John Till-Adams

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AN

EXPERIMENTAL INQUIRY

INTO THE

PROPERTIES of the BLOOD.

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EXPERIMENTAL INQUIRY

INTO THE

PROPERTIES of the BLOOD.

WITH

REMARKS on ſome of its MORBID APPEARANCES:

AND

AN APPENDIX,

RELATING TO

The Diſcovery of the Lymphatic Syſtem in Birds, Fiſh, and the Animals called Amphibious.

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AND TEACHER OF ANATOMY.

Vere ſcire, eſt per cauſas ſcire. Lord BACON.

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MDCCLXXI.

PREFACE.

THE knowledge of the human frame, the preſervation of health, and the cure of diſeaſes, are objects of too great importance to mankind, for the Author of theſe ſheets to doubt, that any attempts to promote them, how ſmall ſoever, ſhould not meet with a candid and indulgent reception from the public. An Inquiry into the Properties of the Blood, it is preſumed, will be thought, in a particular manner, intereſting, ſince there is no part of the human body upon which more phyſiological reaſoning is founded, nor any from which more inferences are drawn for the cure of diſeaſes. And, as the inquiry is made by experiments upon the blood as near as poſſible to the ſtate in which it circulates in the veſſels, it is hoped that the concluſions made from them will ſtand the teſt of a candid examination, and lead to further obſervations and improvements.

THE three firſt chapters of theſe ſheets have already been publiſhed in the Philoſophical Tranſactions, the fourth and fifth contain ſuch obſervations as have occurred ſince, and the

Appendix

Appendix is a vindication of the Author's right to the diſcovery of the Lymphatic Veſſels, in oppoſition to the claim of the learned Dr. Alexander Monro, Profeſſor of Anatomy in the Univerſity of Edinburgh.

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EXPERIMENTAL INQUIRY
INTO THE
PROPERTIES of the BLOOD.

CHAP. I.

Of the ſeparation of the Serum; *the colour of the* Craſſamentum; *and of the cauſes of the* COAGULATION OF THE BLOOD.

WHEN freſh blood is received into a baſon, and ſuffered to reſt, in a few minutes it jellies, or coagulates, and ſoon after ſeparates into two parts, diſtinguiſhed by the names of *craſſamentum* and *ſerum*. Theſe two parts differ in their proportions in different conſtitutions; in a ſtrong perſon, the *craſſamentum* is in greater proportion to the *ſerum* than in a weak one; and the ſame difference is found to take place in diſeaſes; thence is deduced the general concluſion, that the leſs the quantity of *ſerum* is in proportion to the *craſſamentum*, bleeding, diluting liquors, and a low diet, are the more neceſſary; whilſt in ſome dropſies, and other diſeaſes where the *ſerum* is in a great, and the *craſſamentum* in a ſmall proportion, bleeding and diluting would be highly improper. As it is therefore ſuppoſed uſeful to attend to the proportions of theſe parts in many diſorders, and even to take indications of cure from them, it has been an object with thoſe who have made experiments on the blood, to determine the circumſtances on which its more perfect ſeparation into theſe two parts depends; it it being obvious, that till this be done, our inferences from their proportions will be liable to confiderable fallacies. Two of the lateft writers on this fubject agree, that if the blood, after being taken from a vein, be fet in a cold place, it will not eafily feparate, and that a moderate warmth is neceffary: this is a fact that is evinced by daily experience. They likewife fay, that the heat fhould be lefs than that of the animal, or than 98° of Fahrenheit's thermometer; and that, if frefh blood be received into a cup, and that cup put into water heated to 98°, it will not feparate; nay, they even fay, that it will not coagulate; but this, I am perfuaded from experiments, is ill founded.

EXPERIMENT I.

A TIN-VESSEL, containing water, was placed upon a lamp, which kept the water in a heat that varied between 100 and 105 degrees. In this water was placed a phial, containing blood that inſtant taken from the arm of a perſon in health; the phial was previouſly warmed, then filled, and corked to exclude air. In the ſame water was placed a tea-cup half full of blood, juſt taken from the ſame perſon; a third portion of the blood was then received from the ſame vein into a baſon, and was ſet upon a table, the heat of the atmoſphere being at 67°. Now, according to their opinion, the two former ſhould neither have coagulated nor ſeparated, when that in the baſon began to ſeparate; but, on the contrary,

trary, they were all three found to coagulate nearly in the ſame time; and thoſe in the warm water, not only did ſeparate as well as the other, but even ſooner.

EXPERIMENT II.

The ſame experiment was repeated on the blood of a perſon that laboured under the acute rheumatiſm, whilſt the heat of the atmoſphere was no higher than 55°, and that of the warm water was 108°; and the reſult of this experiment was not only a confirmation of what was obſerved in the firſt, but it even ſhewed, that this degree of heat was ſo far from leſſening, that it increaſed the diſpoſition to coagulate; for the blood in the cup and in the phial was not only congealed, but the ſeparation was much advanced before the whole of the blood in the bafon was coagulated. Thence I am led to conclude, that the feparation of the blood in a given time, is in proportion as the heat in which it ftands is nearer to the animal heat, or 98°; or greater in that heat than in any of a lefs degree. And I am confirmed in this inference by experiments hereafter to be related, where the blood in the living animal, whilft at reft, was found both to coagulate and to feparate.

It is well known, that the *craffamentum* confifts of two parts, of which one gives it folidity, and is by fome called the fibrous part of the blood, or the *gluten*, but by others with more propriety termed the *coagulable lymph*; and of another, which gives the red colour to the blood, and is called the *red globules*. Thefe two parts can be feparated by wafhing the

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craſſamentum in water, the red particles diſſolving in the water, whilſt the coagulable lymph remains ſolid. That it is the coagulable lymph, which, by its becoming ſolid, gives firmneſs to the *craſſamentum*, is proved by agitating freſh blood with a ſtick, ſo as to collect this ſubſtance on the ſtick, in which caſe the reſt of the blood remains fluid *.

The ſurface of the *craſſamentum*, when not covered with a cruſt, is in general of

* It may be proper to mention here, that till of late the coagulable lymph has been confounded with the ſerum of the blood, which contains a ſubſtance that is likewiſe coagulable. But in theſe papers, by the *lymph*, is always meant that part of the blood which jellies, or becomes ſolid ſpontaneouſly when blood is received into a baſon, which the coagulable matter that is diſſolved in the ſerum does not; but agrees more with the white of an egg, in remaining fluid when expoſed to the air, and coagulating when expoſed to heat, or when mixed with ardent ſpirits, or ſome other chemical ſubſtances.

a more

a more florid red than the blood was when first taken from the vein, whilst its bottom is of a dark colour, or blackish. This floridness of the surface is justly attributed by some of the more accurate observers to the air, with which it is in contact; for, if the *crassamentum* be inverted, the colours are changed, at least that which is now become the upper surface assumes a more florid redness. This difference of colour, others have endeavoured to explain from the different proportions of the red particles, or globules as they are called, which, say they, being in a greater proportion at the bottom of the *crassamentum*, makes it appear black; but, if inverted, the globules then settle from the surface which is now uppermost, and that becomes redder. But this I think is not probable; for the lymph in the *crassamentum* is so firmly coagulated, as to make

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it too denſe to allow of bodies even heavier than the red particles to gravitate through it; for example gold. That air has the power of changing the colour of the blood, has been long known; and the following experiment ſhews it very ſatisfactorily, and hardly leaves room to refer the appearance to another cauſe.

EXPERIMENT III.

Having laid bare the jugular vein of a living rabbit, I tied it up in three places; then opening it between two of the ligatures, I let out the blood, and filled this part of the vein with air. After letting it reſt a little, till the air ſhould become warm, I took off the ligature which ſeparated the air from the blood, and then gently mixed them, and I obſerved that the

the venous blood aſſumed a more florid redneſs, where it was in contact with the air-bubbles, whilſt in other parts it remained of its natural colour.

THERE is a difference between the arterial and venous blood in colour; the former is of a florid red like the ſurface of the *craſſamentum*, the latter is dark or blackiſh like the bottom of the *craſſamentum*. This change in its colour is produced on the blood as it paſſes through the lungs, as we ſee by opening of living animals *; and as a ſimilar change is produced

* That this change is really produced in the lungs, I am perſuaded from experiments, in which I have diſtinctly ſeen the blood of a more florid red in the left auricle, than it was in the right. But ſome authors of the greateſt authority ſay, that they could not obſerve any ſuch difference in a great number of experiments which they made; but this I ſhould

ced by air applied to blood out of the body, it is presumed that the air in the lungs is the immediate cause of this change; but how it effects it, is not yet determined.

As the blood is changed to a more florid red in passing through the lungs, or from the venous to the arterial system, so it loses that colour again in passing from the arteries to the veins in the extreme parts, especially when the person is in health; but every now and then we observe the blood in the veins more florid than is usual, and it likewise frequently happens in venesection, that the blood

I should attribute to their having been later in opening the left auricle after the collapsing of the lungs than I was; for it seems probable, that whatever is the alteration produced on the blood in its circulation through this organ, that change cannot take place after it is collapsed.

which comes firft out is blackifh, and that which comes afterwards is more florid: in fuch cafes, the arterial blood paffes into the veins without undergoing that change which is natural to it.

Some of the neutral falts have a fimilar effect on the colour of the blood to what air has, particularly nitre; thence fome have attributed the difference of colour in the arterial and venous blood to nitre, which they fuppofed was abforbed from the air whilft in the lungs. But we know that this is a mere hypothefis, for air contains no nitre. Indeed nitre is far from being the only neutral falt which has this effect on the blood, for moft of them have fome degree of it. In making fome experiments on this fubject, I have obferved a more remarkable effect which neutral falts have upon the blood; and that is,

being

being mixed with it when juſt received from the vein, they prevent its coagulation, or keep it fluid, and yet, upon adding water to the mixture, it then jellies or coagulates. Thus, if ſix ounces of human blood be received from a vein upon half an ounce of Glauber's ſalt reduced to a powder, and the mixture agitated ſo as to make the ſalt be diſſolved, that blood will not coagulate on being expoſed to the air, as it would have done without the ſalt; but if to this mixture about twice its quantity of water be added, in a few minutes the whole will be jellied or coagulated, and on ſhaking the jelly, the *coagulum* will be broken, and the part ſo coagulated can be now ſeparated as it falls to the bottom, and proves to be the lymph.

In these mixtures of the blood with neutral salts, the red particles readily subside (especially if human blood be used) and the surface of the mixture becomes clear and colourless; and being poured off from the red part, it is found to contain the coagulable lymph, which can be separated by the addition of water.

