I have dried about two thouſand plants, among which are ſeveral new genera, and ſome ſpecies hitherto undeſcribed.

I ſometimes amuſe myſelf with electricity. This country is ſo dry, that the experiments often ſucceed without any ſtand of bitumen, pitch, ſilk, glaſs, &c. Our carpets and beavers are moſtly ſufficient to retain the electrical virtue, and prevent its ſpreading to the floor. Ten men ſtanding upright, one before the other, have been made electrical, and, being touched, have produced ſparks.

XLII. *Some Obſervations proving, that the Fœtus is in part nouriſhed by the Liquor Amnii.* By Malcolm Fleming, *M. D.*

Read Nov. 13, 1755.

BEſides the myſterious affair of generation, beſides the manner of the formation of the embryo, and the changes it undergoes, while it continues extremely ſmall, concerning all which we ſhall ever remain very much in the dark; there are not a few things relating even to the mature fœtus, which create matter of diſpute amongſt anatomiſts and phyſiologiſts. Of theſe the manner of its nouriſhment *in utero* is not the leaſt important.

The preſent ſtate of the controverſy concerning this matter turns on the following preciſe point, to wit, whether the fœtus in utero be nouriſhed ſolely by the blood, which is tranſmitted to it through the umbilical

umbilical cord; or whether it is likewiſe nouriſhed in part by the liquor amnii, in which it ſwims. I have ſaid the preſent ſtate of the controverſy; for I believe very few, if any at all, will maintain now-a-days with Claudius de la Courvée and Stalpart Vander-wiel, that the whole of its nouriſhment is conveyed by the mouth, and none at all by blood-veſſels.

Both ſides of the queſtion have been eſpouſed by able writers, and ſupported with great acuteneſs, erudition and induſtry. And as there ſtill ſubſiſts a diviſion amongſt candid and intelligent inquirers concerning this matter, it would appear, that ſome deciſive fact, ſome demonſtrative argument hath hitherto eſcaped notice, which, if clearly known and duly attended to, would have by this time ended the diſpute, and rendered the propoſition, which declares the truth, on which-ſoever of the ſides, an object of ſcience; which before was only matter of opinion.

An obſervation, which fell in my way two years ago, together with ſome ſubſequent ones, which I purpoſely made in purſuance of the hint ſuggeſted by the firſt, will, I hope, enable all, who candidly conſider the facts themſelves, and the conſequences that naturally flow from them, to determine certainly, on which ſide the truth lies.

Before I relate my obſervations, let me lay down a neceſſary preliminary, which I obſerve the writers on both ſides are either explicitly or implicitly agreed in; to wit, that if it be clearly made out, that the liquor amnii is naturally received into the mouth, ſtomach, and inteſtines of the fœtus, ſwimming in it;

it; in that caſe we are to conclude, that the fœtus is in ſome part nouriſhed by it. The whole tract of the alimentary paſſage abounds with abſorbent veſſels in the fœtus, more than in the adult animal; and eſpecially the ſmall inteſtines have lacteals plentifully opening into them. The liquor amnii is concretable by heat, like the white of an egg; which characteriſtic in animal juices is, I believe, denied by none to be a proof of their alimentary nature. To ſuch as will not grant this poſtulatum, if any ſuch there be, this paper is not addreſſed. *Contra negantem principia non eſt diſputandum.*

But I proceed to relate the obſervations.

On the 25th of July 1753, being informed, that a calf, come to full maturity was juſt then brought forth dead in this town (Brigg, in Lincolnſhire), which had been alive, and appeared ſtrong a very ſhort time before its birth. I begged it of the owner, ſuch inſtances being rare. The ſkin being of value, for it was an extraordinary large calf, it was ſent to my houſe flay'd. I firſt examined the thorax, which was my chief motive for begging it. All I ſhall mention now of what I obſerved there, is the experiment of the lungs of a new-born animal their ſinking in water. After cutting out the lungs and heart, I clipped off a piece of the former with ſharp ſciſſars, about an ounce weight, or more, and threw it into a baſon full of water. It quickly ſunk to the bottom, and ſettled there. Immediately after, I blew into the remaining part of the lungs, through the trachea; and though I could by that means diſtend them but very little, becauſe the air flowed out readily through the cut bronchia, and therefore acted but faintly

