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PART FIRST.
Analytical and Critical Reviews.

ART. I.

1. *Parturition, and the Principles and Practice of Obstetrics.* By W. TYLER SMITH, M.D. Lond., Lecturer on Obstetrics in the Hunterian School of Medicine.—*London*, 1849. Fcap. 8vo, pp. 396.
2. *Obstetrics: the Science and the Art.* By CHARLES D. MEIGS, M.D., Professor of Midwifery in the Jefferson Medical College at Philadelphia, &c. &c. With 121 Illustrations on Wood.—*Philadelphia*, 1849. 8vo, pp. 686.

It seems to us not a little strange that writers who show a considerable amount of acuteness on other topics, should continue to entertain such vague and confused ideas with respect to the relation between Science and Art, as are presented by the authors of the two works before us, in the opening sentences of their respective treatises. According to Dr. Tyler Smith,—

“Labour: the study of the act of parturition itself, and of all that relates to the prevention or alleviation of the pangs and dangers in which women bring forth children, and to the preservation of their offspring, are the principal aims of the Obstetric Art.” (p. 1.)

We do not precisely understand the relation of the first word in the above quotation to the rest of the sentence from which it is separated by a colon; according to all ordinary rules of punctuation, it is entirely isolated; and although certain clever writers of fictitious or imaginative works have lately broken through all these for the sake of *effect*, we shall set our face against the introduction of any such method, or rather want of method, into treatises in which the clear and sober exposition of *truth* is the only object. Leaving the word Labour, then, to stand alone, like the monosyllabic noun-verb which in the Chinese language forms a sentence by itself, we shall inquire into the meaning of the remainder of the passage. Inverting the sentence, we find Dr. Tyler Smith asserting that

“the principal aims of the *Obstetric Art*” are “the study of the act of parturition itself, and of all that relates to the prevention or alleviation of the pangs and dangers in which women bring forth children, and to the preservation of their offspring.” In our apprehension, this is rather a definition of the *science*, than of the *art*, of obstetrics. The object of the art is to alleviate the sufferings, and preserve from the dangers, incident to the performance of the function in question; that of the science is to *know how* to alleviate. The art consists of a set of rules for practice, deduced from the scientific study of the function in its normal and abnormal conditions; and these may be successfully applied by a practitioner, who knows nothing of their rationale. On the other hand, the science is evolved from the philosophic contemplation of the phenomena presented by clinical observation, guided by an acquaintance with the general principles of physiology; and this may be successfully pursued by a man who has never conducted a case of labour for himself. Although we are far from thinking that a mere observer is the man best qualified for such a pursuit, yet, on the other hand, we are confident that he would be much more likely to contribute towards the advancement of the science, and the consequent improvement of the art in precision and certainty, than the mere practitioner who isolates this department of biology from every other, and looks at the parturient woman as a being with whom no one but himself has anything to do.

Let us see whether Dr. Meigs is more successful in his definitions. The following are *his* opening sentences:

“Midwifery is the art of assisting women in labour.

“Obstetrics comprises the sciences of anatomy, physiology, and pathology, as relates to the reproductive organs, and the arts of therapeutics and surgery, as applied to sexual affections in women.

“Midwifery is an Art.

“Obstetrics is a Science.

“A Midwife or Accoucheur is one who assumes the conduct of cases of labour.

“An Obstetrician is a physician, who, in addition to a general knowledge of physic and surgery, adds the special information that is necessary for one having the peculiar charge of all sexual affections, whether in the department of midwifery proper, or in other complaints of the sex.

“Notwithstanding obstetrics is composed of several different branches or sorts of knowledge, it claims to be considered as a distinct science.” (p. 17.)

In these oracular dicta, we have an attempt at greater precision of statement; and we believe that, by a little amendment, they may be made to express the true view of the subject. We will take Dr. Meigs's definitions of *Obstetrics* as the *Science*, and *Midwifery* as the *Art*; and inquire what are their respective provinces. Obstetrics, strictly speaking, consists of those departments of the sciences of anatomy, physiology, and pathology, which relate to the reproductive function; but as the obstetric practitioner is the one generally consulted upon the diseases and injuries to which the female sexual organs are liable (unless these diseases, like syphilis, be purely constitutional in their nature), the whole pathology of these organs (with the exception we have named) must be considered as included under the designation of obstetrics. Now so far as obstetrics is a science,—in other words, to use the definition of Mr. John Mill, as it consists of a collection of truths, whose language is “this is, or this is not; this does,

or does not, happen,"—it is perfectly independent of any art; and we cannot, therefore, admit the justice of Dr. Meigs's assertion that the science of obstetrics comprises the arts of therapeutics and surgery as applied to sexual affections in women. These arts, again to use Mr. Mill's definition, are collections of rules, whose language is "do this, avoid that;" and therefore they cannot constitute a department of obstetric *science*, but belong to the *art* which is founded upon it. To include the whole of this art under the designation midwifery, would be to make a wide extension in the application of that term; which is usually restricted to the care of the act of parturition, and its antecedent and consequent phenomena. To such an extension there are many obvious objections; and we do not see what else is to be done, than to distinguish carefully between obstetric *science* and obstetric *art*,—just as we distinguish between the science and the art of medicine or of surgery,—and to let midwifery continue to relate to that division of the art, which the term has hitherto been employed to designate.

Although we have not considered either of our authors very successful in the formal definitions, with which their treatises respectively commence, yet the distinction between *principles* and *practice* is most fully recognised by both—in their teachings, as well as in their titles. In fact, Dr. Tyler Smith's object is to give to Obstetric Science a far higher elevation than it had previously attained, by connecting it with those general principles of Physiology of which a large proportion of obstetric practitioners, and too many obstetric writers, are profoundly ignorant. For his labours in this vocation he deserves great credit, whatever we may think of his success; and if we shall find it our duty, in the discharge of our critical office, to express our dissent from several of the doctrines which he has propounded in the treatise before us, we do not the less feel that he deserves the gratitude of the profession, for having pursued the study of obstetric science in a more enlarged spirit than any preceding writer, and for having diligently attempted to improve and extend it.—We quote his preface in full, as expressing what he conceives to be his claims as a discoverer, and as justifying the careful examination of these claims upon which we shall presently enter.

"I began to study Reflex Obstetrics in 1842; and the present work is the result of seven years' close and earnest attention to the subject. I may say truly, that during this time, though much occupied by other matters, it has scarcely ever been absent from my waking thoughts.

"I have no wish to deprecate criticism; but I trust I shall not be considered in the light of one who applies facts and principles already known to his own department of practice. I believe every candid person conversant with the current knowledge of the reflex function, and of obstetrics, when I began to write, must admit that I have both added to reflex physiology, and made extensive applications in practice, which had eluded previous observers. Indeed, reflex obstetrics is a new department of the reflex function and its applications. Taking the whole range of reflex physiology, the cause of labour is only second in importance to the cause of respiration; and no one had perceived that the relation of the ovarian nerves to parturition is the same as the relation of the pneumogastric nerves to respiration; while, in the investigation of the causes of the Genesial Cycles, in the Twelfth Lecture, I have entered upon a new field, altogether distinct from the reflex motor function.

"When I published my first 'Observations,' reflex physiology had not found

even a verbal home in any work on Obstetrics; but I do not think it will be possible to say the same of future works in this department of medicine." (p. viii.)

Dr. Tyler Smith's treatise, like the lectures already published in the 'Lancet,' of which it is almost entirely always made up, is much less an introduction to the study of obstetrics, than an exposition of the author's peculiar views; and is consequently less fitted for the pupil who needs a comprehensive text-book of the science, than it is for the advanced student, who is prepared by his previous acquirements to eliminate its truths from its errors, and to profit by the former without being led astray by the latter. As a treatise on the "Science and Art of Obstetrics," it is deficient in much that the pupil needs to be taught; as a dissertation upon "Reflex Obstetricy," it is replete with interest. The latter title would therefore, in our estimation, have been more appropriate than the former; and we shall discuss the subject-matter of the treatise as if it had been thus designated. This, in fact, will be the fairest method of procedure in regard to the author; for we are sure that he will be best satisfied by our bringing into prominent relief those portions only of his treatise upon which he bases his claims as a discoverer, passing by with little or no mention those which contain no distinct novelty. We shall find that the former will afford such ample food for critical discussion, as to necessitate our restricting ourselves in the present article to the theoretical portion only of Dr. Tyler Smith's work.

The corresponding department of Dr. Meigs's treatise presents so little that varies from the beaten track, that a few words will suffice to dismiss it, especially as we had occasion not long since to criticise some of Dr. Meigs's opinions on these subjects, as well as his manner of expressing them. With regard to the style of the present treatise, we have the satisfaction of informing our readers that whilst it presents many of the peculiarities which characterised Dr. Meigs's "Letters to his Class on Females and their Diseases," it is free from the most offensive faults which disfigured that work. The colouring is in fact considerably 'toned down;' and would perhaps be altogether not more lively than is sufficient to make the book very agreeable reading, were it not that the old affectations every now and then force themselves in, with somewhat unpleasant obtrusion.—We noticed on a former occasion (Vol. III, p. 142,) Dr. Meigs's notion that the "corpus luteum" is a vitellary body. On this subject he expatiates somewhat fully in the present treatise; and informs us that, when boiled, the corpus luteum becomes hardened like yelk boiled hard, and that, when roasted, it gives out a strong odour of roasted eggs! From the result of this truly refined and trustworthy procedure, in addition to the microscopic observations formerly referred to, Dr. Meigs deduces what we must regard as a most loose and unphilosophical analogy between the corpus luteum and the vitellus. Their similarity in composition, histological and chemical, however close it may be, does not indicate any real analogy, their position and functions being so essentially different. A vitellus is *not* a vitellus because it is composed of albumen, oily matter, sulphur, &c., which the chemist finds in it; or because of the floating corpuscles which the microscopist detects; but because it is so placed *within* the ovum as to afford a supply of nourishment to the developing embryo, which shall be converted into its own textures. This is the essential *idea* of a yelk or vitellary body; and to give the designation of

vitellary body, therefore, to a substance which is only developed after the ovum has escaped from the ovisac, and even on the exterior of the ovisac which enveloped the ovum, and having no function whatever as regards the embryo, appears to us a complete perversion of language.

We are surprised to find Dr. Meigs greatly behindhand in his knowledge of the structure of the placenta. For anything that his account of it contains, it might have been written ten or twelve years ago, when as yet neither Professors Weber, John Reid, or John Goodsir had unravelled its complicated arrangement. The old questions about the foetal and maternal portions of the placenta, are discussed in the old style; and the author gravely comes to the conclusion that the placenta is entirely a foetal organ, and that the notion of the passage of vessels, or of maternal blood, from the uterus to the placenta, is all a delusion. He rests his convictions upon the evidence of his own senses, having on more than one occasion seen nothing but "mucous tractus" passing between the two surfaces when torn apart from each other, and having thus satisfied himself of the total absence of vascular communication. When he shall have prosecuted a dissection of the parts in question, under water, and with the requisite slowness and care (that of Dr. J. Reid occupied him several hours a day, for some weeks, we believe months); when he shall have followed, with Professor Goodsir, the development of the placenta, and shall have seen how it is made up of uterine tufts and decidual growth; and when he shall have microscopically examined the placental tissues, injected and uninjected, and shall have accounted for the presence of maternal blood in the placental cells, whilst the foetal blood is restricted to the tufts which constitute the ultimate subdivisions of the umbilical vessels; we will allow him to have an opinion of his own. Until then his assertion of the entirely foetal nature of the placenta is altogether valueless, and he is bound to teach the opinions of those who *have* made such examinations. For ourselves we can say, that having witnessed a considerable part of Dr. J. Reid's dissection, we can bear unequivocal testimony to the correctness of his results; and that having also gone carefully through the microscopical examination of the placenta, we can equally confirm the statements of Professor Goodsir in regard to the combination of foetal and maternal structures which it contains. Dr. Meigs lays great stress on the fact that the whole placenta is thrown off in the human female, instead of the foetal portion only, as in the lower mammalia; but he takes no account of the circumstance that the decidua also, of which the maternal portion of the placenta is only a peculiar development, undergoes the same detachment. In regard to the real nature of the decidua, too, we are surprised to find Dr. Meigs apparently ignorant of the satisfactory elucidation which it has received in this country; he quotes M. Coste and other continental authorities glibly enough; but for him Professors Sharpey and Goodsir have observed and described in vain, and the decidua reflexa is still pushed before the advancing ovum as in old times.

