

## VIII.

*On the Maternal Fœtal Circulation.* By DAVID WILLIAMS,  
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**T**HE inquiry relative to the nature of the medium, and the way by which the fœtus of the viviparous animal is nourished in utero, has engaged the attention of the philosopher from an early period. Of the doctrines which have been entertained on the subject, none, however, as yet, has been supported by conclusive observations, although the modern authors, whose

writings I am acquainted with, appear to be unanimous in giving their approbation to an ingenious hypothesis, to which we shall have occasion to refer hereafter.

The communication between the viviparous animal and its offspring in the womb, is effected in the secundines by two different orders of arteries and veins, and the placenta is generally described as being apparently composed of those vessels, blended either with cellular substance, or perhaps some peculiar, and not yet ascertained tissue. One order, the fœtal system of vessels of the placenta, is a continuation of those of the funis or umbilical cord; the other, the maternal or uterine system of vessels, arises from the vessels of the uterus. Equivalents in some respects to the secundines of the viviparous animal, are observed to be connected with the embryo of every organized production of nature, and in the ovum or seed of the oviparous species, the embryo and secundines communicate with each other by vessels, in like manner as in the viviparous species, but under many modifications. In those classes which exclude their young in the ovum, the secundines seem to be intended for two purposes;—one, to be a reservoir of pabulum, suited to the nourishment of the embryo for a limited period after its vital principle has been excited into a new action;—the other, the effecting a change in the circulating fluid of the embryo, analogous to that which it undergoes in the organs of respiration of its own adult species. Experiments and observations on the discotyledons, or plants whose seeds are furnished with two placentæ or cotyledons, prove the principal part at least of the nourishment of the incipient vegetation, to be derived from the pabulum deposited originally in the cotyledons; and also its aeration to be carried on through the vessels of the latter. Thus during germination, the vegetable is nourished by the cotyledons, until its roots and leaves are sufficiently matured to perform their adult function. Then the communication ceases between the cotyledons and the infant plant, for the same reason that the communication ceases between the secundines, and the fœtus and uterus in the viviparous class at the time of parturition. Experiments and observations likewise demonstrate, that the vitellus and albumen of the ovum of the fowl, contribute, during incubation, to the nutrition and growth of the chick, and that the exterior membranes which envelope them act as organs of aeration.

Notwithstanding our intimacy with the channels and fountains which supply the embryo of the fowl and vegetable with food and air, our information continues to be extremely defective, relative to the nature of the immediate mode of communication

which exists between the vessels of the fœtus of the viviparous species and the sources of its nutrition. The absence in the uterus of reservoirs of pabulum adequate to the support and increase of the fœtus during gestation; the connexion of the uterus with the secundines, and the latter with the fœtus; the fatal effects which speedily follow the compression of the umbilical cord, whilst the offspring is *in utero*; and also, the consideration that the fœtus itself is incapable of creating materials for its own increment, have induced philosophers to infer, that the fœtus of the viviparous animal must needs receive its nutrition from the system of the parent, according to its present exigency. Though all have acquiesced in this opinion, yet different hypotheses have been entertained by different individuals respecting the quality of the nutrition which passes from the uterine system of vessels into the fœtal.

Many physiologists have supposed the blood itself to be circulated between the vessels of the uterine system, and the vessels of the umbilical cord. Hieronymus Fabricius was one of those who assented to this opinion. He imagined that the veins conveyed the blood, and the arteries the spirits (*spiritus vitalis, seu calor cordis*), from the system of the uterus into the system of the fœtus, uninterruptedly, and by continuity of canal. So, agreeably to the popular philosophy of the times, Fabricius fancied that the fœtus received its blood and spirits directly from the liver and heart of the parent. \* Julius Cæsar Arantius, a contemporary of Fabricius, agreed with those who maintained a different notion. He supposed the nutriment (*humor*) and spirits to be imbibed by the small orifices of the extreme ramifications of the umbilical vessels, from the uterine system of the placenta. † These were the prevailing doctrines of the day, when our illustrious countryman made the grand discovery of the greater circulation. The author of the important physiological revolution, approved of, and zealously supported the main principle of the hypothesis subscribed to by Arantius; and he expressed his sentiments very explicitly, both on the nature of what he conceived the circulating medium to be, and on the structure forming the communication between the vessels of the fœtus and those of the uterus. Dr Harvey says, that he thinks the placenta to be appropriated to the secreting of pabulum for the nourishment of the fœtus, and he compares its function to that of the liver and mammæ. “*Jecur, inquam, est organum nutritivum corporis, in quo est; mamma, in-*

\* De formato fœtu, pars secund. cap. De vasorum umbilical. utilitatibus.

