lower uterine hemispheroid had ceased. Now, to all this ingenious theorising fatal objections are easily raised, closely analogous to those raised to the former theory. There is no evidence that the lower uterine hemispheroid is more developed during the latter half of pregnancy than the upper, where the placenta is generally attached. Indeed, I am disposed to think that evidence could easily be found to the contrary effect. Were this theory true, then hæmorrhages should be the invariable rule, or nearly so, in cases of placenta prævia during pregnancy; which is not the case. Were this theory true, there should be in every case of placenta prævia, at or near the full time, an atrophied portion of placenta where the separation took place, which is not the case. Besides, this theory does not account for the separation taking place just at the internal os uteri.¹

Very recently another ingenious theory of this hæmorrhage has been propounded in the graphic pages of Barnes.² This theory may be described as

¹ For a good account of this theory see the work of Cazeaux, *Traité Théorique et Pratique de l'Art des Accouchements*.

² *Lectures on Obstetric Operations, etc.*, 2d edition, p. 401.

the converse of that which I have last discussed. It alleges that the disproportionate growth is on the part of the placenta, and that the hæmorrhages are the result of loss of relation, or separation produced by the placenta shooting beyond its site. Now the
× whole of this theory is a hypothetical structure without any basis of facts. But this is far from being all that can be said against it. It is a theory which, if true, should have in support of it the occurrence of similar hæmorrhages when the placenta is inserted in its normal site, for every part of it
× applies equally well to cases of natural pregnancy. Further, the theory does not explain why the separation should take place at the internal os uteri. It does not explain how, in many cases, there is no hæmorrhage during pregnancy; and lastly, the fact that no atrophied portion of placenta is found corresponding to the supposed detachment, is hostile to or inconsistent with it.¹

My theory, as explained in the preceding pages, is that the hæmorrhages during pregnancy are accidental, not necessary, and that their occurrence is favoured by the extraordinary anatomical conditions existing in placenta prævia, as well as by other circumstances, some of which are known, as the increased
× pressure of the blood above what it would be

¹ Other theories, such as that of Behm (*N. Zeitschrift für Geb.* X. Band, S. 232) that the hæmorrhage depends on irregular and spasmodic uterine contractions, do not demand additional remarks.

were the placenta inserted high on the uterine walls.

I believe much harm has arisen from the custom of authors to treat unavoidable as quite distinct from accidental hæmorrhage ; whereas their whole pathology, though not identical, is very nearly so ; and the study of them is made easier by so regarding them. ✕

CHAPTER XXII.¹

ON THE SPONTANEOUS SEPARATION OF THE PLACENTA WHEN IT IS PRÆVIA.

CASES of placenta prævia are, as is well known, divided into classes—central, lateral, and marginal. As these designations are extremely ill-defined, and as, besides, different authors use the terms with different meanings, I shall avoid using them altogether as terms of classification, but resort to them as words bearing meanings generally understood.

I shall not, in this paper, discuss that part of the separation of a placenta prævia which, if the whole organ has not previously been detached from the uterus, is delayed till the usual time of separation, that is, when the birth of the child is being completed or is completed; nor shall I discuss those separations, from accidental causes, to which the placenta is probably more liable when prævia than when it occupies its natural site. My remarks will be confined to that total or partial separation which takes place before the child can be born, and which may be described as almost a necessary result of the mechanism of delivery in such cases.

¹ Read to the Obstetrical Society of London, October 1, 1873.

I shall refer in the course of the paper only to the more important positive errors which are to be found in well-known and esteemed authors ; but it is desirable also to point out that the whole ordinary mode of describing the anatomy and physiology of this subject requires to be corrected to bring it into agreement with anatomical and physiological facts.

The importance of the subject lies not merely in the settlement of points of scientific interest, but in their settlement being the only basis of a rational practice, and peradventure the guide to the establishment of such practice.

The lower part of the cavity of the uterus, as it is distended in advanced pregnancy, is part of a spheroid or a hemispheroid, with which the cervix is in connection, and of whose surface the internal os of the cervix occupies the vertex. To form the genital passage for the child, this hemispheroidal shape is destroyed by the expansion of the above-named internal os of the cervix and of the adjoining parts. That which formed during pregnancy a portion of a spheroid forms during the passage of the child a small part of a nearly cylindrical tube.

This expansion is effected by uterine contractions. The circular fibres of the lower part of the body of the uterus are either in a state of relaxation while the longitudinal are in action, or their action is overpowered by that of the longitudinal. While expansion of this lower portion of the body of the uterus

in every direction transverse to its axis is going on, there is simultaneous shrinking in a meridional direction.

During labour every portion of the surface of the body of the uterus undergoes shrinking in some direction, and I know no fact or argument which indicates that one part shrinks more than another. In the lower hemispheroidal portion the shrinking or retraction can be only in the meridional or longitudinal direction, and the same is true of the part of the cylindrical body of the uterus immediately above this lower hemispheroidal portion. In the lower hemispheroidal portion there is expansion simultaneously with shrinking; in the part above there is only longitudinal shrinking. Although there are no facts or arguments indicating any variation in the amount of shrinking in different parts of the uterus, the matter is one which may come to be the subject of conclusive demonstrations. At present it is not easy to suggest any method of reaching sound conclusions in this matter, but the comparative thicknesses before and after shrinking, in different regions of the uterus, measured in many cases, may give good results. This plan Sc^hatz¹ has used with some effect, but his basis of facts is as yet far too limited for any argument. I have also resorted to it, so far as possible, in several cases; but without attaining, as yet, good results, and certainly without showing

¹ *Archiv für Gynæk.* Band vi. S. 403.

any probable error in the conclusion as to equal shrinking which I adopt at present.

This shrinking is the cause of the tendency of the body of the uterus during labour towards a nearly hemispheroidal shape with its large inferior opening into the cavity of the cervix.¹ The shrinking is possible, for the body of the uterus embraces the ovum tightly during pains, and part of the ovum is pressed into the newly forming large cavity of the cervix, or through it into the vagina.

There is nothing known as to the arrangement or strength of the muscular fibres of the body of the uterus which would induce any doubt as to the meridional shrinking being the same in all its parts. This meridional shrinking similarly affects the placental site or insertion wherever it may be. The placenta is so constructed and so connected with the uterus that this shrinking does not separate it in early labour, that part of the process with which alone the subject of this paper is concerned. The separation, indeed, does not take place till the child is partly born, that is, until a very much greater degree of shrinking has been produced than can ever exist during the separation of the placenta when prævia. Although the lower part of the body of the uterus is, in early labour, expanded greatly, that condition does not imply greatness of shrinking or retraction; this latter may be very slight while

¹ See page 275 of this book.

rapid expansion is going on. Now clinical observation assures us that a small proportional part of the whole amount of expansion that must take place separates the part of placenta nearest the internal os, and it cannot be supposed that at this time shrinking has any influence on the separation, for the shrinking must be to a very slight degree. If this amount of shrinking or retraction detached the placenta, or any amount of shrinking that takes place in early labour, then this organ would be detached in early labour in every case of childbirth. I therefore conclude that the placenta when prævia is not detached by uterine contractions or uterine shrinking.

There is no doubt that uterine contractions do, in all normal cases, effect the detachment of the placenta when the child is nearly or completely born, the uterus being then nearly empty, and the shrinking of the placental insertion very great; and it is extremely natural to attribute to the same cause its detachment in early labour in the abnormal state of placenta prævia; but I think I have shown that, at least in the present state of our knowledge, it is not rational to attribute it to this cause: yet that it is generally attributed to this cause is well known, and I shall cite a very recent author on this subject to show the prevalence of the error. In the work referred to¹ is the following passage:—"The mouth of the womb must open to give passage to the child.

¹ Barnes's *Lectures on Obstetric Operations, etc.*, 2d edit., p. 408.

This opening, which implies retraction or shortening of the cervical zone, is incompatible with the preservation of the adhesion of the placenta within its scope. In every other part of the womb there is an easy relation between the contractile limits of the muscular structure and that of the cohering placenta. Within the cervical region this relation is lost. The diminution in surface of the uterine tissue is in excess."

The shrinking or retraction of the uterine wall, to which the placenta is attached when prævia, taking place as it does in the direction of the longitudinal axis of the womb, while it does not detach the placenta, produces an amount of detrusion of the whole of the organ when separated or of the separated portion. The detruded portion is somewhat hardened by thrombosis of the placental cells, and its lying in the cervical cavity has been a fruitful source of error. It may be remarked that if, as labour advances, it hangs with its lower edge far down in this cavity, it has probably been originally attached so as to cover the internal os, and its lower margin has been originally attached to the side of the uterus opposite to that to which it is now adjacent. There can be no doubt this detruded portion has been generally regarded as attached to the cervix, often even as far down as the margin of the os uteri externum. One cannot read authors without seeing that the error, though often stated, is

still oftener implied; and P. Müller¹ deserves the credit of ingenuousness for pointing out his own fall into it. Moreover, deceitful adhesion by clotted blood, between the cervix and placenta, and imaginary detachment by breaking it up, are, no doubt, common occurrences. When authors have supposed that they have worked out the pathology of placenta prævia and its rational treatment, they have been proceeding too hastily, for they have only been groping in the dark, ignorant of the behaviour of the cervix during labour, and of many other points necessary to be known and considered before reaching the desired results.

The paramount errors which run through writings on this subject are, that the cervix uteri is or may be the seat of placental attachment, and that in early labour the surface of the cervical portion of the uterus is diminished in extent. The truth is that the placenta is never attached to the cervix, and that the behaviour of the cervix in labour, and the amount of opening of the os uteri externum, have only indirect and remote relations with the pathology of placenta prævia.² A placenta prævia is attached to the body of the uterus above the cervix, and may cover its internal os. The surface of the cervix uteri,

¹ *Untersuchungen über die Verkürzung der Vaginal-Portion in der letzten Monaten der Gravidität*, S. 126. For some remarks in connection with this topic see my *Researches in Obstetrics*, p. 261.

² For an example of this error of attaching too much importance to the condition of the external os, see Barnes's *Lectures on Obstetric Operations*, 2d edit., p. 412.

instead of being diminished during labour, is greatly increased in every direction.¹

It is important for students to know that well-known authors have written under the influence of such fundamental errors ; and the circumstance is the more remarkable because antecedent authors, whose works they have carefully studied, have distinctly disavowed the erroneous views. In the following passage from the works of Simpson,² for example, we have the chief error referred to, as well as others :—“ There are, according to most anatomists, few or no contracting fibres in the structure of the os and cervix uteri, and certainly after delivery, I have generally, if not always, found it remaining open, gaping, soft and flaccid, even when the proper cavity of the uterus above felt shut and contracted, and its parietes hard and firm. Still, when the placenta is attached to the surface of this uncontracting portion of the uterus, hæmorrhage is not common after its separation, unless some laceration of its vessels has occurred.”

In like manner Barnes, although showing in some passages a degree of appreciation of accurate views, gives a figure at page 410 of the second edition of his lectures on obstetric operations, which is reconcilable only with the erroneous views ; and at page

¹ See *Edin. Med. Journal*, June 1873, p. 1066, and p. 274 of this book.

² See *Selected Obstetrical and Gynæcological Works*, edited by J. Watt Black, M.D., p. 237.

421, imitating Simpson, he has the following passage embodying great errors:—"In every labour" (says he) "the cervix, having to suffer great distension and contusion under the passage of the child, and possessing less contractile elements in its structure than the rest of the uterus, is liable to paralysis for a time. This state is more likely to occur in labour with placenta prævia, and it is doubly dangerous because
 × the cervix is the placental site."

It is at present impossible to give an exact statement of the changes which take place during labour in the lower hemispheroid of the uterine body, especially as to whether its area is diminished or increased; but of some points there is no doubt. Like the cervix, this lower hemispheroidal portion has its extent
 × greatly increased in all directions transverse to its axis. Unlike the cervix, it has its meridional extent
 × diminished, that is, in a direction nearly parallel to its axis. I know at present no means of deciding
 × whether the expansion in one direction does or does not make up for shrinking in the other.

The placenta may be spontaneously detached from the uterus in various ways, as by utero-placental hæmorrhage pushing it off, or by traction of the cord pulling it off; but the ordinary mechanism of
 × its separation is contraction of the seat of its attachment, with which a detrusive force may be combined. In cases of placenta prævia, none of these mechanisms is in efficient action, but that which operates is ex-

pansion of the seat of its attachment. The separation by shrinking of the placental insertion, and that by expansion of it, have this in common, namely, that both consist in the production of an incompatibility between the area of attachment and the organ attached. Shrinking of the placental seat, producing compression of the cotyledons and substance of the placenta, may take place to a considerable extent, without producing detachment, as in the first stage of ordinary labour; and, as already said, there is no ground for believing that this contraction is, in the first stage, in cases of placenta prævia, greater than x in the first stage of ordinary labours, or goes so far as to be incompatible with the maintenance of the placental connections. Expansion of the placental insertion in the first stage of labour is the distinguishing specialty of placenta prævia, and has detachment as its distinguishingly peculiar result. This kind of expansion is only rarely a cause of detachment under any other circumstances. Hydramnios may produce placenta membranacea, and is liable to produce detachment by uterine expansion. In placenta prævia expansion of the lower part of the body of the uterus, in early labour, regularly produces, first, stretching of cotyledons and expansion of placental surface, and then separation. While detaching expansion of the uterine site of a previous placenta or of a previous portion of a placenta is going on in directions transverse to the axis of the uterus, shrinking is going

on in a direction parallel to the uterine axis, and this contraction pushes the detached placenta or detached portion into the developing or expanding and elongating cervix, and in the early part of the first stage brings it within easier reach of the examining finger ; while, at the same time, the seat of former placental attachment is getting more and more distant from the external uterine orifice and the examining finger.

This problem of the separation of the placenta when *prævia* is made easier of comprehension by directing attention to a case of what is called marginal attachment, or when the placenta encroaches on only one side of the lower uterine hemispheroid, and does not cover its vertex, that is, the internal os uteri. The mechanism of separation is no doubt the same when the placenta is inserted centrally, that is, when its centre is at or near the vertex of the hemispheroid, but then one is confused by the reflection that, if the os uteri internum opens, it cannot do so without separating from the placenta, unless it is disrupted. This disruption sometimes does actually occur, and it may be the mechanism of some cases of birth through a hole in the placenta, the organ being torn through from below upwards, and a way thus made for the *fœtus* ; not perforated by the head being forced through it from above. But this is a digression from the main subject. The placenta is, in fact, seldom disrupted, and the opening of the cervix is then incompatible with continued cohesion of the

placenta and uterus, because of the great amount of the expansion, as has been described.

The detachment will progress as the pains and the first stage of labour proceed ; and as the detachment of the spontaneously separable portion may not be completed till the internal os is dilated to a diameter of about four inches, the process of gradual detachment may not only occupy a long time, but may also occupy a great proportional part of the whole duration of labour. Indeed it may safely be asserted that in labours which go on with ordinary activity, and uninterfered with, in multiparæ, the gradual process of detachment will occupy by far the greater part of the time of the labour. The detachment will be completed when the first stage of labour is completed, that is, when the external os uteri is completely dilated. But the detachment may be completed, and probably will be completed, much earlier than this, namely, when the internal os uteri is completely dilated, or even somewhat earlier, for it is probable that the limit of necessary expansion of the part is reached without separation going on during the last part of the stretching.

The extent or amount of placenta detached when prævia will depend on the degree of approximation to centrality of attachment over the internal os, and can therefore be settled only by direct observation in each individual case.

A more important law of detachment may be

discovered by directing attention to the shape of the lower hemispheroidal portion of the pregnant and non-parturient uterus, in order to find the extent, measuring along a meridian from the vertex, over which the expansion is such as to produce detachment.

Here we must make a digression as to the extension of a surface according to the direction of the traction or resultant of expanding forces to which it is exposed. Take as the simplest and quite sufficient instance a spherical surface exposed to a uniform traction in all directions perpendicular to one diameter. If it be converted into an oblate spheroid, different portions of the surface are increased in very different proportions; the areas towards the ends of the diameter being increased in the greatest ratio, while the equatorial areas are increased in the smallest. In other words, those portions of the surface which are most nearly perpendicular to the diameter or axis, that is, those which are most nearly parallel to the plain of retraction, are relatively most extended.

Thus we see that those parts of the lower hemispheroidal uterine portion, which, before dilatation of the internal os uteri begins, are nearest the internal os or vertex, are necessarily the most extended, while those parts which are more remote, and whose surface is more nearly parallel to the uterine axis, undergo comparatively little extension. At the remote parts this extension may be, and no doubt is,

so slight as not to disturb adhesion, seeing that the placenta has some extensibility of its own. It thus happens that the area of spontaneous detachment is not coextensive with the whole area of expansion. It is thus also easily explained why detachment begins at the internal os, for there expansion is at all times greatest. ✕

With these principles in view I have examined the shape of the lower uterine hemispheroid in actual or authentically depicted uteri, and I find that a meridian leaves the vertex or centre of the internal os in a direction nearly at right angles to the uterine axis, and that, after it has described an arc of one and a half to two inches in length, it becomes nearly parallel to it. At about two and a half inches from the vertex the diameter of the uterine cavity is four inches, and this is about one and a half inch above the vertex, measuring along the uterine axis. ✕ A canal ✕ of about four inches in diameter is large enough to ✕ transmit the fœtus. There is therefore no need for expansion to any considerable amount above that circle of latitude which is distant two and a half ✕ inches measured along a meridian from the centre of the internal os. The measurement will of course be less than two and a half inches if taken from the edge of the internal os instead of its centre. It is thus seen that the area of the lower uterine hemispheroid over which dilatation of the internal os necessarily involves great expansion of uterine sur-

face nearly corresponds with the extent of area which must be expanded in order to transmit the foetus. Expansion beyond a diameter of four inches would insure very slight extension of uterine surface and a consequent slight detaching power, so slight indeed as to be probably more than counterbalanced by placental expansibility.

I therefore conclude that the detaching area of the lower uterine hemispheroid, covering a portion whose vertex is the centre of the internal os uteri, has a meridional semi-length of about two and a half inches, and that its chord measures about four inches.

This measurement of the extent of the spontaneously detaching area is, I believe, quite consistent with all the facts of the matter hitherto ascertained. Measurements founded on the extent of the thrombosed portion of placenta in cases of unavoidable hæmorrhage require to be much more carefully considered than they have been. I have often seen thrombosed segments of from three to four inches across, measuring along a line extending from the edge to the centre of the placenta. Before such thrombosed segments of placenta can be taken as tests in this matter, the following points must be settled:—Is the thrombosis limited exactly to the detached portion? Was the case examined before dilatation of the internal os, and the placenta ascertained then to be attached marginally to the internal

os? The mere statement that an edge of the placenta was felt in the os or cervix in early labour is quite insufficient, especially when made by authors ignorant of the behaviour of the cervix in labour, and holding erroneous views as to the insertion of the placenta when prævia, for such edge may belong to a portion of placenta which had originally been attached so as to cover the os uteri internum, and now presents its edge in the cervix, having become detached, retaining, however, its connection with the still adhering major part of the placenta. The extent of such thrombosed parts, being three or four inches across, is quite compatible with the views which I am enunciating when thus explained.¹

Cazeaux² speaks of the whole lower third of the uterine cavity as the seat of spontaneous detachment, but he gives no reason for this except the imaginary one, that the lower third increases more than, and disproportionately with, the placenta inserted on it. I reject his view, which is derived from Jacquemier, as having no good evidence in its favour either from theory or from facts.