We are sorry to be obliged to speak in these terms of Dr. Meigs's sins of omission. He takes up many other novelties with such delight and earnestness, that we are sure that he cannot be unwilling to receive information or correction; and we should therefore recommend him to work up his physiology a little better from the later general treatises in which these discoveries are embodied. We shall now quit him for the present,

that we may devote ourselves without hinderance to the examination of Dr. Tyler Smith's production; in doing which we shall take leave to follow the order which best suits our purpose, not that which the author has adopted. This will lead us in the first instance to inquire into the muscular structure and nervous endowments of the uterus.

Muscular structure of the uterus.—It is not long since the muscularity of the uterus was a subject of grave discussion; the affirmative being maintained by some eminent authorities, and the negative by others. The microscope has now settled the question, by showing that the uterus is made up of fibres which belong to the non-striated kind of muscular structure, corresponding in this respect to the muscular coat of the alimentary canal. It has been affirmed that, towards the completion of the term of gestation, fibres of a faintly striped character, resembling those of the heart, are to be seen in the uterus; but in the unimpregnated state, the fibres bear a close resemblance to those of less elevated development.

Nervous supply of the uterus.—Notwithstanding the difference of opinion which exists in regard to amount of the nervous matter of the uterus, all are agreed, we believe, that its nerves are *immediately* derived altogether from the sympathetic system. Dr. Tyler Smith, indeed, speaks of the uterus as "principally supplied with nerves, by the hypogastric and sacral nerves, and by branches from the spermatic plexus;" but this is a very loose method of stating the fact. The nerves of the uterus are all traceable—as Dr. Smith subsequently points out—either (according to Dr. Lee's views) into ganglia of its own, which form part of the sympathetic system, or into the ganglia and plexuses of that system from which the other pelvic viscera are supplied. No anatomist, so far as we are aware, has traced any branches of the sacral nerves into the uterus itself; although it is unquestionable that branches of several nerves of the cerebro-spinal system pass into the plexuses of the sympathetic, and are in that way distributed to the organ. It may be said that the distinction is unimportant; but we cannot regard it as being so; for it is obvious that the endowments of the cerebro-spinal fibres which enter other parts of the sympathetic system are greatly affected by their association with it, neither sensory impressions nor motor impulses being as readily conveyed by these fibres as they are by the ordinary cerebro-spinal nerves. So struck were the older physiologists with this fact, that (as is well known) they assigned to the ganglia the office of "cutting off sensation."

We are not about to offer any decided opinion upon the controversy which has arisen respecting the validity of Dr. R. Lee's claims to the discovery of additional nerves and ganglia in the virgin uterus, and of a great increase in the nervous supply during the enlargement of the organ in gestation. These claims are strongly advocated by Dr. T. Smith, who gives the following statement of them:

"Below the bifurcation of the aorta, the aortic plexus divides into the two hypogastric nerves. Dr. Lee describes the hypogastric nerve as forming, in its descent to the cervix uteri, the hypogastric plexus. This plexus, when it reaches the cervix, terminates in a large ganglion, which Dr. Lee has called the hypogastric ganglion. The hypogastric ganglion is, in the unimpregnated state, on the authority of the same anatomist, from half an inch to three quarters of an inch in diameter; and is made up of numerous lesser ganglia with their rami of communication. Into the outer and lower surface of the hypogastric ganglion, numerous branches

enter from the third, and sometimes from the second and fourth cervical nerves. This ganglion, thus composed and reinforced, is regarded by Dr. Lee as the centre from which each lateral half of the uterus is supplied; from the hypogastric ganglia, nerves pass in various directions to the os, cervix, body, and fundus, and are distributed extensively to the muscular structure and the internal surface of the uterus. In the course of their ramifications over and in the substance of the uterus, numerous ganglionic enlargements occur. In the virgin uterus, Dr. Lee has specially directed attention to a beautiful ganglion, in front of the hypogastric ganglion, extensively connected with the surrounding nerves, which he has called the Laurentian ganglion, in honour of Mr. Lawrence; and in the gravid organ, Dr. Lee has described numerous *sub-peritoneal ganglia and plexuses* on the anterior and posterior surfaces of the organ. These ganglia and plexuses maintain extensive communications with each other and with the hypogastric ganglion below, and the spermatic plexuses and ganglia above." (p. 63.)

According to this description it would be supposed that Dr. Lee was the first to call attention to the "hypogastric ganglion;" whereas Tiedemann and other anatomists, who had carefully dissected the inferior hypogastric plexus, had described the existence of numerous small ganglia united by connecting laminae, some of them containing ganglionic matter, forming a *ganglionic plexus* opposite the neck of the uterus. And on Dr. Lee's own showing, such a designation is far more appropriate than one which brings this collection of small ganglia, with their rami of communication, into the category of the large simple ganglia. We are not aware that any satisfactory proof has been given that the so-called "Laurentian ganglion" is anything else than a portion of the same plexus, with minute collections of ganglionic matter possibly interspersed among its rami.

It is, however, upon the question of the augmentation of the uterine nerves and ganglia during pregnancy, that the hottest discussion has taken place. Now for ourselves, we have no hesitation in saying that we have not the slightest *a priori* objection to believe in such an enlargement, with respect at least to the ganglia and fibres of the true sympathetic system; and no one has asserted, so far as we are aware, that the filaments derived from the cerebro-spinal axis undergo any increase. We do not pretend to say what are the functions of these ganglia and nerves. We do not believe them to be independent centres of reflex action; the whole analogy of the sympathetic system is against this view. On the hypothesis of Dr. Radclyffe Hall as to the general functions of that system, their office would be to regulate, through their action on the arterial coats, the afflux of blood to the organ, in accordance with its functional activity. Whatever be their office, it certainly does seem to us anything but unreasonable to suppose that, with the enormous development of the muscular and other structures, the vast increase in the supply of blood, and the elevation of the entire functional activity of this wonderful organ, there should be a necessity for an augmentation of its nervous supply. To assert the contrary, except upon positive evidence, appears to us almost equivalent to asserting that the nerves and ganglia, whose existence is admitted by every one, are of no use whatever.—It is maintained, however, by many excellent anatomists, that Dr. Lee's sub-peritoneal nerves and ganglia are *not* nerves and ganglia at all, but are carved out of the fibrous texture of the uterus; and that no essential difference exists between the nervous supply of the gravid and that of the virgin uterus. We cannot see that

any real obstacle can exist to the determination of this question, if Dr. Lee will only deign to afford the materials for its settlement: for every histologist well knows that the microscope now presents an unfailling test as to the presence or absence of nerve-fibres and ganglionic corpuscles in supposed nerves and ganglia, provided these be examined in the recent state. From this test we must say, on our own knowledge, that Dr. Lee has held back; alleging precisely the same grounds for his unwillingness, as are set forth in the following extract from Dr. Tyler Smith's discussion of the subject:

"The opponents of Dr. Lee confidently appeal to the microscope to prove the justice of their rejection of his labours; but the fact of such an appeal would seem a prejudice against him, and a willingness to deny the fidelity of his dissections. The microscope is used to ascertain the intimate physical appearances of nervous and other tissues, but hardly, on ordinary occasions, to distinguish grossly as between nerve, muscle, and cellular tissue. It must be borne in mind that the nerves of the body generally were dissected, and held to be nerves upon other evidence than such as that furnished by the microscope, long before this valuable instrument was invented. If, in the case of the uterine nerves, we deny that the tracing of the scalpel or dissecting-needle, continuity with other nervous structures, and careful ocular examination—for the debatable structures are quite large enough to be inspected by sight and touch,—if, I say, these are not sufficient to help us to a decision in the case of the uterine nerves, neurological anatomy should begin again, and we ought to question the reality of every nerve in the body; for they hold their title to be considered nerves only on evidence which, if applied to the uterine nerves, answers directly in the affirmative." (p. 47.)

Nothing can be more unsatisfactory than such arguments. There is not a single point of view, from which their irrelevancy is not obviously discernible. Dr. Lee finds nerves and ganglia, where accomplished anatomists, accustomed to minute dissection, and carefully prosecuting similar researches, had failed to discover them. His dissections are scrutinised by many other anatomists, several of whom deny *in toto* their claim to the character of nerves. Yet every one of these would say,—if Dr. Lee will *prove* these to be nerves, by that which every histologist now recognises as the only legitimate test, we will readily accord to him all the credit he demands. Will Dr. Lee or his partisans continue to affirm the existence of these nerves and ganglia, if microscopic research proves that neither nerve-tubes nor ganglionic cells exist in them?

A case of suspected poisoning occurs. One chemist examines the contents of the stomach, and can find by the liquid tests no indications of arsenic. Another, by some variation in the mode of using the same tests, discovers what he considers sufficient evidence of the presence of the metal. Is not the court of justice, in such a case, fairly entitled to demand *all* the evidence that the case admits of, and to require that the reduction of the arsenic to the metallic state should furnish the only unmistakeable proof of its previous existence in the stomach?

Even supposing, then, that the uterine nerves were as clearly displayed by Dr. Lee as the nerves of the face, or of any other part about whose nervous supply there is no question, the very fact that others have failed and do fail to bring out the same results, is of itself a sufficient reason for requiring more evidence. But there is ample ground for the assertion that this evidence is more required in the case before us than in any other.

For, on the one hand, the nerves of the sympathetic system in general contain a much larger amount of ordinary fibrous tissue mixed up in their trunks, than do those of the cerebro-spinal system; whilst, on the other, the tissue of the uterus itself is so indistinctly muscular, so little different—either to the naked eye, or under a low magnifying power,—from ordinary fibrous textures, that there is not the same facility of isolating the supposed nerve-trunks from it, as there is in the case of ordinary nerves and muscles. When, moreover, we look at the supposed *centres* of the sympathetic system, and see how small a part of the ganglionic enlargement is frequently due to the presence of vesicular matter, the greater portion arising from the junction of nerve-trunks, and from the introduction of a considerable amount of fibrous tissue, we feel that we have a right to demand that any *new* ganglia should be submitted to the test which has been applied to most of the old ones, and that the presence of the characteristic ganglionic corpuscles should be demonstrated in them, especially when a doubt exists as to their connexion with the nervous system at all. Dr. Lee, we have understood, affirms that he takes his stand upon the definitions of nerves and ganglia which have been in vogue amongst anatomists for the last two thousand years, and refuses to admit that the microscopic test *can* demonstrate a fallacy in his researches. If this method be adopted by other anatomists, we may bid adieu to all progress in our science. The wisdom of our ancestors is to be our only guide. Of that which *their* eyes saw and their hands touched, we are forbidden to learn anything more. Everything must remain exactly as they left it, except when the extension of our knowledge is obtained by their own methods.

It must be specially remembered in the present case, that the term “ganglion” has acquired of late years a definite *physiological* signification, very different from its original sense, which was purely *anatomical*. It formerly implied, as its name imports, merely a knot-like enlargement upon a nervous cord; and of the uses of such enlargements no definite idea could be entertained, when it was believed that the brain was the sole source of nervous power in the body. *Now*, on the other hand, we mean by a ganglion a collection of vesicular matter, which serves as a centre of nervous power to certain fibres connected with it; such a ganglionic centre may exist without any obvious external enlargement; and enlargements may exist on nervous cords without the presence of ganglionic corpuscles in their interior. Hence when Dr. Lee justifies his statement by appeals to the usage of the older anatomists, he entirely leaves out of view the complete change which, by the tacit consent of the whole scientific world, has taken place in the acceptation of the term ganglion. His assertions only go to the extent of proving the existence of knot-like enlargements upon the nervous cords,—supposing these last *to be* really nerves; the physiologist cannot accept the statement, as proving the development of new centres of nervous power during the enlargement of the uterus, until it shall have been shown that these enlargements contain vesicular neurine. If Dr. Lee had accepted the offer of experienced histologists, to test his assertions by the microscopic examination of portions of his supposed nerves and ganglia dissected from a recent uterus, the question would not have remained in the uncertainty which still clings around it. No such examination of preparations preserved in spirit can be at all satisfactory; since the “gelatinous” nerve-fibres, when hardened by

alcohol, lose their distinctive characters to such a degree as to be nearly if not quite unrecognisable.