I have tried all the neutral salts, and have made a table of their effects on the blood; but this table I shall not trouble the reader with, it being sufficient to observe that in general they agree in producing this change *. And it is less necessary to be particular in this point, as we do not see of what use this could be in medicine; because we must not conclude that

* Those made with the volatile alkali, and with the earth of allum, are to be excepted.

their

their effects within the body would be the ſame as out of it. Indeed, theſe experiments, as well as ſome others, were not made ſo much with a view of any immediate application to medicine, as to determine the properties of the blood chemically; for, having ſet out with a perſuaſion, that a more particular acquaintance with the properties of this fluid was neceſſary before we could arrive at the knowledge of ſome of the animal functions, ſuch as the manner in which the bile and other ſecreted fluids are formed, I therefore was anxious to throw ſome more light on this ſubject. With this view I have made ſome experiments even on living animals, being convinced that my inquiries would not otherwiſe be ſatisfactory.

When blood is thus kept fluid by neutral ſalts, it ſtill retains its property of being coagulable by heat, and by other ſubſtances as before, air excepted. This method of keeping the blood fluid may therefore be uſeful, by affording an opportunity of making ſome experiments upon it, which we could not otherwiſe do, from its coagulating ſo ſoon when taken from the veſſels.

THIS property of one of the neutral ſalts has been long known amongſt thoſe who prepare the blood of cattle for food; for it has long been a practice with ſuch people, to receive it into a veſſel containing ſome common ſalt, and to agitate it as faſt as it falls, by which means the coagulation is prevented, and the blood remains ſo fluid as to paſs through a ſtrainer, without leaving any *coagulum* behind: by this means they have an opportunity

of

of mixing it with other ſubſtances for culinary purpoſes.

ALTHOUGH the coagulable lymph ſo readily becomes ſolid when expoſed to the air, yet whilſt circulating it is far from that conſiſtence: it has indeed been ſuppoſed to be fibrous, even whilſt moving in the blood-veſſels, but erroneouſly.

IT is this coagulable lymph which forms the inflammatory cruſt, or *buff*, as it is called. It likewiſe forms *polypi* of the heart, and ſometimes fills up the cavities of aneuriſms, and plugs up the extremities of divided arteries. It is ſuppoſed, by its becoming ſolid in the body, to occaſion obſtructions and inflammations; and even mortifications, from the expoſition to cold, have been attributed to its coagulation. In a word, this lymph is ſuppoſed to have ſo great a ſhare in the cauſe of ſeveral diſeaſes, that it would be a deſirable matter to be able to aſcertain the cauſes of that coagulation, either in the body, or out of it.

THE blood, when received into a baſon and ſuffered to reſt in the common heat of the atmoſphere, very ſoon jellies or coagulates; the part which now becomes ſolid is the coagulable lymph, as has been ſhewn above. The circumſtances in which it now differs from what it was in the veins, are theſe: it is expoſed to the air, to cold, and is at reſt; for whilſt in the body air is excluded, it is there of a conſiderable warmth, and is always in motion. The queſtion is, to which of theſe circumſtances its coagulation whilſt in the baſon is chiefly owing. This queſtion, I believe, cannot well be anſwered

anſwered from the experiments that have hitherto been made. It has indeed been ſaid, that the cold alone coagulated it; for, ſay they, if you receive blood into a baſon, and ſet that baſon in warm water, and ſtir the blood well, it can be kept fluid. But in the experiments from which this concluſion was made, I find there has been a deception. In ſhort I have found that it coagulates as ſoon when kept warm and when agitated, as it does when ſuffered to reſt and to cool. As the ſubject ſeemed to me of importance, I have endeavoured to aſcertain the circumſtance to which this coagulation is owing by ſeveral experiments, in each of which the blood was generally expoſed to but one of the ſuſpected cauſes at a time. Thus, in order to ſee whether the blood's coagulation out of the body was owing to its being at reſt, I made the following experiment:

EXPERIMENT IV.

HAVING laid bare the jugular vein of a living dog, I made a ligature upon it in two places, ſo that the blood was at reſt between the ligatures; then covering the vein with the ſkin, to prevent its cooling, I left it in this ſituation. From ſeveral experiments made in this way, I found in general, that after being at reſt for ten minutes, the blood continued fluid; nay, that after being at reſt for three hours and a quarter, above two-thirds of it were ſtill fluid, though it coagulated afterwards. Now the blood, when taken from a vein of the ſame animal, was completely jellied in about ſeven minutes. The coagulation therefore of the blood in the bafon, and of that which is merely at reft, are fo different, that reft alone cannot be fuppofed to be the caufe of the coagulation out of the body.

To fee the effects of cold on the blood, I made this experiment:

EXPERIMENT V.

I KILLED a rabbit, and immediately cut out one of its jugular veins, proper ligatures being previoufly made upon it; I then threw the vein into a folution of fal ammoniac and fnow, in which the mercury ftood at the 14th degree of Fahrenheit's thermometer. As foon as the blood was frozen I took the vein out again, and put it into lukewarm water till it thawed and

and became ſoft; I then opened the vein, received the blood into a tea-cup, and obſerved that it was perfectly fluid, and in a few minutes it jellied or coagulated as blood uſually does. Now, as in this experiment the blood was frozen and thawed again without being coagulated, it is evident that the coagulation of the blood out of the body is not ſolely owing to cold, any more than it is to reſt.

NEXT, to ſee the effects of air upon the blood, I tried as follows:

EXPERIMENT VI.

HAVING laid bare the jugular vein of a living rabbit, I tied it up in three places, and then opened it between two of the ligatures and emptied that part of its blood. I next

I next blew warm air into the empty vein, and put another ligature upon it, and letting it reſt till I thought the air had acquired the ſame degree of heat as the blood, I then removed the intermediate ligature, and mixed the air with the blood. The air immediately made the blood florid, where it was in contact with it, as could be ſeen through the coats of the vein. In a quarter of an hour I opened the vein, and found the blood entirely coagulated; and as the blood could not in this time have been completely congealed by reſt alone, the air was probably the cauſe of its coagulation.

From comparing theſe experiments, may we not venture to conclude, that the air is a ſtrong coagulant of the blood, and that to this its coagulation when

taken

taken from the veins is chiefly owing, and not to cold, nor to reſt?

It may not be improper to obſerve here, that there are none of theſe experiments I have been obliged to repeat ſo often as the 4th, which was made with a view to determine whether the blood would coagulate by reſt. In the firſt trial, the vein was not opened till the end of three hours and a quarter; and juſt before it was opened I had obſerved through its coats, that the upper part of the blood was tranſparent, owing to the ſeparation of the lymph. On letting out this blood, it ſeemed to me entirely fluid: a part indeed had been loſt, but the greateſt part was collected in the cup, and which afterwards coagulated as blood commonly does when expoſed to the air. From this experiment

ment I imagined that the whole had been fluid; but from others made ſince, I am perſuaded that the part which was loſt had been coagulated; for, from a variety of trials, I now find, that though the whole of the blood is not congealed in this time by reſt alone, yet a part of it is. But as it would be treſpaſſing too much on the reader's time to relate every experiment I have been obliged to make for this purpoſe, I ſhall only mention the general reſult of the whole.

After fixing a dog down to a table and tying up his jugular veins, I have in general found, that on opening them at the end of ten minutes, the blood was ſtill entirely fluid, or without any appearance of coagulation *. If they were opened at the end

* I ſay *in general* it was fluid at the end of ten minutes; but I muſt likewiſe mention that in one dog

end of fifteen minutes, at firſt ſight it alſo appeared quite fluid; but on a careful examination I have found ſometimes one, and ſometimes two or three ſmall particles about the ſize of a pin's head, which are coagulated parts of the blood. When opened later than this period, a larger and larger *coagulum* was obſerved; but ſo very ſlowly does this coagulation proceed, that in an experiment where I had the curioſity to compare more exactly the clotted part with the unclotted, I found, after the vein had been tied two hours and a quarter, that the *coagulum* weighed only two grains; whilſt the reſt of the blood, which was fluid, on being ſuffered to con-

I found two very ſmall particles of beginning coagulation, even at this period; yet in another I could not obſerve any ſuch appearance, even at the end of fifteen minutes.

geal,

geal, weighed eleven grains. I can advance nothing farther in this part of my ſubject with preciſion. Nor can I pretend exactly to determine the time at which all the blood between the ligatures is coagulated. I have indeed opened ſuch a vein at the end of three days, when I found a thin, white *coagulum*, which was a mere film; the *ſerum* and red particles having diſappeared. But the whole is undoubtedly congealed long before this period. The manner in which the blood coagulates, when at reſt in the body, has appeared to me curious, and therefore I have taken the more pains to diſcover how it happens, eſpecially as it may aſſiſt us in judging whether or no it coagulates in the heart, ſo as to form thoſe ſubſtances called *polypi*. The abovementioned times will, I believe, be found to be thoſe at which the blood congeals in the veins of healthy dogs: and as I have found, by experiments, that the blood of a dog and of the human ſubject in health jellies out of the body, nearly in the ſame time, that is, it begins in three or four minutes, and is completed in ſeven or eight; I ſhould therefore conclude that the blood in the veins of the human body coagulates nearly at the ſame period with that of a dog. But it may be neceſſary to add here, that from experiments which I have made, I have reaſon to believe that the time at which the blood coagulates, is different in different conſtitutions, and in different diſeaſes. For though the blood of a perſon in health is completely coagulated in ſeven minutes after it is taken out of the veins, yet in ſome diſeaſes, I have found the blood fifteen or twenty minutes, nay even an hour and an half, before it was completely jellied.

As

As we ſee in the above-related experiments, that the blood coagulates in the body when ſuffered to reſt for a little time, is it not probable that to this cauſe its coagulation in thoſe true aneuryſms, which are attended with a pouch, are owing *? For in ſuch enlargements a part of the blood is without motion, which will congeal when at reſt, and in contact with the ſack; and thus one layer may be formed; and the ſack afterwards enlarging, another portion of the blood will then be at reſt; and ſo a ſecond layer may be formed; and thence probably is the origin of thoſe laminated *thrombi* met with in ſuch ſacks.

Likewiſe, to the blood's being at reſt, is probably owing its coagulation in the

* An inſtance of which may be ſeen in the Med. Obſ. and Inq. vol. i. article xxvii. fig. iii.

large arteries which are tied after amputation, or other operations; for after moſt of ſuch ligatures there will be a part of the artery impervious, in which the blood can have no motion. The *coagulum* after amputation might indeed be ſuppoſed owing to air; but, conſidering the manner in which arteries are tied whilſt the blood is running from them, it does not ſeem probable that the air has any effect on what is above the ligature.