Barnes estimates the extent of what he calls the

¹ I have dissected a case where there was no considerable flooding, and yet the placental insertion was found after death to be almost touching the internal os uteri. Heiberg (*Die puerperalen und pyämischen Prozesse*, S. 23) relates a case of unavoidable hæmorrhage when the placenta reached only almost to the internal orifice.

² *Traité Théorique et Pratique de l'Art des Accouchements*, etc., ed. iv. p. 700.

“cervical zone,” or that part of the inner surface of the womb bounded above by what he calls the “lower polar circle,” and which forms his “region of dangerous placental attachment,” as measuring from three to four inches from the os uteri. But this is far too indefinite, for he does not state whether or not he measures along a meridian or its chord ; nor does he state whether or not he measures from the external or internal os, and this latter circumstance introduces a possible variation or error of at least an inch during pregnancy and at least three or four inches during labour. If he measures from the internal os, as he may be supposed to do, then his three or four inches along the placental insertion would conduct far beyond where any dilatation takes place or where any mechanism of detachment could be established. Besides, he makes no allowance for the diminished amount of extension of uterine wall produced by the same amount of dilatation, according as the distance from the os uteri is increased.

Measurements, founded on the distance of the laceration of the membranes from the edge of the placenta, to which Barnes confides, following Von Ritgen and Carmichael, I regard as delusive ; for, although the membranes burst at the os uteri, the rent may, within the limit of this part, be an inch or inches nearer or more remote from the margin of the placenta, and spontaneous extension of the rent may cause additional confusion and error.

The determination of this measurement or of the limit of the area of spontaneous detachment is a matter of much interest, for it is the criterion for distinguishing between what is and what is not placenta prævia. Hæmorrhages produced by detachment of placenta or of a portion of it inserted above this limit are accidental; those produced by detachment, not accidentally caused, below this limit, are unavoidable. Speaking of the placenta as a mere mechanical obstacle to the birth of the child, we cannot call it prævia unless it is inserted centrally or nearly so, overlapping the internal os. But the placenta is not regarded in this light, for its importance as a cause of hæmorrhage is paramount; and it is, in this point of view, prævia when any part of it is attached within the detaching area, whose limits we have been discussing; or rather, that part of it is prævia which is attached within these limits.

The separation of the placenta, which alone I have hitherto been discussing, is that separation which is both spontaneous and a necessary result of the mechanism—that which ordinarily occurs in cases of this nature. But I see no reason to doubt that those uncommon accidents, which produce detachment in cases of what is called accidental hæmorrhage—that is, when the placenta is not prævia—may have a like effect when the placenta is prævia, and to these I shall not make further reference. But there is one occurrence which demands special consideration, that

is, the total separation before the birth of the child. Now, although the placenta when prævia, being occasionally very thick, has sometimes a small diameter, it cannot be so small and so exactly placed centrally over the internal os as to have its whole surface within the detaching area of the lower uterine hemispheroid. How, then, is the total separation to be accounted for? It appears to me that its explanation is suggested by the observation of cases where the head perforates¹ the placenta before birth, and of cases wherein a caul is produced. In the latter accident the membranes are not lacerated at or near the centre of the os uteri but higher up, and a caplike portion, sometimes enclosing liquor amnii between it and the head, is pushed out along with it. As a caul is sometimes expelled long before the child, so may the completely separated placenta. In the former accident, where the head is forced through the placenta, we have a more or less central attachment of the organ, and the advancing head finds it easier to perforate it than to detach its margin, which lying beyond the spontaneously detaching area proper, remains adherent after the central parts are separated. So, in some cases of more or less central

¹ It would be interesting to know if, in cases of perforation of the placenta, there was thinning of placental structure or absence of it in the part perforated. Of course perforation happens because less force is required to push the child through it than to separate it at one side to let the child pass, or to separate it entirely and expel it before the child.

attachment, it may happen that the liquor amnii or the head, propelled against the organ, may find it easier to detach the adherent margin and to push down the whole separated organ than to perforate it. The complete separation of the placenta is thus an accident beyond the common course of cases of centrally attached placenta prævia, and having a peculiar mechanism superadded to that of ordinary cases. This mechanism is detrusive force applied to the upper surface of the placenta, such as, when applied to membranes, produces a caul, and such as sometimes results in perforation of the placenta when it is prævia.

Separation of the placenta is universally held to be the explanation of those hæmorrhages in cases of placenta prævia which frequently occur in the latter months of pregnancy, and much ingenuity has been expended in the production of theories to account for them. I do not believe that in these hæmorrhages any separation necessarily takes place. To this subject I have made a slight contribution at page 261 of my *Researches in Obstetrics*, and I shall not here enter further upon it, because doing so would lead me into the subject of hæmorrhage which I exclude from consideration at present.

Before concluding, I call attention to the interesting observations of Haussmann¹ on the separa-

¹ *Beiträge z. Gynäk. u. Geburtsh.* Berlin, 1873. See also a case of Hélié's, reported by Joulin, *Traité complet d'Accouchements*, p. 966.

tion of the mucous membrane of the womb in a variety of circumstances. These indicate that this takes place first at the junction of the cervix and body, that is, where a similar phenomenon occurs in placenta prævia ; and it appears to me probable that an analogy in several respects may exist between the two sets of phenomena, though they are at first sight widely different.

CHAPTER XXIII.

ON THE CAUSES OF UNAVOIDABLE HÆMORRHAGE DURING MISCARRIAGE OR LABOUR WHEN THE PLACENTA IS PRÆVIA.¹

THERE is in obstetrics no subject more important, or which has attracted more attention, than unavoidable hæmorrhage. Its causes, sources, and the mechanism of its arrestment, have been variously described; and most authors, especially those of recent times, base their practical recommendations on their theoretical views on these subjects; and, as the theories differ, so of course do the practices enjoined. The treatment, therefore, in this very grave class of cases, is in an unsettled state, and it will probably remain so until the whole theory of the hæmorrhage is surely established.

I have in former chapters discussed the separation of the placenta when prævia, and the hæmorrhages which take place during the continuance of pregnancy; but the topic of the present chapter is beset with more difficulties, and cannot, I fear, be regarded as so well cleared up as those of the preceding.

¹ See *British Med. Jour.*, November 22 and 29, 1873.

The hæmorrhage during miscarriage or labour at the full time is the only loss that can be truly called unavoidable, and it is so in two different senses. It is, first, unavoidable because we have no means of preventing it. It is, second, unavoidable, because labour (and here, as well as in future in this chapter, we shall include under the term labour both miscarriage and labour at the full term) cannot go on without inducing it. No doubt many authors¹ take particular care to describe cases without any hæmorrhage; Cazeaux, indeed, says, "without the loss of a drop of blood." But such statements require consideration, and do not disprove the inevitable character of the hæmorrhage. The placenta is never separated without loss of blood. It is easily conceivable, that a case of placenta prævia may go on in labour without serious loss of blood or great hæmorrhage. The function of parturition is sometimes conducted with so great energy and rapidity, or with so early discharge of the liquor amnii, that such cases, complicated with placenta prævia, may have very little attendant hæmorrhage. I have repeatedly induced premature labour in placenta prævia without any hæmorrhage occurring worthy of the name of flooding, or without more than is seen in a healthy confinement. Another explanation may account for such cases without flooding, namely, that the part adhering to the area of spontaneous premature detachment,

¹ Mercier, Moreau, Cazeaux, Simpson, Barnes, etc.

or within the cervical zone of Barnes, may have been separated long before labour. This explanation was long ago given by Jacquemier¹ and Cazeaux; but it is plain that here the placenta is only nominally prævia. The previous portion of it is no longer attached and useful, but detached, probably atrophied and perfectly useless. Cazeaux² gives the explanation in the following words:—"In others, it may be as M. Jacquemier remarks, that the accomplishment of the delivery without accident is due either to the entire separation of the placenta, or to its detachment on one side only to a point just beyond the uterine orifice; so that the dilatation can progress without increasing the detachment, the vessels previously torn having been stopped by coagulated blood."

Still another explanation of absence of flooding is adopted by Jacquemier³ and Cazeaux,⁴ who derive it from Moreau, namely, that death of the child some time before the labour, having given rise to coagulation of blood in the placenta, will lead to arrest of the supply of maternal blood to the organ, and that, in consequence, separation may take place without loss. This explanation was adopted by Simpson,

¹ *Manuel des Accouchements*, 1846, tome ii. p. 240.

² *Theoretical and Practical Treatise on Midwifery* (Bullock's Translation), 1868, p. 757.

³ *Manuel des Accouchements*, tome ii. p. 243.

⁴ *Traité Théoretique et Pratique de l'Art des Accouchements*, edit. vi., 1858, p. 701.

but is, I believe justly, contested by Legroux¹ and Barnes.² I am not at present in possession of knowledge sufficient to enable me to speak exactly regarding it, yet I may make three statements adverse to its acceptance. First, I believe in no mysterious attractive or "sugescent" power in the living fœtus or maternal placental vessels, such as is described by Gendrin³ and Simpson.⁴ Second, cases are not rare where death of the fœtus is found to have no controlling influence over uterine hæmorrhage, whether accidental, or unavoidable, or *post partum*. Third, the circulation in the maternal part of the placenta does occasionally go on long after the fœtus is dead, even after it is decomposed. Although this is contrary to the general opinion of obstetricians, I have been strongly impressed with its truth by a recent case, where, long after the death of the fœtus, the placenta was full of fresh blood and healthy-like.⁵

We have no hesitation, then, in saying that, when the placenta is truly prævia—that is, attached when labour begins to the area of spontaneous premature detachment—hæmorrhage is unavoidable.

The cause of the hæmorrhage is separation of the placenta produced by expansion of the lower

¹ *Archives Gén. de Médecine*, 1855, vol. ii. p. 646.

² *Lectures on Obstetric Operations*, 2d edit., p. 409.

³ *Traité Phil. de Médecine Pratique*, tome ii. p. 275.

⁴ *Selected Obstetrical Works*, pp. 226 and 238.

⁵ See a similar statement by Delore, *Archives de Tocologie*, Mars, 1874, p. 192.

uterine hemispheroid in order to form part of the genital passage.

Wherever, within the spontaneously and prematurely detaching area, the placenta may be inserted, the first part of it separated will be that next to the internal os uteri, and the rest successively, as distance from the os is increased, till the latitudinal limit or distance of the extreme parts of the spontaneously premature detaching area from the os internum or the vertex of the hemispheroid, is reached. This is both clinically true and a consequence of the laws of separation already elsewhere described.

When the placenta is inserted so as to overlie the internal os, hæmorrhage will commence as soon as the expansion of the part of the uterine body next the internal os is greater than can be met by the slight expansibility of the placenta, and this may be simultaneous with the commencement of active or painful labour. Bleeding, however, frequently begins before painful uterine action begins, when expansion is going on under the influence of silent or painless labour. On this subject I have elsewhere¹ remarked, "Practitioners are not frequently called upon to make a vaginal examination just before the obliteration of the cervix begins, that is, generally some days before active or painful labour supervenes. But to this general statement, cases of placenta prævia form an exception. In them, it is well known that,

¹ *Researches in Obstetrics*, p. 260.

at about the full time, hæmorrhage commences quite unexpectedly. It, in fact, begins with the commencement of the dilatation of the internal os uteri—that is, with the commencement of the ordinarily painless contractions which precede active labour.” I might adduce, in illustration of these remarks, many well-observed cases that have occurred in my own experience, or that are recorded by well-known and accredited authors.

Braun’s¹ opinion that long delay of dilatation of the internal os, even till the end of pregnancy, is a peculiarity in cases of placenta prævia and transverse presentation of the foetus, is, I believe, quite unfounded. He accounts for the peculiarity by mechanical uterine conditions existing in these cases; but, as he does not give sufficient evidence of the truth of what he accounts for, I shall not discuss it.

When the placenta does not overlies the internal os, hæmorrhage will be delayed proportionally to the distance of the placental margin from the internal os. Considerable dilatation may, indeed, occur before hæmorrhage commences, when the placental margin is comparatively distant from the internal os. Such considerable dilatation may be the result of active painful labour, or of silent painless labour. Clinical experience often illustrates these statements. In the last case of placenta prævia which I attended

¹ *Lehrbuch der Geburtshülfe*, S. 625.

(along with Dr. Macfarlane of Polmont), the hæmorrhage began quite unexpectedly, shortly after the completion of the seventh month of pregnancy ; yet, although there was no painful uterine action, separation was caused by painless uterine action, producing dilatation of the lower uterine hemispheroid. ✕ The internal os uteri was as large as a florin ; and it was not till this amount of dilatation of the internal os was reached, that the corresponding amount of dilatation at some distance from the internal os was sufficient to produce detachment. In this case, premature labour was induced, with a successful result to both mother and child. After delivery, the detached marginal segment of placenta was easily recognised by its peculiar ordinary thrombosed condition.¹

The delay of detachment and bleeding, in cases where the margin of the placenta only approaches the internal os but does not reach it, arises from the circumstance, which I have elsewhere accounted for, that a detaching amount of expansion is first produced at the margin of the internal os. This does not arise from the absence of expansion over the whole spontaneously detaching area ; on the contrary, the expansion will simultaneously affect the whole of it. But, at parts remote from the internal os, the whole or final amount of expansion or dilata-

¹ See confirmatory remarks by Jüdel, *Archiv f. Gyn.*, VI. Band, S. 463.

- × tion is slight, and it is only when it has reached its maximum, or near it, that detachment of the placenta is produced. At the internal os, on the other hand, the detaching amount of expansion is reached at a very early stage of its dilatation and of labour, whether silent or painful ; and, of course, long before the whole or final amount of expansion or dilatation is reached.

The same circumstances account for the renewals or aggravations of the hæmorrhage, which observers uniformly describe as occurring while the early part of labour advances, in all cases of extensive attachment over the spontaneously detaching area. As the lower uterine hemispheroid is dilated and opened up, successive zones, arranged concentrically round the internal os, are, according to distance, successively

× brought to a detaching amount of expansion or dilatation till the limit of spontaneous premature detachment is reached. This limit will be, of course, reached when the internal os uteri is fully dilated, and probably sooner ; but the reaching of this limit has no direct relations, so far as we yet know, with the degree of dilatation of the external os uteri. The limit of spontaneous premature placental detachment may be attained before the external os uteri is much dilated.

The view of the causation and progress of unavoidable hæmorrhage here just given is that which many long ago held, as the following passage from

Ingleby¹ shows :—“ Pain (says he) efficacious as it is in the accidental form of hæmorrhage, unless adequate to the expulsion of the child, is neither to be expected nor yet desired, to any material extent, in the unavoidable form, as it only renders the effusion more abundant. For, though a certain degree of relaxation is necessary for delivery, it must be remembered that, in exact ratio as the cervix uteri is successively developed, and the os internum progressively dilated, will an additional mass of placenta be detached from its connecting medium and hæmorrhage necessarily be renewed.” Since the time of Ingleby, and of others equally well informed whom I might quote, much progress of knowledge has been made, many erroneous notions dissipated, and some vague ideas reduced to exactness. It was well known that, in ordinary cases, only the part of the placenta adjacent to the os internum was spontaneously detached in early labour, while that attached higher up retained its uterine connection till the child was born. But some authors, not writing with the care of Ingleby, who speaks of detachment as a consequence of development of the cervix and dilatation of the internal os uteri, appear to wish to describe an unlimited progress of detachment during the whole labour. Even Ingleby is inaccurate and inexact; but he correctly limits the bleeding to the time of the progressive dilatation of the os internum, and he does

¹ *Uterine Hæmorrhage*, p. 140.

not, like Churchill, put no bound to the spontaneous premature detachment. The latter author says:—¹ “The flooding is the necessary consequence of the dilatation of the os uteri, by which the connection between the placenta and uterus is separated, and the more the labour advances the greater the disruption and the more excessive the hæmorrhage.” That there is a natural and early limit to separation and bleeding was well known to Jacquemier,² who writes, regarding the separation and consequent bleeding during pregnancy, as follows:—“Elle peut se manifester lorsque le placenta recouvre l’orifice interne, et lorsqu’il en est seulement plus ou moins rapproché par sa circonférence. Dans le dernier cas, et même dans celui où il recouvre l’orifice par son bord seulement, la perte, après s’être produite une ou plusieurs fois, peut cesser définitivement, et l’accouchement avoir lieu à terme sans qu’elle se reproduise. En effet, lorsque la portion du placenta qui avoisine l’orifice ou le recouvre a été décollée, et que l’extravasation sanguine a cessé, la tendance à de nouveaux décollements partiels peut disparaître, parce que les progrès de l’ampliation de la partie inférieure de l’utérus éloignent très sensiblement le bord du

¹ *Theory and Practice of Midwifery*, p. 465, 3d edition, 1855. In subsequent editions, Churchill has, by the addition of the words “until the head passes through the os uteri,” corrected his former error and made a good statement, which for practical purposes is as near the truth and as useful as any other.

² *Manuel des Accouch.*, 1846, tome ii. p. 238.

placenta le plus exposé à de nouveaux décollements du centre du segment inférieur, qui est la partie la plus distendue mécaniquement, et parceque le tiraillement diminue du bord inférieur du placenta au bord supérieur, qui se trouve assez haut dans la cavité de l'uterus." Barnes, however, has distinguished himself by the care with which he has justly insisted on the natural limit of spontaneous premature separation, and has shown that it is not the case, as Churchill says, that the more the labour advances the greater is the separation; the utmost limit of spontaneous premature separation being reached before the first stage of labour is completed. We do not at present enter upon the question, whether or not the hæmorrhage ceases when the limit of spontaneous premature detachment is reached, or artificially hastened, as Barnes believes. This is quite another matter, to which we hope subsequently to direct attention.

It is well known that the amount of blood lost may be very great, and produce alarmingly sudden death before any placental detachment takes place, and in every variety of placenta prævia; but it is a natural consequence of the laws of spontaneous premature detachment that, as a general rule, the hæmorrhage will be greater in proportion to the centrality of the placental attachment, and in proportion to the largeness of the body to be expelled—in other words, the age and size of the child; and I

believe these statements are confirmed by clinical experience. When the placenta is centrally attached, the hæmorrhage begins with the early part of the dilation of the internal os, and continues as long as in any other kind of previous placental insertion.

When, on the other hand, the placenta is prævia, but does not cover the os internum, nor has its margin close to it, then there may be some dilatation and progress of labour before detaching expansion takes effect or begins. x

When the child is not premature and small, but at the full time and big, then the amount of spontaneous premature separation will reach its utmost, and may be considerably more than if the child were small; for a small child will pass through the internal os uteri and lower uterine hemispheroid when these are much less expanded, than the degree of expansion, and consequent placental separation, required to transmit a big child.¹

Most authors, among whom Legroux² enumerates Duparcque, Desormeaux, Dubois, and Depaul, ascribe to the pains an influence in increasing the hæmorrhage. Legroux, on the other hand, believes the hæmorrhage to be, in unavoidable as also in acci-

¹ "Suivant Puzos, les hémorrhagies sont rarement mortelles avant le cinquième mois de la grossesse. L'expérience confirme tous les jours la vérité de cette assertion." Desormeaux et P. Dubois, *Dict. de Médecine*, article Métorrhagie, 2d edit., tome xix. p. 663.

² *Archives Gén. de Médecine*, 1855, vol. ii. p. 643.

dental cases, not systolic, but diastolic ; not increased by pains, but arrested by them. He has satisfied himself of this by feeling it duly flow and stop in an actual case, and he points out that the increase of discharge simultaneous with the coming on of a pain is the result of the expulsion of blood accumulated during the absence of a pain ; that the increase with the prevalence of a pain is apparent, not real. In these views Barnes¹ coincides. I also agree generally with the view of Legroux ; but, in actual practice, I have not observed increase of bleeding with pains in unavoidable and stoppage in accidental hæmorrhage, with any such marked constant regularity even of appearance or seeming, as would lead me to accede to the diagnostic principle, so generally laid down, of increase with each pain in the flooding of placenta prævia and decrease with each pain in accidental hæmorrhage. The opinion of Legroux is adopted also by Gendrin² Charpentier,³ and by Jüdel,⁴ who has recently made observations on this point.