Our own opinion, then, is decidedly in favour of the *à priori* probability of Dr. Lee's assertions as to the increase of the nervous structures of the uterus during pregnancy; whilst, on the other hand, we feel ourselves called upon to refuse our assent to the doctrine, until Dr. Lee shall have furnished the evidence in its support, which is so decidedly called for, in our opinion, by the peculiarities of the case.

A considerable stress is laid by Dr. Tyler Smith on Dr. R. Lee's alleged discoveries, as bearing out his own views. We do not see, however, that the two have much connexion, since the novelty of Dr. T. Smith's doctrines chiefly consists in the stress he lays on the *reflex* actions of the uterus and other parts of the genital system, which, as he says over and over again, are dependent upon the nervous connexions of the organs with the spinal cord, the proper ganglia of the uterus (if such exist) not taking part in them. Hence we shall discuss the questions which he raises, as to the movements of the uterus, on their own merits alone.

Movements of the uterus. The chief novelty which we find under this head is in regard to the relative importance of the "reflex" and the "peristaltic or immediate" actions of the uterus. The latter are thus described by our author :

"All muscles contract when subjected to immediate irritation, after they have been cut off from the influence of the cerebral and spinal centres, and when they have been as far as possible deprived of their nerves. In the case of muscles supplied by cerebral or spinal nerves, the contraction is limited to the spot irritated, and ceases with the removal of the irritation; but, in organs partly or wholly supplied by ganglionic nerves, as the heart, bladder, intestines, œsophagus, &c., the motion produced is of a peristaltic kind, spreading, generally in a vermicular manner, to a distance from the point of irritation, and continuing for some time after the irritation has ceased. The uterus is eminently endowed with this form of contraction. When one point of the uterus is stimulated through the abdominal parietes, the contraction excited extends with the utmost rapidity to the whole organ; the same occurs when the fingers are made to irritate any point of the internal surface of the uterus. Harvey beautifully described this peristaltic form of action in the uterus of the doe; William Hunter saw it in the cat and rabbit; Professor Müller in the uterus of the rat and the oviduct of the turtle; I have seen the same thing in the uterus of the guinea-pig and other animals. *This form of motor action is the basis of the other uterine actions.* In natural labour in the human female, it is, so to say, disguised by the reflex actions; but, under certain circumstances, it is able to effect, unaided, the expulsion of the child." (p. 39.)

All this is perfectly correct; and the sentence which we have italicised most accurately expresses a truth, some of whose applications we consider that Dr. T. Smith has overlooked, in his zeal to make the most of the reflex actions of the uterus. In fact, the sentence which concludes the foregoing extract, contains, in our apprehension, an essential fallacy. The peristaltic contractions of the uterus in ordinary labour are in no respect "disguised" by the reflex actions; for the whole character of the uterine action is as completely peristaltic in the spontaneous "pains" of labour, as it is when it responds to mechanical stimulation; and, though we are far from denying the participation of the nervous influence radiating from the spinal cord in exciting this contraction, yet we believe that we shall

be able to show that its operation is very different from that which is exerted upon the muscles usually styled "voluntary," and consisting of striated fibre.

That the spontaneous contractions of the uterus may, of themselves, effect the expulsion of the child, is proved by instances in which delivery has taken place by this means alone; as in cases of paraplegia from disease of the lower part of the spinal cord, and of post-mortem parturition. Yet, as to the former, Dr. T. Smith remarks, that "patients in this state, and animals reduced to this condition by experiment, have been delivered by the stimulus of galvanism applied to the uterus itself;" and then goes on to say, that, "when cut off from the spinal centre, the uterus *requires* the stimulus of galvanism or some external irritation to excite it to contraction." He does not refer to any other experiments than those of Brachet in support of this last statement; and the testimony of that rather doubtful authority is as follows. In his first four experiments, the spinal cord was divided high up in the dorsal region, at the commencement of labour; the uterine contractions were nearly or entirely suspended, and the animals died undelivered. In the fifth experiment, labour having been fully established, the spinal cord was divided at the last dorsal vertebra, and all contraction was at once suspended; upon the application of a voltaic current, however, the uterine action recommenced, and three young ones were brought into the world; but the animal died, after five hours, with five young ones still in its womb. In the sixth experiment, after a poor guinea-pig had been in labour for an hour, the spinal cord in the sacral region was entirely destroyed, and severe flesh wounds were inflicted over the vertebral column; yet the uterine contractions continued, and five young were delivered within two hours.—Now it is perfectly obvious to us, that in the former of these cases, supposing the results to have been as described by M. Brachet, the suspension of the uterine action was due rather to the *shock* of the operation, than to any other cause. In fact, the severance of the spinal cord would by no means cut off the uterus from reflex influence, although it would prevent the agency of volition or emotion. This point does not seem to have occurred to Dr. T. Smith. The influence of the shock produced by severe injuries of the cerebro-spinal nervous system, upon organs whose habitual actions are independent of it, is abundantly manifest in the case of the heart; and every accoucheur is well aware how liable is the action of the uterus to suspension from remote causes of this nature. The speedy death of the animals which had sustained this injury, is an indication of the severity of its general effect upon their systems. The influence of galvanism, in the one case in which it was applied by M. Brachet, seems to have been exerted simply in *recalling* the uterine action, which the shock had suspended; the animal died notwithstanding. This, so far as we are aware, is the only case of the kind on record, in which galvanism has been employed; and it by no means bears out Dr. T. Smith's assertion. In the cases of uterine inertia in the human parturient female, in which it has been applied by Dr. Simpson and others, we believe that its results have been by no means satisfactory.—The sixth of M. Brachet's experiments shows that the uterine contractions are independent of the *sacral* portion of the cord; but this is obviously not sufficient to prove their complete independence of the cerebro-spinal axis.—To these he adds a case of pa-

raplegia, apparently occasioned by disease of the lower part of the spinal cord, which paralysed everything below the pubes, so that coitus took place without any sensation of pleasure. After the usual term of gestation, however, labour commenced, but in a very imperfect manner; the uterine contractions were extremely slight, but they were sufficient to dilate the os uteri and to expel the liquor amnii when the membranes were ruptured; and at last the delivery was effected by the forceps. It is evident that, in this case, the contractility of the uterus itself was very imperfect; for it could not be called into existence by the usual stimuli applied to itself; this might be owing to that imperfection in the nutrition and muscular development of the uterus, which we usually see in paralysed parts. Here, we suspect, galvanism would have been of no use whatever.

The most complete proof of the independent contractility of the uterus is afforded by the occasional occurrence of post-mortem parturition. On this point Dr. T. Smith observes:

“A slow reflex action of the uterus may possibly continue long after the rhythmic respiratory actions have ceased, as long, indeed, as the body retains its warmth. But we know that the heart, œsophagus, and intestines may be excited to peristaltic action after death; violent peristaltic action is often a part of the act of dying; and I am of opinion that, in the human uterus, it is peristaltic action chiefly which expels the child when the mother has died during labour, undelivered. There is, however, another source of post-mortem muscular contraction, which I believe has never been referred to the case of the uterus,—this is, the post-mortem muscular spasm. We know that, in the case of an analogous organ,—the heart,—the *rigor mortis* occurs to such an extent as to empty the ventricles of blood, and even to simulate concentric hypertrophy.....The *rigor mortis*, therefore, ought to be considered as another cause of post-mortem delivery.” (p. 40.)

Now, in the foregoing extract, three distinct causes are assigned for the phenomenon in question. The first is purely hypothetical. We have no reason whatever to believe in the possibility, much less in the actual existence, of “a slow reflex action of the uterus,” continuing after somatic death. For the two others, we have obvious evidence derived from analogy; and we are glad to find that Dr. T. Smith so distinctly recognises the resemblance of the action of the uterus to that of the heart and alimentary canal, which, as is well known, may persist when these organs are altogether separated from their connexions with the cerebro-spinal system. We have ourselves been accustomed to refer the occurrence of post-mortem parturition to the peristaltic action of the uterus; but we have little doubt that Dr. T. Smith is perfectly correct as to the third cause, which was for the first time assigned by himself,—the post-mortem rigidity being far more remarkable in the muscular walls of the visceral apparatus, which are least subordinated to nervous influence, than it is in the muscular system of animal life.

We do not find any unwillingness, then, on Dr. T. Smith's part, to admit the fact that the uterus may contract powerfully on its contents, so as even to expel them, in virtue of its own inherent endowments, when the requisite stimulus has been given; and he even states that this form of motor action is the basis of the other uterine actions. But he seems to consider that this peristaltic movement must be excited by a stimulus *ab extra*, in order that parturition may be properly induced; and for this

stimulus he looks to the influence of the spinal cord, called into reflex action by some remoter excitant. The amount of this influence, and its *modus operandi*, next require our attention; and we shall in the first place cite Dr. T. Smith's account of *reflex* uterine action:

"The reflex actions of the uterus are very numerous; and it is upon these, and the numerous extra-uterine reflex actions excited during the process, that the natural performance of parturition essentially depends. Contraction of the uterus from irritation of the mammæ, as in the act of suckling the child; contraction of this organ from the cold-water douche, applied to the vulva or the abdominal surface; contraction excited by irritating the rectum, as by stimulating enemata; or of the stomach, by drinking a gulp of cold water; of the ovaria, by the presence of the menstrual nîsus; of the vagina, by manual irritation, as in "taking a pain;" of the os uteri by irritation, as in the introduction of the hand into the uterus,—are all to be considered as so many instances of reflex spinal action. Thus, in parturition, the uterus may be excited, in a reflex form, by irritation of the mammary incident excitor nerves; the pubic and abdominal branches of the intercostals; the rectal; the gastric division of the pneumogastric; the ovarian nerves; and also by the nerves of the vagina, and the os and cervix uteri." (p. 35.)

All this is perfectly true: the facts are familiar to obstetricians; and the rationale of them will at once occur to every one acquainted with the physiology of reflex action. If they are not so explained in recent obstetrical works, it has been because too many of the so-called practical men think it quite unnecessary to keep pace with the progress of science. That the facts in question *had* been recognised and explained in the very same manner, before Dr. T. Smith first directed public attention to them, will be obvious to the reader of the following extract from a review of Dr. M. Hall's 'New Memoir on the true Spinal Marrow,' contained in the 'Brit. and For. Med. Review,' for Jan. 1844 (p. 179). After quoting Dr. Hall's anticipations of improvement in the obstetric art, from the application of reflex physiology to that department of medicine, the reviewer continues:

"We rather wonder that Dr. Hall should not have adverted to the operation of several of the means employed by the accoucheur to bring about the contraction of the uterus in cases of hemorrhage after parturition, as examples of this kind of action. Every one knows the efficacy of friction upon the abdominal surface; where this fails, the sudden contact of the hand, previously cooled by immersion in cold water, will often produce the effect; if this should not succeed, the uterine contraction may often be brought about by the sudden immersion of the patient's hands in cold water; and the continuance of the hemorrhage requires the still more violent remedy of a dash of cold water over the surface of the trunk. Or the application of the child to the nipple has been said to produce uterine contraction, when other mild remedies have been unsuccessfully tried. It can scarcely be necessary to point out, that reflex action is evidently the channel of all these operations."

Illustrations of reflex uterine action may further be drawn, as Dr. T. Smith very justly remarks, from the phenomena of abortion, which frequently exhibit the influence of remote irritation, acting through the spinal system of nerves, in a way that can scarcely be mistaken. A due appreciation of these facts is of the greatest importance in guiding our endeavours for the prevention of abortion.

Fully coinciding with Dr. Smith in the foregoing particulars, we yet feel ourselves unable to discern that they have any direct bearing on the *normal* physiology of parturition. That the uterus is susceptible of being

excited to contraction by reflected stimulation, no more proves that its ordinary actions are dependent on the spinal cord, than the influence of remote excitants upon the actions of the heart and alimentary canal proves that these actions are essentially reflex in their nature. In fact, it does not appear to us at all difficult to show that the influence of the nervous system on the uterus is precisely of the same character with the influence of any stimulus directly applied to that organ, and is very different from the ordinary forms of reflex action as seen in the muscular system of animal life. In the latter case, irritation of a motor nerve supplying a muscle excites immediate and vigorous contractions in every part of it at once; whilst direct irritation of any part of the muscle will produce a contraction limited to that part. In the former, it is very difficult to obtain any evidence by experiment as to the influence of the nerves in producing muscular contraction, so slight, in comparison, is the power they exert; and observation of the contractions which do follow the reflex irritations already alluded to, shows that, instead of being consensaneous and powerful through the whole organ at once, they are equally *peristaltic* with those which originate in direct irritation. Thus it appears that, when uterine contraction is excited through the nervous system, it is not by a new and powerful agency, taking the place of the feebler powers of the organ itself, but by the application of a new stimulus, which causes *remote* irritation to have the same effect as one of a direct or immediate kind; so that the peristaltic contraction of the uterus is really its *only* mode of expelling its contents.