To the blood's being without motion in the cavity of the *uterus*, is its coagulation therein probably owing; hence the origin of thoſe large clots which ſometimes come from that cavity; and which, when more condenſed by the oozing out of the *ſerum*, and of the red globules, aſſume a fleſh-like appearance, and have often been called *moles* or *falſe conceptions*.

In Experiment the 5th, we found that the blood could be frozen and thawed again, without being coagulated: this likewise is an experiment which I have repeated several times, that I might be sure of the fact. I have also varied the experiment by sometimes putting the vein into a phial of water, and freezing the whole in a solution of sal ammoniac in snow; and sometimes I have put the vein into the solution itself; and three or four times I have thrown it into oil, and then frozen it; but after all these trials, the result was the same. The blood was always evidently fluid on being thawed, and as evidently jellied when exposed to the air.

Besides being coagulated when exposed to the air, the coagulable lymph, as well as the *serum*, is known to be fixed by heat; but the degree of heat has not, so far as I know, been determined. It has been ſuppoſed to require a degree almoſt equal to that which coagulates the *ſerum* *; but one much leſs is neceſſary, as will appear from the following experiments.

EXPERIMENT VII.

HAVING found, from a number of trials, that blood, kept fluid by means of the neutral ſalts, had its lymph coagulated by a heat of 125° of Fahrenheit's thermometer, I ſuppoſed that the degree neceſſary for fixing it in its natural ſtate could not be very different from this. I therefore prepared a lamp-furnace with a ſmall veſſel of water upon it; this water was heated to 125°; and then laying bare

* See Traité du Cœur. T. ii. p. 93. Schwenke Hæmatolog. p. 138.

the

the jugular vein of a living dog, I tied it properly, cut a piece of it out, and put it into this water: after eleven minutes, I took out the vein, opened it, and found the blood entirely coagulated; thence I concluded, that 125°, or leſs, was ſufficient to coagulate the blood of a dog. It may be neceſſary to obſerve here, that the part coagulated was only the lymph; for the *ſerum* requires a much greater heat to fix it, that is a heat of 160°, as will appear hereafter.

EXPERIMENT VIII.

The ſame experiment was repeated in ſuch a manner, that the heat never went higher than 120° and an half; and I found, that on opening the vein at the end of eleven minutes, the lymph

was

was entirely coagulated, even in this heat.

EXPERIMENT IX.

I REPEATED the experiment with a heat no higher than 114°, and at the end of eleven minutes, the vein being opened, the blood was found to be fluid, and in a few minutes after, being laid open to the air, it coagulated as ufual. Now, as the blood in the laft experiment but one was coagulated, when the heat had never rifen above 120° and an half; and in this experiment was fluid, though it had been expofed to a heat of 114°; we may therefore conclude, that the coagulable lymph in the blood of a dog, in health, is fixed in a degree of heat between 114° and 120° $\frac{1}{2}$ of Fahrenheit's thermometer.

As to the degree of heat at which the lymph in human blood coagulates, I have not yet had an opportunity of trying it in a more ſatisfactory way, than with the mixture with Glauber's ſalt, in which ſtate it coagulates at 125°. But, as we find that the human blood and that of a dog jelly near in the ſame time, when expoſed to the air, I think it probable that the preciſe degree of heat at which the lymph of the human blood coagulates, is between 114° and 120° $\frac{1}{2}$. I have thought of making the experiment on the umbilical cord of a recent *placenta*, which perhaps is the moſt likely way of coming at the truth.

The degree of heat, at which the *ſerum* of the blood (which ſhould not be counfounded with the lymph) coagulates, is generally ſaid to be 150°; but from

my

my experiments I am inclined to believe it requires a greater heat to fix it. They were made in the following manner:

EXPERIMENT X.

I took a wide-mouthed phial, containing ſome *ſerum*, and placing a thermometer in it, I put it into water which was kept warm by a lamp underneath; and, in making this experiment with as much accuracy as I could, I found the heat required was 160°; which is about forty degrees more than is neceſſary for the coagulation of the lymph.

As the blood is coagulable by heat, and as the heat of an animal is increaſed in fevers, it has been ſuppoſed that the blood might be coagulated by the animal heat, even whilſt it is circulating in the

veſſels;

vessels; but there is little foundation for such an opinion, since the animal heat is naturally only 98° or 100°, and in the most ardent fever is not raised above 112°.

CHAP. II.

Of the inflammatory Cruſt, or Size.

I SHALL next proceed to inquire into the formation of the inflammatory cruſt, or *ſize*, as it is called.

THIS remarkable appearance is frequently met with in inflammatory diſorders, and is formed by the coagulable lymph's being fixed, or coagulated, after the red particles have ſubſided. It has indeed been ſuppoſed to be formed from the *ſerum* of the blood; and an excellent writer on this ſubject ſeems in doubt to which of the two it ſhould be attributed. But that

that it is formed by the coagulable lymph alone, after the red particles have ſubſided, appears from the following experiments.

EXPERIMENT XI.

In the month of June, when the thermometer in the ſhade ſtood at 67°, I bled a man who had laboured under a *phthiſis pulmonalis* for ſome months, and at that time complained of a pain in his ſide. The blood, though it came out in a ſmall ſtream, yet flowed with ſuch velocity, that it ſoon filled the baſon. After tying up his arm I attended to the blood, and obſerved that the ſurface became tranſparent, and that the tranſparency gradually went deeper and deeper, the blood being ſtill fluid. I likewiſe obſerved that the coagulation firſt began on the ſurface, where it was in contact with the air, and formed a thin pellicle; this I removed, and ſaw that it was ſoon ſucceeded by a ſecond. I then took up a part of the clear liquor with a wet tea-ſpoon, and put it into a phial with an equal quantity of water; a ſecond portion I kept in the tea-ſpoon; and I found afterwards that they both jellied or coagulated, as did alſo the ſurface of the *craſſamentum*, making a thick cruſt. On preſſing with my finger that portion which was in the tea-ſpoon, I found it contained a little *ſerum*.

From this experiment it is evident, that the ſubſtance which formed the ſize was fluid after it was taken from the vein, and coagulated when expoſed to the air; and as this is a property of the coagulable lymph alone, and not of the *ſerum*, there can

can be no doubt that the cruſt was form-ed of the former, and not of the latter.

THE following experiment, made on the blood, without expoſing it to the air, likewiſe proves the ſame fact.

EXPERIMENT XII.

IMMEDIATELY after killing a dog, I tied up his jugular veins near the *ſternum*, and hung his head over the edge of the table, ſo that the parts of the veins where the ligatures were might be higher than his head. I looked at the veins from time to time, and obſerved that they became tranſparent at their upper part, the red particles ſubſiding. I then made a ligature upon one vein, ſo as to divide the tranſparent from the red portion of the blood;

and opening the vein, I let out the tranſparent portion, which was ſtill fluid, but coagulated ſoon after. On preſſing this *coagulum*, I found it contain a little *ſerum*. The other vein I did not open till after the blood was congealed, and then I found the upper part of the *coagulum* whitiſh like the cruſt in pleuritic blood *.

It has been a very generally received opinion, that inflammation thickens the blood, and makes it more ready to coa-

* This is not the only apparently healthful animal whoſe blood had a cruſt; I have ſeen it in others: whence I at firſt ſuſpected that merely keeping the blood fluid for a little time was ſufficient to produce this appearance; but I altered my opinion, on ſeeing, that in the greateſt number of animals it did not occur: nor is it commonly met with in the hearts of thoſe perſons who die a violent death, though the blood remains longer fluid in ſuch caſes, than it does in the baſon where a ſize appears.

gulate

gulate. Nay, ſome have gone ſo far as to ſay, that in thoſe diſorders where the inflammatory cruſt is ſeen, the blood is almoſt coagulated even before it is let out of the vein. Now I am perſuaded from experiment, that the contrary of this is true; or that inflammation, inſtead of increaſing the diſpoſition of the blood to coagulate, really leſſens it; and inſtead of thickening the blood, really thins it; at leaſt that part which forms the cruſt, viz. the coagulable lymph.

In the firſt place, that inflammation really leſſens the diſpoſition to coagulate, will appear evident to every one who attends to the jellying of ſuch blood as has a cruſt. For in all thoſe caſes the blood will be found to be longer in congealing, than in its natural ſtate. To this opinion I was

I was firſt led by attending to the phthiſical patient's blood abovementioned; but I have ſince made a compariſon, which ſeems to prove the fact. For, from a variety of experiments made on the blood of perſons nearly in health, or at leaſt who had no inflammatory diſorder, and no cruſt on their blood, I found, that after being taken from a vein, it began to jelly in about three minutes and an half. The firſt appearance of coagulation is a thin film on the ſurface near the air-bubbles, or near the edge of the baſon; this film ſpreads over the ſurface, and thickens gradually till the whole is jellied, which is in about ſeven minutes after the opening of the vein; and in about ten or eleven the whole is ſo firm, that, if the cake be cut, the gaſhes are immediately filled up by the *ſerum*, which now begins to ſeparate from

from the *craſſamentum*. But in thoſe perſons whoſe blood has an inflammatory cruſt, the coagulation is much later; and in general, I believe, is lateſt in thoſe caſes where the cruſt is thickeſt, and *vice verſa*. The following experiments ſeem to confirm this opinion.

EXPERIMENT XIII.

I BLED a woman who was ſeven months gone with child, and the blood was received into a baſon. In five minutes after the vein was opened, a film firſt appeared; but this ſpread ſo ſlowly, that in ten minutes it did not cover the whole ſurface; in fifteen minutes it had nearly ſpread over the ſurface; but the reſt of the blood was quite fluid, at leaſt for ſome depth, and even in half an hour it

was

was not ſo firmly jellied as it was afterwards. In this caſe there was a very thick and ſtrong cruſt or ſize.

EXPERIMENT XIV.

Having bled a perſon with a violent rheumatic pain in his breaſt, the blood was received into three tea-cups, and each of them had afterwards a cruſt. In the firſt I obſerved the progreſs of the coagulation, as follows. The beginning of the coagulation was not marked, but at the end of half an hour the film was not thicker than common writing-paper; and this being removed, a little of the clear lymph was taken up with a wet tea-ſpoon, put into a clean cup, and was twenty minutes more in coagulating. Even at the

end

end of an hour and an half, the whole of the blood was not jellied; for at this time I removed the film or pellicle, and took up a ſecond portion of clear lymph with a ſpoon, and put it into a tea-cup, where it jellied afterwards; though this jelly was not indeed quite ſo firm as the *craſſamentum* itſelf.

EXPERIMENT XV.