At the bedside it is very difficult to get these various points verified, in consequence of confusion of blood flowing from vessels with blood flowing which has already been for some time extravascular, that is, in the uterus or vagina. One would, *à priori*, expect the following course, and I believe it to be

¹ *Lectures on Obstetric Operations*, 2d edit., p. 413.

² *Traité Philosophique*, etc., p. 196.

³ *Archives de Tocologie*, Juin 1874, p. 360.

⁴ *Archiv für Gynæk.* vi. Band, S. 464.

consistent with most observations. Hæmorrhage will take place from vessels, in a more or less considerable gush, during and immediately after every act of separation or of increase of separation, but not necessarily with each pain. Such hæmorrhage will be with pains, not between them. It often happens that, while pains go on hæmorrhage stops for a time, and it is hereby explained. But hæmorrhage may go on between the pains, before the limit of premature spontaneous separation is reached, as in cases of accidental hæmorrhage. After the limit of spontaneous premature separation is reached, the hæmorrhage, if any, will be, as in accidental cases, during the absence of pains or between them. In thus describing the hæmorrhage, reference is made only to hæmorrhage from vessels, not to expulsion of blood from the vagina or uterus, which is so apt to obscure the evidence derivable from observations.

The latter part of this statement is clearly implied in the following passage from Gendrin.¹

“Dans les cas où les eaux se sont écoulées peu à peu par une rupture des membranes opérée dans l’utérus, quelle que soit la partie du placenta qui réponde à l’orifice du col, l’hémorrhagie ne se montre plus que dans les intervalles des contractions utérines, et encore dans ces cas est-elle toujours peu considérable, et ne devient-elle véritablement alarmante que lorsque la matrice tombe dans l’inertie.”

¹ *Traité Philosophique, etc.* p. 197.

In hæmorrhage from placenta prævia during labour, as in all cases of flooding during pregnancy or labour, uterine action will constrict the bleeding vessels, and produce diminution of the area fitted to discharge blood ; and although, in cases of unavoidable hæmorrhage, expansion of the bleeding area in one direction must go on during early labour, yet uterine action, by diminishing or contracting it in another direction, must always keep the area liable to bleed less than it would be were the uterine action or pain absent. Expansion without uterine action or pain must enlarge the bleeding area. But expansion goes on under the influence of uterine action or pain. Expansion under the influence of uterine action or pain may possibly increase the area liable to discharge blood, but instead of increasing it, it probably diminishes it. Certainly, the effect of the pain or uterine action must be to make the area liable to bleed less than it otherwise would be. Besides, the statements already made in explanation of cases of placenta prævia, which pass over without considerable bleeding, and the further considerations to be hereafter stated regarding the arrest of the bleeding, all tend to support the view that uterine action or pain does, *per se*, restrain or stop the hæmorrhage instead of increasing it, as most authors allege.

I am not, in this paper, discussing the subject of the arrestment of the hæmorrhage, but that of its

causes. Yet, to avoid misconception, I make a slight digression. I do not say that uterine action or pain stops the hæmorrhage. It may, I believe, have this beneficial effect in some cases. It certainly, always and in every case, does, *per se*, tend or help towards
 X its arrestment, and not indirectly, but directly, by constricting vessels and diminishing the area liable to discharge blood.

Indirectly, in the early part of the first stage of labour, uterine pain causes or increases hæmorrhage by producing separation or increasing its amount, as has already been said. But this increase of hæmorrhage is not the direct result of the pains, nor even a regular consequence of their supervention.

Simpson and Barnes, erroneously believing that the cervix is the placental site, and knowing that this part does not contract during and immediately after labour, as the body of the uterus does, have been led to unnecessary and misleading reflections based on these anatomical points. Simpson¹ calls attention to the rarity of bleeding when the placenta is separated from this uncontracting portion of the uterus. Barnes² describes the cervix as liable to paralysis for a time, and thinks this state is more liable to occur in labour with placenta prævia, and that it is then doubly dangerous. But such reflections on the influence of the placental site on the

¹ *Selected Obstetrical and Gynæcological Works*, p. 237.

X ² *Lectures on Obstetric Operations*, 2d edition, pp. 401 and 421.

hæmorrhage are to be utterly disregarded, because founded on anatomical error. The cervix is never the placental site.

It is desirable, however, to have more knowledge of the condition of the placental site in placenta prævia, after premature spontaneous separation. In general features its condition is, no doubt, closely similar to the placental site after separation anywhere else. But what are its peculiar features?

While the area of the placental site is diminished in the longitudinal or meridional direction, it is much stretched and extended in the direction transverse to the uterine axis. The shrinking in the longitudinal direction is the direct result of muscular action, while the expansion in the transverse direction is passive, or a result of relaxation and an indirect result of the same muscular action. The same change of shape will affect the sinuses and their orifices. From these, blood may be discharged when the uterus is not in action, especially if the blood-pressure is great. It appears to me that the peculiarity of shape of the vascular orifices and the peculiarity of the muscular action in this part, in early labour, in cases of placenta prævia, will have little or no special effect on the liability to hæmorrhage from it. In other words, the longitudinal shrinking, in cases of unavoidable hæmorrhage, will be as efficient in checking hæmorrhage as the corresponding amount of shrinking in every direction which may occur in certain cases of

accidental hæmorrhage, or the longitudinal shrinking, without transverse expansion, which may occur in other cases of accidental hæmorrhage.¹ Many authors, among whom I mention Legroux,² regard the openings of the uterine sinuses in cases of placenta prævia as being made to gape (*béants*) by the peculiarities of the uterine retraction. But this is plainly erroneous; for while the longitudinal contraction will tend to close the vessels by bringing the upper and lower edges of their openings nearer to one another, so also will the transverse expansion, while at the same time it may render somewhat more distant the ends of the transverse diameters of the openings, giving them a more and more elliptical shape.

The more elliptical form, while it involves diminution of area, not increase or gaping, may perhaps involve slight increase of area above what would be the case were there only longitudinal shrinking without transverse expansion. But truly the state of the mere mouths or openings is a matter of little importance. They are big enough, when at their smallest, for the injurious purpose of transmitting a large

¹ The shrinking of the placental site, when it is above the region of spontaneous premature detachment, and below the fundus, must be almost entirely in the longitudinal or meridional direction. In the region of spontaneous premature detachment there is the same shrinking, but it is accompanied by transverse expansion. When the placental insertion is the upper uterine hemispheroid or fundus, the shrinking will be in every direction.

² *Archives Gén. de Médecine*, 1855, tome ii. p. 653.

quantity of maternal blood. The important matter is the alteration of shape of the sinuses, and this is outlined by the shape of the orifices. Now, the whole of this alteration of shape is favourable not to increased discharge of blood, but to its diminution; and there can be little doubt that any possible increase of area by transverse expansion will have much less effect in favouring increased discharge of blood than the change of shape will have in producing diminution of discharge. All this is a matter of hydraulics, and the principle involved may be thus stated. Diminution of area of a tube, with approximation of its walls, such as would be produced by pressure on the tube in one direction, will have very much more effect in diminishing the flow of a passing fluid than the same amount of diminution of area without change of mere shape, or such diminution of area as would be produced by equal pressure in all directions on the outside of the tube.¹ It must be admitted that, at present, it is impossible to apply this principle of mechanics exactly to the case on hand, but it must have important bearings such as are here pointed out.

¹ This seems to follow at once from the fact discovered by Poiseuille, that, under given circumstances, the flow through tubes of circular section is as the fourth power of the radius. This note and the relative text I owe to Professor Tait. See the *Proceedings of the Royal Society of Edinburgh*, vol. viii. 1873-4, p. 208. On this subject see some remarks by M'Donald, *Edinburgh Med. Journal*, May 1874, p. 997.

Farre and Ferguson¹ believe that hæmorrhage may be recommenced by extreme uterine contraction squeezing out from the sinuses plugs of coagulated blood which had been sufficient to restrain it. I know no evidence in favour of this view, and satisfy myself with mentioning it.

The last cause of hæmorrhage, to which I shall refer, generally acts indirectly, but it may be even a direct cause of vascular rupture and hæmorrhage : it is the condition of the abdomen as to retentive power. When this important power is absent or annulled, and still more when the abdomen has an expulsive tendency, as in cases of hernia or of uterine procidentia, bleeding, in cases of placenta prævia, may even be induced, and certainly will be greatly encouraged. Mere plethora will, by loss of blood, soon be—for the time at least—destroyed, and no ulterior increase of hæmorrhage will arise from it. But when the mechanical conditions of the abdominal cavity are such as to favour evacuation, bleeding, in such cases as we are now considering, may receive a persistent and very baneful encouragement. In other words, the increase of blood-pressure due to mere local hyperæmia or general plethora may cause hæmorrhage, but will soon be annulled ; whereas the increase of blood-pressure due to mechanical conditions of the abdomen may persist and favour the

¹ See introduction (p. xlii.) to new Sydenham Society's edition of *Gooch on some of the most important diseases peculiar to Women*.

continuance of bleeding till it is checked by means sufficiently powerful; and this will be as true in cases of accidental and *post-partum* hæmorrhage as in cases of unavoidable hæmorrhage. I may here add, that the beneficial influence of a vaginal plug is sometimes probably quite as much owing to its changing the mechanical conditions of the abdomen as to its mere mechanical pressure exerted directly or indirectly on the bleeding vessels.

It appears to me that absence of retentive power in the abdomen, or the existence of an expulsive force, is the cause of those calamitous departures from the general laws of floodings which result in excessive or fatal losses. When this influence is at work there is great probability that the hæmorrhage will not be arrested by any of the ordinary causes of this fortunate event which operate during labour. The blood-pressure, direct or indirect—direct through the right side of the heart, indirect through impeded returning current—will keep blood flowing, chiefly in the absence of pains, from the open uterine sinuses, and perhaps from the marginal sinus of the placenta.

This cause of bleeding is illustrated in many conditions. One woman may have wound of a labium with trifling hæmorrhage; another, probably having varicosity of veins, produced by the same cause, will die of a slight wound entering such a vein in the same situation. One woman will lose little blood from a large wound of a vein in the leg; another,

even while in the horizontal position, may die of bleeding from a very small opening in the same situation. A woman having an uterine fibroid, with a sinus on its surface opening into the uterine cavity, may have no bleeding, except when the blood-pressure is increased by standing or by local congestion ; so most women will have no bleeding, in cases of placenta prævia, after the completion of spontaneous premature detachment, unless there be an unusual amount of blood-pressure in the uterine region.

The subject of the causation of hæmorrhage in placenta prævia cannot be concluded without reference to the anatomical and physiological peculiarities of the part of the uterus to which the placenta is attached, and to which I have directed attention in a previous chapter on the production of *inverted* uterus.¹

¹ See p. 296 of this book.

CHAPTER XXIV.¹

ON THE SOURCES OF HÆMORRHAGE DURING MISCARRIAGE OR LABOUR AT FULL TERM IN CASES OF PLACENTA PRÆVIA.

WE shall in this chapter consider only the ordinary or regular sources of hæmorrhage in cases of this kind. When the placenta is spontaneously lacerated, as it is in some rare cases, there will be no peculiarity in the hæmorrhage, so far as the mother is concerned, for the intra-placental blood-spaces communicate so freely with one another and with the surface of the placenta, that disruption of its structure can have little extraordinary effect. But disruption of the placenta may, as well-recorded examples show, have a calamitous effect on the foetus, for the foetal vessels may be torn across, and the child may be rendered anemic, or may bleed to death. This accident to the child may lead to an increase of the amount of blood lost.²

At one part of his essay, Legroux³ speaks as if

¹ See *Obst. Journal of Great Britain and Ireland*, Dec. 1873.

² Gendrin (*Traité Philosophique de Médecine Pratique*, tome ii. p. 197) makes much more of this accident than is, I think, generally admitted by obstetricians.

³ *Archives Gén. de Médecine*, tome ii. 1855, pp. 646 and 652.

there might be bleeding from vessels destined only to nourish the placenta. But this need not be further dwelt on, as no special nutritive vessels are known to exist, and Legroux himself does not distinctly state his meaning.

The *first* ordinary source of hæmorrhage is one which, so far as I know, has not been clearly described. It is a gush from the maternal placental cells, or blood-spaces, at the time of separation, or gushes from freshly detached portions of placental surface at each renewed separation or increase of detachment. It is inconceivable that the large utero-placental vessels, having no mechanical arrangement to prevent bleeding, should be divided without a gush of blood pouring chiefly from the placental blood-spaces, but also from the utero-placental and uterine sinuses, thus largely laid open. This gush will not be very copious, and it is generally soon arrested by thrombosis of the placental cells or blood-spaces.

The practitioner and the student, being familiar only with the partially or wholly separated placenta, and finding it a thick solid mass of considerable consistence, will not readily recognise the mechanism of this gush. They should consider the placenta as an attached living organ, performing its functions in a healthy manner. Then, it presents to the observer's fingers a very remarkable and interesting feeling, not such as naturally arises to the mind familiar only

with separated placentæ, more or less collapsed and thrombosed, but just such as would be expected in a bag of its own well-known shape, filled with fluid. It feels as if it had no resisting thickness, so compressible is it and so perfectly soft ; and, when distended with liquid blood, it retains the fluid character, as perceived through the hand ; but its chorionic surface, felt through the amniotic membrane, is fretted over with smooth elevations like the surface of a quilt. When the bag is even only pricked, the fluid contents will at least partly run out, and it will at least partly collapse.

This gush will be all the more liberal on account of the free intercommunication between different parts of the system of maternal cells or blood-caverns of the placenta in the same and in different cotyledons. Although this opinion as to free intercommunication is almost universally entertained¹ by anatomists, it has recently been contested by Sirelius,² who, however, gives no satisfactory evidence in favour of his own view that the maternal blood-spaces of each cotyledon are separated, or do not communicate with those of neighbouring cotyledons.

The *second* source of hæmorrhage is the surface of the separated portion of the placenta, and is identical with the first, but involves a continuation or repeti-

¹ See *Edinburgh Medical Journal*, January 1873, p. 601.

² *Archives Gén. de Médecine*, tome ii. 1861, pp. 298 and 456.

tion of the bleeding—not a mere gush. Simpson,¹ describing the general subject of these hæmorrhages, and not this source alone, says that the blood “proceeds principally, if not entirely, from the maternal vascular cells belonging to the separated portion of the organ being still, more or less freely, supplied through the utero-placental vessels of the adhering portion.” “The blood issues principally, if not entirely, from the uncontracted and uncontractible maternal orifices that belong to the external surface of the separated portion of the organ.”² This source of the hæmorrhage has been believed in by various foreign obstetricians, but it is chiefly in this country that it has found supporters, among whom are Rawlins, Kinder Wood, Hamilton, and Radford. But Barnes³ justly remarks that he does not think this hypothesis of the placental source of the blood is now entertained by any one of authority.

When a portion of placenta is separated, the natural current of maternal blood through it is stopped. Blood indirectly supplied to it from neighbouring portions becomes coagulated in it; the separated portion, and possibly some adjacent portions, become peculiarly thrombosed. In the hæmorrhage of placenta prævia, the blood does not flow incessantly, as it should do from such a supposed placental

¹ *Selected Obstetrical Works*, p. 225.

² *Ibid.* p. 222.

³ *Lectures on Obstetric Operations*, 2d edition, p. 404. On this subject, see the opinion of Turner at p. 318 of this book.

source, a bag full of fluid. When a portion of placenta is detached in placenta prævia, it must collapse more or less. It is inconceivable that the blood should find its way through all the mazes of the blood-spaces of a detached portion. It will certainly coagulate. It is also to be remembered that, while a portion of placenta is separated, there are openings of sinuses on the corresponding portion of uterine wall which will offer free vent to any blood that might otherwise be propelled into the placental blood-spaces of the separated part. Besides, we have no knowledge of a separated portion of placenta, in an actual case, retaining its soft condition of a fluid bag. It is invariably found thrombosed, the blood being coagulated in it. In this condition it cannot be a source of hæmorrhage, as Gendrin¹ and Simpson² themselves point out. To give their view any reasonable place, these authors should have given evidence of the, at least occasional, existence of a separated portion in situ, still soft and discharging blood, not solidified and thrombosed. They do not do so, and I know no author who does.

Were there free bleeding from the maternal surface of the separated portion of the placenta, it is inconceivable that the child should survive the separation, for many minutes, of even a small part of this organ. For there is certainly free inter-communic-

¹ *Traité Philosophique de Médecine Pratique*, tome ii. p. 223.

² *Selected Obstetrical Works*, p. 222.

tion between the blood-spaces of all parts of the placenta, and the blood would, through the separated portion, be so effectually drained from the whole organ as to lead to collapse of it and general thrombosis. We know that the child survives partial separation, and that general thrombosis of the placenta does not occur; and this is explained by the early thrombosis of the separated portion providing a dam to prevent the blood flowing from the cotyledons, which still remain in connection with the uterus. This early thrombosis, which seems necessary for the preservation of the fœtus, is scarcely reconcilable with the admission of the utero-placental vessels in the maternal surface of the placenta as a source of continued bleeding in cases of placenta prævia. Were there no other free vent for the maternal blood, such as is offered by the opened uterine sinuses, it might perhaps be forced through the thrombosed portion of detached placenta to issue at its surface; but the existence of such free vent and other arguments render this course of any part of the maternal blood highly improbable.

The thrombosis of the separated portion is peculiar, for the part is distended with blood and consequently thicker than the rest of the placenta which has been separated in the usual way at the child's birth. This circumstance suggests that either the blood clots in it when the maternal cells are distended, or that, after clotting, more is forced in from

the neighbouring parts to produce the distension. The separated portion has not the appearance of a collapsed portion through which blood had easily flowed, or of a portion which had been compressed.

Legroux¹ believes that the occurrence of hæmorrhage, in cases of partial separation, when the child is already dead, is proof that it cannot in any case be derived from the placenta. He and others hold that, when the child is dead, the maternal circulation through the placenta is always permanently arrested; and that therefore, in such cases, the bleeding cannot occur by this route. But I have already stated² elsewhere that Legroux's major proposition is false. Besides, his evidence if admitted shows only that in some cases there is another sufficient source; it does not show that the placenta is never the source.

Sirelius, again, urges his view, that the blood-spaces in different cotyledons have no intercommunications, as adverse to the admission of the placental source. But his own anatomical doctrine is untenable, and his argument therefore falls.

Holding the placental surface as a source or a chief source of the hæmorrhage during labour in placenta prævia, Gendrin,³ Desormeaux, Dubois,⁴ and Simpson,⁵ advance an ingenious hypothesis to account,

¹ *Archives Gén. de Médecine*, tome ii. 1855, p. 644. ² See p. 360.

³ *Traité Philosophique de Médecine Pratique*, tome ii. p. 274.

⁴ *Dictionnaire de Médecine*, édit. 2, tome xix. p. 660.

⁵ *Selected Obstetrical Works*, p. 225.

partly at least, for the increase of the hæmorrhage during pains. They assert that the separated portion takes the place of the barrel of a syringe, and that, after it is filled in the absence of pains, it is squeezed empty into the vagina again and again by the successive returning uterine contractions. To this hypothesis everything is opposed which is adverse to the placental source generally; and it is quite inconsistent with the thrombosis of the separated portion and its congested condition as felt at all times before delivery and seen after it.