It is by no means impossible that, although he does not say as much, Dr. T. Smith may be ready to admit the correctness of our view; his main object being, as we have already seen, to establish the dependence of the *commencement* of the parturient action upon a reflex stimulation derived from the ovaria, rather than to show that the uterine contractions, when once established, derive their force from nervous power. To this question we shall return when we have considered the other muscular movements concerned in parturition. The following is Dr. T. Smith's general account of them:

"But besides the uterine reflex actions, there is, in the expiratory actions supervening in the course of labour, from the irritation of the presenting part of the fœtus, another remarkable series of reflex parturient actions, extra-uterine in their seat, but which combine and harmonize in a remarkable degree with the reflex actions proper to the uterus. It is found that tumours in the vagina, or the introduction of the hand of the accoucheur, may produce the same result as the pressure of the presenting part of the child in labour. It has been noticed of tumours, which produce little or no motor disturbance while they remain in the cavity of the uterus, that no sooner do they pass from the uterus into the vagina, than 'bearing pains,' as they are termed, are produced. In other words, the action of the expiratory muscles is excited in a reflex form by irritation of the vaginal mucous surface." (p. 36.)

In the subsequent more detailed account of the phenomena of parturition (pp. 153 et seq.), we find Dr. T. Smith maintaining still more decidedly, that the extra-uterine reflex actions are not called into play until after the stage of dilatation is completed, and the fœtal head is beginning to press upon the vaginal walls. We cannot but regard this as an error, being very sure that the expiratory effort is often involuntarily connected with the uterine "pains," when as yet the os uteri is only partially dilated,

and nothing but the bag of membranes is protruded through it. In his general doctrine, however, that the remote actions are excited by the pressure of the ovum against the several parts of the genital canal, in the course of its expulsion, we heartily coincide; and it seems to us, that the germ of this view is distinctly contained in the following passage, cited from the first edition of Dr. Carpenter's 'Human Physiology' by Dr. T. Smith, in order to show the entire ignorance which prevailed as to the phenomena of parturition before his own researches began to throw light upon it:

"It seems by no means unlikely that the uterus, though not itself dependent upon the spinal cord for its power of contraction, may contain numerous afferent or excitor fibres, and that these, being compressed by the efforts of its own muscular structure, may propagate to the spinal cord the stimulus necessary for the consentaneous action of the assistant muscles. Those who may watch a labour with an attentive consideration of its phenomena, will find that the 'pains' usually commence in the uterus itself, and that it is only when they become decided, that the power of other muscles is called into operation." (p. 25.)

We really do not see that Dr. T. Smith has advanced our knowledge of the matter in any great degree beyond this, save in the more distinct statement, that the excitor stimulus to the remoter actions is probably given by the pressure of the presenting part against the portions of the genital canal which it successively encounters: and in limiting their first excitation to the entrance of the head into the vagina, we believe him to be decidedly in error.

We now, however, approach the topic which involves what Dr. T. Smith regards as his great discovery; and this topic we shall introduce with the question,—*What is the primary stimulus to the uterine actions in parturition?* In order to present Dr. T. Smith's views on this subject in their full force, we must follow him in his survey of the phenomena of menstruation, gestation, and abortion.

Menstruation. Dr. T. Smith adopts to its full extent "the modern theory of menstruation, which makes it dependent on the periodical excitement of the ovaria, and the escape of mature ova from the Graafian vesicles; the secretion of the catamenial fluid being secondary to the ovarian excitement;" and regards as fully proved "the fundamental analogy between menstruation in the human female, the œstruation of mammalia, and the oviposition of oviparous animals." (p. 77.) In the greater part of these expressions we entirely concur; but, as we have recently observed in another connexion (Vol. III, p. 141), we cannot admit their complete accuracy. No one, we believe, has made a larger series of observations on this subject than Dr. Ritchie, of Glasgow; and he distinctly states, as the results of his inquiries,—1. That even during the period of childhood, long before the appearance of the catamenia, there is a continual rupture of ovisacs and discharge of ova at the surface of the ovarium. 2. That although, after the establishment of the catamenial flow, the discharge of ova takes place *most frequently* at the menstrual period, the two occurrences are not necessarily coexistent; for menstruation may take place without any such rupture; whilst on the other hand, the maturation and discharge of mature ova may take place in the intervals of menstruation. These *facts* are alluded to by Dr. T. Smith, with a

strange confusion of terms, as "aberrations of the ovular *theory* of menstruation (p. 82); and he thus expresses himself in regard to them :

"The conclusion I should draw from Dr. Ritchie's papers is, that in certain cases the ovarian action of menstruation is incomplete; that though it excites the catamenia, it is insufficient to cast off an ovule. The ovules shed in the intervals between the catamenial periods, without any uterine discharge, are probably either immature, like those of childhood, or they may escape from the ovaria without a sufficient degree of ovarian excitement to set up the catamenia, being in this respect the antithesis of those other cases in which there is uterine secretion without the extrusion of ova from the ovarium. In perfect menstruation, both ovarian excitement, and the escape of an ovule concur; but the ovarian excitement is absolutely indispensable." (p. 82.)

From the statements quoted by Dr. T. Smith, with laudable impartiality, in his appendix, we gather that he finds himself obliged to allow a somewhat greater force to these exceptional cases. He quotes from a letter addressed to him by his *magnus Apollo*, Dr. Power, which is confirmatory of the doubt which we formerly expressed. "I am not satisfied," says Dr. Power, "that the bursting of a Graafian vesicle and escape of the ovule of Baer is the general occurrence or law, and am inclined to believe that the ovule, when impregnation does not take place, having lost its vitality, is not unfrequently absorbed within the ovarium." And he cites the cases recorded by Dr. Ashwell, which go to prove that the menstrual flow may take place with regularity for some years, and yet no signs of the rupture of Graafian vesicles or of the escape of ovules may be discernible on the surface of the ovarium. At the same time, he cites Dr. Girdwood's explanation of the case of the Jewish women referred to by us (loc. cit.); which is to the effect that conception in them generally takes place immediately *before* the catamenial period,—which we think by no means unlikely.* Our own inquiries on this subject lead us unhesitatingly to subscribe to the doctrine that conception is far more apt to take place immediately before or after the catamenial period, than during the interval; but, on the other hand, we have obtained distinct evidence that impregnation *may* occur at a period midway between two periods. We cannot, therefore, admit the validity of Dr. T. Smith's conclusion, that, in the exceptional cases in which ova are discharged independently of the menstrual flux, these ova are always imperfectly developed. Still less can we admit that these exceptional cases are to be passed by, in drawing from the phenomena in question any general conclusions which are to serve as a foundation for other doctrines. It is Dr. T. Smith's object to show that the uterine excitement is dependent upon the ovarian; the catamenial secretion from the uterus being the result of a sympathetic action propagated from the ovaries. To us, on the other hand, it appears that the two phenomena are in some degree independent, though closely linked together,

* We are surprised to find among those who adopt the modern doctrine of oestruation and menstruation, so little recognition of the possibility of conception occurring from a coitus anterior to the catamenial period. Dr. Meigs does not once allude to it. Among many of the lower animals, it is well known that a single coitus will fertilize a long succession of ova, of which the latest could not have been near maturity when the spermatic fluid was received by the female; and there is no reason why that fluid should not retain its vitality for a few days, as well in the passages of the female, as in those of the male genital apparatus. We are satisfied that the occurrence is by no means unfrequent in the human female; the catamenia being sometimes interrupted altogether, sometimes diminished in amount and duration, but sometimes recurring nearly as usual.

as are other phenomena having a common purpose. The ovarian excitement tends to the maturation and discharge of ova; the uterine, to the formation of a decidua. The former may never be perfected, and yet the latter may completely take place. On the other hand, the former may take place effectually, and yet the latter may not occur in any degree consentaneously. Again, the phenomena of suspended and of vicarious menstruation appear to us to indicate that the periodicity is not simply ovarian but constitutional; the periodical loss of a certain part of the contents of the sanguiferous system being required, for the maintenance of the healthy condition of the system at large. Fully admitting that in a large number of cases amenorrhœa is rather the result than the cause of the general disorder, we yet think it impossible to contemplate the results of the suspension of normal menstruation by some local cause, without coming to the conclusion that, whether the menstrual flux consist of pure blood, or of certain of its constituents more or less altered, its removal from the sanguineous current is required to keep the latter in its due condition. This we can understand, even if these constituents should be no others than those adapted to become organized into a decidual membrane; since, as Mr. Paget (following Treviranus) has pointed out, the constituents of any organized tissue stand to the remainder of the body in the condition of materials destined for excretion.

We are disposed, then, to regard the uterine excitement as by no means so entirely dependent upon the ovarian as our author would have us believe. Although usually consentaneous, either *may* occur independently of the other; and there is no constant correspondence between the degree in which the two organs respectively are affected. Here, as elsewhere, we see the necessity of attending to the *exceptional* cases. To put them aside as mere "aberrations of a theory," is obviously unphilosophical. No theory can be complete which does not include the unusual as well as the ordinary phenomena; and it frequently happens that the greatest amount of information is obtained respecting the conditions of the latter, from the careful observation of the former.—In his zeal for the prerogative functions of the ovaria, Dr. T. Smith goes so far as to assert his conviction that the greater part of the pain in dysmenorrhœa consists in ovarialgia; "the deep lumbar pain being decidedly ovarian and not uterine." Dr. T. Smith's experience of this disorder must, we apprehend, be rather limited, or his observations must be very much swayed by his preconceived hypothesis; for many a woman can assure him that her monthly pains bear a close resemblance to those of the first stage of labour; and he will not, we presume, assert that *these* are entirely ovarian. "Uterine disturbance must be considered as a secondary condition—an aggravated symptom of ovarian excitement in painful menstruation." He is "decidedly opposed to the view that dysmenorrhœa is *caused*, in the majority of cases, by chronic inflammation of the os and cervix uteri. The relation between dysmenorrhœa and inflammation of the os uteri is generally one of coincidence, not of causation, nay, it is often a symptom induced by ovarian irritation, a symptom requiring palliation, but the relief of which by no means constitutes the whole of our treatment." We would not go so far as to assert that the cause of dysmenorrhœa is to be found in the inflamed or strictured condition of the os uteri "in a majority of cases;" but that in a large

number of instances, the violent pains and spasms attending the return of the catamenial period are directly consequent upon the morbid condition or malformation of the uterus, is proved by the immediate relief which the judicious treatment of these conditions affords. If ovarian disorder in such cases participate at all in the general distress, it must therefore be as a secondary consequence of the undue uterine excitement.

Here, as in numerous other cases, Dr. T. Smith mistakes his own convictions for facts, and reasons upon them accordingly. For example, at p. 52 he tells us that the Fallopian tubes have a peristaltic movement, and that, during œstruation in the lower animals, and menstruation in women, they apply themselves firmly to the ovaria. Not content with this undeniable fact, he goes on to speculate (p. 53) that during coition there "may be an action of the Fallopian tubes similar in kind to that affecting the ejaculator muscles, and the bulb of the urethra. At all events," he continues, "it is extremely probably that during coitus contractions of the tubes upon the ovaria take place." Now in what this strong probability consists, we must own ourselves to be at a loss to discover. The close application of the trumpet-shaped mouths of the Fallopian tubes to the surface of the ovary, and the peristaltic movements of the tube itself, have evident reference to the reception of the ovum at the time of its escape from the Graafian vesicle, and to its transmission to the uterus. The occurrence of such phenomena at any other period than that at which ova are being matured preparatory to their escape, would be altogether meaningless; and the supposed analogy of the contractions of the ejaculator muscles and the bulb of the urethra is no analogy at all, because these contractions form the very essence of the coitus. Notwithstanding the entirely gratuitous character of the hypothesis, Dr. T. Smith goes on to argue that this movement of the tubes must be reflex in its character, dependent upon ovarian excitement; whereas, if he had ever looked at the parts in a rabbit in heat, he must be aware that they are all so turgid with blood, as to be in a state legitimately comparable to erection, and that the movements are of that peristaltic kind which, as we have before urged, is independent of nervous stimulation. Nevertheless, this supposed action of the tubes in coitus is repeatedly referred to by our author, as if it were all but an established fact, and as affording an additional example of reflex action excited by ovarian stimulation.