† De humano fœtu, cap. De generatione et usu uterini jecoris.

fantis; placenta, embryonis."\* In examining the doctrine of inoculation of the maternal and foetal systems of vessels entertained by Fabricius ab aquapendente, Dr Harvey remarks, that there can be no greater reason for supposing the umbilical and uterine vessels anastomosing one with the other, than the sanguiferous vessels of the mammæ anastomosing with the excretory ducts of the lacteal glands. Also, in speaking of the cotyledons or placentulæ of glanduliferous animals, he has these words: "Cavitates istæ, spongiæ majoris loculamenta magnitudinæ non excedunt; usque singulas earum, totidem vasorum umbilicalium ramuli tenuissimi profunde penetrant: quippe in iisdem alimentam fœtui reconditur; non quidem sanguinetum (ut Fabricius voluit), sed mucosum, ovique albumen crassius plane referens. Unde etiam manifestum est (ut dudum diximus) bisulcorum fœtus (ut et alios omnes) sanguine materno non ali." †

In consequence of an Essay on the Nutrition of the Fœtus in Utero, by Mr Gibson of Leith, ‡ who defended the doctrine of the fœtus being nourished both by the mouth and by the vessels of the umbilical cord, the attention of Professor Alexander Monro *primus* was particularly called to our subject. In an Essay, in reply to Mr Gibson, by the Professor, we have quotations from several experiments that have been published; in which it is asserted by their respective authors, that injections had passed from the uterine arteries into the umbilical vessels. As I have not been able to procure a perusal of the original works, in which the experiments are detailed, and as Dr Monro has brought forward very material observations in discussing their merits, I have taken the liberty of quoting the following information from his interesting production. § The first experiment alluded to by Dr Monro, is reported by Dr Drake, in his *Anthropologia Nova*, as having been executed by Mr William Cowper. Dr Drake says, "Mr Cowper kept by him of a cotyledon and part of a uterus of a cow, in which mercury poured into a branch of the uterine artery, went into one of the cotyledons of the uterus, and filled those branches of the umbilical veins which went from the cotyledon to the navel of the fœtus." *Secondly*, Dr Noortwyk, in his *Uteri Humani Gravidæ Anatomia et Historia*, affirms, in his description of the dissection of an injected impregnated human uterus, that his injection passed from the uterine arteries into the vessels of

\* Exercitationes de Generatione Animalium. De Placenta.

† Exercitatione de Generatione Animalium. De Acetabulis.

‡ Edinburgh Medical Essays. Vol. I. Art. XIII.

§ The Works of A. Monro, M.D. Essay on the Nutrition of Fœtus.

the chorion and placenta. The force of this assertion, however, is entirely done away with; for Professor Monro makes it quite evident, that Dr Noortwyk committed an error, in mistaking for the placenta the cellular substance situated between the muscular coat of the uterus and the villous membrane which forms its internal covering. In the same narration, Dr Noortwyk also says, that "the injection into the impregnated womb did not penetrate into the fœtus or umbilical rope." And again, he says, "On the side of the placenta farthest from the womb, the red colour of the injection shined through the chorion; and in one part, where the chorion was taken away, the small injected vessels were to be seen in the preparation." *Thirdly*, "Slade is quoted by Blasius for saying"—"The liquor injected into the arterial vessels of the uterus (of a cow), was carried to the cotyledons, and by the cavities of the cotyledons into the substance of the placenta." *Fourthly*, and *lastly*, Dr Monro quotes Dr Raymond Vieussens, who is said to have made the following experiment. "He tied the left carotid artery of a living bitch with young; and then, having put a small ivory funnel into the right carotid, he poured quicksilver at different times towards the head, till it amounted to about four pounds. By the time this quicksilver was poured in, the creature appeared to be quite dead, and he dissected her before a great many witnesses." After describing the progress which the quicksilver had made in the vessels of the bitch, he says, "That fluid (the mercury), without breaking any vessel, or the effusion of one drop of blood, passed through the placenta surrounding each whelp, and was pushed into the umbilical veins themselves." In another treatise, he (Dr Vieussens) gives an account of this experiment in these words: "Mercury being poured into the right carotid artery of a bitch about two months gone with whelp, the left carotid artery being tied, passed into the umbilical vein of the whelps, without any breaking of the vessels."