In 1870 Sir James Simpson advanced a new theory¹ of the source of this hæmorrhage—namely, “that the bleeding came from the line of vessels between the placenta and the uterus, which are on the stretch.” I make only a passing reference to this, for besides being at variance with the previously mentioned theory which he elaborately propounded, laboriously defended, and never withdrew, it does not appear susceptible of vindication. There is no line of vessels between the placenta and the uterus. The utero-placental vessels, when divided, have openings on either side of the line of division, which are well known, and which have both been amply considered as the sources of the hæmorrhage. There are no other vessels and no other openings of vessels between the placenta and uterus. Further, the circumstance that vessels are on the stretch is generally and truly

¹ *Selected Obstetrical Works*, p. 295.

regarded as favouring arrest of hæmorrhage, not its occurrence ; unless, indeed, there be something peculiar in the kind of stretching, and nothing peculiar has been demonstrated in the stretching in this situation.

The *third* source of the hæmorrhage during labour in cases of placenta prævia is the so-called circular sinus.¹ This remarkable vessel, if present at those parts of the margin of the placenta which have been last separated, will be laid open in two places—that is, at each extremity of the separated margin, near to where it joins the still attached margin, and from these openings maternal blood may flow freely.

The circular sinus is not a continuous uniform canal encircling the placenta, as it is very erroneously represented to be in drawings in several well-known text books ; it has indeed never been fully described, ^x but the following statements regarding it are easily verified by anatomical observation. It is an interrupted canal, its different parts having no direct or evident intercommunication. At some parts of the placental margin it may be absent. When it is present, it may vary greatly in arrangement and dimensions. It is not on the surface of the placenta, but as it were channelled out in the thick decidua which surrounds it. It has numerous free communications with the adjoining maternal blood-spaces of

¹ For some remarks on this source, see Simpson, *Selected Obstetrical Works*, p. 221.

the placenta. It seldom contains any foetal tufts, although these may occasionally be easily seen through the openings of communication between the
X sinus and the maternal cells or blood-spaces in which the foetal tufts lie.¹

This last circumstance is an important one, for the absence of foetal tufts, which would fill up and block the sinus, if partially emptied and collapsed, or if the circulation through it were otherwise deranged, is probably the reason why this sinus is not thrombosed as are the maternal placental cells. In examining placentæ I have not observed any blood clots in this sinus.

This sinus runs continuously for various lengths, having no regular anatomical distribution in this respect. It may be a means of draining maternal blood from still attached and healthy acting portions of placenta, and conducting it to a point where it is torn at the side of a separated and thrombosed portion. In the separated portions of placenta throm-
X bosis is inevitable because of the impossibility of a properly distributed current of maternal blood through them, this fluid having virtually unlimited openings of quick escape out of them. Indeed, the freeness of escape of maternal blood through this sinus, from still attached portions of placenta close to the de-

¹ For a careful and original description of this sinus, see Turner, in the *Journal of Anatomy and Physiology*, No. xi. Nov. 1872, p. 130, etc.

tached portions, may lead to thrombosis of attached parts adjacent to the detached. But we know that the great mass of the still attached part of placenta does not get thrombosed, and retains its healthy condition, supporting the life of the child. From these healthy attached portions there may be a continuous partial draining into a piece of circular sinus which is at a farther part of its course laid open.

In laying these views before the profession, I have to lament the absence of anatomical proof of their efficiency and existence in actual cases of placenta prævia. I have often carefully observed all that is stated regarding the common anatomy of the so-called circular sinus, but I have not verified in an actual placenta prævia the adaptation of the anatomical conditions to the conveyance of the hæmorrhage. It is only recently that I have been led to entertain the special views here expressed. My opportunities of examining the necessary specimens are not very frequent; and while I shall myself not neglect any in future, I can certainly also count upon the assistance of my professional brethren in the elucidation of this matter by new anatomical inquiries.

The *fourth* source of hæmorrhage in placenta prævia is the uterine sinuses laid open on that part of the uterine surface from which the placenta has been partially or entirely separated. Those obstetricians who hold the corresponding placental surface to be the chief source do not deny altogether that

this may be also one. The great mass of the profession regard this as the chief source, and it is natural that this should be so, for it is the most direct and easily understood; and, besides, it is certainly the source of those great post-partum floodings with which every obstetrician must be familiar. Yet there is difficulty in completing the theory of hæmorrhage from the uterine sinuses, nearly as great indeed as is connected with the other theories of the origin of the flow of lost blood.

The apertures through which the blood runs are numerous according to the amount of placenta separated. But the number is not of great importance, for even one of them is capable of transmitting a large and rapidly fatal current of blood. These openings are distributed over the site of former insertion of the placenta. They are rounded openings of considerable size. As seen in a post-mortem examination, they may have a diameter of a quarter of an inch. But this dimension would be very misleading, were it supposed that the tubes so opening had, in a transverse section, an area whose diameter was a quarter of an inch. These openings are very oblique sections, and it is these oblique sections that have the measurement named. These oblique sections are through tubes whose transverse section has an elongated spindle shape. According to the amount of blood running through them, they are less or more flattened out, as seen in transverse section. It is

certain that, during life, the amount of blood in these vessels varies greatly, and their function of accommodating very various quantities of blood is no doubt of great importance in the economy. It is probable that during life some of them are frequently quite empty. When the placenta is separated, at whatever time, when gentle pressure against the orifices is not closing them, and when, as is mostly the case, no hæmorrhage is flowing, there is no blood running in at least the layer of vessels next the uterine cavity. This layer of sinuses will, from its position, be most affected by pains and diminution of uterine bulk. Were blood passing through them, in the absence of closing pressure, some of it would inevitably flow through the openings just described, and constitute flooding. When no hæmorrhage is flowing, and when the openings of the sinuses are not closed by pressure from the uterine cavity, no blood is permeating the sinuses near the openings.

This source of hæmorrhage requires no further description, as it is well known to all obstetricians. The great difficulty in regard to it lies in the frequent absence of bleeding while the source is available. Why is bleeding ever absent during labour in placenta prævia? Why is increase of this source of bleeding, as in Cohen's, Barnes's, and Simpson's plans of treatment, frequently followed by diminution or arrest of the hæmorrhage? Such and similar questions have as yet received no sufficient answer, and

the difficulty in completing the theory of this universally admitted source of hæmorrhage in placenta prævia remains. As I am not here discussing the mechanism of the arrestment of the hæmorrhage, I evade at present the attempt to answer these questions, or to remove the difficulty. Meantime, I am satisfied to join in the general admission of this source, and conclude by stating my belief that the difficulty referred to is to be solved by a study of the variations of direct blood-pressure within the vascular system, and of indirect blood-pressure through differences in the general abdominal pressure or retentive abdominal power.

Another source of bleeding has to be considered, namely, the curling arteries laid open by separation of the placenta. This view of the derivation of the blood is maintained by F. W. Mackenzie¹ and by Snow Beck.² The bleeding proceeds from the same surface as that last described, but from different vessels, according to these authors. This opinion is not only contrary to that almost universally entertained, but, as it appears to me, not consistent with our knowledge of the course of arterial bleedings generally, or of such bleedings as are now under discus-

¹ *Association Medical Journal*, December 1853.

² *Obstetrical Journal of Great Britain, etc.*, December 1873. In corroboration of Beck's view, see the ingenious reflections of Dr. Angus Macdonald in the *Edinburgh Medical Journal*, May 1874. P. 997.

sion. Among the circumstances which favour the ordinary opinion and oppose that of Mackenzie and Beck, may be mentioned the invariable arrestment by uterine contraction, a condition which would not be equally efficacious in arresting arterial hæmorrhage; the occasional absence of hæmorrhage, when there is deficiency or absence of uterine contraction, a condition scarcely reconcilable with the notion of the hæmorrhage being arterial; the arrestment and recommencement of the hæmorrhage, with considerable intervals, a circumstance not likely to occur in arterial hæmorrhage. But the chief argument for the arterial origin of the blood is its arterial colour. To this it has first to be subjoined that generally the blood lost in uterine hæmorrhage has a colour more nearly that of venous than that of arterial blood, according to my eyes and those of most observers. X
But knowing the difficulty of judging of tints, I have no doubt of the observations of Mackenzie and Beck, and I admit that I have sometimes seen it arterial or nearly so in colour. This, however, has not struck me as evidence conclusive as to its arterial origin; for, not recognising uterine hæmorrhage as a hæmorrhage by retrogression, I traced the blood from the uterine arteries into the sinuses with an extremely brief course; and would, therefore, not expect much change in its colour, as it would run from the arteries through the sinuses, unchanged or nearly so from its arterial state. There are yet other considerations

which may be urged against conclusions from mere colour in the circumstances before us. These I shall give in the form of an extract from a paper by Dr. Lauder Brunton¹ :—“The condition (says he) of the vessels in fainting has not been ascertained, and the only observation bearing on the subject that I can find is one by John Hunter.² While engaged in bleeding a lady she fainted, and during the continuance of the faint he observed that the blood which flowed from the vein, instead of being dark and venous, was of a bright scarlet colour, like that of arterial blood. Now the only condition in which we know this to take place is when the arterioles are greatly dilated, and the blood flows so quickly through them, that there is no time for it to be deprived of oxygen during its passage. This is seen in the submaxillary gland during irritation of the chorda tympani nerve, and it was observed by Mayer, the celebrated propounder of the doctrine of conservation of energy, in persons whom he bled in the tropics, and who had their vessels dilated in consequence of the heat; and it was also noticed by Crawford³ in animals bled during immersion in a warm bath. Mayer’s⁴ explanation of the occasional red colour of

¹ *On the Pathology and Treatment of Shock and Syncope*, p. 13.

² *Works of John Hunter*, edited by Palmer, 1837. Vol. iii. p. 91.

³ *Experiments and Observations on Animal Heat*. 1788. P. 308.

⁴ R. Mayer, *Die Organische Bewegung in ihrem Zusammenhang mit dem Stoffwechsel*. 1845. P. 84.

venous blood is different from the one I have given above. We both agree that the slightness of the alteration it has undergone in its passage from the arteries into the veins is due to the fact that but little oxygen has been taken from it by the tissues as it flowed through the capillaries. Mayer considers that the tissues adapt themselves to the wants of the body, and take little oxygen from the blood when the external air is warm. The oxidation which usually goes on within the body is thus diminished, the production of the heat lessened, and the temperature of the animal prevented from rising too high. This hypothesis, though very plausible, is rendered improbable by the experiments of Bernard,¹ which show that the tissues of animals which have been exposed to a high temperature absorb oxygen (after death at least) much more quickly than usual. I therefore attribute the florid colour of the blood to dilatation of the arteries and capillaries, allowing it to flow so quickly through them, that the tissues have not time to abstract much oxygen, however great their avidity for it may be.”

¹ *Revue Scientifique*, 1871-2, pp. 133 and 182.

CHAPTER XXV.

ON THE MECHANISM OF ARRESTMENT OF HÆMORRHAGE IN CASES OF PLACENTA PRÆVIA.¹

THE restraint or absolute stoppage of hæmorrhage in cases of placenta prævia may be produced in a variety of ways. The flow of blood through tubes of a peculiar shape is a pure mechanical phenomenon, and its arrest is also such. No doubt, in uterine floodings, feebleness of the heart's action, or actual fainting, will have a hæmostatic effect, but such influences have no special bearing on uterine floodings, and do not fall to be considered here. In cases of placenta prævia there is little that is peculiar in the arrestment of this flow, for accidental hæmorrhages are of course subject to, and also brought under, the direct influence of the same causes of arrestment as act on the former class of cases.

Among causes of arrestment have to be considered, —clots in the vessels; clots over the bleeding surfaces; the change of shape of the uterine sinuses; uterine action; local anemia; general anemia; diminution of, or absence of indirect blood-pressure; pressure by

¹ See *Edinburgh Medical Journal*, December 1873.

the entire ovum or by the fœtus ; partial or complete, spontaneous or artificial, separation of the placenta ; evacuation of liquor amnii ; advance of labour.

The coagulation of blood in the maternal cells, or blood-spaces, of the separated portion of placenta, is well known and recognised as the cause of the stoppage of the flow of blood through them from the corresponding cells of the still attached portion. This forms the peculiar thrombosis of a prematurely separated portion of the placenta ; and the numerous fœtal villi entangled in the clotted blood, and having connections with the walls of the maternal cells, give the coagulated blood a fixity and obstructing power which otherwise it would not possess.

In the so-called circular or marginal sinus of the placenta, I have never observed such a clot as would form anything like a complete obstruction to the passage of blood along the course of the vessel. Bleeding through this circular sinus may be arrested by complete separation of the placenta, or by thrombosis of all those portions of placenta from which the opened sinus gets its supplies of maternal blood. This will be sure to happen if these portions are detached ; and it may also happen if they are attached. But bleeding from a portion of circular sinus may be arrested by complete separation of the portion. Its complete separation may no doubt lead to the opening of another portion of circular sinus unconnected with the former ; but it may also leave the line of

x separation at a point where the sinus is absent, or where the new portion is uninjured.

3 In regard to the uterine sinuses, I am not aware of any observations which would lead us to suppose
 X that clots of blood in their course are formed during labour. At their orifices, it is probable that clots are formed during labour, which may enter them, cork-like, for a very short distance. Such cork-like clots may form an obstacle to the flow of blood from them, but I do not believe their resisting force is so great as to be ever of much consideration in the stoppage of bleeding.

In like manner, I regard the formation of a layer of coagulum on the detached surfaces of the uterus and placenta as not altogether unimportant in arresting or helping to arrest hæmorrhage, but also as exerting a very slight force in this way.

x There is a general consent, and I believe justly so, that the divided curling arteries are not a source of ordinary uterine hæmorrhage, whether unavoidable or accidental.¹ It is therefore unnecessary to enter upon the mode of arrestment of bleeding from them.

q When the placenta is partially separated, the uterine sinuses corresponding to the detached portion will naturally be expected to collapse, on account of the failure of the current of blood, from the now unconnected portion of placenta, which formerly kept them more or less distended. When the blood-pressure is considerable, and especially in the absence of

¹ See p. 392 of this book.

uterine action, they may no doubt be kept nearly as replete as formerly, or as the neighbouring sinuses which retain their direct connection with the placenta, and in this case there will be hæmorrhage from them. This absence or imperfection of collapse may be to any degree, and the bleeding in proportion. But in most cases the collapse appears to be sufficient, as mere diminution of area, to completely or nearly completely arrest the passage of blood. Now, the sections of uterine sinuses are always elongated, spindle-shaped openings, whose increasing collapse will bring their two opposite sides nearer and nearer together, and in this respect diminish their area. The area of a section of a uterine sinus, in early labour in a case of placenta prævia, will be liable to this collapse, and it is not probable that the expansion of uterine wall will materially increase it by elongation. In any case, it may be regarded as certain, as we have elsewhere shown, that this change of shape, even supposing the area to be somewhat increased by elongation the result of uterine expansion, will have a very powerful influence in retarding or stopping the flow of blood through sinuses so collapsed or compressed. The less the amount of blood flowing through the sinuses, the more will they collapse. The more the uterine sinuses collapse, the greater resistance do they offer, by retarding friction, to the passage of blood through them.¹

It is in this way—namely, by collapse and change

¹ See p. 374 of this book.

✓ of shape—that the bleeding is arrested in cases of placenta prævia, while uterine action is absent and whether labour is going on or not. It is inconceivable that hæmorrhage should not go on if the sinuses remained as full as they were before the separation of the placenta. Did they remain thus filled, blood would certainly run from them freely. The uterine
× sinuses form an elephantine reticulation, the whole of which may be filled, without regurgitation or reflux of blood, from any neighbouring sinus. The blood-pressure is, in cases of arrest of hæmorrhage, so slight in those sinuses connected with the placenta, through which blood is necessarily flowing, as not to force it into the sinuses corresponding to the separated portion, the change of shape in which makes such entrance of blood more difficult than it otherwise would be. In the same way is the arrest of bleeding accounted for, in cases of successful resort to Simpson's or Barnes's method of treatment, by total or partial separation of the placenta; for in such
× cases there is not necessarily uterine action, but only detachment and stoppage of the ordinary course of the circulation of the whole or of a portion of placenta in this region. The arrest of the bleeding in all cases of separation of the placenta, when the uterus is not in action, is to be explained in the same manner; among such are to be included the cases of which Simpson¹ has given an interesting account, where, in

¹ *Selected Obstetrical Works*, p. 229.

twin-pregnancy, the placenta of a born twin was separated and expelled before the birth of the other, where the placenta of a still unborn twin was separated and expelled before the placenta of an already born twin, and where both placentæ were expelled after the birth of a first of twins. It is common, in order to account for this kind of stoppage of bleeding, to refer it to the formation of clots in the uterine sinuses ; but it is plain that such clots will not be formed until the bleeding has already stopped. ×

Uterine action will increase the change of shape favourable to the arrestment of bleeding, by compressing the uterine wall and its contained vessels between its own contracting tissue and the separated portion of placenta or unruptured sac of the ovum, or the adjacent foetal part. We do not say that such compression will stop the hæmorrhage flowing from the uterine sinuses, but it may do so, and it will certainly help towards this end.

Mere uterine action must have a direct hæmostatic effect, for it will make the bleeding area less than it otherwise would be, and it will so alter the area and shape of each open uterine sinus as partially or completely to arrest the passage of blood through it. Pains, or mere uterine action, have been very generally supposed to be a cause of hæmorrhage, but it is plain that they cannot be so directly, as I have elsewhere¹ attempted to show. ×

¹ See p. 369.

In cases of placenta prævia, there are frequently only moderate or even small gushes of blood, both before and after labour commences. It is both natural and proper to regard these bleedings as being the result of emptying of replete vessels, and as stopped by the production of local anæmia.¹

In great bleedings, general anæmia will be rapidly produced; and such general anæmia, being accompanied by great diminution of blood-pressure, will certainly lead to diminution or even to arrest of flooding. When a woman has already lost large quantities of blood, there is now comparatively little to lose; and in such an exsanguine patient, a little, in mere measured quantity, may be equivalent, in vital importance, to very much while the original stock of blood was as yet entire, or drained away only to a slight extent. This circumstance, which endows the epithets, "little," or "slight," or "inconsiderable," with two meanings, one referring to measured quantity, the other to vital importance, renders the use of such terms a cause of ambiguity and of error. This mistake is well exemplified in authors who are over-zealously supporting any plan of treatment. Thus, we find them describing the results of their treatment as being the reduction of the bleeding to a

¹ On this point, see Jüdel, *Archiv für Gyn.* VI. Band, S. 462; see also some remarks on the influence of anæmia in arresting hæmorrhage from a uterine fibroid in *Edinburgh Medical Journal*, January 1867, p. 637.

now "slight" or "inconsiderable" amount, in complete forgetfulness that the patients' previous losses and previous danger were perhaps compatible only with such anæmia as rendered losses large in measured amount now impossible; and also rendered losses, small in measured amount, quite as great in vital importance as the grand gushes in the earlier history of the cases. In Simpson's well-known and laboriously-collected statistics of separation of the placenta before the birth of the child, we find this great mistake of confusing the two meanings of "slight" or "inconsiderable," and a consequent loss of value in the conclusions drawn from them.

Already, elsewhere,¹ I have directed attention to the influence of indirect blood-pressure, the result of absence of retentive power or adspiratory or sucking-in force in the abdomen, or the result of presence of a positive expulsive force in this cavity, as causing or favouring uterine hæmorrhage. Here I have only now to refer to the influence of a strong retentive power or adspiratory force in helping to stop or restrain uterine hæmorrhage. This force cannot prevent hæmorrhage while the placenta is being detached; but other hæmorrhage in connection with placenta prævia, it may not only restrain or stop, but it may more truly and frequently be described as preventing. This subject² has been much discussed,

¹ See p. 376 of this book.