Parturition. We pass over, for the present, Dr. T. Smith's hypothesis of the cause of the abdominal movements felt by pregnant women; as the discussion of it would lead us too far away from our inquiry into the validity of his doctrine regarding the origin of the parturient actions. The following extract concisely states the views which he has been led to entertain upon this subject, and which are believed by him to constitute an important advance in the physiology of parturition:

"I believe that at the time of parturition in mammalia, the uterus and the uterine nervous system are excited by the ovaria; that it is the ovarian excitement which induces both the permanent contraction of the uterus immediately before the coming on of labour, and the tendency to those reflex, emotional, and peristaltic actions, by which parturition is completed. In menstruation, a small synergic and reflex arc is described between the ovaria and the Fallopian tubes: in parturition, a larger arc is in operation, extending from the ovaria to the uterus. According to my researches, the excitement of the uterine nervous system at parturition, upon

the presence of which the due performance of this function depends, is caused by ovarian excitement. At the time of ordinary menstruation, the ovarian irritation which excites the contraction and rigidity of the Fallopian tubes is manifest. Throughout utero-gestation, the ovarian excitement returns in a slight degree at each periodic date; but at the eleventh period after conception (reckoning the last catamenial period inclusively) the ovarian excitement returns in full force, and, as a consequence, the uterine excitability and the uterine actions of labour begin." (p. 111.)

According to Dr. T. Smith, therefore, the muscular excitability of the uterus at the full term is dependent upon the ovarian excitement; and the occurrence of its contractions on the 280th day (or thereabouts) after conception is a reflex phenomenon, for which the exciting cause is to be found in the eleventh periodical access of ovarian excitement. We quote the following passage from the Appendix, in order to show how much of this doctrine is regarded by Dr. T. Smith as his own, and how far he considers himself to have been anticipated in it by those obstetricians, who have held similar views with himself as to the coincidence between the ordinary duration of pregnancy and ten menstrual periods:

"The following extract from Dr. Fleetwood Churchill's work on the 'Theory and Practice of Midwifery,' shows how perfectly the idea of the regular duration of pregnancy, and the relation of parturition to the catamenial periodicity, may exist, without any idea of the cause of labour. Dr. Churchill is perfectly conversant with 'the fact,' that the average duration of labour is 280 days, or a time equalling ten catamenial periods; and he suspects that labour comes on at the tenth period after impregnation; but the 'exciting cause' is quite undiscovered. It is the knowledge of an excitor spinal nerve in the ovary which is the one great point wanting, and without which the mere fact of periodicity is confused and of little value. I insist upon this point strongly, because attempts have been already made by those who confound *cause* with mere *coincidence* and *time*, to set aside my claim to whatever merit may lie in the detection of the cause of labour. The passage I quote proves that it is quite possible to have a tolerably accurate notion of the 'utero-ovarian periodicity' of pregnancy, and yet to consider that 'all search has hitherto failed in discovering the exciting cause of labour.' Obstetricians might have gone on, as they had done before, from generation to generation, repeating to themselves the fact, that the duration of gestation is a multiple of the catamenial period, without any conception of the ovario-uterine nervous arc, which excites the uterus to parturient action." (p. 392.)

If we recall to the minds of our readers the expression we have previously cited from the preface, that "no one had perceived that the relation of the ovarian nerves to parturition is the same as the relation of the pneumogastric nerves to respiration," we think that our readers will have a distinct notion of the claim which Dr. T. Smith puts forward as an original discoverer. Granting for a moment that his doctrines are correct, and that the evidence which he has adduced in their support is adequate to establish them, we are as ready as he can desire to accord to him the credit to which he considers himself entitled; since we fully recognise the distinction which he draws in the preceding extract. But, unfortunately, we can neither admit that he *has* produced sufficient evidence in support of his doctrines to entitle them to be received as physiological verities, even if no opposing facts could be adduced to render them otherwise than probable; nor can we think that Dr. T. Smith has taken sufficiently into account the facts which run counter to his views. We are far from asserting that he has *wilfully* shut his eyes to these; but we

trace here, as elsewhere, the indication of a want of that comprehensiveness of survey, which is an essential part of the truly philosophic character.

We shall now go into this question in some detail; endeavouring to give a fair summary of the evidence actually adduced by Dr. T. Smith in support of his views, whilst we point out the deficiencies in his proof, and the objections to his doctrine. Let him supply the deficiencies, and remove our objections, and we will most readily concede to him all the credit he demands.

In the first place, it might be supposed, from the language used by Dr. T. Smith, that he had *experimentally demonstrated* "the ovario-uterine nervous arc, which excites the uterus to parturient action;"—that he had furnished some kind of visible evidence that "the relation of the ovarian nerves to parturition is the same as the relation of the pneumogastric nerves to respiration." Physiologists have a right to expect such evidence. They do not believe that the pneumogastric nerve is an excitor of the respiratory actions, on the strength of Dr. Any-body's "conceptions" or "convictions," or on the ground of inferences founded on its anatomical distribution and connexions; but because it has been shown, over and over again, that irritation of the pulmonic branches of the vagus excites an inspiratory movement through the nervous centres, and that section of the trunks of that nerve is constantly followed by a great diminution in the rate of the respiratory actions. So, again, in the case of the act of deglutition, we rely upon experimental demonstration for the conviction that the glosso-pharyngeal nerve is the chief excitor, and the pharyngeal branches of the par vagum the chief motor nerve, concerned in the process. Now Dr. Tyler Smith has not added one particle, either by new anatomical researches, or by experimental demonstration, to our knowledge of the "ovario-uterine nervous arc." He has not proved that any more direct communication exists between the ovaries, the uterus, and the spinal cord, than that which is established through the spinal fibres intermingled with the sympathetic; whose endowments differ so much from those of ordinary spinal nerves, that any arguments founded upon the analogy of the latter are unsafe in their application to the former. He has not shown that irritation of the uterine nerves will at any time excite parturient actions in the uterus. He has not shown that section of these nerves prevents the access of the parturient actions at the usual period. Still less has he shown that irritation of the *ovarian* nerves will excite uterine contractions by reflex agency; or that section of the ovarian nerves, by interrupting the continuity of the "ovario-uterine nervous arc," will prevent the access of the parturient efforts of the uterus at the regular period. All this he must demonstrate experimentally, before he can be admitted to have established his position, that the ovarian nerves stand in the same relation to the act of parturition as do the pneumogastric to that of respiration. Not one particle of evidence on this subject is adduced by him, except that afforded by the experiments of MM. Brachet and Segalas, of which we have already disposed. Before leaving this part of the inquiry, we must remark that Dr. T. Smith seeks to establish a relation even more intimate than that to which he refers for illustration; for every physiologist knows that there are many excitors to reflex action in addition to the pneumogastrics; so that section of the latter diminishes the frequency of the respiratory movements, but does not suspend them altogether;

whereas Dr. T. Smith refers so exclusively to the ovarian nerves as the excitors of parturition, that this action would seem, in his view, to be entirely dependent upon the stimulus which they supply.

The evidence upon which Dr. T. Smith relies, is to our minds of far too vague and unsatisfactory a nature to be admitted as *establishing* a principle of such importance, even if there were no opposing considerations of any weight. It consists of little else than a reiteration of the facts and arguments he has already adduced respecting the coincidence between uterine action and ovarian excitement; together with other facts, the relevancy of which we cannot admit. "Very early in the present inquiry," he says, "I saw that the cause of labour in the human female must also be the cause of all the parturient phenomena of the animal kingdom; and this set me to observe and deduce from the parturient actions of the lower animals, in order to explain those of the human subject." Now very nearly all these illustrations are derived from the phenomena of *ovi-position*, between which and *parturition* there is no real analogy. We cannot but feel surprised that so acute an observer as Dr. T. Smith should not have perceived that what he terms the "parturition" of oviparous animals is analogous to the transit of the ovum through the Fallopian tubes into the uterus of mammalia; and that the whole of the subsequent phenomena peculiar to the latter class must be considered as belonging to a distinct category. Dr. T. Smith speaks of the well-known phenomena attending the reproduction of frogs as *proving* "that the activity of the ovaria and testes produces a long-continued and involuntary contraction of certain muscles; and further, that they increase the excitability of the nervi-motor apparatus devoted to the function of reproduction." (p. 110.) How easy it is to find proofs, when such *coincidences* as these are regarded as sufficient! At a certain time of the year, the testes of the male frog and the ovaria of the female undergo a great increase of development, and attain a condition of functional activity. At the same period, certain parts of the nervous system exhibit a peculiar excitability, closely resembling that of tetanus; and this excitability, called into action by the mechanical contact of any object with the general surface, but particularly with the tubercles then developed upon the thumbs of the male, is evidently destined to maintain the prolonged sexual congress of these animals. The two phenomena are *coincident*; but what proof have we of the dependence of either of them upon the other? A physiologist who regards the nutritive actions as resulting from the agency of the nervous system, might quote this as an illustration of the activity of nutrition in a certain organ, consequent upon the excitement of the nervous centres connected with it, with just as much propriety as Dr. T. Smith quotes it in support of his doctrine of the dependence of the nervous excitability upon the development of the sexual apparatus. *Both* phenomena appear to us as likely to be the conjoint results of that which must be admitted, on either hypothesis, to be the cause of *one* of them,—namely, the seasonal change. Dr. T. Smith says that the motor phenomena "would not appear at all if the ovaria and testes were wanting;" what proof has he of this? Granting, however, that he is right upon all he affirms on this point, we again ask, what has all this to do with the phenomena of *parturition* in the human female, save in the way of remote analogy? This excitability of the nervous system has relation to the sexual congress, not to the ovi-

position ; the latter seems anything but a reflex act, judging by the gradual way in which the ova are voided during several weeks ; and even if it were, the uterine contractions of mammalia, by which the fœtus is detached from its placental connexion with the parent, and expelled alive, has nothing in common with the deposition of an ovum which has previously escaped from the ovarium, and which is lodged as a foreign body in the oviduct.

The whole of the evidence adduced by Dr. T. Smith from the comparative physiology of the mammalia, which alone, we maintain, can be admitted as bearing upon the question, is contained in the following paragraph :

“In many of the lower mammalia, we may witness the processes of oviposition or œstruation, parturition, congress, and conception, all going on as nearly as possible at the same time. In the guinea-pig, for instance, immediately that the young are dropped, the female admits the male, conception takes place, and a new utero-gestation commences, dating from the very hour of parturition. There is, in these animals, œstruation and ovulation going on in the ovaria, while parturition is taking place in the uterus ; and during or immediately after the expulsion of the young, the sexual heat develops itself. The same phenomena are present in all the mammalia, in a greater or less degree. In those animals of which we know the order of the œstrual periods, as the rabbit, the horse, and the cow, the duration of pregnancy is a multiple of the œstrual period. Doubtless this law is as extensive as periodical œstruation itself. Not only is gestation a multiple of the œstrual period, but the time of parturition is positively an œstrual period. The maturation of ova, which has ceased during utero-gestation, is resumed, and the sexual instinct is predominant, just as though the uterus did not contain the product of a former ovulation and conception. The presence of heat or œstruation, in parturient animals, did not escape the observation of Harvey, though the fact has never hitherto been utilised in physiology.” (p. 115.)