A WOMAN, with a ſlight inflammation in her throat, had eight ounces of blood taken from her arm; the blood was received into a baſon, and the bleeding finiſhed in four minutes and three quarters, when a film began to form near the air-bubbles; in ſeven minutes a tranſparent ſize appeared over a conſiderable part of the ſurface which was quite fluid, whilſt the

the reſt of the blood was coagulating, there being now a very diſtinct red cruſt over the reſt of the ſurface.

Now, from comparing theſe experiments with what has been obſerved of the coagulation of the blood, where there is no inflammatory cruſt or ſize, is it not evident that the blood remains longer fluid after being expoſed to the air, and has leſs diſpoſition to coagulate, in thoſe caſes where there is a ſize, than where there is none? for where there was none, it was found to coagulate completely in ſeven minutes; but in one of the others, where the ſize was very thick, it did not completely coagulate in leſs than an hour and an half.

THE power that inflammation has in leſſening the diſpoſition of the lymph to coagu-

coagulate is likewiſe plain from the following experiment, where the blood in the heart of a dead animal ſeems to have congealed very ſlowly.

EXPERIMENT XVI.

A DOG was killed eight hours after receiving a large wound in his neck. The wound had during this time inflamed conſiderably. Upon opening him next morning, when he had been dead thirteen hours, a large whitiſh *polypus* was found in the right ventricle of his heart; under this was a little blood ſtill fluid, which being taken up with a tea-ſpoon, coagulated ſoon after being expoſed to the air.

IT may be proper to obſerve here, that in the hearts of animals which had died without any inflammation, I have found the blood entirely coagulated long before this time. And that from opening them at different times, I have ſeen it coagulate in their hearts after death, in the ſame gradual manner that it does in their veins, when its motion is ſtopt by ligatures; as related in page 25.

In the next place, that the blood is really attenuated in inflammatory diſorders, where the whitiſh cruſt or ſize appears, is probable from the following circumſtances: firſt, it even ſeems thinner to the eye; 2dly, the red particles or globules ſubſide ſooner in ſuch blood, than in that of an animal in health. This ſeems proved by obſerving that in the above-mentioned experiments, where the blood was at reſt in the veins, it was not cover-

ed

ed with a cruſt, except in one or two inſtances, though in all thoſe caſes it remained longer fluid than the blood commonly does in a baſon, after bleeding, where the cruſt appears. And again, the blood in the heart of an animal that dies a violent death, is not generally covered with a white cruſt, notwithſtanding it is ſo late in being congealed. Theſe circumſtances ſhew, that ſomething more than merely a leſſened diſpoſition to coagulate is neceſſary for the forming of the cruſt or ſize. 3dly, The globules more readily ſubſide in inflammatory caſes, from the ſurface of the whole maſs of blood, than they will afterwards do from the ſurface of a mixture with the *ſerum* alone, of which the following experiments are a proof. But, before I relate them, let me obſerve, that they were made with a view to difcover, whether the inflammatory cruft could be owing to any other caufe than to the attenuation of the coagulable lymph, and to its difpofition to coagulation being leffened: and as the fame appearance might be fufpected to arife from an increafed fpecific gravity in the red particles, or from the *ferum* alone being attenuated, I endeavoured to decide the queftion in the following manner.

EXPERIMENT XVII.

INTO a phial, marked A, I put an ounce of the *ferum* of the blood of a perfon, whofe *craffamentum* had an inflammatory cruft.

INTO another, marked B, I poured an ounce of the *ferum* of a perfon whofe blood had.

had no cruſt; then to each of theſe, I added a tea-ſpoonful of *ſerum*, loaded with the red particles of a perſon whoſe blood had no inflammatory cruſt or *ſize*. In attending to them, I could not obſerve that the red particles ſubſided at all ſooner in the *ſerum* of the blood that had a cruſt, than they did in the *ſerum* of that blood which had no cruſt. Thence I conclude, that the *ſerum* is not attenuated in thoſe caſes where the inflammatory cruſt appears.

Lastly, to ſee whether the ſpecific gravity of the red globules was increaſed, I proceeded as follows.

EXPERIMENT XVIII.

I POURED into a phial C a portion of the *ſerum* of the blood which had no cruſt; and likewiſe into another D a ſecond portion of the ſame *ſerum*. I then added to C a tea-ſpoonful of the ſame *ſerum*, loaded with red particles from the blood which had an inflammatory cruſt. And into D I poured a tea-ſpoonful of the ſame *ſerum*, loaded with the globules of that blood which had no cruſt. In viewing theſe, I could not obſerve that the globules of the blood which had an inflammatory cruſt ſubſided ſooner than thoſe of the blood which had none: whence I inferred, that the ſpecific gravity of the red particles, or globules as they are called, is not increa-

ſed

ſed in thoſe caſes where the cruſt appears. And, therefore, ſince that inflammatory cruſt or ſize ſeems neither owing to the *ſerum*'s being attenuated, nor to an increaſed ſpecific gravity in the red particles, it probably depends ſolely upon a change in the coagulable lymph. And what ſeems farther to confirm this inference, in none of theſe experiments did the red particles ſubſide from the ſurface of the *ſerum* in 20 minutes, though, where the cruſt appears, they ſubſide from the ſurface of the blood in half that time; ſo that the whole maſs of blood ſeems to be thinner than the *ſerum* alone; or, the coagulable lymph ſeems to be ſo much attenuated in theſe caſes, as even to dilute the *ſerum*, which at firſt ſight appears a paradox.

May we not, therefore, conclude, that in thoſe caſes where the inflammatory

cruſt

cruſt appears, the coagulable lymph is thinner, and its diſpoſition to coagulation is leſſened; both of which circumſtances contribute to the ſubſiding of the red globules from the ſurface of the blood, which then coagulating gives riſe to this appearance, called the inflammatory cruſt or ſize, in pleuritic or rheumatic blood *.

How contrary to the concluſion, which theſe experiments lead us to, are the opinions of ſome medical writers on this ſubject! How frequently do we find it ſaid, that the blood is thicker in inflammatory diſorders, where that ſize occurs; and

* This remarkable appearance might indeed be accounted for, by ſuppoſing that the lymph had aſcended to the ſurface of the blood in thoſe caſes; but this is improbable, from conſidering, that, in its coagulated ſtate, it is of greater ſpecific gravity than the ſerum, and ſinks in it.

that

that a large orifice is neceſſary to let out the vitiated blood! That a large orifice is preferable to a ſmall one in many caſes, where ſuch blood is found, I believe to be true; but that its advantages are owing to its letting out the thickened blood, ſeems improbable from what we have ſeen in the experiments above related: they are perhaps nearer the truth, who attribute it to the ſuddenneſs of the evacuation.

IT may be proper to obſerve here, that this ſize or whitiſh cruſt is not a certain ſign of inflammation; it being often met with where there ſeems to be no ſuch diſeaſe, in particular in the blood of pregnant women. And that it differs much in denſity in different caſes; in ſome it is extremely firm, in others it is ſpungy or cellular,

cellular, and contains much *ſerum* in its cells. Theſe diverſities we ſhall endeavour to explain hereafter, when we have laid before the reader ſome more obſervations on the coagulation of the lymph.

CHAP.

CHAP. III.

Of the cauſes of the inflammatory cruſt's appearing at different times in blood-letting; of the ſtopping of hæmorrhages; and of the effects of cold upon the blood.

IT has been obſerved by thoſe who have written on the blood, that it ſometimes happens in blood-letting, that the firſt cup has an inflammatory cruſt, whilſt the laſt has none; but no ſatisfactory reaſon has been given for this difference. One might ſuppoſe that it was owing to ſome circumſtance in the bleeding, ſuch as in the different velocity with which the

the blood flowed into each cup, or to the laſt cup's being agitated ſo as to prevent the ſeparation of the lymph: but I have ſeen it where there was no difference of this ſort, nor in any other circumſtance that I could obſerve. I therefore ſuſpect that in ſuch caſes the properties of the blood are changed, even during the time of the evacuation; and in this opinion I am confirmed by the following experiments.

EXPERIMENT XIX.

NINE ounces of blood were taken from a woman who had been delivered two days before, and who at that time laboured under a fever, with a conſiderable pain in her ſide, and in her *abdomen*. The blood was received into a baſon, and her arm was tied up; when, on looking at the blood, I found its ſurface tranſparent for ſome depth, an indication of a future cruſt; and as her pain was not abated, and as her pulſe could bear it well, I removed the ligature from her arm, and took away about ſix ounces more, into three tea-cups; but what appeared to me remarkable, although the blood flowed as faſt into each of the cups as into the baſon, and when full they were immediately ſet down on the ſame window, yet there was no inflammatory cruſt on the blood in the cups, though a very denſe one on that in the baſon. And again, although the blood in the baſon had been taken away ſome minutes before that in the cups, yet it was later in being completely coagulated; as was evident on comparing them.

I HAD an opportunity of repeating the experiment in the evening; for the ſymptoms of inflammation ſeeming equally violent, it was thought proper by the phyſicians who attended her, to take away more blood; which was done by opening the ſame orifice, when three tea-cups were nearly filled, and ſet in the ſame place; and it was obſerved, that the firſt had a cruſt, though not ſo thick a one as in the firſt bleeding; but the other two cups were without this appearance, though the blood had flowed into them even more quickly than into the firſt.

EXPERIMENT XX.

A GENTLEMAN, who laboured under an inflammatory complaint, had about nine ounces of blood taken from his arm. This

quantity

quantity was divided into four portions; the firſt was received into a cup, and was in meaſure little more than an ounce; the ſecond, into a baſon, to the quantity of two ounces; the third into a cup, which held one ounce; and the fourth into a baſon, to the quantity of three ounces. Each veſſel was immediately placed upon the window; and it was obſerved that the blood in the firſt was lateſt in coagulating, and had a cruſt over the whole ſurface; that in the ſecond had a cruſt only upon a part of its ſurface; but that in the third and fourth had none, and manifeſtly coagulated before either of the other two.

Now, ſince in theſe experiments the blood in the firſt cups was later in coagulating than that in the laſt, and ſince the blood in the firſt cups alone had a ſize, is it not probable, that even during the ſhort time taken up in the evacuation, the properties of the lymph had been changed, and that it was owing to this change that the ſize diſappeared? It might indeed, at firſt ſight, ſeem poſſible, that the bleeding had only let out the vitiated part; but this is not at all likely; for, ſuppoſe a part only of the blood was vitiated, that part muſt have been equally diffuſed through the whole maſs, and there is no probability of its getting out of the veſſels before the reſt of the blood; and conſequently it ought to have appeared in the laſt equally as in the firſt cup, but it did not. Bleeding, therefore, in thoſe caſes alters the nature of the blood, not by removing the vitiated part, and giving room for new blood to be formed, as has been ſuppoſed; but probably by changing that ſtate of the blood-veſſels on which the thinneſs, and leſſened tendency of the lymph to coagulation, depends; which ſurely is a very curious circumſtance, and muſt diſprove the doctrine of thoſe who maintain that this vitiated blood is the cauſe of the diſeaſe, ſince the diſeaſe remains, though the properties of the blood are changed *.