² See the Author's *Researches in Obstetrics*, p. 409.

especially by German obstetricians, and its laws have been made the subject of many special inquiries by Schatz.¹ It is a very difficult and complicated matter, but already its practical importance has been proved in many different ways; and it may be expected that further research will discover the means by which this retentive or adspiratory power is to be augmented with a view to favouring the arrest of uterine bleeding.

Many authors, among whom are Gendrin,² Ramsbotham,³ Campbell,⁴ and Radford,⁵ regard pressure by the child's head as, when duly applied, a means of stopping the flooding in cases of labour complicated with placenta prævia, and there can be no doubt that this good result will follow such application of pressure. At the same time there can be as little doubt that these authors were very far mistaken in ascribing to such pressure, in actual cases, very much more influence than it has or can have. Such pressure, if required to stop the bleeding, can scarcely be efficient, except when pains are present, and hæmorrhage may go on in their absence. Such pressure cannot prevent the spontaneous premature detachment of the placenta, which is the real cause of hæmorrhage. Such pressure of a presenting head

¹ *Archiv für Gynækologie*, 1872, etc.

² *Traité Philosophique de Médecine Pratique*, tome ii. p. 275.

³ *Observations on Midwifery*, part ii. p. 191.

⁴ *System of Midwifery*, p. 369.

⁵ *Provincial Medical and Surgical Journal*, 22d January 1845.

can act only during the early part of the first stage of labour, while spontaneous premature detachment is going on, and can therefore have no influence on the hæmorrhage, which may persist after that time is past. Simpson¹ has ingeniously shown, so far as his statistics are of value, that great or considerable "hæmorrhage, after the expulsion or detachment of the placenta, is not more liable to occur when the retained foetus afterwards presents by an upper or lower extremity, than when it comes down with the head or breech upon the exposed surface of the cervix uteri." Although these last words indicate an erroneous view of the source of the bleeding, yet the statistical conclusion is of interest and value. At the time when the head or breech is pressing upon the exposed surface of the cervix uteri, the real site of placental attachment may be pulled high up above such pressure. Barnes² also speaks of pressure on the vessels of the cervix, and refers to Legroux,³ who regards the erect position as having a certain influence in increasing this pressure by bringing the weight of the child to bear upon the bleeding surface. But these authors are very far wrong, forgetting the demonstrable fact, that the erect position will take the weight of the child off the supposed bleed-^xing part, instead of bringing it to bear upon it. Had

¹ *Selected Obstetrical Works*, p. 217.

² *Lectures on Obstetric Operations*, 2d edition, p. 420.

³ *Archives Générales de Médecine*, tome ii., 1855, p. 656.

the erect position the effect erroneously imagined, it might be of use ; but, so far as I can see, it must have in fact baneful results, and such only.

It is impossible to contemplate the sources of bleeding in placenta prævia without recognising the beneficial influence of pressure, however exerted, on the bleeding parts. Such pressure may, no doubt, be exerted slightly during the intervals of pains, more strongly during their continuance ; and the medium of pressure may be the separated portion of placenta, the bag of waters, or some part of the child. Authors greatly mistake in regarding the cervix as the part to be pressed. That is not the bleeding part. They also mistake in regarding the
x lowest or presenting part of the child as the part pressing. The part of the ovum pressing, or that may press, the bleeding surface, is the presenting part of the child only in very early labour, and then, in most cases, only after rupture of the bag of membranes. If bleeding lasts, which it generally does not, after the first stage is well advanced, then the part of the child pressing, or that may press, on its source, is high above the presenting part, the height varying according to the pulling up of the internal os uteri, and the pushing down of the ovum. Both placenta and placental site retreat, as Gendrin very well knew, with the advance of the first stage of labour.

In my own practice, and in the records of experi-

ence in placenta prævia, I have not been able to make sure, at any particular time in any case, that pressure, of the kind we have just been discussing, was the direct cause of diminution or arrest of hæmorrhage ; yet, as I have already said, when exerted, it must have a beneficial influence. ✓

Spontaneous complete separation of the placenta when it is prævia is frequently accompanied or followed by arrest of hæmorrhage, frequently by diminution of its amount. The value of diminution in amount of hæmorrhage it is impossible to estimate nicely in consequence of our ignorance of the degree of the already existing and recently produced general anæmia, and in consequence of our at present possessing no good means of measuring this degree in any actual case. But the complete arrest or stoppage of the bleeding is a distinct statement, and one that may be verified. Now, the frequency of arrest of the bleeding by spontaneous separation of the placenta has been very much exaggerated by the enthusiastic Simpson. Nothing can be more unsatisfactory than the character of the statistical data on which he founds his opinion, that "in one only out of every twenty-two labours does there appear to have been a continuation of hæmorrhage to a great or profuse degree after the placenta was detached," and the statement is, in my opinion, of little value. If, however, we analyse his table of statistical data, as described by himself, we find only forty-seven cases

fitted to throw light on the question of the arrest of hæmorrhage by complete separation of the placenta. These forty-seven were cases in which an interval, varying from ten hours to ten minutes, elapsed between the expulsion of the placenta and the birth of the child. In these forty-seven there was opportunity to judge whether bleeding was stopped or not. In the rest of his cases, as statistically arranged, the interval between the separation of the placenta and the birth of the child was either less than ten minutes, or it was not known ; and in the former of these two x last categories there was not a sufficient lapse of time to allow of secure observation of the effect of the separation. Now, in the forty-seven cases, where there was a sufficient interval for this observation, I find that in twenty-five cases only, or in little more than one in two, was the bleeding arrested. In all the rest it appears to have continued in a greater or less degree. But this numerical statement is also of little value, in consequence of the unsatisfactory character of the data in every respect.

In cases in which complete spontaneous separation of the placenta takes place, and in which the bleeding is arrested, it has to be asked, Is the stoppage of the bleeding the consequence of the complete separation of the placenta ? I can see no reason whatever for thinking so. Were the bleeding through the placenta, having for its source the open utero-placental vessels in the placental surface, then the

bleeding should always be arrested by complete separation. We have just seen that this is far from being the case, for we find from Simpson's great table, that it continued in nearly one-half of the cases in which spontaneous separation took place. But in addition to this, we must recall the general belief of obstetric pathologists, which we have endeavoured to confirm, that the bleeding in placenta prævia is not through the placenta. The bleeding, which, in nearly one-half of the cases, continued after complete separation, must have come from the uterine site of the detached placenta. Bleeding from the so-called circular sinus is of course arrested by complete separation; but bleeding in nearly one-half of the cases persisted after the separation, and the stoppage of the placental source did not stop the bleeding, which flowed on in greater or less quantity from its chief origin, the open vessels on the placental site.

But, if the frequent arrest of the bleeding after complete separation is not accounted for by the detachment of the placenta, how is it explained? Now, it appears to me to be so natural a result of the circumstances as scarcely to require explanation, and the causes of it have already been, or will hereafter be, fully considered. Meantime, I may briefly state those not already discussed. Spontaneous separation of the placenta is indisputable evidence of energy of labour and of great progress of the first stage. Now,

it is well known that these two conditions do in any case generally arrest the flooding, and Barnes has contributed greatly to the explanation of why it should be so. It is only in certain cases, chiefly of central or nearly central attachment, that the placenta is spontaneously prematurely separated entirely. In the majority of such cases, if the labour goes on with energy and rapidity, no further hæmorrhage of importance is to be expected, whether the placenta be completely detached or not. In short, complete spontaneous separation of the placenta indicates such characters in a labour as would lead us to expect early arrest of the bleeding, apart entirely from the remarkable circumstance of the complete separation.

In itself, complete separation of the placenta must have an injurious tendency, if there is any truth in the views universally entertained as to the source of the hæmorrhage, for it increases the bleeding area to the greatest possible extent. The evil effects to be expected from it are, no doubt, to a great extent averted by the characters of the labour which produce it. If in any case there were a great tendency to uterine hæmorrhage, direct or indirect blood-pressure being great, nothing but strong and rapid labour could save the woman from the evil effects of extension of the bleeding area. Fortunately, in most cases of placenta prævia there is no great mechanical tendency to uterine hæmorrhage,

for contraction of the uterus cannot be relied on to check it so long as the child is unborn ; and some authors have erroneously supposed that it can.¹

Artificial premature detachment of the whole placenta I shall not here discuss, because, with some change, most of the remarks made on the spontaneous premature total detachment apply to it as well. Besides, I have no good collection of data whence to derive a satisfactory basis for remarks. At the same time I add, that I have no doubt that artificial premature separation is frequently followed by arrestment of the bleeding ; for, as already said, most cases of placenta prævia have little tendency to bleed after the unavoidable amount of separation is effected.

Artificial partial detachment of the placenta is resorted to in the treatments of Cohen and of Barnes, and in both, apparently, with considerable effect in arresting or diminishing the hæmorrhage. The method of Barnes acts very much as complete separation does, and is liable to nearly the same criticism. In it, as in complete artificial separation, the same amount of benefit cannot be expected as follows when the partial and complete separation are spontaneous, and for the same reasons. Spontaneous partial separation shows energy and progress of labour, which are not ensured by artificial partial separation. Artificial complete separation has the advantage over partial separation, whether spontaneous or artificial,

¹ See Barnes's *Lectures on Obstetric Operations*, 2d edit., p. 404.

that the possible bleeding during partial detachment, from a utero-placental sinus, is rendered impossible. But, apart from mere therapeutical considerations, which I am not here considering, the method of Barnes has the advantage, in theory at least, of not extending the bleeding area beyond what it must necessarily reach as labour advances. The method of Barnes stops, or rather unites into one, all the gushes from the renewed partial separations as labour advances, and only those gushes are so united which must necessarily take place as labour advances ; whereas the complete separation, even if successful, will have, during its artificial production, a greater amount of gushes united than were inevitable or possible. The method of Barnes does for a time, or prematurely, increase the bleeding area above what it would otherwise be ; not, in theory at least, above what it would ultimately be as the first stage of labour progressed. In this respect it is far less injurious than the method of complete separation ; and it is possible that even this evil may be counterbalanced by the advantage of having united in one all the bleedings consequent on renewed partial separations ; for such single gush will probably be less in amount than the united gushes of repeated separations of parts, which, taken together, constitute an amount of separation equal to that represented by such single gush. Against this possible advantage there has, however, to be placed the possible great disadvantage of extending

the bleeding area earlier than is necessary in those cases in which there is a great, or even a slight, tendency to persistent bleeding. Most cases have, no doubt, no such tendency when the partial separation of Barnes is completed ; but there is reason to believe that the cases requiring greatest consideration and energy in treatment are those in which the partial separation, according to Barnes's plan, has not the usual or desired effect. For, as this author truly remarks, though with a very different application, "there is no independent virtue in the mere detachment of the placenta, as post-partum hæmorrhage abundantly proves."

The method of Simpson and that of Barnes frequently conduct to arrest of bleeding, but far from invariably, as both authors admit. Their methods do not nearly always stop the bleeding. They both prematurely extend the area fitted to discharge blood.

This is not the place for trying to estimate their value as therapeutic measures, but we may quote Barnes's statement that, "when the dilatation of the cervix has reached the stage at which the head can pass, and all that part of the placenta which had been originally adherent within the cervical zone is detached ; and if, as is the constant tendency of nature to effect, the intermitting active uterine contractions arrest the hæmorrhage, a stage is reached when the labour is freed from all prævial placental complication ; the lateral portion of placenta retains its connection, supporting the child's life ; the labour is,

in all respects, a natural labour.”¹ This allegation as to labour being, in such circumstances, in all respects natural, is a misleading rhetorical trope, not justifiable in a scientific work, but which the generous enthusiasm of the author may be allowed to excuse.

It is a great mistake to suppose that “the one constant condition” of arrest of bleeding, in any circumstances, “is contraction, active or tonic, of the muscular structure of the uterus.”² Very many cases, indeed, of arrest of bleeding occur quite independently of uterine action. Were it not so, there could be little advantage expected from artificial separation of the placenta, whether complete or partial; for these means do not necessarily imply uterine action. But we have already shown that uterine action is always a restrainer of hæmorrhage, if not a stopper.

Although it has no reference to placenta prævia, yet the following passage from Dr. R. Ferguson illustrates the argument of the previous paragraph, and we therefore quote it. In doing so we do not mean to admit that plugging of the veins by coagula is the only or even the chief means of arrest of hæmorrhage in the cases referred to. We merely wish to illustrate absence or arrest of hæmorrhage without action or tonic contraction of the muscular structure of the uterus:—“The latter (plugging of the veins by

¹ Barnes's *Lectures on Obstetric Operations*, 2d edition, p. 408.

² *Ibid.* p. 421.

coagula) appears to be the sole means of safety," says Ferguson, "in those cases of intense flooding, in which the uterus flaps about the hand like a wet towel. Incapable of contraction for hours, yet ceasing to ooze out a drop of blood, there is nothing apparently between life and death but a few soft coagula plugging up the sinuses."¹

Uterine action during labour has two direct results: first, the ordinary intermitting efforts, which perform the chief part of the mechanical work of labour, and whose influence in arresting hæmorrhage has already been discussed; second, the gradual progressive decrease of capacity of the organ, progressing from its state of repletion, when labour begins, till its state of emptiness, when complete delivery is effected.

Now, the whole of this second progress does not imply persistent active or tonic contraction of the uterus, but it necessarily involves diminution of the area of its surface, and will be, from first to last, favourable to the diminution or arrestment of bleeding. It cannot advance without diminution of the bleeding area below what it would be were this progressive shrinking absent.

The evacuation of the liquor amnii, whether spontaneous or artificial, is a great means of contributing to this progressive uterine shrinking, and this is the

¹ See page xlii. of Introduction to the New Sydenham Society's edition of Gooch *On some of the most important diseases peculiar to Women.*

x chief part of the explanation of its hæmostatic influence ; but it may also contribute to hæmostasis indirectly by increasing active uterine action or pains. The evacuation of liquor amnii, then, and the advance of labour, both offer ever-increasing security against the persistence or recurrence of hæmorrhage, and the influence of both is easily explained and understood.

This is so well stated by Gendrin, that we quote his words,¹ although we do not altogether agree with his remarks on uterine inertia. “ Dans les cas où les eaux se sont écoulées peu à peu par une rupture des membranes opérée dans l’utérus, quelle que soit la partie du placenta qui réponde à l’orifice du col, l’hémorrhagie ne se montre plus que dans les intervalles des contractions utérines, et encore dans ces cas est elle toujours peu considérable, et ne devient-elle véritablement alarmante que lorsque la matrice tombe dans l’inertie. Si les contractions sont efficaces et si l’enfant s’engage, l’hémorrhagie se suspend définitivement. Nous avons constaté cette suspension de l’hémorrhagie dans tous les cas de cette espèce dont nous avons été témoin ; Rigby l’avait déjà remarquée ; elle est si efficacement produite par l’accomplissement réel du travail expulsif du fœtus, que nous venons de l’observer dans un cas où le placenta était inséré par son centre sur l’orifice du col, et a été

¹ *Traité Philosophique de Médecine Pratique*, tome ii. pp. 197 and 347.

entraîné comme une calotte sur la tête de l'enfant, et cependant le placenta avait été déchiré profondément et en divers sens, en s'engageant dans le col sous l'influence de la pression qu'exerçait sur lui postérieurement la tête du fœtus.

“ La suppression de l'hémorrhagie utéro-placentaire par implantation du placenta sur le col de l'utérus, arrive toujours dans les cas d'implantation par un segment du placenta dès que les contractions utérines expulsives de l'accouchement s'établissent. Dans les cas d'implantation centrale, l'hémorrhagie, augmentée par les premières contractions, cesse dès que le travail d'expulsion a déterminé l'évacuation des eaux de l'amnios ; l'inertie seule de l'utérus peut la faire recommencer. Nous sommes étonné que la plupart des auteurs qui ont traité de l'art des accouchements n'aient pas fait cette remarque, et surtout que quelques uns aient été jusqu'à regarder cette suppression comme un fait rare dans ces cas. Si le sang jaillit du col de l'utérus à chaque douleur utérine, ce n'est que pendant la première période de l'accouchement, et jusqu'à ce que les contractions utérines soient franchement établies pour les cas d'implantation latérale du placenta ; ce n'est que dans les cas d'implantation centrale que cette augmentation de l'hémorrhagie continue jusqu'à l'évacuation des eaux de l'amnios. Les choses se sont ainsi passées dans tous les cas d'hémorrhagie utéro-placentaire dont nous avons été témoin. Nous regardons

done comme une circonstance toujours heureuse l'établissement du travail de la parturition dans les cas d'hémorrhagie utéro-placentaire par implantation anormale du placenta. Nous en déduisons comme conséquence l'indication de déterminer artificiellement l'accouchement dès que ces hémorrhagies, par leur abondance ou leurs fréquentes répétitions, menacent la vie de la femme."

At last, in cases of placenta prævia, as also in cases of accidental hæmorrhage, the stage of complete delivery is reached. All danger of hæmorrhage necessarily connected with the premature separation of the placenta is passed. If hæmorrhage occurs now from the placental site, it is post-partum hæmorrhage. If the uterus is firmly contracted, x hæmorrhage from the placental site does not occur.

But eminent obstetricians have tried to unsettle the professional faith in a contracted uterus as an efficient hæmostatic after delivery. It is the sagacious Gooch¹ who is responsible for this belief, that flooding may come in the usual way from a uterus well contracted after delivery. Gooch founds his belief on a case whose history he gives; and, remarkable to tell, this case presents nothing at all unusual. It is just a case of ordinary flooding post-partum, such as every accoucheur is familiar with.

¹ See his essay on *A Peculiar Form of Hæmorrhage from the Uterus*, in his collected *Essays on some of the most Important Diseases peculiar to Women*.

After delivery, the uterus became well contracted, but there was no flooding from it then. Subsequently, it became relaxed, and flooding began, as Robertson¹ has pointed out; and that it was so relaxed is proved by the fact that Gooch put his hand into it, in order to induce contraction and stop the hæmorrhage!!! Gooch had, in fact, no ground whatever for unsettling the general faith of the profession in a contracted uterus after complete delivery. In my opinion, this article of medical faith is un-assailable **

But Gooch has had many followers, among whom are Velpeau,² Rigby,³ Ingleby,⁴ Michælis,⁵ Ferguson,⁶ and Farre;⁷ and against whom Boivin,⁸ Robertson, and Barnes,⁹ have given evidence.

In order to substantiate the belief of Gooch, it will be necessary to have cases carefully observed by a competent person who is well aware of the various sources of error in judgment. I know of no such cases.

It has been adduced as telling in favour of

¹ *Physiology and Diseases of Women*, p. 362.

² *Traité complet de l'Art des Accouch.* Ed. Bruxelles, 1835, p. 489.

³ *System of Midwifery*, p. 218.

⁴ *A Practical Treatise on Uterine Hæmorrhage*, p. 227.

⁵ See Rigby, *loc. cit.*

⁶ Introduction to the New Sydenham Society's edition of Gooch *On some of the most Important Diseases peculiar to Women*, p. xli.

⁷ See Ferguson, *loc. cit.*

⁸ See Velpeau, *loc. cit.*

⁹ *Lectures on Obstetric Operations*, 2d edit., p. 455.

Gooch's view, that copious hæmorrhage may take place from an unimpregnated uterus. But such hæmorrhage is not post-partum flooding. It is not from the same source. Besides, we cannot tell whether in such cases the organ was in a state of firm contraction or not. Similar remarks may be made in answer to any defence of Gooch's doctrine, founded on the frequently fatal hæmorrhage of uterine fibroids.