Granting, for the moment, the accuracy of Dr. T. Smith's facts, we must again point out that they amount to nothing else than *coincidences*. There is not the shadow of a proof that the œstrum, in the cases cited, is the *cause* of the parturient movements. There is nothing to disprove the assertion, if any one chose to make it, that the activity of the ovaria is dependent upon the accomplishment of parturition, as a consequence of the determination of blood to those organs,—the explanation usually given of the excitement of mammary action after parturition. And if it be affirmed that the relation of the periods of œstruation and parturition is altogether non-essential, we do not see what evidence to the contrary is afforded by the cases cited by Dr. T. Smith. Further, it is to be observed that all these facts are drawn from domesticated or semi-domesticated mammalia ; in which, as is well known, the excitement of the sexual apparatus loses much of its natural periodicity. How Dr. T. Smith can take upon him to assert, with as much coolness as if nobody even thought of questioning the fact, that “the same phenomena are present in all the mammalia, in a greater or less degree,” we are utterly at a loss to understand. He does not refer to a single authority in support of his assertion ; and whilst we are so ignorant of the history of the reproductive function in the greater part of the *feræ naturæ*, as not even to know with certainty whether the period of gestation in the wolf is or is not the same with that of the dog, it must be obvious that unless he has become possessed, by original observation, of a vast accession to our knowledge on this subject, he is altogether unjustified in the assertion we have cited.

Again, with regard to the "œstrual periods of the rabbit, the horse, and cow," of which the duration of pregnancy in these animals is said by him to be a multiple, we must ask whether the existence of these œstrual periods has been satisfactorily established, and by whom? Whatever may be the case with these animals in the state of domesticity,—in which, as in the common fowl, an ample supply of food, with warmth and shelter, tend to produce an increased frequency in the returns of ovarian excitement, as in the functional activity of the male apparatus,—we have no satisfactory evidence, so far as we are aware, that the same holds good of these or similar animals in a state of nature. In fact there is every probability to the contrary. In the cow, for example, the average duration of gestation is about that of the human female; but the period of heat or œstruation, both in the male and female, is only annual. What coincidence is there here? If it be said that there is a monthly œstrum in the female of wild cattle, we have only to reply that we shall not believe this until it be proved; and that all probability is against it, there being no corresponding œstrum in the male. But even were such a coincidence shown to exist, it would no more be sufficient to establish a relation of cause and effect between œstruation and parturition, than the *coincidence* between the periods of heat in the male and female of the same species entitles us to assert that the œstrum of either is *dependent* upon that of the other.

We have next to examine into the evidence adduced by our author from the phenomena of human parturition. Here, again, we shall cite his own words, to avoid the possibility of misrepresenting him:

"The duration of human pregnancy is well known to be a multiple of a catamenial period. It has also been observed that, in rare cases, where the duration of pregnancy exceeds the ten menstrual periods, the function of parturition is deferred to the following period, so as to make pregnancy reach to eleven periods inclusive. This fact came out most clearly in the obstetric evidence on the Gardner peerage case, and will prove of very great importance in cases of supposed criminality in wedlock, or in establishing the legitimacy of posthumous children. On the other hand, it is well known that when utero-gestation is brought to a premature termination, it is at what would have been a menstrual period that abortion usually takes place. There is, in fact, in all women, a tendency to abortion at the times represented by the catamenial periods. In placenta prævia, the hemorrhage occurring in the later months of pregnancy generally happens at the periodic dates. Hemorrhage sometimes occurs for two or three periods in succession before parturition in these cases, with perfect immunity in the intervals. The occurrence of uterine pains at the termination of the natural period of gestation, in cases of extra-uterine foetation, points very decidedly to the operation of some extra-uterine source of excitement. After abortion,—and very speedily, too, in some cases,—it is known that there is a remarkable aptitude to conceive; and during the catamenial climacteric, when the ovaria are irregularly excited, there is frequently an increased tendency both to conception and abortion. These accumulated facts are sufficient to establish the influence of the ovaria and of the ovarian function on the uterine phenomena of gestation and parturition. But to complete the evidence, we must inquire what proofs there are that the sexual excitement which attends exaltation of the ovarian irritability in the lower animals, is present in the human subject. An attentive observer will not fail to perceive the existence of this excitement, though in a rudimentary degree. I have insisted in a former lecture, that there is an actual increase of the sexual emotion during, or immediately after, the catamenial periods. There are also distinct traces of sexual excitement in some cases of parturition. That they are not always present does not tell against my

argument, because the reasons why they are not so present are very evident." (p. 117.)

Dr. T. Smith then goes on to state objections to the use of anæsthetic agents in parturition, on the ground of their removing the moral restraint which ordinarily keeps these feelings in check; and then returns to his position that "parturition does not merely occur at what would otherwise be a menstrual period, but *is essentially a menstrual period.*" The "show" is regarded by him as the equivalent of the access of the catamenial discharge; the lochial flow as its continuation, altered by the admixture of discharges from the placental site. He refers to the lochial flow which occurs after abortion in the early months of pregnancy, as indicating that it is something else than a discharge from the placental site; and he states that having examined the internal surface of the uterus after death in parturition, he has seen the whole of it dyed with the sanguineous secretion.

Now we might give to Dr. T. Smith the full benefit of the evidence by which he demonstrates that parturition ordinarily occurs in the human female at a catamenial period, without allowing that he has thereby advanced a single step beyond those who have gone before him in the demonstration of the *cause* of parturition. The coincidence may be, and we believe (for reasons presently to be given) that it is, totally destitute of any essential connexion; and we cannot see that Dr. T. Smith has gone beyond his predecessors in any other point, than in the origination of the *idea* of such a connexion,—which idea, however satisfactory to its originator, must be regarded by others as a pure hypothesis, until adequate evidence has been collected to render it, at the least, probable.

It is very easy to see how such a coincidence may arise. We will assume for the moment that the period of 280 days, or ten lunar months, has been fixed by the Creator as the term of gestation in the human female, quite irrespectively of the periodical returns of ovarian excitement. Now it is admitted on all hands that conception most frequently, if not invariably, occurs either immediately before, or immediately after, the catamenial flow. The period of parturition, therefore, would necessarily have a certain coincidence with that of the eleventh recurrence of the ovarian excitement in a very large proportion of cases; and yet this coincidence would involve no essential connexion. That this is the true view of the case, we think that we have evidence sufficient to demonstrate.

In the first place, if Dr. T. Smith's doctrine were rigorously true, the duration of pregnancy ought to be 280 days in those cases *only* in which conception takes place *immediately before* the regular period for the return of the catamenia; since the commencement of parturition answers, according to him, to the commencement of œstruation. In those cases, usually considered to form the great majority, in which conception takes place at an early period *after* the cessation of the catamenia, if labour comes on at the eleventh menstrual period, the intervening time must be 280 days *less* the time which elapsed between the date of conception and the commencement of the antecedent menstruation. Thus, for example, if the menstrual flux usually occupy five days, and conception take place on the sixth from its commencement, the eleventh recurrence of the menstrual flux, which ought, on Dr. T. Smith's hypothesis, to fix the date of parturition, will fall on the 280th day from that commencement, but

on the 274th day from the date of conception ; in other words, the duration of gestation would be only 274 days. Now we knew a case in which the date of two conceptions could be exactly ascertained. The first took place on the day before the catamenial period ; the menstrual flux appeared at the usual time ; but its duration and amount were greatly reduced. Labour supervened on the 280th day from the date of conception ; that is, at the time when the eleventh catamenial period would have commenced. But in another pregnancy in the same individual, the date of conception was the fourth day after the cessation of the catamenial flow, and this had lasted seven days ; nevertheless labour supervened as before, on the 280th day, that is *eleven days later* than the time for the eleventh recurrence of the catamenial period. This is not an exceptional case. We are confident that the general experience of accoucheurs will accord with our own, in the conclusion that where the date of conception can be exactly ascertained, labour may be expected to commence on the 280th day, without any reference to the question whether or not the conception took place before or after the catamenial period, which may be considered, on the average, to make a difference of at least a week. We have not found any reference to this difficulty in Dr. T. Smith's discussion of the subject ; in fact, in his 'Periodoscope,' he unhesitatingly assigns 280 days as the regular term of gestation, and gives directions for determining the various periods of expected abortion, &c., all of which are to date from the day of conception,—as if it were a matter of perfect indifference whether that conception had occurred before or after the menstrual period.

We have tested the correctness of Dr. T. Smith's hypothesis by the ordinary experience of obstetric practitioners. We shall now test it by exceptional cases, which afford still stronger evidence of the want of that *necessary* connexion between the two phenomena in question, which alone can give to them the relation of cause and effect. Every accoucheur is acquainted with instances in which the menstrual period is something different from 28 days ; for instance, 21, 25, or 30. Now if the term of gestation be necessarily a multiple of that period, it ought not to be affected by any such irregularity. Of such a modification, however, we have no evidence ; but, on the other hand, we have much proof to the contrary. In the case we have just cited, the catamenial period is only 26 days ; the menstrual flux returning with the usual regularity at this shortened interval. Now if the uterine action in parturition were dependent upon the recurrence of œstruation, the term of gestation in such a case ought to be 260 days ; or, if that term be exceeded, it ought to be protracted to another period of œstruation, in which case it would be 286 days ; yet in three pregnancies it has been 280 days, as usual. We could cite many other cases of the same kind ; all of them equally tending to support our position that the uterine act of parturition is independent of ovarian œstruation ; and that, where the two occur simultaneously, the relation is one of coincidence merely.

Another proof to the same effect is derivable from the phenomena of protracted gestation. Dr. T. Smith affirms, in the passage we have already cited, that "in the rare cases where the duration of pregnancy exceeds the ten menstrual periods, the function of parturition is deferred to the following period, so as to make pregnancy reach to eleven periods

inclusive. This fact came out most clearly in the obstetric evidence on the Gardner peirage case." We could scarcely cite a stronger instance than this of Dr. T. Smith's rashness of statement, and disregard of all facts but such as tend to confirm his own hypotheses. It must be remembered that in the Gardner peirage case, the object of those who sought to establish the legitimacy of the offspring was to show that pregnancy might be protracted to 311 days, or 31 days beyond the usual term; and in the medical testimony that was adduced to prove the possibility of such an extension of the usual term, any minor degrees of prolongation would of course be less attended to. Yet it will be obvious, we are sure, to those who will go through the evidence given on that case, that there is nothing even there to justify the assertion, that pregnancy, if protracted at all beyond the tenth catamenial period, extends to the eleventh. In the table published by Dr. Merriman, who was one of the witnesses upon that occasion, it is expressly stated that whilst 33 women were delivered in the 40th week, 22 were delivered in the 41st, 15 in the 42d, 10 in the 43d, and only 4 in the 44th. Yet, according to Dr. T. Smith's assertion, all the women who passed the 40th week ought to have retained their ova until the 44th. Nature, however, refuses to tie herself down to such rules. Dr. Murphy, in his 'Report of the Obstetric Practice of University College for 1844,' gives a similar table, in which it is stated that whilst 24 women were delivered in the 39th week, 25 in the 40th, and 32 in the 41st, 25 were delivered in the 42d week, 19 in the 43d, 9 in the 44th, and 11 in the 45th. We believe that all these cases are open to the objection that the date of conception was not known, and that impregnation was presumed to have occurred immediately *after* the last recurrence of the catamenia; whereas it might have taken place just *before* the next æstruation, which appears to be the case (see Dr. T. Smith's Appendix, p. 386) with those Jewish females who observe the Rabbinical law of abstinence from sexual intercourse for five days beyond the period laid down by Moses. This would cut off about three weeks from the presumed period of gestation, and would probably tend to reduce a large proportion of the cases of supposed protraction to the regular term. But it will not do so in by any means all cases; and moreover we have an amount of trustworthy evidence, derived from cases in which the day of conception could be fixed, and in which there was no motive for falsification, sufficient to establish the possibility of protraction beyond the usual term, for *any number* of days or weeks *less* than the additional month; how much *more* may be allowed, can scarcely be considered as yet decided.

The evidence of comparative gestation is all to the same effect; and we cannot refrain from asking why Dr. T. Smith has kept it back, when he is evidently acquainted with at least that part of it supplied by Tessier, and himself contends (p. 125) that "a comparison between human and comparative parturition, and all the other forms of parturient action, is essentially and absolutely necessary to a comprehension of this branch of human physiology." If it be admitted that we have a right to reason from the one case to the other, it is obvious that the class of phenomena in question affords most satisfactory evidence in regard to variability in the term of gestation; since the date of conception can be more positively determined than is usually possible in the case of the human female. In addition to the observations of Tessier, we have now a long series carried

on upon the cow, under the direction of the late Earl Spencer; from which it appears that, the average term of gestation in that animal being 284 or 285 days, an excess of a few days is not at all uncommon; whilst a prolongation to the extent of a fortnight or three weeks is much more common than an extension to a month. We shall presently speak of one very curious part of these observations; from which it appears that the *male* parent exerts an influence over the term of gestation, a phenomenon altogether inconsistent with Dr. T. Smith's hypothesis.

