VIII. *An Experimental Inquiry concerning Animal Impregnation.* *By* John Haighton, *M. D.* *Communicated by* Maxwell Garthshore, *M. D. F. R. S.*

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DIFFICILLIMUM aggredior laborem, et exitum vix promitto qui lectori satisfaciat.

This was the sentiment of the justly celebrated Baron HALLER, when he first directed his attention to this subject, when he attempted to produce order and regularity out of chaos, and to show

> "How the dim speck of entity, began
> "T' extend its recent form, and stretch to man."
>
> GARTH.

The difficulties which discouraged so able a philosopher, are but ill calculated to inspire me with confidence; but the disappointment from failure will be attended with this solacing reflection, that if I have miscarried, it is in a great undertaking.

The multitude of physiologists who have sought for laurels in this field, can best bear witness to the difficulty of the pursuit; and the penetrating genius of a HARVEY, though adequate to a full exposition of the circulation of the blood, toils in vain in the mysterious researches of generation. His philosophic

mode of scrutiny by experiment, when pointed to one object, conferred immortality on his name; but when directed to another, reduced him to a level with contemporary reputation.

Others, perhaps from possessing a greater propensity to the subject, have laboured with more success: they have penetrated into the interior recesses of nature, and thence brought to view what preceding investigators had deemed inaccessible to research. On this view of the subject, our acknowledgments are particularly due to the labours of STENO, DE GRAAF, HALLER, and others. TO STENO and DE GRAAF we are indebted for some important facts on the structure of the ovaries. The supposed analogy to the male's testes is disproved, and the vesicular structure, together with a connexion with the ova, or rudiments of the new formed animal, fully established.

From the experiments of DE GRAAF on rabbits, we learn,

First. That the ovaries are the seat of conception.

Secondly. That one or more of their vesicles become changed.

Thirdly. That the alteration consists in an enlargement of them, together with a loss of transparency in their contained fluid, and a change of it to an opaque and reddish hue.

Fourthly. That the number of vesicles thus altered, corresponds with the number of fœtuses, and from these are formed the true ova.

Fifthly. That these changed vesicles, at a certain period after they have received the stimulus of the male, discharge a substance, which being laid hold of by the fimbriated extremity of the fallopian tube, and conveyed into the uterus, soon assumes a visible vesicular form, and is called an *ovum.*

Sixthly. That these rudiments of the new animal, which for a time manifested no arrangement of parts, afterwards begin to

elaborate and evolve the different organs of which the new animal is composed.

To these facts we may add, that the *calyx* or *capsula* which formed the parietes of the vesicles, thickens, by which the cavity is diminished. This cavity, together with the opening through which the fœtal rudiments escaped becomes obliterated, and from the parietes of these vesicles having acquired a yellowish hue, they are called *corpora lutea.*

But though some important facts are clearly ascertained, there are others still problematical. Physiologists are by no means agreed concerning the *immediate cause* of conception. All admit the necessity of sexual intercourse. They acknowledge too the necessity of some part of the female being affected by the direct contact of a fecundating fluid, but what the precise part is which must receive the stimulus, has hitherto been involved in mystery and doubt. Nor are they more unanimous respecting the state or condition of the substance that passes from the ovaries; whether at the time of its expulsion it has a circumscribed vesicular character, or whether it has no determined figure. DE GRAAF and MALPIGHI, in the last century, and some respectable physiologists of the present day, adopt the first opinion; HALLER and some others favour the last.

The subject of conception involves other problematical points not less interesting; the discussion of which I purpose waving at present, in order the better to direct my attention more closely to the preceding questions.

The intention then of this essay is to explore the *proximate cause* of the impregnation of animals, and to trace with more accuracy the visible effects of it from their first appearance, until the rudiments of the fœtus are lodged in the uterus, and

have assumed the proper characters of an *ovum*. As soon as these rudiments manifest that opaque spot, or "dim speck of "entity," which is known to evolve the fœtus by regular and progressive steps; another stage of the inquiry then commences, viz. to trace the visible formation of the new animal through its whole course; but as this belongs rather to the œconomy of the fœtus than the mother, it is not intended to form any part of this paper.

I perceive, however, that I cannot investigate the question of the proximate cause of impregnation in a satisfactory way without first determining what are the evidences or proofs that impregnation has taken place: this then necessarily becomes a preliminary question. I therefore restrict my inquiry to the three following subjects.

First. What are the *evidences* of impregnation?

Second. What is the *proximate cause* of impregnation?

And, *third.* Under what form do the rudiments of the fœtus pass from the ovary to the uterus?

SECTION I.

What are the Evidences of Impregnation?

The investigation of every complicated subject of inquiry comprehends within its range a more or less extended recital of facts, depending in a greater or less degree on each other, but primarily arising from some fundamental proposition.

As this proposition is generally the basis on which this superstructure is raised, or the trunk from which the various ramifications of inquiry proceed, it is essential, to the establishment of the ultimate conclusions, that the antecedent question

be rightly decided. It becomes then indisputably necessary to us in the present subject, to determine *what is the criterion of impregnation.*

That a female is impregnated when a fœtus is sensibly formed, is so obvious to reason that no argument can be necessary to convince us of its truth. But it is important to some conclusions in the sequel of this paper to prove, that a female has conceived before there are any vestiges of a new animal. The test of this condition must then be sought for in the ovaries; and the well conducted experiments of DE GRAAF, in the last century, and of Baron HALLER and others, in the present, bear so forcibly on this point, that the necessity of further investigation is in a great measure precluded.