The proposition I enunciate is, that it has never been shown that uterine hæmorrhage, of the ordinary post-partum kind, ever takes place from the uterus when it is in a state of moderately firm general contraction.

Among the sources of error are the following :—

1. Variations in bulk of the empty and contracted uterus. If an observer has a preconceived idea of the bulk of the empty uterus taken from the ordinary run of cases, he may, in an individual case, if judging from size, be greatly deceived. For some uteri are extremely small (and were, no doubt, proportionally weak) after delivery, scarcely larger than the fist; and such a uterus, if expanded to an ordinary size, would certainly be in a state of relaxation. Other uteri are, after delivery, and when in strong contraction, very large when compared with the ordinary size (and were, no doubt, proportionally strong).

2. Variations in position of the uterus after

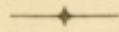
delivery. If the uterus has sunk into the true pelvis, it may appear small and contracted when it is merely collapsed and depressed in the abdomen. If the uterus is high in the abdomen, which is often a consequence of repletion of the bladder, it may appear to be larger than it really is.

3. A uterus may be hard and feel as if contracted, when the hardness is merely the result of passive tension. This is often seen in cases of the kind called hour-glass contraction. On this subject I have elsewhere remarked as follows :¹—“ In their descriptions of, and reasonings upon, cases of hour-glass contraction, of inversion, and of hæmorrhage, many authors have erred in taking a hard part of the uterus for a part necessarily in a state of action. Now, while hardness of uterine wall is in most cases a good and sufficient sign of muscular action in the hardened part, it is not invariably so. For, in cases of hour-glass contraction and of inversion, the hardness may arise from passive tension. The bladder of urine and ovarian cysts frequently illustrate hardness of flaccid bags produced by passive distension. In like manner, a paralysed portion of uterus may be hard from being tightly replete with placenta or other contents retained by stricture ; and the fundus of an inverted uterus may be hardened by the circumstances of its inversion, apart from muscular action of the hard part.”

¹ See p. 297 of this book.

4. Great and even fatal hæmorrhage may be erroneously supposed to be ordinary post-partum flooding, when it is really running from an unsuspected source. I have seen an artery in the
× perineum bleeding freely twelve hours after delivery. Among such unsuspected sources are injuries of the
× cervix, of the vagina, of the perineum, of the nymphæ, aneurismal bursting, so-called varicose hæmorrhage, uterine hæmatocele, etc.

A P P E N D I X.



ADDRESS IN OBSTETRIC MEDICINE.¹

PROGRESS OF MIDWIFERY.

AT one time, and that so late as last century or early in this, the whole field of medicine could be with comparative ease occupied by one mind. The same man practised and also taught all its branches, medicine, surgery, and their subdivisions. If one man proposed to do this now, we should look on him as a foolish pretender. The great medical genius of the scientific Helmholtz already has narrow limits which it cannot overpass; the great practical talents of Thomas Keith and of Spencer Wells are restricted in their beneficence by close boundaries.

Rarely and at long intervals in the course of the ages a powerful mind is produced, such as that of a Newton in physics, who, using the scattered knowledge accumulated by his predecessors, succeeds by his own discoveries and generalisations in restoring simplicity where before there was complexity, and in making for his successors a sort of royal road to the science of his day. But such a master mind, while on the one hand it simplifies and builds by generalisation, does, on the other hand, increase complexity and number of incoherent details by stimulating progressive inquiry. We hope we shall never again see the

¹ Delivered at the Norwich meeting of the British Medical Association in February 1874.

return of those dark days when one man could dare to profess or to practise the whole science and art of medicine.

It is by division of labour that we secure growth, and it is vain to regret the feebleness of our powers. As each department of medicine grows, it is, like its parent stem, doomed to subdivision. He, however, who brings the widest attainments and the most varied talents to bear on his chosen branch, will, *cæteris paribus*, be the greatest in his department. But who is equal to the demands of any department? To tell the truth, there is not among us a single man who has mastered all the details and work of even one single individual topic. One finds that the histology of his branch fully occupies his powers; another is similarly placed in regard to its chemistry; a third finds full occupation in its physical relations; a fourth, gleaning behind all these, finds his hands full in trying to be practically useful to his patients.

IMPORTANCE OF METHOD.

Partly as a result of the subdivision of labour, greater progress is being made now than at any previous period in the history of medicine; and there is no greater exhilarant and stimulant of enthusiasm than that very progress which is the result of well-judged zeal. In the field there is work for minds of every kind, for talents of every description: for him who truly observes and accurately notes details, for him who arranges or classifies, for him who experiments, for him who suggests and demonstrates conclusions, for him who confirms or overthrows by the process of verification. Every one of us should take some part in this work of advancing medicine, a more important part than that humble and useful one in which all should,

in addition, take a share, the attentive listening to and applauding the diligent and the successful.

There can be no doubt as to what is the paramount cause of our more rapid progress in recent times. It is the adoption of proper method ; it is the modern attempt to found or still further build up a science of medicine on what is known as the Baconian plan. Imagination and ratiocination are not even now excluded from the operations, but nothing is held as proved by mere reasoning ; and when the philosopher strays far from the solid shore of rocky facts he loses his intelligent audience, who have no interest in his occupation of beating the air. The medical man proceeds nearly in the same manner as the physical philosopher. He observes by the naked eye, then by the microscope, and this instrument he aids by numerous experimental plans of hardening, cutting, tinting, and staining. He applies so far as possible physical laws to explain natural and morbid processes. He institutes new experiments to elicit truth. He makes the same appeals to chemical science, and to the practical chemist, as he makes to physics and the physicist. He reasons on what he obtains, and tests the truth of his conclusions, verifying by further observations and further crucial experiments.

Every one who knows the history of midwifery must admit that it is by these methods alone that progress has been made. The greater part of what has been attempted, and the greater part of what is being attempted, is not according to this method ; and the labour is vain. The substantial progress, often seen at the time only by a few, and appreciated by a small circle, is achieved by scientific method, and is like the grain of mustard-seed which grows and grows till it is a great tree. Obstetricians can justly

boast that, in some departments, mere obstetrical knowledge is ripening into a science of obstetrics, a boast which happily some other subdivisional branches of medicine can truly make.

IMPORTANCE OF MECHANISM.

The first subject to which scientific obstetricians, as a matter of course, direct themselves, is natural labour in all its varied aspects ; and it is in some parts of this that the greatest advances have been made. The great idea of "Mechanism" so loudly proclaimed by Levret is in process of being worked out. The first stages have been nearly completed by the writings of W. Hunter and Smellie, of Ould, Solayres, Saxtorph, and Nægele. But, now that their inquiries are completed, we are landed in a more difficult and higher stage of this investigation, a stage demanding much more than the good powers of observation and description which sufficed for the former. Standing side by side with contemporary workers at these outposts of science, we are not good critics of the comparative value of their works ; but among them we may signalise names familiar to you all—Hecker, Kehrer, Schroeder, Poppel, Haughton, Hodge, Leishman, Schatz, Küneke, Schultze, and many others.

The majority of practitioners, however, take far more interest in the management of morbid labour than in any other department of obstetrics ; and this for the evident good practical reason that there lie the questions of the highest, immediate, most pressing urgency—their difficult cases and how to manage them—questions which, in the interests of the poor patients and of the anxious practitioners, brook no delay ; which cannot wait for the slow decisions of science, however valuable these may be when

at last they do come. But, even though science is not at hand to offer the solution of difficulties in practice, it is the duty of every obstetrician, with a view to the advantage of his confiding patients, to watch the progress of science, if not to contribute to it, as well as to select more or less empirically among modes of treatment. He should at once contribute to build up science, and at the same time be ready to meet in the field of practice the great difficulties that suddenly come in his way. Like the Jews of old, he should work with one of his hands in the permanent work, while with the other hand he holds a weapon wherewith to fight against Sanballat and Tobiah. He who directs his professional life after this manner will certainly be the best practitioner, the most useful to his immediate patients, and peradventure happily useful indirectly to the patients of all instructed practitioners in all coming time.

MECHANISM OF MORBID PARTURITION.

There is no department of obstetrics, from whose scientific progress more is to be expected by the mere practitioner than that of morbid labour. This is a branch in which a great deal of valuable work has been expended, with invaluable results, but much more still is required before we can style our practice rational or scientific. Morbid labour in contraction of the pelvis is a well-known and typical part of this practical department; and, to permit brevity, as well as to give point to our remarks, let us be supposed as referring to it alone meantime. Now, where is the practitioner to look for guidance in this matter? He can nowhere find a secure resting-place. What views is he to adopt as to mechanism? What treatment is he to pursue? What instruments is he to

use? Whom is he to adopt as his guide—Collins, or Simpson, or Dubois, or Hodge, or Barnes, or Hicks, or Spiegelberg, or Schroeder, or Leishman? He must act, and to the best of his judgment and ability. To decide among these able and ingenious, but discordant advisers, he is unable, and there is no hope of his being very soon in a position to do so. However dogmatically the teacher may write on these subjects, he settles nothing for any one but himself and his disciples. The elementary data necessary for arriving at conclusions on these subjects are not yet acquired; but they are being rapidly elaborated. When they are worked out, then there will be a vast diminution of the area over which there is at present mere difference of opinion; and many practical directions will flow from such knowledge, which, not then resting on opinion, will command, nay compel, general consent.

To secure this position for the practice in contraction of the pelvis, there must be, first of all, a nearly complete view of the whole mechanism of natural labour, not merely of what is popularly called mechanism in our text-books, and we are very far off from this complete view. Then pelvimetry must be improved, and even uterine craniometry. Then the modes of progress of labours in contracted pelvis of different degrees and kinds, and both when assisted and when unassisted, have to be made out. Then we have to study the explanations of these modes of progress. Then we have to accumulate experience and laboratory experiments of complexity and variety in order to discover what is the best direction of treatment. Then we have by similar means to find the tools best suited for effecting our varied purposes.

In every one of these departments, we are proud to say, most valuable labour is at present being expended, and light

is being converged on this great practical matter—delivery in cases of contracted pelvis. Till this light makes the subject clearer than it is at present, we can have no final adjustment of our differences and difficulties. Mere dogmatic empirical teaching must be allowed to prevail. We must, as people do in matters of politics and religion, choose our authorities and do our duty under them as we best can. It would be tedious to enumerate the names of men who have recently contributed to the progress of this subject ; but, at the risk of making omissions which are to be regretted, we may signalise among foreigners Michælis, Litzmann, Hecker, Kehrer, Dohrn, Spiegelberg, Olshausen, Fehling, Cohnstein, Pajot, Joulin, Breisky, Braun, Martin, and von Haselberg ; and among ourselves Radford, Churchill, Simpson, Barnes, Hicks, Playfair, and Kidd.

NATURAL AND MORBID PUERPERALITY.

After natural and morbid parturition come subjects of far more difficult investigation, natural and morbid puerperality. As their secrets are far more deeply hidden, so our progress in bringing them to light has been less, and is more recent ; yet it is made in a way strictly analogous, if not identical, with that which has conducted us to so much acquaintance with parturition. The steps of natural healthy recovery from childbearing are first investigated ; and here much has been attained through the labours of Kölliker, Farre, Priestley, Robin, Friedlander, Turner, and many others. Then, in logical order, we come to morbid puerperality, the subject more immediately interesting to accoucheurs, one of the most important topics, not in obstetrics only, but in the whole of medicine, one which has been discussed with an excessive copiousness, but with very little advantage till recent times. There is

in the whole large library of puerperal diseases not a work of considerable value before the early part of this century. We have to come down to the names of Dance and Cruveilhier, and Robert Lee, and Virchow, and Pasteur, and Sanderson, and Lister, to find the indistinct bridle-path, which is quickly becoming an evident broad and great line of beneficent scientific progress,—a progress whose practical results will overflow, not obstetrics merely, but surgery also and medicine proper. Old books on puerperal diseases are full of disjointed facts and mere discussions. It is only now when we have resorted to statistics, to anatomy, and to researches by the microscopist and the chemist, that our way clears up towards a comparatively full intelligence of the awfully important puerperal diseases. The old works of Puzos, of Willis, and of others, are nearly as valuable as most of those of the last generation of authors. But since the times of phlebitis and lymphangitis began, this department has ceased to move in a circle of no progress, and almost every day we are reaping new increments of knowledge, not only valuable in themselves, but indispensable to still further advances.

In connection with this subject, there is a preliminary inquiry whose importance is self-evident, and which, remarkable to relate, has only recently been discussed formally, and with sufficient means—the mortality in childbed, or total mortality of childbirth and in childbed. How many women die from all causes during childbirth, and in the puerperal state? Of course, such a question, lying on the surface, has been considered and answered, but the responses have been most insufficient and erroneous. The ordinary belief seems to be, that there is, in connection with childbirth and lying-in, no mortality in a

well-conducted practice. Miss Nightingale says that deaths from puerperal diseases ought never to arise after delivery in a properly conducted and managed institution for lying-in women. In a late number of one of our principal medical journals appears a report of one of these properly conducted and managed institutions. The hospital is a military one, and not a death is reported ; and the article is evidently written with a view to show benighted civil obstetricians what is the result of proper conduct and management. I have often heard sanguine medical men say that in the course of a long and large practice they had
× not had a single fatal case. Now all such beliefs, reports, and statements are mere encumbrances of the inquiry, and are to be thrown overboard, if not more ungracefully dealt with. We have no time to trifle with such nonsense, for we are everywhere surrounded by awful deaths in childbirth and in childbed, where there has been, so far as can be discovered by ordinary mortals, nothing but proper conduct and management.

Another response to this great question is familiar to all ; it is derived from Merriman, and has often been repeated, not only as evidence of the mortality of childbirth, but also to show the successful progress of obstetrical therapeutics by the rapid diminution of the maternal mortality. It requires such a stretch of credulity to place the slightest confidence in Merriman's tables, that I do not regard myself as justified in taking up your time any longer with them. More recently, and on much higher, even on official authority, somewhat similar statistics have been proclaimed as evidence of the progress of obstetrical therapeutics from decade to decade ; but, alas for us, the evidence will not bear inspection, and we shall not inspect it. Even now, in 1874, we are only striving to reach a sound

conclusion as to this mortality ; and after all our labour, official and private, have no statistics to be relied on for the comparison of the results of successive decades or of longer periods. The importance of the question needs no demonstration, and it must be solved in a scientific manner. We are seeking not what we fancy or wish, but what is. There can be no doubt that the death-rate does represent marriage and childbearing as "a most perilous ordeal for a young woman to encounter ;" and it is not good reasoning to use this, as a great author has done, to prejudice us against receiving what may be proved. During the whole life, including intrauterine existence, the female half of mankind has a great advantage over the male in point of mortality, except that for a considerable time childbearing brings the female nearly to the level of the male ; and that, for a less time, the risks of primiparity sink her below him. Childbearing is in these kingdoms the special, and, so far as known, the only special great cause of enormous increase of female mortality above what it would otherwise be. Primiparity produces a great exaggeration of the childbearing risks. It is this mortality that we seek to estimate, and it is of the highest human interest ; but it is for medicine of special importance, being a cardinal element in the solution of the question of the value of hospitals. These noble institutions, the lighthouses of practice, have had their reputations tampered with on the most insufficient grounds. If we are ignorant of what may be called the normal mortality of childbirth and lying-in, how can we justly judge the hospital mortality ? If we do not estimate the excess of special, and, in a sense, just causes of mortality in hospitals over those acting in the country generally, how can we fairly measure the salubrity of maternities ? It is impossible to do so, yet there has been

a lamentable and injurious amount of such mere cavilling with institutions whose reputations should be too sacred for any but the most solemn and logical consideration.

Among the Chinese puerperal mortality is held, according to Dr. Jamieson, to rise as high as from 1 in 12 to 1 in 20—an alarming and scarcely credible statement ; yet Dr. Thin, lately of Shanghae, believes it to be true. Some statistics of the city of New York, recently published by Fordyce Barker, yield a puerperal mortality of 1 in 35, a terrible result ; and on the surely extravagant admission that a quarter or even half of the births were not registered, we have here a very high figure. Faye states the puerperal mortality of Prussia as 1 in 84 ; and that of Finland, according to Pippingskjöld, as 1 in 106 ; and that of Norway as 1 in 131. I found that in Edinburgh and Glasgow, in 1855, the mortality of married women within six weeks after delivery was 1 in 107 at least. From a large collection of data, and trying, however rudely, to get an approach to exactness, I estimated the mortality within four weeks after delivery as about 1 in 120.

Hervieux says there are places where, for a long series of years, this rate has not risen above 1 in 1000 ; but he must derive his information from some other planet than this, for certainly there is no such abode of the blest known among the inhabitants of this earth. Le Fort estimates the rate as 1 in 212 ; but his figures and reasoning are such as to render this determination unworthy of any reliance, as has been sufficiently demonstrated. Farr has carefully estimated this rate, and arrived at the conclusion that it is 1 in 190. But, however much we may be disposed to bow to his authority, we are bound to scrutinise his method ; and, on doing so, it turns out to be very unsatisfactory. He is dealing with a system of regis-

tration which is not compulsory ; he seeks to verify the returns relied on by appeal to the returners, which is something like trying to correct an error by itself ; he made no independent search for the deaths of the delivered women ; he made no correction for twins, nor for still-births. On all these accounts I regard his result as being, not only out of keeping with the best of the others, but as not especially reliable. The data of foreign countries which I have given may be very good for aught I know, but then I have no positive knowledge of the care or of the circumstances under which they were compiled ; yet we have always been led to regard the Swedish and Prussian returns as very valuable.

In this state of matters I was not disposed to allow the point to remain unsettled for this country, and I recently undertook the somewhat onerous task of thoroughly searching official returns, with a view of getting a figure that could be relied on. The determination which I am about to give can be erroneous only in the way of making the rate too low.

I found that there were registered in Edinburgh and Glasgow, in 1869 and 1870, about 52,000 births, and I found that within twenty-eight days after delivery at least 1 in 139 of the mothers had died. Now, several mothers additional may have died, and their deaths have been elsewhere registered, they having left their original residences. These would slightly increase the rate if they were found ; but the rate is too low for another reason—namely, that all births of dead children are omitted. Now, this very serious omission of a large proportion of the most dangerous labours leads to this rate of 1 in 139 being far too low. How very far too low, we may to some extent conceive, when I call to mind that, among Collins's 16,414 women

delivered, 164 died ; and of these 164, nearly one-half had dead children ! It may be said, then, as the result of this investigation—the most careful and complete, so far as I know—that at least 1 in 139 died ; and I add, for the reasons above given, and for others, that I have no doubt that at least 1 in 120 died. These terrible results, or something closely approximating, we must accept meantime, however forcibly they may demonstrate that marriage and childbearing are a fearful ordeal for a young woman to encounter.

My estimate of this lamentable mortality of lying-in women is 1 in 120 within four weeks ; and it is useful to have a fixed period of four weeks for various reasons. But we must not allow ourselves to be misled into thinking that puerperal mortality is over in four weeks. You are well aware that many bad cases linger on beyond the month of four weeks, to die beyond the reach of these restricted statistics ; and that many others owe their deaths to puerperality, although the occurrence is later than four weeks after the labour. I have already mentioned that I have most carefully prepared statistics showing a mortality of 1 at least in 107 within six weeks after delivery. Further, I have statistics analysed which do, I believe, show that the mortality of puerperal women does not again fall to its ordinary level till a period, not of weeks, but of months, after delivery. It would be a grand work for our young statisticians to show the wave of special mortality, beginning with conception and ending some months after delivery. Statistics have already shown the great rise of mortality, or the great wave of it that passes over the sex during the childbearing age. But we want much more than this, and especially the wave for the average individual pregnancy, labour, and lying-in. *

I must conclude this already too long discussion by saying that I believe that in this country nearly 1 in every 100 women delivered at or near the full time dies in parturition, or before the puerperal state and its effects have passed over. This is, no doubt, an awful statement for women and for men. Whether it will deter them from marriage or not when they come to know it I cannot say, for I have no analogy to guide me. The risk from railway accidents is comparatively a mere bagatelle, when taken in any point of view, and I have made no inquiry as to its influence in deterring from travel.