Having now tested the value of Dr. T. Smith's hypothesis by applying it to three different classes of facts which are altogether unnoticed by him, and having found it deficient even in the attribute of probability, still more in any claim to be received as a demonstrated truth, we might leave the question where we found it, and say, as many have said before us, that no account whatever could be given of the ordinary limitation of the term of gestation to 280 days. Even if no valid explanation can be given, however, it seems to us that it will be useful to know in what direction we are to seek for one; at any rate, it cannot but be advantageous to point out certain facts which any theory on the subject ought to embrace.

In the first place, then, it must be admitted on all hands that there is a certain disposition or tendency, on the part of the uterus, to disburthen itself of its contents at the termination of the fortieth week of gestation, rather than at any other time. Even if we grant, with Dr. T. Smith, that at each period of œstruation during pregnancy there is a tendency to abortion, we have still a right to ask why, in such a vast majority of cases, the fœtus is retained until the tenth of these periods has elapsed, and is then ejected? Why should not labour take place just as readily at the *tenth* as at the *eleventh* œstrum; and why should it be so very seldom deferred until the *twelfth*? We cannot see that Dr. T. Smith can give any other answer to these questions, than that the uterus and its contents are then in such a condition, as to render the ovarian stimulus far more effectual at that time than at any other. In assigning the ovarian stimulus, then, as *the cause* of parturition, Dr. T. Smith has altogether left out of view the fact that this cannot operate alone; and that unless there be, coincidently with this, a certain peculiar readiness on the part of the uterus to part with its contents, labour would no more come on at the end of the tenth monthly period than at the close of the ninth. As he has not shown whence this arises, we must consider him as having advanced but little in the explanation of the causes of parturition, even if we concede to him all that he demands. The act of parturition was likened by Buffon to the dropping of ripe fruit; and we believe, that in seeking for its cause partly (to say the least) in that condition of the uterus and its contents which may be designated as *maturation*, we are justified by all the facts at present known to us. The phenomena of the separation of the offspring from the parent, as a consequence of the completion of certain stages of growth on the part of the former, the loosening of the connexion between them being a part of this sequence of changes, are seen very extensively through the organized world; and they are obviously analogous to the phenomena of separation and detachment, which occur in the parts of an integral structure during its evolution. No one has any difficulty in understanding that the fall of the leaf is due to the death of the tissue which connects it with the stem; or that the

opening of a flower is the result of the turgescence of its tissue, consequent upon its full development. Every one knows that there are flowers whose first opening takes place with great regularity at a particular hour; we have ourselves repeatedly witnessed this in a West Indian species of *Amaryllis*, the long floral leaves of which became detached from each other, and sprung apart, within a few minutes before or after six o'clock in the evening. Although many vegetable and animal periodicities may be shown to be under the influence of external agencies to a certain extent, yet there is ample proof that they are essentially independent of such agencies, and that they form parts of that cycle of phenomena, of which the whole life of the being, from its origin to its final decay, is made up. We do not see anything more wonderful in the recurrence of certain changes at particular times, than in the progressive evolution of the entire organism. The exuviation of the milk-teeth, for example, in a certain regular succession, and at such uniform times, as to afford one of the best tests of the age of the individual, is a phenomenon which strongly marks the disposition in the human system to the spontaneous severance of a vital connexion which has served its purpose, and to the detachment of the part in which it has ceased to exist.

We have adequate reason to believe that the embryo of warm-blooded animals is extremely regular as to its term of development. Thus, in the common fowl, we may calculate almost to an hour (provided that the eggs have not been detained in the oviduct of the hen) the time when the chick will emerge from the shell. And it is well known that the stages of progressive development in the human fœtus afford indications from which its age can be determined, with a pretty close approximation to truth. That the development of the uterine structure goes on *pari passu* with that of the contained fœtus, appears equally evident from a consideration of its phenomena; especially those attending the formation of the placenta, in which organ there is such a marvellous commingling of parts, formed from two distinct and independent centres of development. Now we see pretty clear indications that the placenta of the fully-developed fœtus is somewhat in the condition of the footstalk of a ripening fruit; that is, that having attained its full evolution as an organ of temporary function, its connexion has a tendency to become dissevered, in virtue of causes inherent in itself alone, and quite independent of external agency. Something of this kind seems to have been recognised by Dr. T. Smith himself, who expresses himself, in his chapter on abortion, to the following effect:

“The placenta has a tendency to become unfit for foetal respiration towards the end of utero-gestation, when Nature is preparing for the change from branchial to pulmonary breathing. There is frequently observed on the surface of the mature placenta crystals of carbonate of lime, which must tend to interfere with its functions as a respiratory apparatus, and generally, I believe, to facilitate its separation from the uterus. This caducous preparation of the placenta, by the deposition of the salt of lime, is probably connected with the demand for ossific matter in the fœtus; but it must also remind you of the deposit of lime upon the egg of the bird, or of silica in the stems of ripe fruit, to facilitate its separation from the parent tree; or we may compare it to the deposit of earthy salts in the lungs of old age, as preparatory to the death of the individual. It is pretty certain that, in some cases of abortion in the latter months, caused by the death of the fœtus, the death has depended on the low respiring power of the placenta, the placental

development having progressed so rapidly as to render the organ prematurely deciduous." (p. 145.)

Another indication of the necessity for this maturation is to be found in the difficulty with which uterine contractions are excited by the ergot of rye before the normal time for parturition has arrived. Perhaps the strongest proof, however, that the parturient contraction of the uterus is in some way dependent upon the condition of the placenta, is derived from the phenomena which have been described under the designation of *superfœtation*. In whatever way we explain these phenomena,—whether we regard the two fœtuses as the products of two distinct conceptions,—or whether we regard them as twins, of which one is more advanced in its development than the other,—the facts remain the same; namely, that one child may be expelled by the parturient efforts of the uterus, whilst the other is detained; that the uterine contractions do not detach the placenta of the latter; but that pregnancy may continue for many weeks or even months longer, until the second child has attained its full term of development. In some of these cases, it is positively affirmed that the first child presented every appearance of maturity; in others, it appears that an abnormal condition of the placenta was the cause of its detachment. In all, however, it is obvious that the retention of the second fœtus was consequent upon *its* immaturity; and we do not see how this could influence the uterine contractions, except through the condition of the placenta.

There is another most important indication, that the immediate cause of the supervention of labour is to be looked for in the condition of the attachment of the fœtus to its parent, rather than in ovarian excitement; namely, that preparatory changes are obviously taking place during the last fortnight, or thereabouts, of gestation. The uterus, as Dr. T. Smith himself tells us, begins to contract more closely upon its contents; and the disposition to relaxation in the soft parts surrounding the outlet of the pelvis manifests itself in the descent of the whole mass. We have no doubt that, during this period, a progressive change is taking place in the placental attachment; for such a change may be easily verified in the placenta of many of the lower animals (such as the cat), in which the fœtal and maternal portions remain more distinct than they do in the human female, these being separable from each other, as the period of parturition draws near, far more easily than at any previous time. And it is not difficult to understand that the completion of this change, involving (so to speak) the death of the entire placenta, shall be the immediate cause of the supervention of labour; just as we know that premature labour may result from the death or abnormal condition of the fœtus and of the fœtal portion of the placenta. This view affords us a rational explanation of the occurrence of uterine action in cases of extra-uterine fœtation, which is considered by Dr. T. Smith as clearly indicating that the exciting cause of labour is to be found elsewhere than in the uterus itself. For if the condition of the placental attachment furnish that cause, instead of the state of the ovary, it will do so equally, whether the placenta be attached to the lining of the uterus, or to that of the Fallopian tube, or to any other organ.—As an additional indication that the exciting cause of labour has its place in the fœtal, rather than in the ovarian, con-

nexions of the uterus, we may notice the fact, ascertained by Earl Spencer, that of 75 cows in calf by a particular bull, the average period was 288½ days; none of these having gone less than 281 days, and two fifths of them having exceeded 289 days. That the male parent could produce any modifying influence on the ovarian periodicities of the female, cannot be thought probable for a moment; but that he should influence the term of foetal maturation, must be admitted to be not unlikely.

We have thus fully discussed the chief theoretical questions raised by Dr. T. Smith, in regard to the cause of parturition; because we consider that his claims are put forward in a manner to attract much attention amongst those who are disposed to accept specious novelties without sufficient investigation into their merits; and because we deem it our duty to subject all such claims to a rigorous and searching examination. We would again disclaim any wish to disparage Dr. T. Smith's merits. He has displayed great ingenuity and acuteness in the manner in which he has built up his hypothesis, and strengthened it with so many props and buttresses, that its inherent weakness may pass unnoticed. And when we come to discuss the practical portion of the work, we shall find that this same ingenuity has led him to originate many useful suggestions, for which he will deserve the gratitude of obstetricians. But in the character of a philosophic physiologist, of which he is obviously ambitious, we are obliged to regard him as having altogether failed, through a want of that comprehensiveness of survey of phenomena, and of logical appreciation of their entire relations, without which no discovery can be established, however ingenious may be the conceptions put forth, and however probable the doctrines advanced.—In order to avoid the possibility of misconception, we shall briefly recapitulate our positions, in such a form as to indicate what we believe to be the true view of the subject, and to point out the chief errors into which we consider that Dr. T. Smith has fallen.

1. The muscular action of the uterus we regard as essentially peristaltic and independent of nervous stimulation. We fully recognise, however, the fact that the induction of this action is *often* brought about by nervous agency; but we consider this agency as operating, not directly upon the entire muscular structure of the organ (as when a voluntary muscle is thrown into contraction), but indirectly, by exciting its peristaltic movements, as in the case of other organs composed like it of non-striated fibre, and receiving, like it, their nervous supply from the sympathetic system. We altogether dissent from Dr. T. Smith's doctrine, that the parturient contractions of the uterus, when once established, and even its tonic contraction anterior to labour, are excito-motor; because no adequate proof has been given that they are dependent upon nervous agency at all; and because all analogy, as well as the evidence of experiment and pathological observation (so far as they extend), go to prove that they are not. Nevertheless, we would not deny that the parturient actions *may be* excited in the first instance by nervous agency; it being very obvious, from the phenomena of abortion, that causes originating at a distance, and not capable of acting in any other way than through the nervous system, may bring about these actions. That the uterus may be thus affected through

the nervous system, has long been taught by physiologists and obstetricians; and Dr. T. Smith's chief movement, in supposed advance, is in referring the regular parturient actions of the uterus to this source.

2. Dr. T. Smith maintains that the exciting cause of parturition is to be found in the recurrence of the periodical excitement of the ovary; which continues during the entire period of gestation, giving a special tendency to abortion at each return; and which, at the eleventh recurrence, acts with such potency as to induce parturition. He assigns no other cause, however, why this eleventh recurrence should be so much more effectual than the rest, than that by this time there is a much greater aptitude to contraction in the uterus itself, and a readiness to be thrown off on the part of the placenta. That this ovarian excitement, acting by reflexion on the uterus through the spinal system of nerves,—the *ovarian* nerves being the *exciters*, and the *uterine* the *motors*,—is the exciting cause of normal parturition, is, we believe, an entirely original doctrine of Dr. T. Smith's; no others having advanced beyond the mention of the usual *coincidence* of the period of parturition with the eleventh ovarian nisis.—From this doctrine, however, we feel called upon to withhold our assent; because, in the first place, no adequate proof has been afforded by Dr. T. Smith, that the coincidence of the two phenomena is in any way essential; and because, secondly, a number of phenomena, altogether left out of view by him, afford a clear indication to our minds that the coincidence is *not* essential, the normal parturient actions of the uterus being entirely independent of the ovarian nisis. We do not deny that abortion may sometimes result from ovarian excitement; but we conceive that this has no special action, and that it is only one out of many causes that may produce the result where a predisposition to it exists.