From this obſervation we may be led to think, that it may be uſeful to receive the blood more frequently into ſmall cups, inſtead of a baſon, and to attend more carefully to the alteration produced upon

* That the properties of the blood can be changed by emptying the blood-veſſels, is likewiſe proved by an experiment hereafter to be related; where the blood in an animal in health was found to have its diſpoſition to coagulation increaſed, in proportion as the veſſels were emptied, and as the animal became weaker. It may likewiſe be proper to mention, that though the inference is here drawn from two experiments only, yet I have likewiſe obſerved the ſame appearance in other caſes, which I have thought unneceſſary to relate.

it by bleeding; as we may by that means perhaps learn to determine better, what quantities ſhould be taken away in particular caſes. For it would ſeem probable that the operation is likely to have the moſt effect on the diſeaſe, in thoſe caſes where the greateſt change is produced by its means, on the diſpoſition of the blood to coagulate; and of that change, we can judge, by comparing the blood in the firſt cup, with that in the laſt; for the firſt cup will nearly ſhew the ſtate of the blood at the beginning; and the laſt cup the ſtate of the blood at the latter part of the evacuation.

It frequently happens, that inſtead of an inflammatory cruſt over the whole ſurface of the *craſſamentum*, there is only a partial one, which appears in large ſpots or ſtreaks. In ſuch caſes I have obſerved, that only a part of the blood had its diſpoſition to coagulate leſſened, as in experiment XV. in which ſome of the blood remained fluid and tranſparent, where thoſe ſtreaks appeared, for ſome time after the coagulation had begun in other parts of the ſurface. Now whether in thoſe caſes there had been the ſame difference before the vein was opened, or whether the whole blood had not been of the inflammatory kind, before veneſection, and a part of it was changed as it ran out, or as ſoon as the general fulneſs was diminiſhed, may be a queſtion; but the probability, I think, is much in favour of its being changed during the time of the evacuation, from what was obſerved in the laſt experiments.

WHEN

When I had obſerved that this diſpoſition of the lymph to coagulate was increaſed by bleeding, or by weakening the action of the blood-veſſels, I ſuſpected that poſſibly in thoſe caſes where the body was very weak, the diſpoſition to coagulate might be ſo much increaſed, that inſtead of being three or four minutes in beginning to do it, after it is let out of the veins (as is the caſe in people in health) it might coagulate in leſs time, or almoſt inſtantaneouſly; for I imagined, that unleſs this took place, we could hardly conceive how the blood ſhould ever have time to coagulate in ruptured veſſels, ſo as to ſtop hæmorrhages, as it is believed to do. And upon this occaſion I recollected a remark of Dr. Hunter's, which is, "that the "faintneſs which comes on after hæmorrhages, inſtead of alarming the bye-ſtanders, and making them ſupport the pa-

"tient

" tient by ſtimulating medicines, as ſpirits " of hartſhorn and cordials, ſhould be " looked upon as ſalutary; as it ſeems " to be the method nature takes to give " the blood time to coagulate." Now as this ſeemed to favour my ſuſpicion, I determined to make the experiment.

EXPERIMENT XXI.

BELIEVING it would be ſufficient for this purpoſe, to attend to the properties of the blood, as it flows at different times from an animal that is bleeding to death, I therefore went to the markets, and attended the killing of ſheep; and having received the blood into cups, I found my notion verified. For I obſerved, that the blood which came from the veſſels immediately on withdrawing the knife, was

about

about two minutes in beginning to coagulate; and that the blood taken later, or as the animal became weaker, coagulated in lefs and lefs time; till at laft, when the animal became very weak, the blood, though quite fluid as it came from the veffels, yet had hardly been received into the cup before it congealed. I have alfo repeated the experiment, by receiving blood into different cups at different times, whilft the animal was bleeding to death; and though the time taken up in killing the animal was not commonly more than two minutes, yet I obferved, on comparing the cups, that the blood which iffued laft coagulated firft. I have obferved likewife, that the blood coagulates with a different appearance in proportion as the animal becomes weaker; that which follows the knife begins to coagulate

late in about two minutes; it firſt forms a film or pellicle on the ſurface, which extends gradually through the whole blood, yet ſo ſlowly that its progreſs may be obſerved, eſpecially if the pellicle be moved from time to time. But the blood that comes from the fainting animal is coagulated in an inſtant, after it once begins. From this circumſtance, that the diſpoſition of the blood to coagulate is increaſed as the animal becomes weaker, we may draw an inference of ſome uſe with regard to the ſtopping of hæmorrhages, viz. not to rouſe the patient by ſtimulating medicines, nor by motion, but to let that languor or faintneſs continue, ſince it is ſo favourable for that purpoſe; and alſo, that the medicines likely to be of ſervice in thoſe caſes, are ſuch as cool the body, leſſen the force of the circulation,

tion, and increaſe that languor or faintneſs *. For, in proportion as theſe effects are produced, the divided arteries become more capable of contracting, and the blood more readily coagulates; two circumſtances that ſeem to concur in cloſing the bleeding orifices †.

It

* Beſides giving ſtimulants and cordials to counteract the fainting, it is a common practice in many parts of England, to give women, who are flooding, conſiderable quantities of port-wine, on a ſuppoſition that it will do them ſervice by its aſtringency. But ſurely, from its increaſing the force of the circulation, it muſt be prejudicial in thoſe caſes. Perhaps many of the remedies called ſtyptics might be objected to for the ſame reaſon.

† It has of late been proved by experiments, particularly by thoſe of the ingenious Mr. Kirkland, that the larger arteries, when divided, contract ſo as to ſtop the hæmorrhage. But the large *coagula* which we ſee in the orifices of the veſſels of the *uterus* of thoſe who die ſoon after delivery, and the ſtopping of hæmorrhages where the blood-veſſels were

IT has been queſtioned whether blood-letting can be properly recommended in hæmorrhages, excepting in thoſe that are attended with evident ſigns of *plethora*: but do not theſe experiments ſhew, that a vein may be opened with propriety, even where there is no *plethora*, in order ſuddenly to bring on weakneſs; by which the momentum of the blood may be ſo diminiſhed, and the diſpoſition of the lymph to coagulate may be ſo increaſed, as to ſtop the hæmorrhage? For, when we conſider how ſoon the blood veſſels contract, and adapt themſelves to the quantity of blood which they contain, it ſeems

were ruptured on their ſides and not entirely divided, make me believe that contracting the bleeding orifice is not the only method nature takes to ſtop an hæmorrhage. Her reſources indeed are great, and ſhe has often more methods than one of producing the ſame effect.

not improbable that in ſome caſes where the hæmorrhage is not profuſe, but long-continued, the ſtrength of the patient may be ſo recruited, that the diſpoſition to coagulate ſhall not be ſufficiently increaſed, or the extremities of the veſſels ſufficiently contracted, for the ſtopping of the bleeding; but, by emptying the veſſels ſuddenly, this effect may be obtained, and the hæmorrhage may be ſtopt by the loſs of leſs blood, than would have happened, had only the ſlow draining been continued.

Although the whitiſh cruſt ſo commonly ſeen in inflammatory diſorders, has ſo very morbid an aſpect, as might induce us to conſider it as inflammatory, and to bleed repeatedly in all thoſe caſes where it occurs, yet I believe we ſhould act im-

properly:

properly: for, to ſay nothing of pregnancy, in which the appearance is almoſt conſtant, there are few phyſicians that have not ſeen patients, who, even in ſuch circumſtances, were the worſe for this evacuation. Nor need we be ſurpriſed that this ſhould happen, conſidering how ſoon in ſome inſtances this ſize diſappears; and if ſo, may we not ſuppoſe, that it may likewiſe ſoon be formed, even by a ſhort exertion of ſtrength in the veſſels? Perhaps this was the caſe in the gentleman mentioned in page 62, who, in leſs than twenty-four hours after bleeding, had ſymptoms of great weakneſs.

As it appears from theſe experiments, that the diſpoſition of the blood to coagulate is increaſed by bleeding, it may be uſeful to attend more to this circumſtance, and to compare the coagulation of the blood in the laſt, with that in the firſt cup, even in caſes that are not attended with the inflammatory cruſt. And it may likewiſe be worth while to make the ſame compariſon in thoſe caſes where every cup has a cruſt; which frequently happens both in rheumatic and in phthiſical complaints. By theſe means we may judge what effect the evacuation has produced on the ſtrength or fulneſs of the veſſels; and may perhaps, by inſpecting the laſt cup, eſpecially if it contains only a ſmall quantity, be able to gueſs pretty nearly at the nature of the blood which remains in the body. In the rheumatic caſe mentioned in page 46, every cup contained this cruſt; and although the blood in the laſt cup coagulated in much leſs time than that in the firſt, yet as it was later in coa-

gulating

gulating than common, I ſuſpected what remained in the veſſels had the ſame diſpoſition; but the patient got well without repeating the evacuation.

It may be mentioned here, that I have once or twice ſeen blood, which, when it firſt began to coagulate, had on its ſurface a red pellicle, and underneath a tranſparent fluid, which afterwards formed a cruſt. In theſe caſes, if the red pellicle had not been removed before the reſt of the blood had congealed, we might have concluded that no part of the blood had this diſpoſition to form a white cruſt. This appearance, I ſhould imagine, was owing to the blood, where in contact with the air, having coagulated before the red particles had time to ſubſide, from that

part of the lymph which had its dispofi-
tion to coagulation leffened.