The subjects of Cowper, Noortwyk, and Slade, had evidently been dead for some time previous to their being injected; consequently, the irritability of their bloodvessels must have been so far exhausted, as to have been incapable of rendering any assistance in the circulation of the injected fluids. As they were placed in that respect under similar circumstances, we shall give them a general consideration, by principally quoting a few of Dr Monro's experiments and observations. "Drake," says the Professor, "expresses himself so little like an anatomist, in comprehending both the glandula of the womb, and the placenta of the secundines, under the name of *cotyledon*,

that I suspected his committing a mistake ; therefore, I repeated the experiment many times, by pouring mercury sometimes into a branch of the uterine arteries distributed to one of the glandulæ, and at other times, I poured the mercury into a branch of one of the umbilical arteries sent to a placenta, but never could make one drop of it go from the vessels of the one into the vessels of the other." In another place the Professor tells us, " Having fixed a pipe into one of the iliac arteries, and having tied the other iliac artery and the veins, I pushed through the pipe fine oil of turpentine. I continued this injection till all the vessels of the womb were in hazard of bursting. Not one drop of this oil was found in any branch of the umbilical vessels, or in the fœtus." Professor Monro, in concluding his observations on the experiments attributed to Cowper and Slade, says, " I have tried injections of very different kinds so often into the vessels of the womb and secundines of cows, prepared in all the different ways I could contrive for making liquors pass from the one to the other, without having once made a drop to pass, that I cannot be more certain of any thing, than that there is no anastomoses or continuity of these vessels in cows." The shining of the injection through the chorion and amnios, as remarked by Dr Noortwyk, is a common occurrence. For, when we inject from the uterine arteries, we render a portion of the placenta turgid, by forcing the injection into its cells and fissures.

We have next to examine the assertion of Dr Raymond Vieussens, respecting the result of his strangely conducted experiment. Although Dr Vieussens's subject was alive at the commencement of the experiment, yet, from the manner in which he conducted it, the sensibility of the animal must have been extinguished for some time previous to the quicksilver reaching the uterine system of vessels. The advantage, therefore, that Vieussens derived from the irritability of the vessels in circulating his injection, can be equally as well insured by the forwarding of the mercury into the uterine vessels, the instant the animal is destroyed, through a more direct channel than the one he selected. Dr Monro says, " I endeavoured to imitate Vieussens's experiment on a living bitch, but the creature died before any success could be expected; and therefore, with the assistance of my colleague Dr Andrew Sinclair, P. M., and of Mr Gibson, I took another way to try if the mercury would pass from the womb into the umbilical vessels." Then Dr Monro gives us an account of two pregnant bitches, into whose carotid arteries he introduced quicksilver in the direction of the aorta immediately after their destruction, at the same time sus-

pending them by the necks, (above where the pipes were inserted into the vessels), so as to give the pressure of a high column. In describing the appearances of one of them on dissection, he says, "After the amnios was opened, there was no mercury to be seen in the fœtus, or in the umbilical vessels, though we (meaning Professor Sinclair, Mr Gibson, and himself) could trace these to their very minute branches in the placenta and membranes."

To make it yet more satisfactory to myself, as to whether quicksilver would pass from the uterine into the umbilical vessels, while the irritability of the system was still active, a bitch that had gone about two months with young was pithed, and immediately quicksilver was introduced into the descending aorta. The animal was suspended by the neck. After continuing the process until the hind feet were pretty tense with the mineral, the uterus was opened, and the fœtus were carefully examined. Not a globule of quicksilver could be detected in their systems, or in any of the branches of the umbilical vessels. When the placentæ were separated from the uterus, a quantity of mercury escaped from the vessels of the latter. In one of the placentæ which had been slit open, a globule of quicksilver was observed sliding below the amnios and chorion. From its bulk, it was evident it was not within the coats of a blood-vessel, which was proved to be the case; for, on gently opening the exterior or uterine surface of the placenta, above the apparent lodgement, and inverting it, the globule dropped out. For the purpose of seeing whether the irritability of the blood-vessels was equal to the propelling of mercury against gravity, another animal was destroyed. Instantly after tying its trachea, four pounds of quicksilver were gradually introduced into the left carotid, (the head being suspended over the edge of the table), and, in imitation of Vieussens's experiment, a ligature was passed round the right carotid. When about one half of the mercury had been poured into the artery, it began to escape at the nostrils. On examination, it was found to have ruptured the vessels of one of the eyes, and to have insinuated itself into its chambers. None of the mercury could be detected in the vessels of the thorax and abdomen, while the head remained suspended. On raising the head above the level of the body, the quicksilver rapidly penetrated into every part of the system. From this experiment mercury would seem to be too heavy a substance for the irritability of the blood-vessels (immediately after the destruction of an animal) to circulate against gravity. The confused description given by Raymond Vieussens, of the progress made by the mercury along the umbilical vessels, in-

clines me to think, as it did Dr Munro, that he must have fancied the mercury, worked into the fissures of the placenta, (as the secundines were handled,) to have been contained within the coats of the branches of the umbilical vessels. The hypothesis approved of by Dr Vieussens seems to throw a doubt of his own firm conviction of the mercury having entered into the umbilical vein. If he had really believed that the mercury had circulated from the uterine vessels into the umbilical, would he not naturally have maintained that red globules of blood were probably circulated to and fro, between the maternal and foetal systems? However, instead of being the advocate for a doctrine of this kind, we find Dr Vieussens giving his approbation to the opposite hypothesis.