But, in order that I might bear evidence of its truth, I examined with great attention the ovaries of some full grown virgin rabbits, and found, as DE GRAAF has represented, that there entered into their composition a series of cells containing a transparent colourless fluid. It was indispensably necessary here to be certain, that these rabbits had never been admitted to the male, lest the remains of former impregnations should be confounded with virgin appearances. I therefore observed with care not only the appearance on the surface of these bodies, but likewise examined with great minuteness the interior parts; yet in none of them could I see any of those circumscribed substances, which, from their yellow colour, are called *corpora lutea.* But when similar observations were made on rabbits that had been impregnated at different periods, and the traces of those corpora lutea were more or less evident, according to the interval of time that had elapsed; I may then say that no corpora lutea exist in virgin animals, and that when-

ever they are found, they furnish incontestible proof, that impregnation either does exist, or has preceded.

But a proper distinction between past and existing impregnation can be made only by tracing the phænomena of recent fecundation progressively, and noting the appearances in the different stages. I was therefore under the necessity of repeating with care several of DE GRAAF'S experiments, in order that I might bear testimony to the truth of them, at least as far as the results coincided with my own.

EXPERIMENT.

Having therefore procured several virgin rabbits in a fit state for impregnation, I admitted one of them to the male. Twelve hours afterwards it was killed, and on examining the ovaries several of the vesicles evidently projected; they had lost their transparency, and were become opaque and red. When punctured, a fluid of the same colour escaped. I made sections through some of them; but at this early period the corpora lutea, which are formed by the thickening of the parietes of the vesicles, were not very evident. I therefore determined to examine them in a more advanced state.

EXPERIMENT.

Another rabbit being admitted to the male, I examined it twenty-four hours afterwards. The colour of the fluid contained in the vesicles was similar to that of the last experiment. The vesicles projected more evidently, and their thickened parietes manifesting the commencement of corpora lutea were become more apparent.

EXPERIMENT.

I inspected the ovaries of another rabbit forty-eight hours *post coitum.* At this period the vesicles seemed to be in the very act of bursting, and a semitransparent substance, of a mucus-like consistence, was beginning to protrude from some of them; others indeed were less advanced. The fimbriated extremities of the fallopian tubes were preparing to receive their contents, as appeared by having quitted their usual position, and embraced the ovaries in such a degree, that only a small portion could be seen until the tubes were taken away. Sections being made into the thickened vesicles, the formation of corpora lutea appeared to have made further advances.

From the appearance of an incipient rupture of the vesicles in this experiment, it was but reasonable to expect that their contents would soon have escaped; but as my views were directed to the formation of a corpus luteum, I deferred the next examination to a more distant time.

EXPERIMENT.

In two days and twelve hours after coition, I examined the ovaries of another rabbit. The fœtal rudiments had escaped; but the cavity of the ovarian vesicles had suffered but little diminution. Bristles were easily introduced by the ruptured orifices. In this experiment the advances towards the formation of a perfect corpus luteum were such as the period of examination would naturally lead us to expect.

The contents of the vesicles having escaped, it was but reasonable now to look forward to a speedy obliteration of the

cavity. I therefore examined these parts under similar circumstances on the third, fourth, and fifth day. In the last experiment there was but little vestige of cavity, consequently the corpora lutea might be considered as perfectly formed.

I think it not improper to remark here, that though in the relation of the above experiments I have constantly kept in view the formation of corpora lutea; yet I did not altogether neglect the opportunity of making other observations, which in this early stage of the inquiry it would be premature to relate. Besides which, several other rabbits were examined at more distant periods, as well with a view of tracing their progress with accuracy, as to afford further evidence of their connexion with impregnation. But as it would be tedious to state in detail the several experiments made on this single question, by reason of the great similarity of result, I decline trespassing on your patience, and therefore lay before you only the conclusion; which is, that in the great variety of experiments on brute animals which my physiological inquiries have led me to conduct, as well as in the extensive opportunities I have had of observing the ovaries in the human subject, I have never seen a recently formed corpus luteum unattended with some circumstance or other connecting it very evidently with impregnation. I have more than once seen a recently formed corpus luteum in the human subject, *without* a fœtus. Nay, even in a subject where there has been a kind of *hymen:* but the uterus in these cases has borne the marks of an early and recent abortion.

SECTION II.

What is the proximate Cause of Impregnation?

The preliminary question concerning the criterion of fecundation being now answered, we are led by a natural transition to show by what means this test has been produced.

Waving all comment on the peculiar circumstances of sexual intercourse, as being both irrelevant and indelicate, we shall note only one important effect of it, the passage of the fecundating fluid of the male into the generative organs of the female, as being an indispensable requisite in the human female, and in such animals as bear an affinity to it. As this effect of sexual communication is so important, it cannot be indifferent to the design of nature, to what part of the uterine system the semen should be conveyed. It admits of no doubt that it either remains in the vagina, passes into the uterus, or else extends its course along the fallopian tubes to be applied to the surface of the ovaries, which it stimulates, and from which the new animal derives its existence; but whether it be one or other of these, has given birth to more physiological controversy, than perhaps any other operation of a living animal.

Those who have entered the lists have ranged themselves either on the side of application of the semen to the ovaries by means of the tubes; or on that of the inutility of this process. These latter contend for an absorption of this fluid by the vagina, a peculiar excitement of the whole frame as a consequence, of which excitement the changes produced on the ovaries are to be considered the local effects. But though the question has been disputed on both sides with all the zeal of argument

and controversy, the arbiters of science have not yet acknowledged a victor on either side.

The advocates for the first opinion allege, that the semen has been seen both in the uterus and tubes, and quote as their authority the observations of MORGAGNI for the former, and RUYSCH for the latter. When seen in this last situation, some have thought that it was conveyed thither by the muscular power of these parts in the manner of a peristaltic motion, beginning at the uterus and ending at the fimbriated termination of the tube; and when at this last, it was supposed that the semen was applied to the surface of the ovaries, and impregnated them by actual contact.