faintly on the other parts; yet a piece about the ſame bigneſs with the firſt, clipped off in the ſame manner, and thrown into the ſame baſon, conſtantly kept at the top. This may ſeem foreign to my preſent purpoſe; but I thought proper briefly to mention it here, not only upon the account of the importance of the experiment, but likewiſe to ſhew, that I was not miſinformed in the account of the calf's being brought forth dead, and that it had not even reſpired; much leſs taken any nouriſhment after excluſion, to influence the appearances, which we are now going to deſcribe.

Having done with the thorax, I opened the abdomen, in order to take a ſlight view of it before the lumbering carcaſe ſhould be removed. I obſerved the thick inteſtines, eſpecially the rectum, extremely diſtended with an incredible quantity of meconium; which for ſeveral inches above the anus was formed into diſtinct ſcybala or balls. It came into my head to make an inciſion in the rectum, where it was very turgid, about two inches from the anus, and to let out about twenty-five or thirty of theſe ſcybala; which I laid upon clean paper to dry, that I might examine them at my leiſure. About three or four days after, when they were dry and brittle, of the colour and conſiſtence of aloes; I was ſurpriſed to find, upon examination, every ball ſtuck full of tough, thick, white hairs, ſome of which were an inch long, or more. There ſeemed to be ſome ſcores in each, though they, being ſhrunk with drying, did ſcarce exceed the bulk of an ordinary pea. This unexpected appearance ſet me a conſidering, whence theſe hairs had come; how got they there? And I

could think on no other tolerable ſolution of the difficulty than to conclude, that they belonged originally to the calf's ſkin; and, being looſened by maceration in the liquor amnii, were propelled into the ſtomach and inteſtines; till they were at length entangled in the meconium. I was confirmed in the belief of this by being informed, upon inquiry, that the calf's ſkin was white; a circumſtance unknown to me before, it having been ſent flay'd, as hath been already ſaid.

From this perſuaſion it was natural to infer, that if hairs looſened from the ſkin of the fœtus, and floating in the liquor amnii, can find a way into the inteſtines, and get entangled in the meconium, it is impoſſible but the liquor amnii muſt enter and paſs through the whole alimentary paſſage along with them; as a fluid may certainly penetrate where hairs cannot: but no good reaſon can be aſſigned, or even conceived, why hairs ſhould be admitted where the fluid is excluded.

The only reaſonable ſcruple, that remained to be got over, was, that this being but a ſingle inſtance, a general concluſion was not to be too haſtily drawn from it; that it was poſſible there might be ſome morbid concretions in the meconium of this particular calf, reſembling hairs, which concretions in a common and natural way might be wanting; or ſome preternatural communication between the primæ viæ in this ſubject, and the liquor amnii, not to be found in the generality of other fœtus's.

To obviate all this, and farther to eſtabliſh and illuſtrate the truth of my concluſion, as inſtances like that, which we have now related, cannot be commanded,

manded, I laid myſelf out to procure the firſt dung of calves after they are brought forth; which always can be nothing but meconium. This was attended with ſome difficulty and delay, as cows in this neighbourhood moſtly calve in the fields. After ſome months, by the good offices of a ſenſible acquaintance in this place, I obtained what I wanted. He was ſo obliging as to watch his own cow's calving, and before he ſtirred from her, to catch the calf's firſt dung, and ſend it me forthwith. Some weeks after another parcel of the ſame kind was ſent me from another quarter. In both which I found a great number of ſtrong hairs all over; ſo as to leave no room for doubting but that this appearance is general in the meconium of calves, in a natural way.