From the above statements, and from the sentiments expressed by the authors I have perused, it appears to me, that neither mercury, nor any subtle fluid that is used as an injection, can be forced from the maternal into the foetal vessels, and *vice versa*, after the irritability of the blood-vessels has become extinct. Also it appears, that although we avail ourselves of the irritability of the blood-vessels the moment an animal has been deprived of sensibility, yet we cannot introduce quicksilver from the sanguiferous system of the parent into that of the foetus.

The vain efforts of Dr Monro and other anatomists, in their repeated endeavours to inject the foetal system of vessels from the uterine, brought the speculation of the blood being the circulating medium into disbelief. In process of time, the failure, by passive consent, came to be admitted to be one of the principal proofs of the formation and circulation of the blood of the foetus being independent of the parent, excepting that the materials for its fabrication could only be derived from the system of the latter. When the influence of the atmosphere on the blood in the adult lungs, and the action of the same agent on the blood of the chick *in ovo*, and on the fluids of the embryo vegetable, became to be duly appreciated, it was conjectured that the blood of the foetus must also be exposed by some means to the influence of the same vivifying element. As the foetal blood, according to the prevailing theory, could have no immediate access (such as the blood has in the adult lung) to the atmosphere, it was hinted by Sir Edward Hulse,\* that the placenta might be an organ of respiration or oxygenation, as well as an organ of nutrition. This idea by degrees gained ground, and at present the placenta is considered as appropriated to the secreting of *pabulum nutritionis*, and to the executing the office

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\* Study of Medicine. Fifth Physiological Proem.



of a lung. The blood of the fœtus, when conveyed into the substance of the placenta, is imagined to be there exposed to the influence of the maternal blood, which is thought to be so charged with the vivifying principle of the atmosphere, as to effect on the fœtal blood an analogous change to that which it undergoes in the lungs after birth. There is something extremely plausible and fascinating in this hypothesis, yet the data which have given rise to it appear to me to be deduced from analogies and observations of too ambiguous a nature, to sustain satisfactorily the inferences drawn.

While reflecting on the power which I had observed the blood-vessels to possess, of circulating oily fluids for a time after an animal was destroyed, and also on the obstruction to the circulation in the lungs, which takes place on the stoppage of respiration,\* they appeared to me to present a clue by which the nature of the communication between parent and fœtus might be disclosed. The following investigation then was undertaken, from a preconceived idea that the *vis insita* of the blood-vessels might mainly contribute to the establishing, by experimental observation, one of the physiological speculations we have been reviewing above. To give an insight of my preconceived views, I shall briefly mention the phenomena which I thought would necessarily result from, and consequently indicate, each mode of communication, on injecting oil into the maternal arteries, immediately after the destruction of an impregnated animal. First, it was presumed if the communication should be carried on between the fœtal and maternal vessels uninterruptedly, that the injection would permeate the vessels of the fœtus in the form injected into the system of the parent. Secondly, if the communication should be through a secreting system, (secreting *pabulum nutritionis*), that the injection would not pervade the fœtal system of vessels under the form introduced into the vessels of the parent. Thirdly, that if the placenta, in addition to the last presumed function, performed also the function of a lung, then, besides the exclusion of the injected fluid from the fœtal system, it was imagined that an obstruction and engorgement would take place in the umbilical arteries, as oil reached the maternal portion of the placenta. For, as an obstruction and engorgement takes place in the pulmonary artery when respiration is stopped, from a deficiency (in the atmosphere pent up in the lungs) of the principle necessary to the decarbonization of the blood; so, on the same principle, it was conceived that a similar pheno-

\* Edinburgh Med. and Surg. Journal, vol. xix. p. 526.

menon in the umbilical arteries would be the result of the presence of oil, instead of the vital stream in the uterine system of vessels. The anastomoses of the umbilical arteries and veins were expected to lessen the engorgement; but, at the same time, it was thought that if the placenta performed the function of a lung, that the mode of communication between the extreme branches of the umbilical arteries and veins could not deviate greatly from that of the pulmonary vessels, therefore that a turgescence of the umbilical arteries would discover the presence of an obstruction in their system. These speculations were made known to my friend Dr Traill some months previous to putting them to the test of observation; and in that interval, with his usual candour, their merits were often discussed. I may truly say, from the important assistance I have received from the same gentleman during the present inquiry, that few could be more advantageously associated in an investigation.