Though I shall prove that this hypothesis is altogether visionary, yet *prima facie* it is far from carrying with it the characters of absurdity. There is nothing repugnant to reason in contending for what analogy seems to favour, particularly when the subject is thought beyond the reach of demonstration or proof. And the analogy favourable to this opinion has probably been taken from the impregnation of frogs and toads, in which process we are told, on the authority of ROESEL, SWAMMERDAM, and SPALLANZANI, the ova are impregnated by the male as they are passing from the body of the female; and that in water newts the ova are impregnated even without copulation. Now here is an appearance of contact between the fecundating fluid and the ova.

Again, on the other hand, the contact of semen with the ovaries has been thought improbable, from an analogy drawn from the vegetable kingdom; for admitting the Linnæan doctrine to be true, which contends for a necessity of sexual intercourse in vegetables, it would be difficult to demonstrate to

the satisfaction of stern philosophers, that the *pollen* pervades the *pistillum*, and stimulates the contents of the *pericarpium* by contact, to the evolution of the *germen*. Such would deny the contact of semen. The advocates for either opinion then may avail themselves of analogies suited to their own mode of thinking. It may be said, however, and with some colour of truth, that the latter analogy, as being more remote than the former, and as being founded on a principle which some have suspected to be gratuitous, should be received with caution and distrust. Before any deduction can be made from analogy concerning the means by which any important end is to be effected, we cannot examine the instruments performing such actions with an attention too nice or too minute. If we find nature employing different instruments, in different animals, to produce the same ultimate effect, I think it but fair to conclude, that the means used are essentially different; but the closer the resemblance in the instruments or organs, the nearer will the means approach. On this principle no conclusions can be drawn respecting the human species, from observations either on vegetables, or even on frogs, toads, and newts. Birds, as being impregnated by semen conveyed into the body, resemble human impregnation more than the former; but they differ so obviously in the mode of perfecting the fœtus from the ovum, that I scarcely dare to rest any thing on their general analogy. There is, however, a curious fact respecting them not altogether inapposite to this question, which is, the permanent effect of one coitus. I have read in the Abbé SPALLANZANI's dissertation, and elsewhere, that all the eggs which a hen will lay in twenty days will be impregnated at one coitus: and Mr. CLINE tells me, that in Norfolk this matter is reduced to a certainty

with respect to turkeys; and that even to a greater extent. There is certainly some difficulty in reconciling these facts to impregnation by contact of semen; but from the very obvious difference between oviparous and viviparous animals, I shall not press this argument farther. Indeed it should always be impressed on the recollection of those who are labouring in the pursuit of truth, that arguments drawn from analogies, unless from those of the nearest relation, are better adapted to the purpose of illustration than of proof: and though they frequently find advocates in confident closet philosophers, they are received with deserved distrust by the more cautious practical physiologists.

Those who cannot admit the passage of semen by the tubes, do not neglect to take the advantage of some difficulties which their opponents have overlooked. They say, implicit confidence is not due to the observations of MORGAGNI and RUYSCH, and that what appeared to them to be semen in the uterus and tubes, was nothing more than the mucus of the parts. They further invalidate the force of this argument by contrasting these solitary observations, with a numerous train of counterfacts; for in all the experiments made by HARVEY, DE GRAAF, HALLER, and others, it does not appear that semen was found beyond the vagina, except in one of Baron HALLER's experiments in a sheep, in which he saw semen in the uterus forty-five minutes after coition. But this fact stands almost alone; and when placed in opposition to the many experiments attended with a contrary result, will weigh but little in the balance of impartial decision. Yet, however, he rested much upon this one fact, and adduced it in support of his opinion, that whenever impregnation happened, the semen passed into the uterus,

and was retained; but when it returned from the vagina, then the animal remained unimpregnated. In this latter case, he supposes the semen had never passed beyond the vagina; for if it had, he says it would have been retained. This argument he thinks is unanswerable.

The insufficiency of this reasoning did not escape the penetration of his opponents; and the immense mass of counter-facts poured out against him, like an irresistible torrent, bore away the very foundation of his doctrine. This brings the advocates for the necessity of the contact of semen with the ovaries into a dilemma, from which they attempt to extricate themselves by contending, that fecundation does not require the application of semen to the ovaries in a palpable form; but that there is exhaled from it a subtile fluid in a vaporific state, called *aura seminalis*, and that the contact of this vapour is fully sufficient to impart to the ovaries their due quantity of stimulus.

But the opinion, even thus qualified, has not passed without animadversion. There are some who cannot comprehend how the tubes should perform two motions in contrary directions, which they must do, if they first convey the aura seminalis to the surface of the ovaries, and afterwards return the rudiments of the fœtus into the uterus. Such a double action they think is repugnant to the œconomy of the part, but assign no reason for their opinion. They might with equal propriety deny the possibility of a peristaltic and inverted peristaltic motion of the intestines, or the opposite actions in the œsophagus of ruminant animals, though I am persuaded very few would acquiesce in their incredulity: but as a minute discussion of this particular

question would be rather extraneous to my investigation, I must decline any further disquisition.

The difficulties which were opposed to the conveyance of the semen by the tubes, were, as we should suspect, intended to prepare the way for a different explanation; therefore physiologists, by a very natural transition of thought, were led to suppose that the presence of semen in the vagina alone was sufficient to account for impregnation.

In order to give support to this opinion, cases were adduced, in which, from some anatomical peculiarities, it seemed almost impossible that the fecundating fluid could be conveyed into the uterus; and yet in several of these cases impregnation had really taken place. It would be digressing too much to state the facts in detail, seeing that in this inquiry I deduce nothing from them; nor would such statement solve the problem before us. The facts are already in the possession of physiologists, but are not admitted as satisfactory proofs. Those who hold the contrary opinion, either cavil at the accuracy of the statement, or draw a different conclusion; therefore to attempt conviction by these materials would be to engage in the service of forlorn hope. It remains then to try whether by a patient experimental investigation, we can make such an accession of new facts to our present stock of knowledge as will enable us to unloose this Gordian knot. This attempt naturally leads us to review the two points of the question, viz. *Is the passage of the semen by the tubes to the ovaries, essential to impregnation? If not, what other means are employed?*

If it be true that the fecundating fluid must pass by the tubes to the ovaries before impregnation can take place, ought it not

to follow, as a consequence, that if, from any cause, both these tubes be obliterated, the animal so affected would be barren? or if the animal be multiparous, would not an obliteration on one side prevent conception in the corresponding ovary?