The reader will pleaſe to obſerve, that in neither of theſe inſtances I could be deceived, if I had ever ſo little reaſon to truſt to the judgment and fidelity of thoſe, who ſupplied me with what I wanted. The colour and conſiſtence of the meconium of a fœtus is ſo very peculiar, and ſo widely different from that of fæces formed out of ingeſted aliments, that none, who have any knowlege in theſe matters, can miſtake the one for the other.

In the mean time I omitted not to open the embryo's of the cow-kind, ſuch as I could procure in the ſhambles of the market-town I live in, and to examine their meconium. The two moſt advanced towards maturity, which I met with, had ſtiff long hairs about the mouth, the eye-brows, the ears, and navel, and a good many on the end of the tail; but none on their ſkins. In neither of theſe, any more than in the younger embryo's, which I examined,

was there ſo much as a ſingle hair to be found in the meconium; for this plain reaſon, if I judge right, to wit, becauſe they had not got hairs upon their bodies of long enough continuance to become looſe, and float in the liquor amnii.

But as opportunities of coming at fœtus's of this ſpecies, eſpecially ſuch as are remarkably nearer to maturity than thoſe two I have juſt now mentioned, are rare with us, I tried to ſupply that defect by opening thoſe of other animals. Accordingly I procured ſix puppies, of the butcher-dog-kind, brought forth at the full time, at one litter. Having taken out the whole meconium of every one of them, after the ſtricteſt ſearch, I could find no hairs in any part of it. I had likewiſe an opportunity of opening a colt, that died either in the birth, at the full time, or immediately after, before its meconium was diſcharged; which I found in great quantities in its rectum and colon. But neither here could I ſpy a ſingle hair, though I examined whole pounds of it, and that portion thereof moſt carefully, which was lodged in the rectum, near the anus.

Theſe obſervations may ſeem at firſt view to claſh with and contradict thoſe I have related: But, upon cloſer conſideration, they will be found in reality to confirm them, for this reaſon, to wit, that puppies and colts, when brought forth, have no looſe hairs on their bodies; but calves have in great numbers. In the puppies and colt, which I examined, the hairs were ſo firmly rooted on their ſkins, that I could ſcarce pull any off with my thumb and fingers; whereas in a ripe calf, new brought forth, many are found quite looſened at their roots, and only adhering

ing to their ſkin by the moiſture on it. Therefore in the latter ſpecies, hairs from the ſurface may be, and actually are, incorporated with the liquor amnii, and along with it enter the mouth and alimentary canal, which cannot be the caſe in the former.

From theſe facts it is eaſy to draw ſuch inferences, as are ſufficient to decide the controverſy, which we are treating of; and prove inconteſtably, that the liquor amnii is in a conſtant natural way received into the mouth, ſtomach, and inteſtines, and therefore muſt contribute to the nutrition of the fœtus.

For if we attentively conſider, that in the meconium of an human fœtus no hairs are found, there being no looſe hairs on its ſkin when it is born; that in the meconium of unripe calves there are none, before they are of ſo long continuance on their bodies as to be looſened, and float in the liquor amnii; whereas in that of the ripe calves they are found in great abundance, of the ſame colour with thoſe of the ſkin, as appears by the example we had occaſion to examine moſt particularly; and of ſuch a length, ſtiffneſs, and thickneſs, and ſo conſtantly to be met with, as cannot with any ſhew of reaſon be imputed either to morbid concretion, or preternatural frame: If, I ſay, all theſe facts are attentively conſidered, and duly weighed, the concluſion I have drawn will, I hope, appear to follow from them ſo naturally, that no candid and intelligent inquirer will call its truth in queſtion.

POSTSCRIPT.