The learned profeffor de Haen has taken notice of a curious appearance of the blood, which he could not account for; but which, I prefume, may be explained from fome of the above experiments. His obfervation is, "that, having bled a per-
"fon in a fever, the blood was covered
"with an inflammatory cruft, and upon
"examining the *craffamentum* in one of
"the cups, he found that it formed a fort
"of fack containing a clear fluid: this fluid
"being let out, and the cup fet by, on
"examining it next morning, he obferv-
"ed a very firm cruft covering the whole
"again, and extending to the bottom of
"the cup *." I once met with a cafe

* Vide Rat. Medendi, cap. vi.

fimilar to this; for, having bled a perfon into four cups at ten o'clock in the morning, on looking at the blood afterwards, at five in the afternoon, I found the *ferum* had not feparated from the *craſſamentum* in the firft cup; but the *craſſamentum* felt as if it contained a fluid in a bag, as profeffor de Haen has defcribed it. Upon preffing it, the fluid gufhed out, and in a few minutes after being expofed to the air, coagulated: there was however this difference in the two cafes, that in mine the fluid was red, fo that it formed a red cruft over the firft, which was white. Now this feems to have been owing to the blood's having firft coagulated, where it was in contact with the air and with the fides of the cup; and the fluid which gufhed out was the *ferum*, with a part of the coagulable lymph, which ftill remain-

ed

ed fluid; but, when expofed to the air, it jellied or coagulated, as it naturally does. That one part of the lymph can remain fluid after the other is coagulated, is proved by fome of the preceding experiments; and I have more than once feen blood, which appeared perfectly jellied foon after bleeding; yet, on cutting into the *coagulum*, a tranfparent fluid has oozed out, which afterwards jellied. And fo flowly does this coagulation proceed in fome cafes, that, in an experiment mentioned before, a part of the blood in a dog's heart was found uncoagulated thirteen hours after death. And I have likewife diftinctly obferved, that in fome cafes where the difpofition to coagulate was much leffened during the evacuation, the blood at the bottom of the cup has jellied, whilft the greateft part of the *fize* at

the

the top was yet fluid; there being only a thin pellicle on its ſurface, where it was in contact with the air.

Another inſtance of a change in the properties of this coagulable lymph, which appears curious, was ſeen in ſome experiments, where I had occaſion to throw the blood into water, and into oil, during the winter ſeaſon, whilſt the heat of the water and of the oil was no greater than 41° of Fahrenheit's ſcale. In all thoſe experiments, I found that the diſpoſition to coagulate was leſſened, the blood becoming more and more viſcid, but did not coagulate whilſt in that degree of cold. I ſhall next relate thoſe experiments.

EXPERIMENT XXII.

THE jugular vein being properly tied, and then cut out from a rabbit juſt killed, was thrown into water of 41° of heat, and taken out at the end of half an hour; when the blood was found to be ſtill fluid, though rather more viſcid than natural; but, after being expoſed to the air for a few minutes, it coagulated.

EXPERIMENT XXIII.

Two pieces of the jugular vein of a dog, juſt killed, were put into water, in which the thermometer ſtood at 41°; one was taken out after twenty minutes, and the

the other after three quarters of an hour; the blood in both was found to be fluid, and to coagulate afterwards.

As it was evident from theſe experiments, that the water had leſſened the diſpoſition of the blood to coagulate, I next enquired to what property in the water this effect could be owing; and to ſee whether water that was warmer would not have the ſame effect, I made the following experiment.

EXPERIMENT XXIV.

On December the thirteenth, I cut out two pieces of the jugular vein of another dog, immediately after his death. One piece was put into cold water, and the

other

other into water kept warm by a lamp, ſo that the heat never varied more than between 90 and 100°. At the end of three quarters of an hour, that in the warm water had in it a *coagulum* as large as a garden-pea; but that in the cold water, being let out into a cup, was quite fluid. Twenty minutes after being expoſed to the air, that which had been in the cold water was coagulating; but that from the warm water neither then nor afterwards ſhewed any ſigns of farther coagulation: ſo that it ſeemed not only to have jellied whilſt in warm water, but to have begun to part with its *ſerum*. From this experiment, it ſeems probable that the *coldneſs* was that property of the water to which the leſſened diſpoſition to coagulate was owing; but, to be more ſure of this, and to ſee whether the blood might not be kept

kept fluid a longer time by theſe means, I tried as follows:

EXPERIMENT XXV.

On January the fourteenth, I cut out a piece of the jugular vein of another dog, and put it into oil, in which the thermometer ſtood at 38°. At the end of ſix hours it was taken out, and the red particles were obſerved through the coats of the vein to have moſtly ſettled to one ſide. The blood was let out into a cup, and was found to be fluid; at the end of fifteen minutes above one half was ſtill fluid; in twenty-five minutes it ſeemed to be quite jellied. Now as in this experiment a ſimilar effect was produced, as when the vein was put into water, it ſeems probable that it was the coldneſs of the water, and of the

the oil, which had leſſened the diſpoſition of the lymph to coagulate.

EXPERIMENT XXVI. *

Another piece of the ſame vein was put into river-water, in which the thermometer ſtood at 38°, and was left till the next morning; when, after twenty-two hours and a quarter, it was taken out. The red particles did not ſeem to have ſubſided, as in the former experiment; but the vein being opened the blood was found to be fluid, though ſo viſcid that it could barely drop from the veſſel. The cup into which it was received was placed upon the window of a

* It is neceſſary to obſerve here, that great expedition ſhould be uſed in making theſe experiments; for, unleſs the vein be cut out in a few minutes after the death of the animal, the experiment may not ſucceed, from the blood's having begun to coagulate.

mode-

moderately warm room, and was examined carefully from time to time; but the blood never had any appearance of coagulation, on the contrary, it remained fluid till it was dried by the evaporation of the water, which happened by the next day. In this experiment the cold ſeemed entirely to have prevented the coagulation of the lymph: ſo ill-founded is the common opinion, that cold coagulates the blood.

As the lymph, on being cooled, is deprived of its power of coagulating when expoſed to the air, may we not thence be led to explain that fact mentioned by Liſter, that the blood of thoſe cold animals which ſleep during the winter-ſeaſon, on being let out into a baſon, does not coagulate?

CHAP. IV.

Some further observations on the coagulable lymph, and on the sudden changes produced upon it.

IF the reader has been persuaded of the common opinion, that the disposition of the blood to coagulate is increased in inflammatory disorders, it may perhaps appear to him, as it formerly did to me, a very extraordinary circumstance that the contrary should be true; and likewise that the blood should in reality be the more disposed to concrete, in proportion as the body is weakened, or as the action of the blood-vessels

veſſels is diminiſhed. And as we are naturally tenacious of old opinions, and unwilling to adopt new ones till fully proved, he may ſuſpect that there has been ſome fallacy in theſe experiments. And indeed I muſt acknowledge, that there is, in appearance, one ſtrong argument againſt my general concluſion, which is, that it has not only been remarked, that the firſt cup has a cruſt, whilſt the laſt has none; but likewiſe, that the ſecond, or the third cup, alone ſhall have a cruſt, whilſt the preceding ones are without it. Now this, I ſay, ſeems contradictory to what I have advanced, concerning the diſpoſition of the blood to coagulate being increaſed in proportion as the body is weakened; for here in proportion as the blood is evacuated, its diſpoſition to coagulate is leſſened; ſince it was more ſizy in the ſecond, or third cup, than in the firſt. But, in anſwer to this objection, I muſt remark, that theſe caſes very ſeldom occur; and that in general the firſt cups are more ſizy, and are the lateſt in jellying; and when the contrary takes place, or when the ſecond or third cup is more ſizy than the preceding, I am perſuaded, that upon a careful examination, inſtead of weakening, they will be found to ſtrengthen my inference; as will appear probable by the following caſe, which has occurred ſince theſe experiments were publiſhed in the Philoſophical Tranſactions.

EXPE-

EXPERIMENT XXVII.

On the 13th of June, I viſited a young man, twenty-two years old, of an athletic habit, who complained of a violent pain in his head and back, with a full ſtrong pulſe; but as he was then in a profuſe ſweat, which had been preceded by a ſhivering, it was not thought proper to bleed him, and the rather, as we were informed, that he had had a ſimilar paroxyſm two days before. But next day, finding that his fever had not left him with the ſweat, and that he ſtill had a pain in his head and back, and that his pulſe, though not now full and ſtrong, yet was quicker than natural, it was then judged neceſſary to take away ſome blood. Upon opening the vein, the blood flowed very ſlowly, and indeed merely trickled down his arm. Imagining that the bandage might be too tight, I ſlackened it, but ſtill the motion of the blood was not accelerated. I then aſked him whether he had not been afraid of the bleeding, and he told me he had; and on feeling his pulſe, in the other arm, I found it very low. I therefore deſired him to move the muſcles of his hand, which he did; but nevertheleſs ſo ſlowly did the blood run, that it was four minutes before I got an ounce and an half into a cup. I then ſtopt the orifice till another cup was brought, into which the blood ran in a full ſtream, to the quantity of three ounces, and that in two minutes, although the orifice was rather ſmall, ſo much was its velocity now increaſed. Into the third cup, which likewiſe held three

ounces,

ounces, the blood ran ſtill faſter, as it was filled in leſs than two minutes. By this time the patient beginning to be faint, I ſtopt the bleeding till he could lie down on the floor, and then about three drachms more of blood were received into a fourth cup: this came away very ſlowly, and the bleeding ſtopt of itſelf. He drank a glaſs of water, and did not faint, and he appeared afterwards to be much relieved by the evacuation. Upon this blood I made the following remarks.

THAT which was taken away laſt was firſt coagulated, and completely too, by the time I had tied up his arm, which was in three minutes from the blood's firſt running into the cup.

THE

The blood which was received into the firſt cup coagulated next, and as I obſerved by my watch, in twelve minutes from its being ſet down on the table.

That which was received into the ſecond cup was the third in order as to coagulation, and was conſiderably later in jellying than the firſt; for in fifteen minutes it was not thoroughly coagulated; nay, even in twenty-two minutes a ſmall part of it was ſtill fluid. It was remarkable, that none of theſe three had any ſize.

But the blood in the third cup differed conſiderably from that in the others; for in five minutes it began to appear tranſparent on its ſurface, an indication of a future ſize, and it was later in coagulat-

ing

ing than that in the other cups; for even at the end of twenty-ſix minutes a great part of the coagulable lymph was ſtill fluid, as appeared on removing the pellicle that covered it; but in thirty-five minutes it was completely jellied. The ſize in this blood was very thick and tough.

Now this caſe, when carefully examined, inſtead of being an objection to my concluſions, will, I preſume, be thought a ſtrong confirmation of them.

For, in the firſt place, as the blood in the third cup alone had a cruſt, and was much later in jellying than the reſt, it ſtrengthens my inference, that the diſpoſition of the blood to coagulate is leſſened in thoſe caſes where the inflammatory cruſt or ſize appears. And as the blood

ran

ran more rapidly into this cup, it ſhewed that the heart and blood-veſſels had begun to act with greater force, and therefore confirmed the opinion, that in proportion as theſe act more ſtrongly, the diſpoſition of the lymph to coagulate is diminiſhed. The ſame opinion is likewiſe ſupported by obſerving what happened to the blood in the firſt cup, which coagulated ſooner than that in the third, owing to the veſſels then acting more weakly, as was evident from the blood's trickling down the arm, and from the lowneſs of the pulſe *.