Now I had some distant apprehensions, even before I made this experiment, that dividing both tubes would produce effects equivalent to an extirpation of both ovaries, which experience has since proved to be well founded; for it not only destroys the power of conception, but even the disposition for using the means.

EXPERIMENT.

Having procured a full grown virgin rabbit, which had betrayed signs of disposition for the male, I made an incision into the posterior part of each flank, exactly upon the part where the tubes are situated. By means of my finger and a bent probe, I drew out a very small portion of the middle of the tube, and cut out about $\frac{1}{8}$ of an inch. The two ends were returned into their former situation, and the wound closed by what surgeons call the quill suture. The same operation was performed on the opposite side, and in a few days both wounds were healed.

As soon as this rabbit appeared in health, it was admitted to the male, but the venereal appetite seemed to be entirely lost. Thinking it possible that its health was not perfectly restored, I kept it a month longer in a state of high feeding, and admitted it to the male a second time, but the same reluctance continued. I began now to suspect that the venereal appetite was irrecoverably gone: but as the season was cold, and of course unfavourable, it appeared proper to persevere in this plan until the

genial influence of returning spring had produced its effect; but instead of discovering signs of the restoration of the female character, it was evidently more averse. It was now killed and examined, the tubes adhered firmly to the loins at the part where they were divided, and at that part their canal was obliterated, so that neither quicksilver nor air could be made to pass. The ovaries were much smaller than they usually are in breeding rabbits; they appeared to have degenerated from their proper character, a circumstance probably the consequence of that destruction of the harmony of action in these parts, which subsists in the healthy state, which is essential to the views and intentions of nature, and for want of which harmony, the sexual indifference, approaching to aversion, was in this instance so remarkable.

In the relation of this experiment, it must be remembered, that a small portion of each tube was cut out, in order to obliterate the canal with greater certainty. It is not altogether indifferent to the present subject to know, whether this apathy depended on the removal of that portion, or whether it would have happened had there been nothing more than a mere division. Nor is it extraneous to inquire, whether a simple division of the tube is sufficient to obliterate it, because less violence is offered to the part, and of course the connection will be less disturbed.

EXPERIMENT.

Being furnished with another rabbit, in high breeding condition, I repeated the experiment, by making only a division of the tubes; in other respects every thing was conducted as before. The venereal appetite declined as evidently in this as

in the former, and notwithstanding many solicitations from a very animated male, during the space of three months, it could never be excited.

On dissection, it appeared that the tubes were as completely obliterated in this experiment as in the last, and the ovaries had equally degenerated.

In the two preceding experiments neither of the rabbits had given any active proofs of fecundity, though they had marks of the venereal heat upon them. I therefore changed my subject for one that had had young ones.

EXPERIMENT.

A healthy rabbit, which had lately been separated from her first litter, was made the subject of a repetition of the experiment. I took the opportunity of feeling for the ovaries, in order to have better evidence respecting their bulk, and by that means to form a juster comparison. The disposition to propagation declined as evidently in this animal as in the two former; and dissection equally evinced a change of the ovaries; for at the expiration of three months, they had lost nearly half their size.

Feeling but little encouragement to persevere in a repetition of these experiments, I determined to change the mode of inquiry, and to try the effect of a division of *one* tube only. From reasoning I was led to think, that if a division of both tubes destroyed the harmony of the generative system, a division of one only might permit that harmony to continue in some degree. I wished likewise, if possible, to have this point determined on a virgin rabbit, the better to guard against any

deception which the remains of a former impregnation might occasion.

EXPERIMENT.

A full grown virgin rabbit had one of the tubes divided at a little distance from the extremity of the cornu uteri. The wound soon healed up, and its health was soon restored, but it betrayed no disposition for the male. I attributed it in part to the coldness of the season, for it was in the middle of December, 1794; but the effects of its inclemency were much moderated by having a fire in the room during the day. I kept her until the first of May; during this interval the male was frequently offered to her, but she always refused, except once in February: it however was unproductive.

From examination after death, it appeared that the divided tube was completely obliterated, but the other was sound: both ovaries were evidently shrunk, proving, in addition to my previous observations, that their actions had been languid.

The result of this experiment disappointed me much; for no reasoning *a priori* had led me to entertain the smallest suspicion that a mutilation of one side only could destroy the harmony of the whole uterine system. But my disappointment originated chiefly from the apprehension that this effect would be uniform, that it was the result of a determined law of the part; and if so, it formed an insuperable obstacle to my research. Its importance to my project was too great to be discouraged from a single obstacle; therefore in justice to my undertaking, I was in some measure compelled to push the inquiry to such an extent, as should enable me to say with precision, whether

it is possible to impregnate an animal in the situation just described.

EXPERIMENT.

Two other rabbits full grown and perfectly healthy were made the subject of a repetition of the last experiment. The male was offered to them several times during the space of three months. They generally refused him, yet received him twice or three times each during this interval; but neither were impregnated. As the signs of degeneracy from their proper sexual character became daily more evident, they were devoted to anatomical inspection, and exhibited appearances in the ovaries like the former, but somewhat less in degree.

The rabbit keeper informing me that those which had already had a litter were more certain of breeding than those which had not; I determined to make a trial of one of this description, with a view to compensate for my former disappointment.

EXPERIMENT.