MORBILITY OF CHILDBED.

Even the fear that women may be deterred from marriage and childbearing must not deter us from unmasking the real extent of the dangers they encounter ; but I must only spend a few words on puerperal morbidity. Besides dying at a rate of nearly 1 in 100, women have to encounter a vast amount of disease and suffering which does not end fatally. This has been called morbidity, in contradistinction to mortality. Miss Nightingale and Dr. Farr, besides having very favourable views of mothers' chances of survival and recovery, go a great deal further than this, and enunciate a doctrine to which it is difficult to believe they have ever given a moment's reflection. They regard ordinary women as having no need of long nursing after lying-in ; for it is all over, say they, in a few days after retirement and delivery in the rude compartment of a hut. It is unnecessary to take up the time of any one of the most moderate experience in confinements and diseases of women among the poor or the rich, among the civilised or uncivilised, with a deliberate demonstration of the tragical injustice of their statement.

I only refer to it here as it forms a contrast with the truth as to puerperal morbidity. This subject has been illustrated by many authors, among whom are Späth and Landau, who confine their researches to morbidity shortly after delivery. The latter, taking temperature as a criterion, estimates puerperal morbidity as affecting 1 in 6. His valuable experience was in an obstetric hospital, and it requires corroboration. Besides this, he takes no account of the many ulterior diseases coming on after so-called recovery.

Deaths during parturition or the puerperal state are often conveniently arranged in three sets—

1. Childbirth deaths.
2. Puerperal or metria deaths.
3. Accidental deaths.

A woman dying during post-partum hæmorrhage undergoes a childbirth death ; a woman dying of puerperal fever undergoes a puerperal or metria death ; a woman accidentally poisoned by laudanum shortly after delivery undergoes an accidental death. In cases such as these the placing of the death in its class is easy, but there are a large number of cases regarding which there may be just difference of opinion as to which of these three groups should receive them : hence the classification as used in practice cannot be relied on as embodying a scientifically accurate statement of any point—a circumstance which, for the conduct of various important discussions, is much to be regretted.

But though this is so, there is unanimity in placing puerperal fever deaths in the second category, that of puerperal or metria deaths, and in giving it the horrid pre-eminence over all other causes of mortality in the three combined categories. For obstetricians and for the

world, then, this is the subject of first importance in midwifery, and it has attracted a corresponding amount of attention, and never more than at present, and certainly never with so much advantage.

HISTORY OF PUERPERAL FEVER.

The oldest writer on puerperal fever, Willis, whose book was published two hundred years ago, propounded a theory of this disease which is remarkably like that now in vogue among the best pathologists. He regarded it as having some connection with an uterine wound ; but then he did not know the nature nor the anatomy of the uterine wound, and he did not think any of the other wounds or injuries of the lying-in woman important in the matter. He regarded the disease as a fermentation in the blood, but his notions as to fermentation, its causes and results, were imperfect in themselves, and extremely unlike ours. He attributed to fancied sulphureous particles somewhat of the importance and place that are now given to the bacteria of Mayrhofer, of Lister, and of Heiberg, or to the micrococci or globular bacteria which Orth, and I believe Heiberg also, regard as holding the supremely baneful position among these noxious beings. Our new theories of puerperal fever must be tolerated because, as philosophers, we delight to frame them ; and they have a certain utility which this is not the place to discuss. The new theories far surpass the old ones, which deserve more thorough displacement and rejection than the profession has yet awarded them. The new theories are based on an accumulation of facts, whose collection is most creditable to modern science. These valuable facts have been the fruit of the ceasing to philosophise, and the struggling to

observe and to experiment, which are the characteristics of modern pathology. As in parturition, so in the puerperal state, the progress has arisen from discovering mechanism, although we do not speak of these advances in such terms. What are our discoveries in the anatomy of the lymphatics but the mechanism of puerperal fever or a part of it? What are thrombosis and embolism? What is the conveying and diffusion of bacteria, or of any septic poison? What are our antiseptic precautions? By statistics, by observation, by experiment, we accumulate facts more or less pertinent to the subject, and we arrive at theories having a wider and more solid basis than those of our predecessors, who long and till recent times, misled by the *ignis fatuus* of an essential puerperal fever, were destined to make little real progress till they threw that notion aside and began working at the matter again according to better method.

THEORY OF PUERPERAL FEVER.

No theory of this subject can be regarded as final or sure. But the time has come when obstetricians should try to leave off the use of the convenient term puerperal fever, because it embodies error. There is nothing essentially puerperal known in it; nor is there anything of the nature of a fever, as that term is generally understood. A new name, already widely used, is to be found in the already comparatively old term, pyæmia. This new name can be of only temporary utility, but that utility will be very great, and continue till advancing science displaces it by a better, as it should now displace puerperal or childbed fever. It will then have served its time by carrying the ideas of generations of practitioners away from the old, flimsy, and extensively erroneous speculations of the past

to the more substantial of this day. It is not to be supposed that pyæmia is a term to be analysed into its component parts and held as implying purulent blood. That was once the meaning of pyæmia, but it is not so now. The crude pathology of Piorry is already almost forgotten, and his term pyæmia is used extensively among the best pathologists as a comprehensive word, identical with or including the septicæmia and ichorrhæmia of certain others. It sounds like an adoption of humoralist views, but in it there is as much of solidism as of humoralism, and there is in it vastly more of modern science than in the term puerperal fever.

Pyæmia occurs in several forms, which are characterised each by more or less peculiar symptoms, but most distinctly by the pathological appearances discovered post mortem. There is that most widely known when you have septic emboli, and scattered abscesses caused by them, and perhaps otherwise also. There is that where you have inflammation of the peritoneum and other serous cavities, including the synovial and endocardial. There is that where the mucous membranes are chiefly affected—the muco-enteritic. And lastly, there is that where the only results found after death are—alteration of the blood, enlargement of the spleen, the liver, and degenerations of their most important tissues, with similar degenerations in other organs. It is this last which, often rapidly fatal, was described by Helm, and is now often called acute septicæmia. These are the cases which the superficial pathology of our young days described as having no post-mortem appearances at all. An autopsy in those days was made by any practitioner, occupied only a few minutes, and the observations made were of corresponding value. Now an autopsy is a matter understood to demand

the labour for a long time, often for hours or even days, of an expert. On these fruitless necropsies, where no appearances were discovered and none supposed to be discoverable, was founded, as you will remember, an argument supposed to be of clenching potency in favour of the essential fever character of the disease. But I confess I have never been able to discover either the logic or the power of the demonstration.

Easily getting rid of this old argument, we come sharply into contact with a new difficulty. When I say that Weber, Bergmann, Billroth, Hüter, Schroeder, Verneuil, and Hervieux, support the doctrine, that Olshausen holds it an open question, and that Sanderson, in his essay on the *Infective Product of Inflammation*, demonstrates truths which seem at least to favour its pretensions, you will see that the matter has already occupied great minds. The doctrine is antithetical to the essential fever notion, for it states that pyæmia, or so-called puerperal fever, does not essentially differ from ordinary inflammatory fever, such as is called healthy, except in degree, and that the modes of induction of these feverish states are identical, or nearly so. From the slightest pyrogenous effect or merest evidence of morbidity, as discovered by the thermometer, up to the most rapid of Helm's cases of acute septicæmia, we have one disease in different degrees or forms, all depending on a chemical poison of Schmiedeberg and Panum, or on the bacterium of Mayrhofer, of Lister, of Klebs, of Waldeyer, of Heiberg, and of Orth, whether this bacterium be the poison, or only its carrier. That there are weighty reasons for entertaining this view must be admitted, and among them not the least is the wonderful results of the antiseptic system of treatment, as not only preventing pyæmia, but as preventing ordinary inflammatory fever. But practitioners of my own age, or greater, will find it

difficult to get rid of, or controvert, the *primâ facie* evidence in favour of the old views afforded by the great array of facts and ideas which forms the basis of our daily reasonings in the guidance of practice in healthy and unhealthy inflammations, and which furnish a set of arguments which have been well stated by a reviewer in a late number of our medical quarterly journal.

The disuse of the term puerperal fever, and the replacement of it by puerperal pyæmia, is a change which has already been carried out by many of our best obstetric authors. The old designation is so impregnated with erroneous and misleading theory, that it cannot, within a reasonable time, be purified, and will probably be most advantageously subjected to destructive cremation. For-
dyce Barker, a recent American author of great intelligence, still upholds the old banner—"an essential fever peculiar to puerperal women, as much a distinct disease as typhus or typhoid." He well knows how pathologists believe they have torn this view into tatters, and he ought to have given us good evidence of its being reparable, if not actually rehabilitated, but he does not even attempt the difficult task. When we are asked for evidence as to the specific characters of typhus or typhoid, we can easily produce them, and defy the further destructive analysis of these diseases. It would be a waste of time to go over the special history of the causes, progress, and results of these diseases. They are well known, clear, and convincing to all. For puerperal fever, we have no such characters—no such evidence. All the evidence brings the disease into
X the closest alliance or identity with surgical pyæmia. The grand modern history of pyæmia is, in fact, at every step of its progress, the history of the elucidation of puerperal pyæmia, or of so-called childbed or puerperal fever.

One error is sure to bring another in its train ; and so we have the widely prevalent belief that this disease is like cholera, or small-pox, or typhus, in occurring sporadically, but chiefly epidemically. Here it must be observed that many authors use the word epidemic carelessly, or as synonymous with endemic ; a common error, which should never be committed. But, knowing this, we find them almost universally believing in real epidemics of puerperal fever, describing them as sweeping over a country or devastating a continent. It is well known, and indeed needs no further proof, that the disease has often the appearance of being endemic in an hospital, that it attaches itself to and follows certain individuals in their practices ; but I have not been able to find anything worthy of the name of evidence to prove its epidemic prevalence at any time or in any large district. You are all, no doubt, familiar with the long descriptions and marvellous statistical compilations adduced as evidence of this doctrine by our best writers, especially by medical historians, among whom Hirsch is pre-eminent. But when these statistics are subjected to scrutiny, they are all found wanting, as may be made plain by one example, and the examples are all from old times. We have few epidemics described in recent times, and these few do not produce respect for the doctrine implied in their description. Yet though there are few descriptions now, there is still everywhere the erroneous belief. Epidemics are described as having occurred in Edinburgh in 1772, 1814, 1825, 1833. But it is mere assertion. There is not a tittle of proof that the disease was not as prevalent in every year as in the years of the so-called epidemics. When a physician, struck with awe by a few cases, writes a description of them, down it goes in the statistical tables as an epidemic ; and the year

of it is not always that of the cases, but sometimes that of the publication of the pamphlet or book. When the horrid mortality rises in an hospital, from overcrowding or other causes, down it goes again as an epidemic ; and with this supply of burlesque evidence the manufacture of epidemics never ceases. Registration arrangements are not required to show the epidemic character of cholera, or of small-pox, or of other fevers ; and, when we do get the valued registration statistics, we get the proof in proper form. But if puerperal fever is to be shown to be ever epidemic, it must be by regular modern registration statistics. The prevalence of it is happily never very great, comparatively speaking, and, again, it is never extensively or long absent ; and, when we appeal to such statistics of metria as are accessible, we do not get proof of epidemic character. We find it always present, in every county, in every community. It is easy to get proof of the epidemic characters of fevers, but not of puerperal fever. He who studies this point will find metria to vary in its ravages as pneumonia does ; he will find cholera or scarlatina varying in their ravages according to a quite different law.

As the disease is erroneously believed to be a fever and to occur in epidemics, so we have a corresponding erroneous theory of its origin or causation. Many authors delight to speak of cosmic or of telluric influences or of miasma as producing the disease, and such subjects are favourites with a certain class of minds which find it most agreeable to enter at great length upon those topics of which they know very little or even nothing. For such there should be a puerperal Zadkiel. It is very difficult to find any evidence for the existence of a miasma even in the air of a pestilential hospital, for puerperal pyæmia prevails in such a manner as is scarcely reconcilable with

the miasma hypothesis, and, on the whole, easily reconcilable with more or less direct communication with diseased neighbours, as Veit has ably shown. Among the circumstances of prevalence to which I allude are the comparative immunity of women brought into the hospital already delivered, the special liability of primiparæ, the special liability of those who have tedious and difficult labours.

CONTAGION OF PUERPERAL FEVER.

Again, as the disease is believed to be a real or essential fever, so, of course, it is believed to be contagious and infectious, or both—whatever these terms may mean. In recent times, this mode of communication has come to be regarded as so certain and yet so subtle and mysterious, that many teachers and a great body of practitioners have been terrified out of their senses by it. One cries out that the obstetrician must not wear gloves, and it would be just as rational to say he must not wear clothes. Perhaps he might be permitted to paint, and go about his practice as the ancient Britons fought. Many say that the practitioner who has a case of puerperal fever must give up his practice and go through various processes, and not return to his avocations for a period varying from a fortnight to six weeks. Others, on this point speaking logically, go further, and say the accoucheur should give up his practice, not only if he has on hand a case of puerperal fever, but of many other contagious diseases, such as scarlet fever, typhus, small-pox, measles, erysipelas, foetid abscess, etc. Of the many who propound or teach such doctrines, I have never known one who practised them, and I cannot say their feelings on reflection are to be envied. If such be good doctrines, they are, of course,

equally good for physicians and surgeons as for obstetricians, but the poor obstetrician is laden with restrictive burdens which his medical and surgical brethren do not recognise or touch with one of their fingers. Yet, the old proverb tells here, that what is sauce for the goose is sauce for the gander, and accordingly physicians and surgeons must
X follow the rules they inculcate on the accoucheur. In truth, these extreme practical doctrines of contagion are absurd, for they render all practice, whether medical, surgical, or obstetrical, an impossibility, or at least reduce the number of patients cared for at a time to one, which amounts to the same thing. In this matter much error and evil are, I believe, introduced by confusing the duties of the practitioner with those of the nurse, two quite different, and almost, if not altogether, incompatible occupations. A practitioner must always, in such circumstances as we are now considering, carefully eschew undertaking the functions of a nurse, for, if he do, he must submit himself to the code of rules that regulates the conduct of nurses. If, avoiding a nurse's duties, a medical man of any kind cannot make himself medically clean in hands and person and dress, all kinds of medical practice, as at present carried on, are impossible with due regard to the safety of patients. Every one who knows the safety of actual medical practice must see that such views of contagion end in absurdity. But it is not, despite all this, to be supposed that practitioners are not bound by the most solemn considerations to take most scrupulous care against being disseminators of disease; and there is no disease with which they deal, where such care is more imperative on them than puerperal pyæmia. The puerperal woman presents in her contused, lacerated, and inevitably wounded passages the most favourable nidus for

the reception of morbid material ; and the woman suffering from puerperal pyæmia in any of its forms, and patients suffering from some of the allied diseases, present this morbid material in its subtlest and most potent essence. A well-demonstrated communicability arises from this source. No other has been demonstrated, but it is possible that in an ill-managed hospital there may be some other. For the existence of another source, several of the best recent authors offer slight evidence ; but, on the other hand, its existence is rendered very doubtful by the alleged absence of pyæmia in those surgical hospitals or parts of hospitals where the antiseptic treatment of Lister is properly carried out.

MATERNITY HOSPITALS.

Another result of this extravagant and superstitious dread of contagion is what I deliberately call the slandering of our noblest and most useful institutions—hospitals—and in that word I include all hospitals for the sick, not those for lying-in women only. Although it is against the latter that most of the foolish talk is directed, it is vain to suppose that they alone suffer. If one kind of infirmary be indefensible, so are all kinds ; they must stand or fall together. The laws of pathology are not varied in nature with a view to the misfortunes specially of lying-in women. No doubt, the slandering is done with an excellent intention, under a good motive, but it is none the less what it is called, censuring injuriously and falsely, or without sufficient evidence. There have been, and there may be now, maternities which are justly calumniated as injurious, but that fact is no excuse for calumniating all. After abundant evidence has been adduced to show the directly and indirectly erroneous character of Le Fort's

statement, that while the mortality of hospitals is 1 in 29, that in private practice is 1 in 212, a recent lecturer on pyæmia repeats it as if it were uncontested, and weakly appeals to authority on a point capable of scientific demonstration. We have reliable and large statistics to show what a moderately good hospital is, and we have no thoroughly reliable evidence that better results are anywhere obtained, whether within an hospital or in private practice. Among the good, I place the Rotunda of Dublin, the reports of whose recent condition you may have seen from the pen of Dr. George Johnston, its present master. So great is this superstitious dread of hospitals and reliance on imperfect statistics, that one eminent author believes he has made out that amputation of the forearm performed upon a poor man in his cottage is thirty times less fatal than if it were performed in an hospital. The paradox is not to be received because its basis is inadequate; and, considering whence come most of our hospital patients, I wish we had from this author some theory of the healing virtues of the concentrated and various filth of a Highland bothy or cottage, or of a den in the Edinburgh Cowgate, or the London Ratcliffe Highway, accompanied, as it often is, by every abomination physical and moral.

As hospitals are in this facile manner made out to be bad, so, of course, large hospitals are worst, and statistics are again appealed to in support of this view. Were it not that a recent lecturer on pyæmia repeats this statement, I would not here allude to it, for it has been shown to be groundless by demonstrations as good as can well be imagined, but which seem not to have reached the eyes or mind of this eminent surgeon. Siebold believed that lying-in hospitals were so useful that shutting them up

would bring far more serious evils than an occasional outbreak of puerperal fever. Whatever soundness there may be in Siebold's judgment, I prefer, with Steele, to adopt a different view of maternities, and to look forward to the time when there will be no endemics of puerperal pyæmia, but only sporadic, or what are called autogenetic, cases. X

Other errors connected with the old and still prevalent opinions regarding puerperal fever, though worthy of comment, must be passed over ; but one, from its importance, demands notice. It is, that this disease is a kind of cholera, or a kind of typhus, or a kind of scarlatina, or owns the same or similar causes. In this country it is almost exclusively the identity with scarlatina that has found supporters. But at home and abroad the doctrine has, in some form or other, been extensively entertained. It is to be carefully distinguished from that reasonable view of Pouteau, Alison, Sidey, Nunneley, and Tilbury Fox, that the disease has close alliance with erysipelas, and, I might add, with the diffuse inflammation of Duncan,—a view which has been partially adopted and signally illustrated by Virchow in his now celebrated paper on X *Diffuse Puerperal Parametritis*. Scarlatina is a source of terrible danger to lying-in women, and scarlatinoid rashes are seen in some of the worst cases of septicæmia ; and herein probably lies the attractiveness of the theory. But, unluckily for its supporters, and especially for the most recent, the theory has been disposed of by the researches of Hirsch and of Veit, who, by statistics of the comparative prevalence of scarlatina and of puerperal fever at different times and at the same time, have shown that there is no relation between the two. This argument against the theory is far stronger than any in favour of it, and X

must meantime be held as conclusive. It had been urged by these authors and by Späth before the *Lancet* did the good service of bringing Farr into the field on the same side.