*1st Experiment.*—A bitch of a diminutive size, that had gone about six weeks with young, was destroyed by tying the trachea. Then an ivory pipe was introduced and secured in the descending aorta, within the cavity of the thorax; through which warm linseed oil, coloured with alkanet, was injected by a small common pewter syringe, with as little delay as possible. To prevent regurgitation during the refilling of the syringe, the orifice of the pipe within the artery was closed by the finger. After continuing to throw the oil in, until we thought it had traversed the system of the womb, Dr Traill and myself commenced the examination. On exposing the viscera of the abdomen, we found that the colour of the oil rendered our injection visible through the coats of arteries, as well as the veins; and from its being of a different tint to the blood, we could distinctly trace the extent of its progress along the vessels on the external surface of the uterus. The womb contained three pups. We extracted two of them, and then drained the vessels of the ends of the umbilical cords attached to the foetus, upon white paper. A film of oil was distinctly seen on each of the drained portions of blood. Also, on making incisions into the bodies of the two foetus, oil was apparent on the surface of the blood, which flowed from the divided vessels. The remaining pup was preserved.

*2d Experiment.*—To ascertain whether there was any peculiarity in the mode of communication between the above subject and her young, another of her own species, and about the same size, was pithed and injected. Films of oil were discovered on the blood received on paper from the vessels of the um-

bilical cords. Oil was also perceptibly seen on the surface of the blood, which escaped from incisions made in the bodies of several of the pups.

*3d Experiment.*—Dr Bostock and Dr Roget being in Liverpool, I had the pleasure of communicating to them the above experiments, and at the same time of expressing a desire to be favoured with their presence to witness a similar one; which invitation they kindly accepted. Our subject being of a large size and surly withal, it was deemed prudent to stun her by a blow on the head, before attempting to pith. Linseed oil, coloured with alkanet, was thrown into the descending aorta, by means of the apparatus already described. In consequence of Dr Traill and myself having repeatedly been able to trace the effect of the momentum given to the injection by the depression of the piston, as far as the extreme branches of the arteries (and no farther, the circulation in the veins being always uniform), an opening was made in the upper part of the abdomen, for Dr Bostock and Dr Roget to observe the like phenomenon. Contrary to my expectation, in this instance the impulse given to the oil at each ejection was but very feebly detected even in the larger branches of the mesenteric artery. This deficiency of power (in the impulse communicated by the discharge of the syringe) to produce shocks or pulsations in the minute arteries, arose partly from the apparatus being imperfect and too small for the size of the animal, and partly from the regurgitation of oil which took place, from the fingers of the assistant and my own being repeatedly seized with the cramp, while stopping the extremity of the pipe inserted in the aorta, at the time of the refilling of the syringe. It hence appears to be necessary, for the production of a visible shock in the terminating branches of an artery, that the quantity of liquid emitted, and the force applied to its ejection, should be in proportion to the calibre of the vessel; and likewise, that none of the injection should be allowed to escape during the process. After finishing injecting, the cells of the womb containing the pups were alternately opened, and the fœtuses extracted and separated from their secundines. The blood of each, as it issued out of the umbilical vessels, and from the incisions that were made in the bodies, was received on paper in several distinct portions. On examining these portions, we discovered films of oil only on a few of them. As oil might possibly have come in contact with the skins of the pups by extracting them out of the womb, and be the source of the few oily films observed, it was proposed, as an *experimentum crucis*, that two of the pups which yet remained without being incised,

should be washed under the spout of a pump. By this precaution, we thought to evade the possibility of being deceived by oil that might be adhering to the external surfaces of the pupa, spreading itself imperceptibly over the blood, which would issue out of the incisions that would be made in their bodies, after their ablution. The two fetuses being washed, their internal cavities were laid open, and the large blood-vessels were divided, but no appearance of oil could be detected on the blood which oozed out.

*4th Experiment.*—Dr Traill present. Our subject measured sixteen inches from the occiput to the coccyx, and was so far advanced in pregnancy, that the superficial blood-vessels of the pups could not readily be observed on account of the development of the hair. Pithing was the mode of destruction. Afterwards, a stop-cock pipe was fixed in the descending thoracic aorta, through which a pint and a half of linseed oil, coloured with alkanet, at the temperature of about 100° of Fahrenheit, was thrown in by means of a three-ounce brass syringe. When half the oil had been injected, an opening was made in the right auricle of the heart, through which oil soon began to issue. It was supposed that this aperture would prevent an accumulation of blood in the cavities of the right side of the heart, and the large venous trunks; therefore, that the oil would be more freely circulated in the uterine vessels, as no stagnation could take place in them from an engorgement in the inferior cava. The pups were extracted, by making incisions across and through the parietes of the uterus into each cell, above the portion of the ovum, at either end, which extends beyond the annular placenta. After the opening was made, the uterus was grasped in such a manner, that the foetus and its membranes were pressed towards the aperture, through which they were made to protrude beyond the cut edges of the uterus. The chorion and allantois were ruptured with hooks, then the amnion and foetus were drawn out. This method was adopted as the one best calculated to reduce the chances of oil from the divided edges of the womb coming in contact with the foetus. The first pup extracted had its funis secured. After washing it in different vessels with warm water, until no oil could be perceived on the surface of the latter, it was laid aside on a plate. Another nidus was exposed, the foetus of which, after dividing the umbilical cord, was placed instantly to bleed in warm water. The action of the heart, which was feeble on its immersion, became so stimulated by the heat, as to project the blood out of the umbilical arteries to some distance, and to pulsate sixty-six times in a minute. Oil immediately began to make its appear-