Being furnished with one of this kind, and from which the young had been taken away three weeks at the age of ten weeks, which, together with the month of gestation, amounted in the whole to four months from the last conception, I made this the subject of the experiment. Now, at this distance of time, it is not very probable that the ovaries should retain very evident vestiges of the preceding conception: but as it was a point of too much importance to be left in doubt, I determined to satisfy myself by ocular examination, which, by a little management, was effected. The traces of corpora lutea were

far from being evident, so that there was no danger of confounding them with any recent mark that might happen. The tube on one side was cut through as before, but to my unspeakable mortification this rabbit was as barren as the former, though tried several times during the space of three months. The generative organs were examined after death, and the appearances corresponded with those of former experiments.

In this case, as well as in a former, I had an opportunity of comparing the shrunk state of the ovaries after death, with the plump and healthy condition before the mutilation; and it affords an additional proof of that sympathetic connexion, or consent, between one part of the generative organs and another; and shows that in the production of a new animal, the co-operation of different parts is necessary; and further, that if the assistance of one part is wanting, the others, as if governed by a principle of intelligence, cease to continue their important work. But I was still in a state of suspense with regard to the end for which these experiments were instituted; and such an uninterrupted succession of failures on a point so essential to my present inquiries, I confess tended but little to animate me in the pursuit. I was beginning to suspect that the barrenness consequent to the division of only one of the tubes, was as determined a law in the œconomy of these parts, as it seemed to be in those cases where both tubes were cut through; and that nothing could prevent this sterility; but my contemplations were directed into another channel by the following experiment.

EXPERIMENT.

Having procured another rabbit, nearly under the same circumstances as the last, I operated precisely in the same mode, and had equal evidence too concerning the condition of the ovary. The result of this experiment was successful; for on admitting the male to her about one month from the operation, she betrayed no reluctance, and became impregnated. Ten days afterwards she was killed, and opened. Both ovaries retained their primitive plumpness, and manifested the evidences of impregnation. These evidences are the presence of corpora lutea, bearing the same precise characters as I have demonstrated in the former part of this essay. Those seated in the ovary of the mutilated side did not differ in any respect from the same bodies on the perfect side: but they were unattended with fœtuses; whereas in the perfect side, there were as many fœtuses as corpora lutea

As this experiment had succeeded, I examined the divided tube with attention, to satisfy myself whether its canal was obliterated; and of this I had the clearest proof; for it would not allow quicksilver, nor even air to pervade it.

Now here is matter for reasoning. Both ovaries, it seems, bear unequivocal proofs of impregnation, but fœtuses are found only on one side.

Now, on what principle shall we explain these phænomena? It is certain that neither semen nor the aura seminalis could have touched the left ovary, and yet it bears the most unequivocal marks of recent impregnation. It must depend on some other cause than the actual contact of semen.

But an important subject for investigation here presents

itself. Why were there no fœtuses on the mutilated side; but only the corpora lutea? Is the application of the semen to the vagina or uterus sufficient to stimulate the ovaries to perform their first procreative operations, without enabling them to achieve any thing more? and does it require the permanent and active energies of this fluid, operating by direct contact on the surface of the ovaries, to produce the full measure of their effects? But as these are queries which cannot be answered from the mere reflexions of the closet, I must engage anew in the business of experimental inquiry. But the first step that ought to be taken in the management of this question, is to give full confirmation to the above fact, by a repetition of the experiment; I therefore engaged a keeper of rabbits to procure me six in high breeding condition, as soon as possible.

EXPERIMENT.

Within the space of a month, I cut through the fallopian tube on one side in six rabbits. The season was warm, and consequently favourable for breeding. As soon as they recovered they were admitted to the male: but out of this number two only were impregnated; and the keeper assured me that one of them had never been impregnated before. When the success in these experiments is compared with that of the former, there was no cause for complaint. Of these two which succeeded, one had three corpora lutea and three fœtuses in the perfect side, with two corpora lutea and *no* fœtuses on the imperfect side. The other, which was the virgin rabbit, had two corpora lutea and two fœtuses on the perfect side, with one corpus luteum and *no* fœtus on the mutilated side.

Having now three indisputable proofs of this important fact,

I consider it a full answer to any objection that can be urged on the ground of accidental appearance; and that what has been stated above, must, under the circumstances described, be considered as a law of the part; viz. *That the ovaries can be affected by the stimulus of impregnation, without the contact either of palpable semen, or of the aura seminalis.*

But I cannot expect that any physiologist, prepossessed with the common notion of the contact of semen, will yield assent to my position, without subjecting it to a severe scrutiny, and exposing every possible objection to which it is liable. It certainly would not be unphilosophic to ask, why fœtuses were not found either in the ovarium, or in the tube between it and the obliterated part, agreeably to the assertion of NUCK, if, as I contend, the ovary was affected by impregnation? Again, a tenacious opponent might further avail himself of this apparent difficulty, by alleging that if the tube had not been obliterated until after coition, the semen or its powers might have affected the ovary by actual contact; and the product of conception might have been more complete. And in support of this idea, he might adduce the result of an experiment said to have been made by NUCK, in which he made an extra-uterine case in a bitch, by tying one of the tubes three days after coition.

These objections have at least speciousness to recommend them to our notice; but it is from experiment alone that we can determine whether they have any solidity.

To the first difficulty I reply, that my experiments were not made under the same circumstances that NUCK's is said to have been; therefore, giving him full credit for what he has advanced, a similarity of result cannot be expected. But it is painful to me to differ from any writer of character in the statement of a

fact, where the truth is equally accessible to us both; and notwithstanding the respect I willingly bear towards a name that has both acquired and deserved considerable reputation, I must confess that it appears to me highly problematical, whether this celebrated experiment be a reality, or only an ingenious device. But some facts, which it will soon be in order to relate, will show (I think very clearly) that I rest my suspicion upon fair grounds. In the mean time I feel it incumbent on me to reply to the general principle of the objection, and to determine by experiment how far it is deserving attention.