2*dly*, It

* In like manner may be explained another variety in the appearance of the ſize, namely where it is found in the firſt and laſt cups, but not in the ſecond or third: this I ſuſpect ſeldom happens, but when it does, it may perhaps be found, on examination, that the veſſels were acting more weakly whilſt the ſecond or third cups were filled. For

2*dly*, It may be obſerved, that the great difficulty in admitting the concluſion made in the former part of theſe ſheets (viz. that the want of ſize in the laſt cup is occaſioned by an alteration in the blood-veſſels) was to conceive how theſe veſſels could poſſibly alter the properties of the lymph ſo ſuddenly, as in the time between receiving the blood into the

For, ſo eaſily does this ſize appear to be removed, or aſſumed, that, I ſuſpect it may ſometimes happen, that when the blood is taken away, in a full ſtream, from a large orifice, the patient may be ſo ſuddenly weakened, and the properties of the blood may in conſequence be ſo changed by the time the ſecond cup is filled, that the ſize ſhall be removed: and yet afterwards the veſſels may recover their former tone, ſo that the third or fourth cup may acquire a ſize again. Nay, I ſuſpect that this appearance may even be affected by the paſſions, particularly from obſerving that the patient abovementioned, as well as others whoſe blood at firſt trickled ſlowly down their arms, had been much afraid of the lancet.

first cup, and into the last. But this case confirms that inference, by shewing the fact in a clearer point of view; for even here, where the appearance of the size was reversed, it was found that the blood which had a crust or size was latest in coagulating, and that it was this blood which was taken out of the vessels when they acted most strongly, as was proved by the rapidity with which it flowed into the cup.

3dly, SINCE the times in which the blood jellied in these cups were so very different (the first coagulating in twelve minutes, the second in about twenty-two, the third in thirty-five, and the fourth in less than three minutes, notwithstanding these cups were filled in less than two minutes after one another), it shews, I say, how

how ſoon that ſtate of the blood-veſſels on which the ſize depends, can be removed and aſſumed, and therefore leads us to conclude, that although this ſize is in general a ſign of an inflammatory diſorder, or a ſtrong action of the veſſels, yet there may be ſeveral circumſtances to be taken into the account, before we can judge from the preſence, or want of it, whether or no veneſection ſhould be repeated: and it likewiſe ſhews clearly, that it would be improper to determine from the preſence of this alone, when bleeding is neceſſary; and yet there have been not a few who have inclined to make ſuch a concluſion, from their conſidering this cruſt or ſize as ſo very morbid an appearance.

4thly, As the blood in the third cup was ſo late as thirty-five minutes in coagulating, and was ſizy, whilſt that in the fourth was not ſo, and jellied in leſs than three minutes, although it had been taken from the veſſels only two minutes after the other, but at the time the patient had become faint; it ſhews how much faintneſs and languor increaſe the viſcidity of the blood, and likewiſe its diſpoſition to coagulate, ſince in two minutes they produced ſuch a change as to remove the ſize, and to reduce the time of coagulation from thirty-five to three minutes. It therefore ſhews clearly how much languor and faintneſs ſhould be encouraged in hæmorrhages, and how carefully we ſhould avoid giving any thing that can ſtimulate, or rouze the patient; that the medicines likely to be of ſervice are nitre and the acids; or ſuch as cool the body, or have the

property

property of diminiſhing the force of the circulation, or of increaſing that languor or faintneſs *; that all anxiety and agitation of mind ſhould, as much as poſſible, be prevented, leſt they increaſe the circulation: that all muſcular motion ſhould be avoid-

* It has been objected here, that nitre would ſeem improper for this purpoſe, becauſe in experiments mentioned before (p. 13.), it was found to prevent the coagulation of the blood, out of the body; but this objection is removed, by conſidering, that, in order to prevent coagulation, the nitre muſt be uſed in the proportion of two ſcruples to every two ounces of blood. But, when we exhibit it internally, we ſeldom give more than a ſcruple every two hours, which can have no effect in attenuating the whole maſs of blood, nor in preventing coagulation; eſpecially as we have reaſon to believe its properties are changed, before it paſſes the digeſtive organs. Its good effects in hæmorrhages, therefore, are probably owing to its action upon the ſtomach. For proofs of its utility, ſee *Medical Obſervations* and *Inquiries*, vol. IV. art. xvi.

ed for the ſame reaſon: for that an exertion of the patient's ſtrength can leſſen the diſpoſition of the blood to coagulate, I am perſuaded from ſome of the abovementioned caſes, and likewiſe from what I have obſerved in dying ſheep, where the ſtruggles of the expiring animal ſeemed in ſome inſtances, when violent, to alter the properties of the lymph.

We have endeavoured to explain the appearance of the inflammatory cruſt or ſize, from the red globules having ſubſided from the ſurface of ſuch blood before it coagulated: this we obſerved was partly owing to the lymph's being later in coagulating in thoſe caſes, but principally to its being thinned. But we may now add, that although the attenuation of the lymph, and its leſſened tendency to coagulate, are connected in moſt of thoſe cafes, yet they do not always go together; for the lymph may have its difpofition to coagulate leffened without being thinned; which was evident in the preceding cafe, on comparing the blood in the fecond with that in the third cup; for the blood in the fecond cup had no fize, notwithftanding it remained fluid at leaft ten minutes after the fize had begun to appear in the third: this I attribute to the blood in the third being more attenuated, and thereby more readily allowing the globules to fubfide.

That the blood may have its difpofition to coagulate leffened, without being attenuated, is likewife probable from the following cafes.

EXPERIMENT XXVIII.

In the month of January I bled a man, who complained of a pain in his head, attended

attended with giddineſs and ſhivering, a pain and ſickneſs at his ſtomach, and with a full and quick pulſe: the blood was found to remain fluid for ten minutes, and then jellied, but no ſize appeared.

EXPERIMENT XXIX.

In another perſon, who was bled merely for a drowſineſs, and becauſe he was accuſtomed to that evacuation in the Spring, I found the blood remain ſeven minutes without coagulating, and yet it was without any ſize.

Now, ſince in theſe caſes the blood remained ſo long fluid, and yet the red particles did not ſubſide, or no ſize appeared, I ſhould conclude, that only the diſpoſition of the lymph to coagulate was leſſened, without its being thinned. And from

from the laſt caſe we may likewiſe conclude, that although the times, at which the blood taken from perſons in health begins to coagulate, be allowed to be about three minutes and an half, as I have found from repeated obſervations, yet there may be ſome variety in this reſpect; for a *plethora* and other circumſtances may make it later in coagulating in ſome caſes, even where the patient is otherwiſe in perfect health.

We have obſerved before, that the ſize is ſometimes very firm, and at other times ſpongy and cellular; theſe differences in its denſity are, I ſuſpect, in proportion to the degree of attenuation and leſſened diſpoſition of the blood to coagulate; for as the coagulation begins on the ſurface, and forms there a film which attracts the reſt of the lymph, the more that lymph is attenuated

attenuated, and the ſlower it coagulates, the more will the film be able to ſeparate it from the red globules, and from the ſerum: thence perhaps it is, that when the blood, beſides being very thin, likewiſe jellies ſlowly, we ſometimes ſee almoſt the whole coagulable lymph collected at the top, forming a firm cruſt, which being free from the ſerum, as well as from the globules, contracts the ſurface into a hollow form. But when the blood has its diſpoſition to coagulate leſs diminiſhed in proportion to the attenuation, then, although the globules ſubſide from the ſurface, yet the whole of the lymph jellies ſo ſoon after the coagulation begins, that there is not time for its being ſeparated from the ſerum, of which it therefore contains a conſiderable quantity, and is of courſe more ſpongy and cellular.

NOTWITHSTANDING bleeding does in general weaken the action of the veſſels, increaſe the diſpoſition of the blood to coagulate, and even thicken the lymph; yet it may happen, that, in the ordinary quantity in which blood is taken away, none of theſe effects ſhall be produced; of this the following caſe ſeems to be an inſtance.

EXPERIMENT XXX.

A WOMAN in the ſeventh month of her pregnancy was bled for a violent pain in her ſide, attended with a cough; the quantity taken away was eight ounces, which was received into four cups; and as the orifice was ſmall, about ten minutes were ſpent in the bleeding. On attending to the

the different cups, I could obſerve no difference in the periods at which the coagulation commenced, and finiſhed in each, allowance being made for the time the blood began to run into each. In every one of theſe cups the blood was completely jellied in about twenty minutes, and each had a cruſt or ſize nearly of the ſame thickneſs. So that the bleeding ſeemed not to have produced any change in the ſtrength of the patient's veſſels, nor was her pain ſenſibly abated by it. She was therefore deſired to live low, to confine herſelf to a vegetable diet, and to take a ſcruple of nitre every three hours in a draught of the *decoctum pectorale*; and if her pain and cough were not abated in a day or two, ſhe was directed to repeat the bleeding. As cloſe attendance was not required, I did not viſit her till four days after,

after, and then ſhe had got free of her complaints, notwithſtanding her blood had been apparently ſo little changed in the time of the evacuation.

In this caſe the bleeding ſeemed neither to have thickened the lymph, nor increaſed its diſpoſition to coagulate, nor weakened the action of the veſſels; but that it generally produces theſe effects, can not, I think, be doubted, from our having obſerved it in ſo many inſtances. Perhaps the dread of the operation might here have made the coagulation of the blood in the firſt cup approach nearer to that in the laſt; or perhaps the ſmallneſs of the orifice prevented there being ſo manifeſt a change produced by the evacuation, from its giving time to the blood-veſſels to adapt themſelves more equally to the quantity they contained, by which means ſhe was not weakened by the loſs of blood.

It has been obſerved by Sydenham, and others, that it ſometimes happens, even in inflammatory diſorders, when the blood trickles down the arm, inſtead of running in a full ſtream, it does not acquire a cruſt or ſize. May not this be explained from what is obſerved in the caſe related in Experiment XXVII? that is, in ſuch inſtances the veſſels, either from a febrile, or from ſome other oppreſſion, act more weakly than they do in the ordinary caſes of inflammation, by which means the lymph is not ſufficiently attenuated to allow the red globules to ſubſide before the coagulation begins, and therefore the ſize does not appear, as in other caſes of inflammation where there is no ſuch oppreſſion.

As many of theſe experiments ſhew how eaſily the diſpoſition of the lymph to coagulate can be altered, even by ſlight changes, as it would ſeem, in the ſtate of the blood-veſſels, they help us to explain how it ſhould happen, that the blood, in ſome diſeaſes, is found without this property of jellying; an inſtance of which is mentioned by Monſ. de Senac *; another was obſerved by the learned profeſſor Cullen; and a third I ſaw lately by the favour of Dr. Huck and the phyſicians of the Britiſh lying-in hoſpital, who, having bled a woman in a fever that came on ſoon after delivery, found her blood did not coagulate on being expoſed to the air, but appeared like a mixture of the red globules and *ſerum* only, the globules ha-

* Traité du Cœur, T. 2. p. 129.

ving subsided to the bottom in the form of a powder. She died three days after, and, upon opening her, we found the blood had coagulated in her vessels after death, and that a tough white *polypus* was formed in each auricle of the heart, one of which I have now bye me. I examined the blood taken away before she died, and found, on exposing it properly to heat, that it did not coagulate sooner than *serum* commonly does, nor under 160°; so that it is probable, that, at the time she was bled, her blood either was without the coagulable lymph, or its properties were changed.