Every change in doctrine or in name does not necessarily indicate progress ; often, indeed, it indicates retrogress. Such unfortunate changes arise more frequently
x from error in philosophising than from error in observing. Among such, there is one in the history of our present subject. It has by some pathologists been proposed, not only to continue the name puerperal fever with all its adherent errors, but to go far towards introducing like errors into surgery by describing the allied diseases there as surgical fever. Had there not been the erroneous use of the word fever in childbed diseases, there would undoubtedly have been one hindrance less of the general acceptance of modern views. Progress in surgical pathology runs no risk of being now impeded by the false name, surgical fever ; and its adoption will certainly not do any good. By adopting, instead of puerperal or childbed fever, the term
x puerperal pyæmia or some similar one, we, by the mere use of words, enforce the argument for a great medical generalisation, making the gains of surgical and obstetrical science mutually beneficial.

It must not be supposed that there is in the mere change of a name any real progress, however much such change may foster right views. Nor is this change of name to be held as even tantamount to a change of theory, from a false to a true one. The present state of our knowledge is not such as to justify a sense of great security in any theory. Yet theories are very attractive, nay useful, and the tendency to frame them, whether prematurely or not, is in vain repressed. All the more ought we to maintain our minds in an unprejudiced attitude, ready to throw

away the worse and cling to the better. Willis, who introduced the name *febris puerperarum* two hundred years ago, held that it was the result of sulphureous particles and fermentation. This has a faint resemblance to the new pyæmic theory, with its bacteria or micrococci, its septic and infective materials; but Willis's theory and every recent modification of it is a mere shadow compared with the pyæmic theory. The former can scarcely be said to explain or embrace any of the chief details of the subject, while the latter explains and embraces a vast number, is incompatible with none, and is the most promising road to still further explanation and generalisation. It is the last philosophic production of a vast array of modern observations and inquiries. It helps to increase the distance between us and the mere philosophisers, and to join us with the advanced guard of modern medical investigators, whose weapons are observation and experiment, not learned talk, however clever. When the physicist finds a theory of light propounded by the greatest of his kind to be inconsistent with his more advanced researches, he not only adopts a new one, but deserts the old nomenclature connected with the misleading hypothesis.

Many researches of different kinds have contributed, and are contributing, to converge scientific light on this greatest of practical, obstetrical subjects; but scarcely one of them can be regarded as being even yet completed, while some are only well begun. In drawing to a conclusion, it will be well to take a glance at these various investigations, with which we may class some more general discussions, such as that of Spiegelberg, on this topic.

Van Swieten, Willis, and many old authors on puerperal fever, regarded it as a wound-fever, and Eisenmann's well-known work on the subject, published in 1837, is

called "Wound-Fever and Childbed-Fever." The wound which these, and even most recent authors, have in view, is that produced by the separation of the placenta; but it is now well known, and has been often found clinically exemplified, that the disease may begin in a contusion or laceration of any part of the genital canal. Many have recognised the beginning of a fatal disease in a diphtheritic state of a slight recent perineal laceration, with surrounding redness and boggy swelling. I have already alluded to the recent advances of our knowledge of the anatomy of the placental wound, but already we know where to look for much more light on this subject. The anatomy of the lymphatics, to which recently Recklinghausen, Klein, Thin, and many others, have contributed, is not yet completed for the uterus. We all know the fine-looking and distinct but very unsatisfactory drawings of uterine lymphatics by Moreau; but we do not yet really know their actual anatomy, though very much progress has been made by the anatomical investigations of Lindgren, Fridolin, and especially of Leopold. The completion of this work will be an addition to the theory of puerperal pyæmia.

Following the as yet indefinite notions of a wound-fever, came a further true advance from Boyer, Legallois, Cruveilhier, Tonellé, Dugés, and Simpson, who combined to demonstrate the identity of what would now be called the rough morbid anatomy of patients dying after surgical and obstetrical wounds.

Then came a grand piece of progress, consisting in the discovery and descriptions of phlebitis and lymphangitis, which we owe to Dance, Duplay, Cruveilhier, Robert Lee, Hecker, and Buhl.

We now reach our own times, and have a still grander

progress of our knowledge to record, in the discovery of thrombosis and embolism by Virchow, Kirkes, Cohnheim, Buhl, and many others.

After these come researches whose bearings on this subject are certainly very important, but which are, in many respects, as yet immature and incomplete. We allude to the investigations as to the potency of septic poisons, connected with the names of Davain, Panum, Kehrer, Thin, and many others ; the great and actively progressing researches as to the production, diffusion, and influence of bacteria of various kinds, by Lister, Klebs, Waldeyer, Sanderson, Billroth, Winge, Heiberg, Orth, and many others ; the new researches of Sanderson on the infective product of all acute suppurative inflammations ; researches also into the distinctive characters of the noxious or septic, and the innocuous bacteria.

Besides all these, many valuable results have been elicited from the analysis of experience in hospital and in private practice. In this way the influence of age has been ascertained, and especially of immaturity and of advanced life ; so also the baneful influence of primiparity and of excessive childbearing, and of twin-bearing ; so also the baneful influence of severe and of complicated labour ; so also the bearings of the duration of labour ; so also the influence of earliness or lateness of attack after delivery ; so also the baneful influence of inclemency of season ; so also the dangers attending hospitals, and the dangers of communications between the sick and the healthy.

But all of these researches, whether finished or still imperfect, do not, when combined, complete our modern view of this great subject. It is a subject in practical medicine, and we are practitioners. The great object of

our work is to prevent or to remove the disease in our patients, and we have to inquire what fruits our knowledge produces for the comfort or healing of the sick.

Many remedies for puerperal pyæmia have been proposed, and their successful application loudly proclaimed and widely believed. Doulcet was even rewarded by the French Government for his discovery of the curability of this disease by ipecacuanha. In our own day new cures do not fail to make their appearance, and the advanced knowledge of our times would lead us to expect that they should be more rational, as the phrase is. But who is there of weight in the profession, now, who believes in any cure or in any system of specific treatment? All have been found wanting. Yet the wise physician of this formidable disease does not despair of guiding his patient through it, although he well knows its very dangerous character. Experience has shown him the utility of several means for relieving sufferings; and the favourable progress of a case may be encouraged, though not secured, by those invaluable directions which he may give as to diet and stimulants, as well as to more direct medicinal treatment of the genital passage, of the skin, of the bowels, and of the system generally.

But in this disease the physician has long been saying, not that prevention is better than cure, but that prophylaxis or prevention is to be chiefly looked after, and not cure. Great credit is due to Semmelweiss for the good he has done, especially to hospital patients, by his enlightened zeal in this cause; but the records of hospital practice sufficiently show that much more has yet to be accomplished. Prophylaxis is still further to be carried out by attention to stop injurious communication between the sick and healthy, by disinfection, and by architectural

arrangement, subjects which are all at present receiving much attention from the profession.

In the course of my remarks I have repeatedly referred to scientific researches, as to the poison producing pyæmia, and as to the effects of its concentration, and as to the connection of this poison with the presence and diffusion of bacteria. These researches have been carried on mostly by observations and experiments on the lower animals, and of their very great value there can be but one opinion. But there is a variety of circumstances which seem to indicate that the lower animals are not subject to exactly the same laws in these matters as man is, and certainly there must be great caution exercised in arguing in human pathology from the analogy of the lower animals. The most important of the researches referred to, however,—those of Lister and his followers,—have been mainly carried out in man, and consist, in a great degree, in the attainment of results in practice equally wonderful and valuable,—results that can, so far as we at present know, be attained in no other way. These results go far to justify the belief that pyæmia is a septic disease, and that puerperal pyæmia may be almost, if not altogether, prevented by the application to delivery of a practice based on antiseptic principles. We know how much has already been, and is, I am happy to say, daily done with success in this direction. But the rules of Semmelweiss, or any other washing of the hands, however carefully conducted, do not constitute treatment according to the manner of Lister. Such imperfect antiseptic precautions, by use of antiseptic gauze and otherwise, I have used with apparent advantage ; but we have yet a long way to go, in order to secure complete antiseptic delivery and subsequent treatment. To reach this desirable object, the efforts of several

good minds are, I know, directed, both at home and abroad; and some recent unpublished cases of successful antiseptic treatment of wounds of the penis, where periodical discharges of urine have to be permitted, supply a sketch in miniature of plans that might be applied to ordinary confinements. To say more about them I have no right; but I conclude by calling upon you to give your best aid to forward the grand cause of the increased safety of lying-in women.

Write a paper on - Show that it was not epidemic
in '72. That events are true - That is not symptomatic
in infection - Go to Bondy House 301 West St.
Woman Hospital. Bellevue - Remond. St. Lukes -
Nursery, Childs House - Surtis pamphlet. Parker's book -
Laws by any case -

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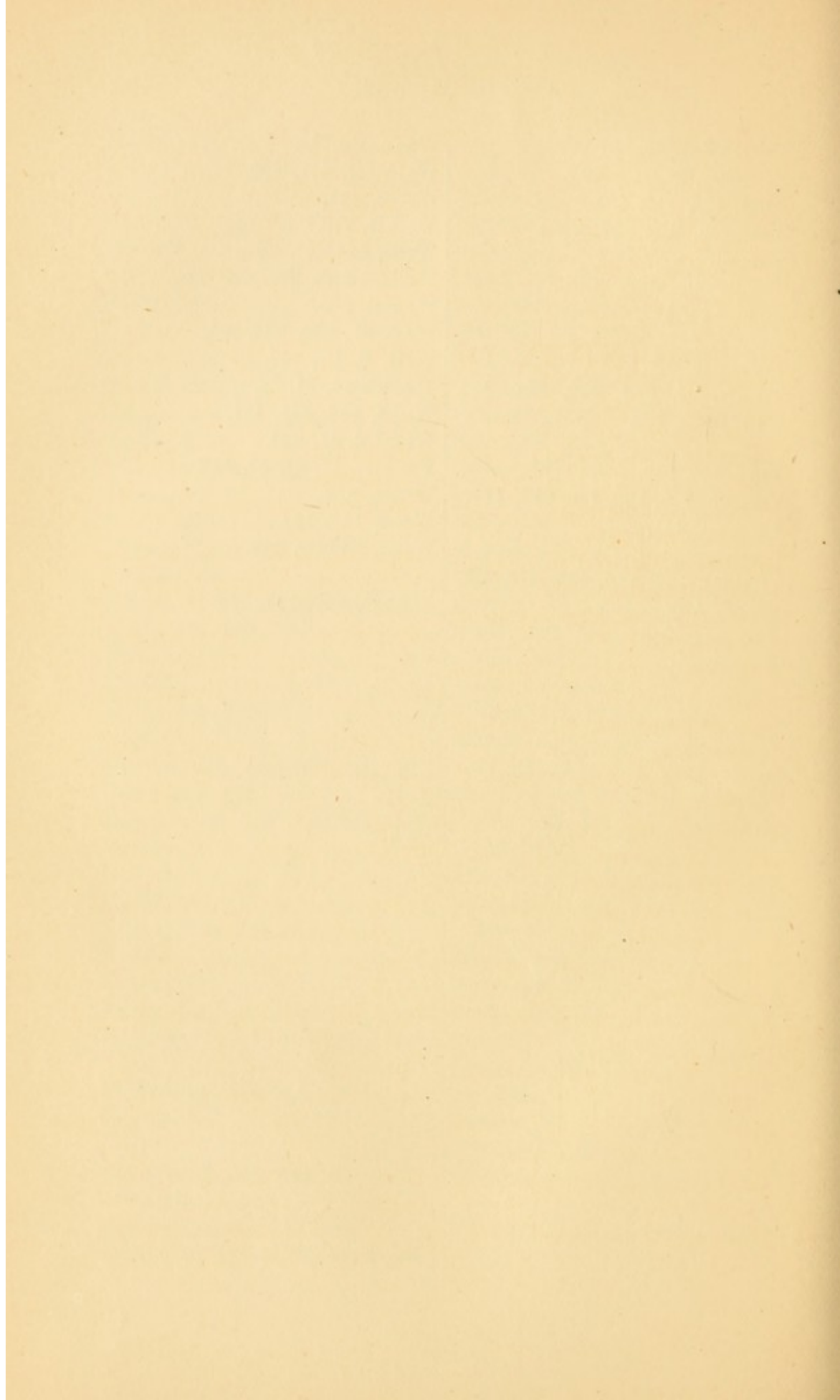
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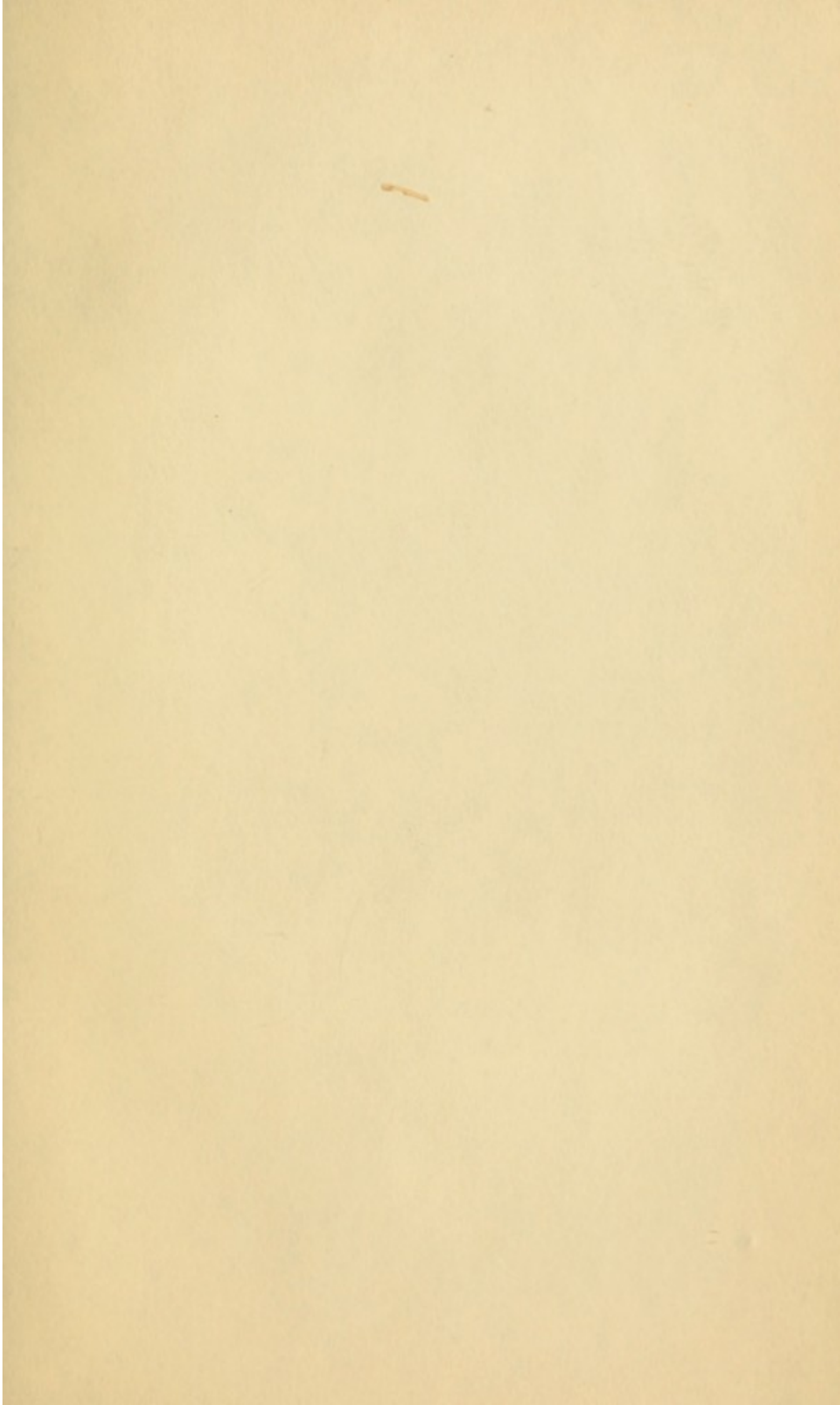
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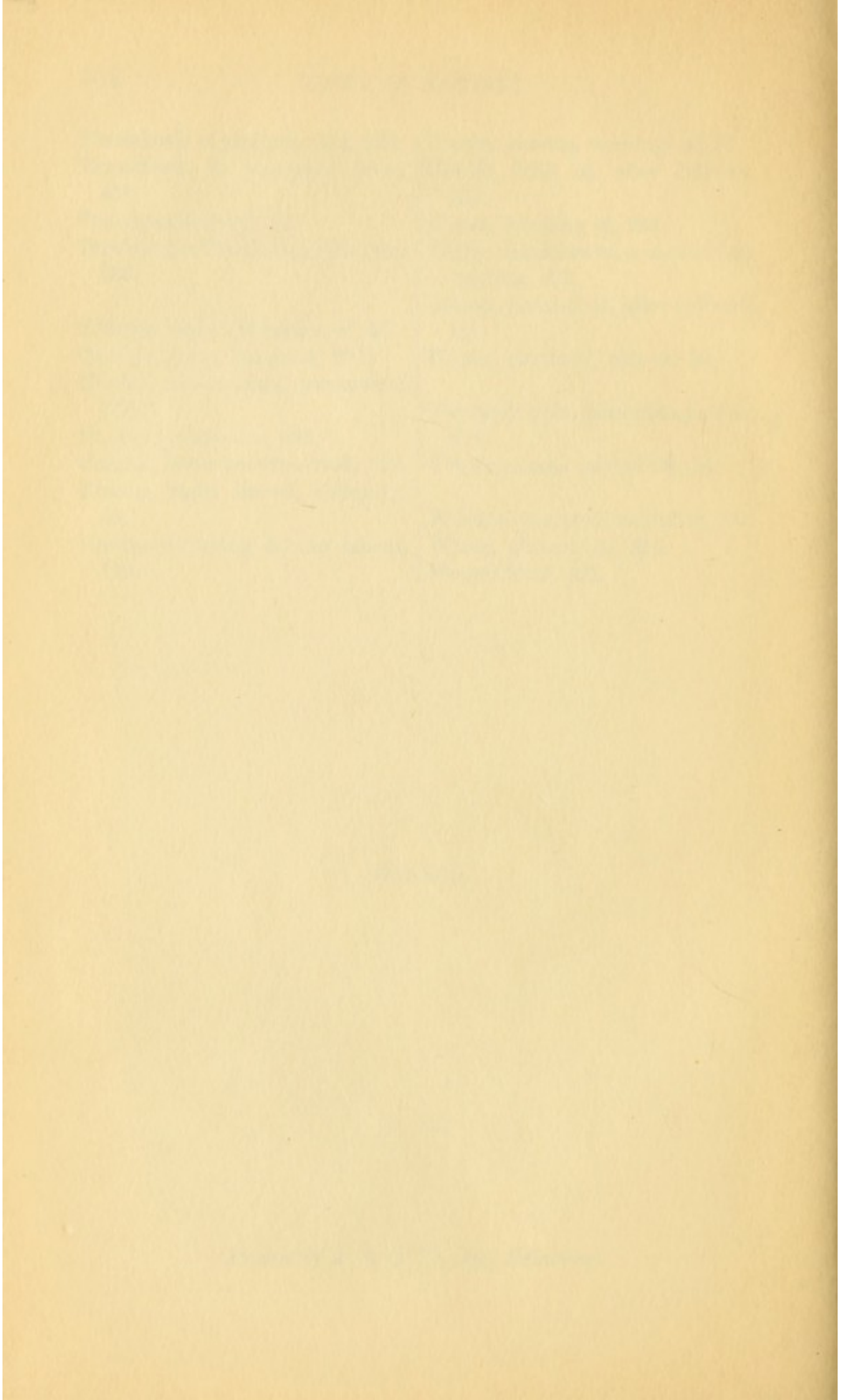
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