Now, if there be any validity in the objection, it should necessarily follow, that if an opportunity was given for the semen to pass by the tubes to the ovaries; we might, by opening an animal at a proper time after coition, detect some disposition in the fimbriated extremities of the tubes to apply the semen, by first approaching, and afterwards embracing the ovaries; and this action ought, according to the common theory, to take place before the usual sign of conception is at all evident on those bodies, which in the rabbit is somewhat apparent in six hours, but unequivocally marked in twelve.

Again, admitting the probability of it, we are led to inquire by what power the semen can be conveyed to such a distant part. It must be either by the male, *vi jaculationis*, or by muscular power in the tubes, analogous to a peristaltic motion. If it were by the first mode, the conveyance would be instantaneous; but in the latter, some little time seems necessary to allow the tubes to be affected by the stimulus preparatory to their peristaltic action. Perhaps this question may receive some light from the sacrifice of a few animals, at different periods between the coitus and the first visible effects of impregnation;

and I considered it by no means inapposite to the subject, to determine whether these conjectures were authorized by any visible changes, either in the condition or situation of the tubes. But the fruits of this inquiry will appear by the following experiments.

EXPERIMENTS.

A female rabbit in high season was admitted to the male, and in a few minutes afterwards the ovaries and tubes were brought into view; but the fimbriæ were in their natural situation.

As soon as proper rabbits could be procured, I repeated this experiment on two others, with precisely the same consequence.

These facts militate strongly against the possibility of the conveyance of the semen to this part *vi jaculationis*, and demonstrably prove, as far as three facts can go, that if the moving power inheres in the female, it is not instantaneously exerted.

But are the powers of the fecundating fluid conveyed *at any time* by the tubes?

This simple question betrayed me into the prosecution of experiments to a greater extent than I at first expected; for the result of several of them was unsatisfactory: but being once engaged in the question, I felt myself compelled to prosecute it, by examining these parts at different periods from the coitus to the manifestation of its effects. But I found from a regular series of observations made on different rabbits, at every hour between the first and the ninth, that the fimbriæ remained nearly in their usual situation; and the only differ-

ence I perceived in the last hours, was a greater turgescency of vessels, as if preparatory to some important action. I desisted from this inquiry at the ninth hour, because the ovaries now bore very evident marks of impregnation; and there appeared to have been no action in the tubes by which the semen could have been conveyed to them.

The impression which these experiments at first made on my mind, was, I must confess, not altogether incongenial to my wish, in as much as they seemed to furnish a satisfactory answer to the question; but reflexions when more at leisure abated my confidence, and in the end convinced me that my proofs did not exceed probability, so that there was still room for the suggestions of scepticism: and indeed it might be said with great propriety, that the tubes might have inclined towards the ovaries in the intervals of the hours above mentioned, and have returned to their former situation, and thus have eluded my research. I think it but candid to acknowledge, that these last experiments do not prepare me to meet that objection.

These reflexions suggested to me the expediency of constructing a plan of inquiry more apposite to the subject; and attended with experiments bearing more directly on the point at issue. Under this impression I determined to obliterate one of the tubes at different periods *post coitum*, and after the lapse of a sufficient length of time, to notice the effect. My particular view in this was to allow sufficient time for the arrival of the semen at the ovaries, supposing it to take place; so that if they were stimulated by an affusion of that fluid, either in a palpable or insensible form, here would be time allowed sufficient to produce its effect; and if in this mode fœtuses could

be formed, while by obliterating the tube *ante coitum* nothing more than corpora lutea were seen, it furnished an argument of no inconsiderable force in favour of impregnation by immediate contact; but if on the contrary, corpora lutea ONLY were found, then such experiments would give additional force to the arguments stated in a former part of this section.

EXPERIMENT.

One of the tubes of a rabbit was divided half an hour *post coitum*, and the wound closed as before. She was kept a fortnight, that I might know the result; but there were no marks of impregnation on either side.

Though a failure of impregnation has been very common in experiments connected with the mutilation of these parts, I apprehended that the derangement in the present instance proceeded from some disturbance given to the procreative operations in their commencement, and therefore determined in the next trial to wait a few hours, the better to avoid this.

EXPERIMENT.

I repeated the operation on two other rabbits, in one at four, and in the other at six hours after coition. On inspecting the parts at the end of a fortnight, the first was not impregnated, but the last was. In this there were four corpora lutea in the right side, answering to the same number of fœtuses in the cornu uteri of that side; but on the left or imperfect side, there were three corpora lutea without fœtuses. The corpora lutea on both sides were cut open, but not the slightest difference could be detected.

Now, if the contact of the semen with the ovaries in any

form be essential to impregnation, here has been an opportunity for such contact during the space of six hours; but it has not been sufficient to advance the procreative operations further than happened in those experiments where the tube had been divided before coition. Let us then for a moment suppose that the interval be lengthened, in order to allow a better opportunity for producing the full effects of impregnation, by exposing the ovary a longer time to the stimulus of the semen.

EXPERIMENT.

I cut through the left tube of another rabbit twelve hours *post coitum*, and examined the parts on the fifteenth day. There were four corpora lutea with the same number of fœtuses on the right side, and three corpora lutea without fœtuses on the left; so that twelve hours supposed exposure to semen, had made no sensible advances in the procreative operations on the mutilated side.

EXPERIMENT.

The same operation was repeated twenty-four hours *post coitum*. Corpora lutea were found in both ovaries, but fœtuses only on the perfect side.

Now I observed in one of the experiments related in the former part of this essay, that the vesicles of the ovaries when examined forty-eight hours *post coitum*, were extremely prominent; they appeared as if going to burst: it is but reasonable then to admit, that at this time they must have received their full measure of stimulus; and if one of the tubes was divided in this state of things, the result would be more decisive.

EXPERIMENT.

The operation was repeated under the circumstances just described, and in fourteen days the result was ascertained, viz. three corpora lutea and as many fœtuses on the perfect side, and two corpora lutea without fœtuses on the imperfect one.