ance on the surface of the water, over which, by degrees, an oily film was spread, and ultimately, in several places, particles of oil congregated and formed extremely minute flakes. The first pup extracted, after having laid on the plate awhile, was put in a basin containing warm water, at the same time the funis was divided between the ligature and umbilicus. The action of the heart was not renewed, but the blood flowed from the vessels of the cord pretty freely, and oil gradually ascended to the surface. The third pup extracted had its funis tied, was washed, then after dividing its cord, between the knot and umbilicus, it was dropped into a vessel which held warm water; the vessels of the cord bled freely, and by degrees oil became visible on the water. The accumulation of oil on the water in the two latter vessels was not so considerable, as on that of the first. The fourth and fifth pups had their umbilical cords tied. From a deficiency of warm water, some time elapsed before they could be washed and placed in the vessels in which it was intended that they should bleed. When they were placed in them, no blood would issue out of the umbilical vessels; incisions were therefore made into the cavities of the chests, and the large venous trunks were punctured. From the one that was first operated on, a few pellicles of oil ascended to the surface, but from the last none appeared.

*5th Experiment.*—All our subjects having been pithed with the exception of the first, this trial was instituted for the purpose of ascertaining whether more oil could be introduced into the fœtal system by destroying the parent by securing the trachea, than by pithing. The animal was nearly of the same size as the one described in the last experiment, and about the same period of pregnancy. After we had injected, a fœtus was extracted, and the like method was observed with it, as with those of the fourth experiment. Oil ascended to the surface of the water, but Dr Traill and myself could not determine whether one of the modes of destruction alluded to, would be more favourable than the other to the introduction of oil from the maternal into the fœtal system.

My friend, Professor T. Thomson of Glasgow, did me the favour of transmitting the above experiments to the Royal Society, before which they were read.\* Shortly after the reading of my paper, it was suggested, that it would be advisable, before committing it to the press, that a less equivocal mode of experimenting should be had recourse to. The objection raised against the validity of the foregoing experiments was, that the

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† See *Annals of Philosophy* for April 1825.

skins of the fœtus might have been accidentally besmeared with oil in the extraction from the uterus, and that this might have been solely the source of the oil perceived both on the water and on the blood. It was therefore thought, that as the manner of extracting the fœtus in our trials, did not undeniably guard against the possibility of its being besmeared with oil, nor the after part of the process, in the event of the accident, afford an unquestionable removal of the same, it was desirable that a more positive mode of experimenting should be devised. The relation of the vain trials that were made for the purpose of finding a method that would set aside all doubts on the question; would be of little interest. It will suffice to say, that the strong affinity of rape oil for the alkalies was availed of to do away with the argument advanced against the use of oily injections.\* This chemical affinity renders rape oil perfectly manageable, so that we can expeditiously and satisfactorily in the manner described in the following experiment, remove any of it that may come in contact with the fœtus during its extraction from the uterus.

*6th Experiment.* Dr Roget (being in town) was so kind as to assist Dr Traill and myself. Our subject had gone about seven weeks with young. Rape oil at the temperature of 60° Fahrenheit, was injected into the aorta, by means of the apparatus used in the fourth experiment. Three of the pups were alternately extracted, and then they were immersed in a strong warm solution of subcarbonate of potass, from which they were soon removed, and put in vessels with warm water. When we had satisfied ourselves that no oil ascended from them to the surface of the water, the umbilical cords (which had been secured previous to separating the fœtus from the placentæ) were divided between the ligatures and the navels; and also, the vessels of the cavities of the chest were opened. As the blood-vessels of the first and second fœtus examined, discharged their contents, oil continued ascending to the surface of the water. Oil also ascended from the third, but the quantity was much less than from either of the other two.