3. In searching for the real exciting causes of parturition, we conceive that there is ample evidence, both from the analogy of similar phenomena in the lower mammalia, and from observation on the human subject, to justify the expectation that it will be found in the condition of the placental attachment; and we can see no reason why the changes in which maturation consists, and on which, according to Dr. T. Smith, the peculiar efficiency of the eleventh ovarian nisis depends, should not, when complete, be themselves as potent in exciting the first uterine contractions, as he supposes the ovarian stimulus to be.

4. The parturient efforts of the uterus having gradually advanced the presenting part of the child into the vagina, the other expulsor muscles are called into action through the spinal system of nerves; the requisite stimulus being supplied, either by afferent nerves of the uterus itself, as first suggested by Dr. Carpenter, or by those of the walls of the parturient canal, against the several divisions of which the presenting part of the child successively presses, as maintained by Dr. T. Smith. We are inclined to regard the latter as the correct view of the source of the action; but if any merit be due to the idea of the local origin of the stimulus which excites the reflex action of the expulsors, it appears to us that it may be fairly claimed by the first-named author.

Having thus discussed the main subject of the first portion of Dr. T. Smith's treatise, we may now revert to one or two of the minor topics, of which we thought it better to postpone our notice until this should have

been disposed of.—In regard to the *fœtal movements* supposed to be felt during pregnancy, our author maintains that, for the most part at least, they are in reality local spasmodic contractions of the *uterus*. In fact, all his arguments go to prove that there is no such thing as fœtal movement at all; and he does not even attempt to suggest any diagnostic marks by which the true fœtal movements may be distinguished from the uterine. That *many* of the supposed movements of the fœtus are really uterine, we quite agree with Dr. T. Smith in believing; but that the fœtus is capable of executing reflex movements within the uterus, and that many of its supposed movements are really due to itself, we are equally satisfied. There is no mistaking the form of the infant's foot, which often causes a projection of the uterus in the later months of pregnancy, and which may be distinctly felt when the abdominal parietes are thin; there is no possibility of error when we can feel this withdrawn and pushed out elsewhere, so prominently that we can even count the toes. Nor can there be any mistake when fœtal movements are excited during labour by irritation of the presenting part. On the other hand, the spasmodic twitchings of the uterus itself communicate a different sensation to the hand laid over the part; and we can scarcely imagine that any practitioner endowed with the *tactus eruditus*, and possessed of the knowledge of both sources of movement, can have much difficulty in the diagnosis when either occurs separately. We are disposed to think, however, that the two are frequently concurrent; and that either may excite the other. Nothing would be so likely to excite local contractions of the uterus, as the pressure or impulse of the limbs of the fœtus against its walls; and nothing would be so efficient in calling the reflex mobility of the fœtus into play, as the spasmodic twitchings of the portion of the uterus that might happen to press on any prominent part. We would commend to the attention of Dr. T. Smith, and of our readers, the very interesting papers on the reflex movements of the fœtus, as the cause of its position in the uterus, contributed by Professor Simpson to the recent numbers of the 'Edinburgh Monthly Journal.'

Active dilatation of the os uteri.—Great stress is laid by Dr. T. Smith upon what he terms a positive dilatation of the os uteri, over and above the mechanical dilatation of this structure by the pressure of the amniotic bag, and the tension exerted on the opening mouth of the uterus by the contractions of the rest of the organ; and upon the reflex nature of this dilatation. Other authors have spoken of a "disposition to dilatation," by which they implied (we suppose) a *passive* condition of the sphincteric fibres, which caused them to yield before the propulsive force exerted by the active contractions of those of the remainder of the uterus. But Dr. T. Smith considers that the dilatation of the os uteri is itself an *active* process, the result of an operation of that same reflex function which he has called in to the assistance of the uterus in explaining the other parturient actions. We are unable to appreciate the force of the evidence which has led him to this assumption; a certain hypothesis that other sphincters have a similar power of "active dilatation" being the chief groundwork of his argument. Now we cannot, for our own part, see the slightest proof that the cardiac sphincter, or the sphincter ani, are ever in any condition different from those common to other muscles,—namely, a state of contraction more or less powerful, and a state of relaxation more

or less complete. In the former condition they tend to keep the orifice closed, even when a powerful force is in action to distend it; in the latter they are disposed to yield to the expulsoir power of other muscles, still retaining contractile force enough to keep it closed when not thus distended; or if the relaxation be rendered complete and permanent by paralysis or section of the nerves, they no longer maintain the closure of the orifice. Now we do not admit that the dilatation of the os uteri is in any degree more dependent upon nervous agency than is the contraction of the fundus; and we cannot see that there is any more difficulty in accounting for its "disposition to relax" at this particular period, than there is for the "disposition to contract" on the part of the fundus; both changes being due, as it seems to us, to the very same cause, and that cause residing in the maturation of the uterine structures connected with the function of gestation. We are acquainted with only one sphincter in the body which has even the appearance of an active dilating power; this is the iris, upon the nature of whose actions (as our readers must be well aware) physiologists are not agreed. At any rate, if the dilatation of the pupil, as well as its contraction, be truly muscular, it is produced by the antagonistic action of a set of fibres passing radially across the circular fibres of the sphincter. Some have attributed the dilatation of the os uteri to the contraction of a set of fibres supposed to be implanted in the lip, and to pass in a straight line towards the fundus. Dr. Meigs favours this idea; but it is altogether repudiated by Dr. T. Smith, who does not need the assistance of any such fibres, finding all that he seeks in the "active dilatation" of the sphincter itself, excited through the nervous system.

The Genesial cycle.—We have perused with great attention the lecture in which the theory is propounded that bears the above designation; and we must own ourselves at some loss to discover what are the special *ideas* which the author desires to develope. We shall do our best to give a concise expression of them; being only deterred from doing so in the author's own words, by the length of the quotations that would be required to convey them. Dr. T. Smith in the first place brings systematically together the various "sympathies" and "synergies," physiological and pathological, which indicate a close relation between the ovaries, uterus, and mammæ. This relation was previously, we think, sufficiently appreciated both by physiologists and practitioners; the former having been in the habit of looking at these three organs as parts of the same apparatus, and as performing three distinct but mutually adapted offices in one function; the latter having the connexion forced upon their attention by phenomena of daily occurrence.

Dr. T. Smith divides the reproductive life of the human female into a series of "great genesial cycles," each of them composed of a period of ovarian, of uterine, and of mammary activity;—the ovarian commencing with puberty, continuing until conception takes place, and then giving place to the uterine; the uterine lasting for the term of gestation, and then being succeeded by the mammary; and on the termination of lactation, a fresh cycle commencing with the return of ovarian activity. Now in this it is obvious that there is but a formalization of facts which everyone knew before; and this formalization (very different from the inductive process by which a true *law* is evolved) is valuable only in so far as it leads to further results. To refer the metastasis of activity, as

Dr. T. Smith does, to the operation of the reflex function, is about as gratuitous a hypothesis as we have anywhere met with, although, as we have shown, such hypotheses are by no means unfrequent with our author: we might as well, it appears to us, refer the withering of the stamens and floral envelopes of a blossom, and the increase of the fruit, to a nervous system. Nothing is more common than this alternation of action among the different parts of the same apparatus; and where this involves the organic functions only, we do not see any difficulty in accounting for them in accordance with the general doctrines of nutrition. The succession of phenomena designated as the "great genesial cycle," is repeated, however, on a small scale, according to our author, during every catamenial period; and he thinks that he thus satisfactorily accounts for the regularity of the monthly return of ovarian excitement.

"We know that it is the state of the ovaria at the ovarian period which produces the uterine secretion. This action continues for two, three, four, or five days or more, and then ceases. Now I believe that during the rest of the days of the catamenial cycle of twenty-eight days, the breasts and uterus are exerting an influence on the ovaria of a slower and more moderate character, which gradually prepares them for the next ovarian excitement. Thus in a case in which the catamenial secretion and mammary disturbance lasts four days, there is, so to speak, a neural discharge from the ovaria to the uterus and mammæ during four days. At the end of this time, the activity of the ovaria declines, and there is now a neural discharge set up in the contrary direction, so that for twenty-four days the mammæ and uterus are modifying and directing the actions of the ovaria. During the intervals between the catamenial flow, the uterus and mammæ are acting upon the ovaria, causing them to prepare and mature the ovule or ovules of each ovulation, just as, during the days of the catamenial flow, the ovaria are acting on the uterus, causing it to pour out the catamenial secretion—the reason of the distinct ovarian periodicity being, that this action of the uterus and mammæ upon the ovaria is prolonged for a greater number of days than the reaction of the ovaria upon these organs. According to this view, the twenty-four days is as much a periodicity as the four days of the catamenial flow. The twenty-four days are the term of the uterine and mammary periodicity, the four days are the term of the ovarian periodic excitement, and the entire catamenial period of twenty-eight days is the record of the time taken in one revolution of the reciprocal actions of the mammæ and uterus upon the ovaria, and of the alternative action of the ovaria upon the uterus and mammæ. If the catamenial flow had lasted fourteen days, and the apparent quiescence of the uterus had continued just an equal time, the cause of the periodic times of the sex would have been evident long ago; but owing to the disproportion between the uterine and ovarian times, the uterine and mammary phenomena of the intervals between the catamenial flow have never been recognised. All the facts of the case—and they might be multiplied—seem to me to point to a power of neural induction, excitation, or polarity, existing between the three principal organs of reproduction, and regulating and producing their functional revolutions or periods, just as simply and decisively as gravitation causes the motions of the heavenly bodies." (p. 181.)

At the conclusion of the lecture which contains the foregoing passage, we find Dr. T. Smith giving utterance to the following speculation, which forms an appropriate pendant to it.

"By an extension of the same principle as that which I have been tracing between the organs of reproduction, I think we may acquire a better knowledge of intermittent fevers, perhaps of Asiatic cholera itself." (p. 188.)

We cannot think it requisite to inflict on our readers any laboured

demonstration of the utter baselessness of this hypothesis, which, though at first brought forward merely as the author's belief, is afterwards spoken of as if it were the expression of observed facts. We can imagine the unsophisticated student imbibing it with all the gravity with which it is propounded; and learning with surprise, as he grows wiser, that the "neural discharge" is a pure invention of the author's, and that there is about as much ground for the belief in this "reciprocal action of the mammæ and uterus upon the ovaria," as there is in the doctrine of "lunar influence," which, we recollect, was brought forward afresh in a new and specious form in our earlier days, and was very attractive to our young imagination. The consideration does not seem to have occurred to Dr. T. Smith, that such mammary sympathies as present themselves in connexion with this monthly periodicity are *coincident with* the ovarian excitement, not reciprocal to it; many women having pains in the breasts, and some even forming a lactescent secretion, during every catamenial flow. But a theorist of his wonderful ingenuity, can have no difficulty in accounting for such apparent obstacles; in fact we shall not be in the least surprised if Dr. T. Smith should convert every one of the objections which we have advanced against his hypotheses, into triumphant demonstrations of their truth. Nothing is more easy to a sanguine speculator, mounted upon his Pegasus, than to dash through the petty obstacles which sober and cautious reasoners would build up in his course; and there is a certain fascination in thus giving oneself up to the exhilarating influence of rapid motion, without being troubled about the payment of turnpikes over which we leap, or the value of the crops we trample down, that is for a time extremely enticing. The misfortune is, that instead of making real progress, we are too often led back in our chase to the point from which we started, or are carried in the direction precisely opposite to that of our goal. And, as in the case of the hare and the tortoise, the cautious reasoner, who is content with a less rapid advance, but whose movement, however slow, is steadily progressive, generally wins the race in the end.

It has been with much regret that we have felt ourselves compelled to say so much in disparagement of Dr. T. Smith's hypotheses, and of the method of philosophising which he has thought fit to adopt. With nothing but a friendly feeling towards himself, we yet consider the interests of truth paramount; and the questions which he has raised, and the manner in which he has raised them, call for a full and free discussion. We trust that neither he nor our readers will consider that we have in any degree transgressed the bounds of candour and good feeling, in criticising his production as we have done. We again repeat that we consider that great merit is due to him for his labours, although we cannot coincide in his own estimate of the value of his results; and we may add that the reader of his treatise cannot but be favorably impressed by the author's range of acquirement, his ingenuity and fertility of thought, and his very agreeable style, even when most widely dissenting from his views.

We must postpone until our next Number the discussion of the practical portions of the two works before us.