Now, what mode of reasoning ought we to adopt here? Has the mutilating process suspended the effect of that stimulus which impregnation had begun? and are those appearances in the ovaries, any thing more than incipient relapses into evanescence? Such really appears to be the state of things, and seems to mark in a decided manner, a sympathetic connexion between one part of the uterine system and another. And were I to adopt the language of a late celebrated physiologist, I should say "that the ovary on the imperfect side, "feeling the inability of the tube to transmit its contents to "the uterus, the proper receptacle, had suspended the usual "operations of these parts, from a consciousness of their in-"utility."

This reasoning will probably appear not perfectly consentaneous to certain well established facts on the subject of extra-uterine fœtuses; for dissection has fully evinced the possibility of a fœtus being perfectly evolved, and of acquiring considerable bulk, either in the ovary, abdomen, or tube.

I do not hesitate to acknowledge the full force of these facts; but I cannot admit that they subvert the principle I wish to establish from experiment; because I conceive there is an essential difference whether nature spontaneously dispenses with her usual modes, and attempts to effect her ultimate purpose by irregular means; or whether, proceeding in

the ordinary course of her operations, she suffers an impediment which a physiologist may have produced to thwart her designs. In the first case, she may be provided with an expedient; in the last, she will probably be left without resource.

Here again we may notice the experiment mentioned by NUCK, which, though under similar circumstances, was attended with a different result. Some who feel themselves disposed to venerate his authority, will probably oppose his experiment to mine, and think it incumbent on me to account satisfactorily for the difference. I can by no means acknowledge such an obligation; for to confer validity on experiment by reasoning, is to invert the order of inquiry, and support facts by conjectures. It is sufficient for my credit to be able to adduce evidence of the truth of what I advance, and for this evidence I rely on my preparations.

The train of reasoning which I have lately pursued, led me to extend my inquiries into this particular question still further; and as in the last experiments the vesicles were known to be just on the point of bursting before the tube was cut through; the next step in the inquiry appeared to be, to determine the consequences of dividing the tube a short time after the rudiments of the fœtus had passed. Will the procreative operations be suspended, if the tube be cut through after the ovum is deposited in the uterus?

EXPERIMENT.

I repeated the operation on two rabbits, one of which had received the male two days and eighteen hours, the other two days and twelve hours. I knew from my own experiments, as well as those of DE GRAAF, that the vesicles had

discharged their contents before either of these periods. The examination of these at the usual time, proved that the actions of these parts suffer no interruption by a division of the tube made after the rudiments of the fœtus have been conveyed into the uterus; for there were corporea lutea in both ovaries, and fœtuses in both cornua uteri.

These experiments I think overturn (as far as experiment can) every argument which has hitherto been adduced to support the hypothesis, that the affusion of the semen on the ovaries, either in a sensible form or in that of aura seminalis is essential to impregnation: for if the ovaries were susceptible of their proper excitement only by the contact of semen, by what accident has it happened that the effects of that excitement are not more obvious and further advanced in those experiments, where nothing was done to intercept its course for forty-eight hours, than in those where all communication between the uterus and ovary had been cut off before the means for impregnation had been employed? We should expect in the one case to find the full effects of impregnation, and in the other no traces of it would be seen; instead of which, the procreative actions are no further advanced where there has been an opportunity for the passage of the semen, than in those cases where the passage has been impossible. But if we defer the mutilation until the ovary has perfected its work, which it does in a rabbit in something more than fifty hours from the approach of the male, then the generative process is not disturbed, and the evolution of the fœtus goes on in the usual manner; for now all the different parts of the uterine system being in a condition to act, each performs its peculiar office.

First. The semen by its presence stimulates either the vagina, os uteri, cavity of the uterus, or all of them.

Secondly. The impression made on these is propagated to the ovaries by consent of parts.

Thirdly. One or more of the ovarian vesicles enlarges, projects, bursts, and discharges its contents.

Fourthly. During this process in the ovary, the tube is undergoing a state of preparation for the purpose of embracing the ovary, and receiving the rudiments of the fœtus.

Fifthly. This preparation consists in part of an increased turgescence of its vessels, and a consequent enlargement of its fimbriated extremity. When thus prepared, it approaches the ovary.

Sixthly. After the tube has performed its office by a peristaltic motion, commencing at the fimbriæ, and terminating in the uterus, it gradually returns to its former situation and condition.

Seventhly. While these different actions are going on in the appendages of the uterus, others not less important to the design of nature are instituted in the uterus itself: for the tunica decidua, where it is obvious, is formed ready to secure firmness of connexion between the tender ovum and internal surface of the uterus, until a proper attachment by means of placenta can be effected.

Eighthly. By way of guarding with additional security against a premature escape of the ovum, an apparatus, seated in the neck and mouth of the womb, now begins to develope its real structure, and perform its proper action, consisting in the secretion of a mucus-like substance, sufficient in quantity to fill completely the whole length of the neck, and by that

means to seal up the communication between the cavity of the uterus and vagina.

Ninthly. Nor does the care of nature for the preservation of the new animal terminate here; for while she is by various means forming and perfecting her work, at least as far as comes within the province of the uterine system, she is at the same time making preparation for its nourishment after birth, by instituting the proper secretion of the breasts.

When we take a reflected survey of these successive operations, I think it must appear, on tracing nature's steps through the different stages of this work, *that they are the product of that law in the constitution which is called* SYMPATHY, *or consent of parts.*

That the semen first stimulates the vagina, os uteri, cavity of the uterus, or all of them.

By *sympathy* the ovarian vesicles enlarge, project, and burst.

By *sympathy* the tubes incline to the ovaries, and having embraced them, convey the rudiments of the fœtus into the uterus.

By *sympathy* the uterus makes the necessary preparation for perfecting the formation and growth of the fœtus. And,

By *sympathy* the breasts furnish milk for its support after birth.

Having now investigated this intricate question, I hope with some regularity; the design of this essay next leads me to consider the state or form of that substance which passes from the ovaries in consequence of impregnation.