We shall now examine the results of our experiments. In the first, second, and fifth experiments, oil in every trial was seen on the surface of either the blood or the water, according to the mode of examination resorted to. In the third, it will be remarked, that oil was detected on a few only of the portions of blood received on paper, and that none could be perceived on the blood which issued out of the wounds inflicted on the bodies

† Linseed oil unites less readily with the alkalies.

of the two pups that were washed under the spout of a pump. From the subject of this experiment having been but indifferently injected, and from the partial appearance of the oil, doubts may reasonably be entertained respecting the source of the oil on the portions on which it could be detected. In the fourth and sixth experiments, oil ascended to the surface of the water from every pup that was examined, excepting from the last pup in the fourth. In these two latter experiments, it will be noticed, that a marked diminution in the quantity of oil on the water, could be discovered the longer the bleeding of the foetus was suspended; and after a time, it ceased to appear altogether. Could this gradual diminution and final disappearance, be attributed to the oil adhering more and more tenaciously to the sides of the bloodvessels as the circulation flagged? It is evident from our details, that in the most successful of our experiments, we introduced into the systems of the foetus but a small quantity of oil in proportion to the blood which we found in them. The present state of our knowledge may be too imperfect to solve the reason of this partial introduction of oil only, yet before I conclude, I shall venture to hint at the causes which I conceive to have obstructed the entrance of a greater quantity. What could have been the reason in Dr Monro's trials and my own, of the non-admittance of quicksilver into the foetal systems from the uterine, at a period that would have been favourable to the admission of oil? Did it arise from the quicksilver presenting a mechanical obstacle? Or, from its being obnoxious to the umbilical system?

From secretion being a function, the end of which is the formation of fluids which have no previous formal existence in the blood: the passage of the oil into the vessels of the foetus in our experiments, would appear to be inconsistent with the idea of its having passed through a secreting system. This position granted, it is highly probable that the capacity of the communication between the *uteri* and *foetus* in the subjects of our experiments, had been capable of allowing the passage of the red particles of the blood. Accordingly, I am disposed to conclude the maternal and foetal systems in the canine species, to be parts only of a common uninterrupted sanguiferous system. The analogous situation, in which the foetus of viviparous animals in general are placed *in utero*, as to the influence of external physical agents, appears to me to declare very obviously, the mode of communication between parent and foetus to be similar in all. If this analogical inference, and the previous conclusion be admitted, we shall have reason to doubt the validity of the doctrine of the maternal foetal communication as taught



by Dr Harvey, together with its modern superstructure. For if my experiments and deductions be correct, we can no longer subscribe to the hypothesis of there being two independent sanguiferous circulations in the impregnated state, and to the placenta being an organ of aeration.

Admitting the quality of the medium of communication between the uterine and umbilical vessels to be blood, it would seem to me, from the small portion of oil discovered in the foetal systems in our experiments, and from the foetus not being found deficient of blood when the parent is bled to death, that the maternal foetal circulation must be obstructed, previous to either the maternal, or the foetal vessels, becoming too debilitated to propel their contents. It is well known, that the foetal system does not appear to be in the least drained of its blood when an impregnated animal is killed by the butcher. In this case it cannot be questioned, that the heart of the foetus continues its action, and that the irritability of the blood-vessels of the maternal and foetal systems continues vigorous a while, after the left ventricle of the heart of the parent has ceased to propel blood to the uterine arteries. A cow far advanced with calf was slaughtered; presently, on examining the calf, it was found to contain a large quantity of blood, which issued out in a stream on dividing the jugular vessels. In this instance, the maternal foetal circulation must have been obstructed soon after the cow had been "stabbed," and while the irritability of the foetal heart and of the maternal and foetal vessels had been yet active. For the blood of the cow naturally rushing towards the point where there was the least resistance, the uterine vessels could only have replenished the system of the calf for a very limited period. Therefore, if the blood propelled to the placental by the umbilical arteries had been forwarded to the uterine system, as long as it had been able to have conveyed it towards the inferior cava, the calf would not have had in its system, in my opinion, so large a portion of blood as that which escaped out of its jugular vessels. It would appear to me to be a law of the animal economy, that as soon as blood ceases to enter the umbilical vessels from the uterine, that blood also ceases to enter the uterine vessels from the umbilical. Thus the draining of the foetus of blood, (in the event of the sudden death of the parent), would be prevented, and means would be gained towards its preservation for a time in the womb, independent of the maternal sanguiferous circulation. Likewise, as an additional means of preservation, the foetal blood propelled to the placenta, (after the maternal foetal communication has been cut off), would appear to pass from the umbilical arteries, through the