After I had gone thus far upon my own bottom, and while I was looking about me, in order to pick up

up more proofs, that might, as it were *ex ſuperabundanti*, confirm and illuſtrate the doctrine, which I was endeavouring to eſtabliſh, before I ſhould venture to publiſh it, I caſually found what, as I here ſolemnly declare, I knew nothing of before, to wit, that I was not the firſt, who had obſerved hairs in the meconium of ripe calves, two authors having been before-hand with me, to wit, Aldes and Swammerdam; the former in his *Epiſtola contra Harveium*, publiſhed in the firſt volume of the *Bibliotheca Anatomica* of Mangetus and Le Clerc: the latter in the *Biblia Naturæ*. Aldes, a feigned name under which Slade, an Amſterdam phyſician, conceals himſelf, barely mentions the obſervation, without drawing any inference from it, either on the one ſide or the other. Swammerdam draws the ſame concluſion with me; but, I think, he goes farther than the obſervation can well juſtify, maintaining, that it evidently follows from thence, that a calf licks his own ſkin *in utero*, thereby looſening the hairs, and ſwallowing them down along with the liquor amnii, its nouriſhment. See the whole paſſage, *Bibl. Natur.* pag. 319.

However that be, it is enough for my purpoſe to have two ſuch authors concurring with me, as to the reality of the fundamental fact, and quite ſaves me the trouble of making any farther trials to confirm it.

It is no wonder, that theſe paſſages eſcaped me, who have ſpent almoſt all my time in practice, and practical ſtudies, and who never was poſſeſſed of either of theſe books; but, after I found them quoted, was obliged to trouble two learned friends, at a di-

ſtance, upon this occaſion, for my own ſatisfaction, and that very lately. The one was ſo obliging as to ſend me the *Bibliotheca Anatomica*; the other to write out at large the paſſage from the *Biblia Naturæ*. I ſay, it is no wonder the paſſages eſcaped my notice, ſince they ſeem to have been overlooked by not only all the particular writers on the fœtus, ſince Aldes and Swammerdam, but likewiſe, for aught I know, by all the lecturers on phyſiology in Europe, preceding Auguſt 1753; at which time I began to communicate by letters, what I then took to be a diſcovery, to ſeveral correſpondents verſed in theſe matters, both at home and abroad, as I can vouch by their anſwers.

So that I hope the public will do me the juſtice to believe, that though I was not the firſt in point of time, that made the fundamental obſervation, yet I was obliged to no author for it; and that I have drawn an important truth relating to the animal oeconomy out of obſcurity, and eſtabliſhed it by ſupplemental obſervations, as well as by reaſoning.

Before I conclude, let me acquaint the reader, that an ingenious ſtudent of anatomy and ſurgery *, to whom I had communicated my firſt obſervations, and recommended to him to examine the liquor contained in the ſtomachs of ripe new brought forth calves, if he had a proper opportunity, acquainted me laſt harveſt, that he had not long before actually found hairs in abundance in the liquor contained in

* Mr. John Clark, now ſurgeon to the Lock-Hoſpital, near Hyde-Park Corner.

one

one of the ſtomachs of a calf, brought forth dead at the full time, of the ſame colour with thoſe on its ſkin. As this obſervation renders the proof, however clear and unexceptionable it was before, fuller, I was unwilling to omit it here.

Brigg, July 12, 1755. Malcolm Flemyng.

XLIII. *An Account of the Succeſs of* Agaric *in Amputations*, &c. *in a Letter from Mr.* William Thornhill, *late Surgeon to the Infirmary at* Briſtol, *to* Robert Dingley, *Eſq*; *F. R. S.*

Novem. 2, 1755.

Read Nov. 13, 1755. I Have made uſe of the agaric in four amputations: the firſt was on the arm, between the elbow and wriſt; the ſecond and third were below the knee; and the fourth on the crural artery. In every caſe it anſwered beyond my expectation; the patients had little or no fever, and no ſpaſms at all, which are frequently the conſequences of the ligature, and ſometimes fatal. I am ſurpriſed, that any objections are made to the uſe of it. I fear the true reaſon of its not being uſed in general practice proceeds from a narrow ſelfiſh way of thinking; but it is my opinion it will make its way, to the honour of its firſt author. I am well ſatisfied, it is the beſt medicine in all the materia medica yet diſcovered for that purpoſe.

I have