SECTION III.

What is the Form of that Substance which passes from the Ovaries in consequence of Impregnation?

No sooner had the researches of the physiologists retraced the existence of the new-born animal to the ovaries, than their curiosity was excited to discover the form it assumed while resident in these bodies, and especially at that particular time when the fœtal primordia are about to escape from them. The analogous phænomena of oviparous animals, and the structure of the ovaries as described by DE GRAAF, concurred to favour an opinion, that in viviparous animals there existed ova in these bodies, and indeed from this very circumstance they received their name. But though several physiologists have concurred in this opinion, there has not been any strict coincidence respecting their state while in the ovary. Some have thought that the vesicles described by DE GRAAF were the true ova, and that these are the bodies that are expelled by impregnation. Others, with greater probability, have considered these vesicles as the apparatus destined by nature, under the influence of the proper stimulus, to form the ovum: and though at all times they contain a glairy kind of fluid, from the stimulus of impregnation this fluid becomes a small vesicle or ovum seated within the larger vesicle, which now becoming thickened, and acquiring a yellow colour, is called the corpus luteum: from this body the interior vesicle or ovum is protruded.

Others again refuse assent to both these opinions, and contend that the substance extruded from the corpora lutea has

no vesicular appearance; and though by some it has been called an ovum, yet that name is not applicable to it from any resemblance of figure, but rather from its agreement with an egg in being the substance in which the rudiments of the future animal are contained,

De Graaf contended that the primordia fœtus while in the ovary is vesicular, as appears in his work; in which, after describing the enlargement of the proper vesicles usually connected with his name, he says, "præterea aliquot post coitum "diebus tenuiori substantia præditi sunt, et in sui medio "limpidum liquorem membranâ inclusum continent, quo unà "cum membranâ foras propulso, exigua solum in iis capacitas "superest." He is therefore decidedly of opinion, that as soon as the product of conception becomes the subject of notice, it has a vesicular form, and this he thinks takes place at the end of the third day, though the substance passes from the ovaries several hours before this time. He seems rather to assert, that it passes in a vesicular form, than to prove it; for in fifty-two hours after the approach of the male, he found the ovarian vesicles were empty, though he could not now find the new vesicles either in the uterus or the tubes. But in seventy-two hours they were so evident, that he could distinguish with ease the two membranes of which they are formed, viz. the chorion and amnios; so that they cannot be very small at this time. Hence it would follow, that if on a repetition of this experiment on the third day no vesicles should happen to be found, it would not be from minuteness that they would escape observation; therefore should any one be disposed to search for them, he need not bend his sight, as if looking at microscopical objects.

VALISNERI on the contrary searched for these eggs with great industry, accompanied with an ardent wish to find them; but though his experiments appear to have been judiciously conducted, he never succeeded.

HALLER also maintains, from a regular series of experiments made on sheep (whose term of utero-gestation is five months), that some days elapse between the escape of the substance from the ovaries, and the appearance of a circumscribed body in utero, which can properly be called ovum: and that this does not happen until seventeen days from impregnation. In the mean time, nothing but irregular masses of mucus are found. The circumscribed form at this time acquired seems to depend on the formation of the fœtal membranes now bounding the contained mucus-like substance. This apparently homogeneous mass, on the nineteenth day undergoes a change of character; an opaque spot is seen within it, which subsequent observations prove to be the first evident marks of the evolution or formation of the fœtus. From this dim speck of animal existence we may observe a series of regular advances, from an inorganized mucus-like mass to the most beautiful and complicated machine in nature. But to trace her progressive steps through this important work, forms no part of the design of this dissertation.

The chief difference between DE GRAAF and HALLER on this subject, consists in their opinions respecting the form of the substance that is passing from the ovaries, whether it is vesicular at this time or not; for in the subsequent processes they differ but little. No solution can be given of this question by force of reasoning; it is from experiment alone that we can receive conviction, notwithstanding the two contrary

opinions that prevail. All that can be expected from an individual in such a case, is to add the result of his own labours to one side or the other, so that in the end the preponderance must depend on the weight of evidence.

The experiments I have made on this simple question do not allow me to incline to the side of DE GRAAF; for in the rabbit I have never found any thing in the uterus which had a regular circumscribed form earlier than the sixth day, and even then the substance was bounded by a covering so very tender, that it scarcely had firmness sufficient to support the figure. Before the sixth day, I have never seen any thing but irregular mucus-like masses in the uterus; but after this time the substance has firmness sufficient to admit of preservation in spirits, a specimen of which I have in my collection of preparations.

This acquisition of figure does not depend so much on a difference of consistence, as on the formation of membranes inclosing this substance. These membranes when in a more advanced state of formation, are known by the names of *chorion* and *amnios*. The product of conception being arrived at this stage, may with some propriety be called an ovum, as it has acquired a determined figure; but the different constituent parts of it are not apparent at this early period; on the tenth day, in the rabbit, an opaque spot is seen in this ovum, which increasing daily in its bulk, progressively manifests the formation of the fœtus.

It is a little remarkable that in the rabbit, where the term of utero-gestation does not exceed thirty days, a third part of that time should be required to make that opaque spot obvious to the sight, whilst the remaining two-thirds should suffice to complete the formation of the fœtus. It appears as if it

required a more elaborate exertion of the formative powers of these parts to produce what might figuratively be called the nucleus of a fœtus, than to go on and complete the work. But this remark applies only to the rabbit; for in the human female, abortions at the third month clearly prove that the evolution of the fœtus has been perfected some time before. Such an obvious difference cannot fail to impress our minds with doubts and distrust, whenever we are drawing inferences from analogical reasonings: but to trace the formative process of nature through this work, and to compare her progressive advances in the different periods of utero-gestation, are foreign to the design of this essay.

It remains then for me to beg pardon for having so long trespassed on the patience of this Society.