anastomosing branches existing between them and the umbilical vein, into the latter, to be again returned into the foetus. In this way, the foetal circulation after the cessation of the maternal foetal, might be carried on uninterruptedly as long as the irritability of the heart and vessels of the foetus would be able to circulate the blood. Is it not this adjustment of means to ends that regulates occasionally in some animals the foetal circulation during parturition, from the instant the sanguiferous communication is cut off between the offspring and the parent, until the former respire? And, is it not to the above law and prospective contrivance, that the infant owes its preservation when its life is saved by the Cæsarian operation after the death of the mother? Pray, does any blood pass directly from the umbilical arteries into the umbilical vein, while yet the maternal foetal circulation is going on uninterruptedly? If we accede to the affirmative of this interrogation, then we shall admit that a smaller quantity of blood enters the umbilical vein from the uterine system in a given time, than is returned to the placenta by the umbilical arteries from the foetal system within the same period. Or in other words, that the blood which is conveyed by the umbilical vein to the foetus is not *entirely* received *directly* from the uterine arteries, but partly from them, and partly from the umbilical arteries. This view, and the foregoing one which supposes an early obstruction of the maternal foetal circulation, on the destruction of the parent, offer, in my opinion, a probable explanation why comparatively to the quantity of blood, so small a proportion of oil, could be found in the systems of the foetus in our experiments.

Admitting that the uterine arteries supply *directly*, but a *portion only* of the blood returned to the foetus by the umbilical vein, it may be a query, whether the foetus of certain animals do not circulate blood that is more of the nature of arterial blood, than the foetus of other species? If so, then the young animals that are the least capable of enduring the privation of air, and that have the greatest power of generating heat at birth,\* may most likely be expected to be the more liberally supplied with blood *direct* (during the foetal state) from the uterine arteries, and *vice versa*.

In injecting the foetus from the maternal system, and in detecting the oil in the foetal, I think attention to the following suggestions might be of advantage. The impregnated subject should be deprived of sensibility, by having the trachea tied at the acme of inspiration, so that the lungs may be in a state of

\* De l'Influence des Agens Physiques sur la Vie. Par W. F. Edwards.

full inflation after the chest is opened. This mode of destruction gives the operator time to remove away the sternum and the cartilaginous ends of the ribs; and also of passing a ligature under the aorta, while yet the circulation is going on uninterruptedly. After this preparative step has been taken, puncturing the aorta, and the inserting and securing a pipe in it, with the ligature already prepared, should be the next object. As soon as this has been accomplished, the injection should be thrown into the pipe *immediately*. On the speedy execution of this act mainly depends the success of the experiments. Therefore, the sooner the shock, caused by the depression of the piston, pervades the uterine system of vessels, after the communication has been cut off with the heart, the better. The injection should be emitted from the syringe, not in an even stream, but in reiterated, quick, and smart discharges. This method greatly accelerates the motion of the injected fluid in the arteries, and, consequently, its passage to the placenta. What would perhaps send the oil still more expeditiously towards the placental system than the common syringe, though ever so expertly handled, from the time taken up in the refilling of it, would be an appropriate injecting apparatus, on the principle of Mr Reid's patent one for the stomach. For the same reason, such an apparatus would also be better adapted than the common syringe, for the purpose of exhibiting the nature of the progress of the blood in the artery and vein, as alluded to in third experiment. After the second syringe-full of oil has been injected, the right auricle should be punctured. This aperture will be a guide in directing when to desist injecting; perhaps, we should continue the process for two minutes, after the oil has commenced issuing out of it. This opening also, from its preventing engorgement in the vessels of the uterus, enables the oil to be circulated in them more freely. The moment we finish injecting, the examination of the fœtus should be entered upon without delay, in the manner described in the sixth experiment. The observance of this suggestion is of much importance. For, as the irritability of the sanguiferous system of the fœtus becomes more and more feeble, the presence of the oil on the water becomes less and less distinct, until at last none can be detected.

I shall here observe, that experiments similar to those adopted in the above inquiry, are extremely well calculated to show the state of the blood-vessels of the stomach and small intestines, under the different alternate conditions of the alimentary canal. To demonstrate the vessels in the opposite extremes of these conditions, it would be necessary that ani-

imals should be examined, after being destroyed in the following states. The stomach and small intestines of one animal should be empty of aliment; another, after twenty-four hours of fasting, should be allowed a full meal of animal food, shortly before its death; and the lacteals of another should be actively engaged in conveying chyle. After injecting subjects with coloured oil, under these conditions of the alimentary canal, the blood-vessels ramifying on the stomach of the animal lately fed, would be found to be much more conspicuous and larger than the corresponding vessels of the one with the stomach empty. Likewise, the mesenteric blood-vessels of the animal with the lacteals in a state of activity, would be much more conspicuous than those of the one with the lacteals in a state of rest. In a trial instituted in the presence of Dr Traill, the difference in the diameter of the gastric vessels of the animal injected with its stomach full, and that with its stomach empty, was very apparent; and to me it appeared much greater than I had imagined it could have been.