After a blow or contusion, the blood now and then bursts from the vessels into the cellular membrane, sometimes forming an *ecchymosis*, and sometimes a tumor,

mor, and it is a queſtion with ſome, whether ſuch blood coagulates or not; but that it coagulates in moſt of theſe caſes, is proved by opening ſuch tumors. Yet it has likewiſe been obſerved, that now and then theſe tumors have been attended with a fluctuation, and that, after ſome time, their contents have been abſorbed, and it has alſo been found, that, upon opening ſome of them, even ſeveral weeks after the accident, the blood was fluid. In ſuch caſes the blood had probably undergone a change ſimilar to what was obſerved to take place in ſome of the preceding experiments; that is, the lymph had been deprived of its property of coagulating, in paſſing from the blood-veſſels into the tumor: a circumſtance, which, how remarkable ſoever it may appear, agrees with what we have above obſerved of the lymph, whoſe properties ſeemed to vary with the ſtate of the blood-veſſels.

CHAP.

CHAP. V.

Of the white Serum *of the blood.*

THE *ſerum* of human blood is naturally tranſparent, and a little yellowiſh, but it is frequently found to have the appearance of whey, and ſometimes to have white ſtreaks ſwimming on its ſurface like a cream, and now and then to be as white as milk, whilſt the *coagulum* is as red as uſual. In all theſe three caſes of whiteneſs, I have examined it in a microſcope with a pretty large magnifier, and have found it to contain a number of very ſmall globules, although naturally, when tranſpa-

tranſparent, no globules can be obſerved in it, notwithſtanding what has been affirmed by ſome authors. Theſe globules differ from the red particles (improperly called globules) in their ſize, which is much ſmaller; and likewiſe in their ſhape, which is ſpherical, whilſt the red particles are flat. They agree more with the globules of milk. I have compared them with thoſe of woman's milk, and have found, that in the milk the globules are of different ſizes, ſome being three or four times as large as others, and the ſmalleſt little more than juſt viſible, when viewed with a lens of $\frac{1}{23}$ of an inch focus, whilſt thoſe of the white *ſerum* are more regular, and are all of them about the ſize of the ſmalleſt globules of milk. Of this white *ſerum* I have met with the following inſtances in books. In Tulpius, one in-

ſtance,

ſtance *, in Morgagni two †, in the Philoſophical Tranſactions ſome inſtances ‡, in Sckenckius's Obſervations two caſes are related from other authors ||. I have likewiſe heard of the ſame appearance having been obſerved by the learned Sir John Pringle, Dr. Pitcairn, Dr. Hunter, Dr. Watſon, Dr. Bromfield, and Dr. Garthſhore. And other inſtances have lately occurred to perſons of my acquaintance, who have favoured me with a ſhort account of them.

Mr. French, apothecary in St. Alban-Street, having informed me, that he had ſome blood bye him, taken from a

* Tulp. Ob. l. 1. cap. 58.

† Morgagni, Ep. xlix, Art. 22.

‡ Philoſoph. Tranſact. N° 100, and 442.

|| Sckenckij Obſ. lib. 3.

woman

woman the day before, whofe ferum was as white as milk; he favoured me with a fmall quantity of it for examination, and with it the following particulars of the cafe. "Mary Rider, about twenty-"five years of age, of a frefh complexion, "and lufty, has not had her menfes for "thefe feven months. She difcharges "blood fometimes by vomiting, and fome-"times by ftool; complains of a pain "in her left fide, and in her ftomach: "fhe has an inclination to eat, but when "fhe tries, fhe foon after loathes her food. "She complains of great laffitude and "fleepinefs; her pulfe is ninety-five in a "minute. She has been bled twelve times "within thefe fix months, and every time "the *ferum* was as white as milk."

Mr. Robertson, apothecary in Earl-Street, acquainted me, that " Mr. Her-" bert, a publican, of about thirty-five " years of age, and corpulent, had been " ſubject to a bleeding at the noſe, to the " piles, and to ſuch profuſe ſweats in the " night, as to be frequently obliged to " change his ſhirt in the morning before " he got out of bed, but that, for ſome " time paſt, his ſweats had ceaſed. That, " on September the 23d, he was ſeized " with a bleeding at his noſe, which had " been preceded by a pain in his head for " two or three days. That his bleeding " continued till he had loſt about two " pounds of blood, and then ſtopt; and that " the ſerum of his blood was as white as " milk. That at ten o'clock the ſame " night, the hæmorrhage returned, and " he loſt a conſiderable quantity; never-

" theleſs,

" thelefs, it was thought proper to take " fixteen ounces of blood from his arm, " during which evacuation he fainted, " but his bleeding at the nofe ftopt. That " the *ferum* of this laft blood was likewife " very white. That on the 25th, in the " morning, he again complained of a " pain in his head, and about ten o'clock " his nofe began to bleed again; but the " *ferum* now appeared no whiter than " whey. That he continued to lofe blood " during moft part of the night, fo that " it was fuppofed he could not lofe lefs " than two or three pounds, the ferum " all this time being a little whitifh, but " fo little, that the bottom of the veffel " in which it ftood could now be feen " through it. That his bleeding return- " ed repeatedly till the third of October, " when it entirely ftopt, the *ferum* having

" become more tranſparent towards the " laſt."

Mr. Eustace, apothecary in Jermyn-Street, ſent me a phial of white *ſerum* from one of his patients, by trade a butcher. " This man," he told me, " was tall, of " a ſtrong make, a hard drinker, ſubject " to puke every morning, took little food, " ſweated a good deal, but did not waſte " in his fleſh. He was bled for a ſlight " *aſthma* to which he was ſubject, and of " which he had always been relieved by " bleeding. In other reſpects he was in a " good ſtate of health, ſo as to follow his " buſineſs without much inconvenience."

Besides theſe caſes, my friend Mr. Lambert, ſurgeon at Newcaſtle upon Tyne, told me, " that he had a patient

" ſome years ago with a violent rheuma-
" tic pain in his hip, whom he was obli-
" ged to bleed thrice, and every time his
" *ſerum* was as white as milk, but the *coa-*
" *gulum* of its natural colour. This gentle-
" man," Mr. Lambert adds, " was a free
" liver, of a full make, but rather muſ-
" cular than corpulent, and remarkable
" for being a great walker."

When I firſt ſaw this unuſual colour of the *ſerum*, I was inclined to adopt the opinion of thoſe who have attempted to explain it by the patient's being bled ſoon after a meal, or before the chyle was converted into blood. But afterwards, on conſidering the caſes above related, I found this could by no means be the cauſe, as none of theſe patients had taken a ſufficient quantity of food to occaſion this appear-

appearance; on the contrary, moſt of them had a bad appetite, and had taken remarkably little food, and were ſubject to vomitings. I therefore concluded it was owing to ſomething elſe, and what confirmed me in this opinion, was an obſervation I had repeatedly made in diſſecting geeſe, whoſe *ſerum* I had frequently ſeen white, whilſt their chyle was tranſparent; although they had been killed only three or four hours after eating. And as the whiteneſs, in all the caſes that I examined, was owing to a quantity of ſmall globules like thoſe of milk (which are known to be oily) I concluded that theſe in the human *ſerum*, when white, were ſo too, and recollecting to have read ſomewhere of an experiment by which butter had been got from ſuch human *ſerum*, I tried, by agitating ſome of it a little diluted, to ſeparate its oil, or to churn it, but without ſucceſs. I then inſpiſſated ſome of it to dryneſs, and compared it with the natural *ſerum* of human blood prepared in the ſame way, and found it leſs tenacious, and much more inflammable; and when thus dried, its oil oozed out ſo much as to make the paper in which it was kept greaſy. Another portion of this white *ſerum* being kept ſome days, putrefied, and when putrid, it jellied as milk does when become ſour; but it differed from milk, in being extremely fœtid.

Now, as the white globules appear from theſe experiments to be of an oily nature, and as it is improbable, from theſe patients having taken little food, and from the tranſparency of the chyle in birds, that this whiteneſs of the *ſerum* ſhould

ſhould be owing to unaſſimulated chyle, accumulated in the blood-veſſels; we muſt therefore believe it to be owing to ſome other cauſe. And as we know there is a conſiderable quantity of oil laid up in the cellular ſubſtance of animals, which is occaſionally re-abſorbed, is it not moſt probable that this curious appearance was, in the abovementioned caſes, owing to ſuch a re-abſorption? And as all theſe patients had ſymptoms of a *plethora*, and were relieved either by ſpontaneous hæmorrhages, or by blood-letting, is it not probable, that, to whatever purpoſe the oil is applied in the body after it is re-abſorbed from the cellular membrane, in theſe patients it had been re-abſorbed faſter than it was applied, and by that means was accumulated in their blood-veſſels. This conjecture ſeems to be confirmed, from conſidering that in moſt of theſe caſes the people were inclined to corpulency, and that two of them laboured under a ſtoppage of a natural evacuation *.

ANOTHER concluſion which theſe obſervations lead us to, is this, that ſince the chyle of the birds which I diſſected was not white, but tranſparent, at whatever time after eating it was examined, it follows, that the fat (in theſe animals at leaſt) is not merely the oily part of the chyle or

* Although it appears probable that the whiteneſs of the *ſerum* in the abovementioned caſes was not owing to the chyle, yet I would not conclude that the chyle does not in the human ſubject occaſionally colour the *ſerum*. We frequently obſerve the *ſerum* of ſuch people as are bled a few hours after a meal, a little turbid, like whey, which I believe may be owing to the chyle. But if the milk-like ſerum was occaſioned by a full meal, it is likely we ſhould oftener ſee it than we do.

of

of the food; but is a new ſubſtance, or a new combination of the principles or elements, which is made probably in the ſecretory organs of the adipoſe membrane: the form of oil being made uſe of by Nature in preference to any other for the nutritious ſubſtance of the body, from its being the leaſt liable to putrefaction, and from its containing the greateſt quantity of nouriſhment in the leaſt bulk. This circumſtance was clearly proved by my valuable and ingenious friend the late Dr. Stark, who, in a courſe of curious experiments, made by weighing himſelf after living for ſome time on different ſorts of food, diſcovered that a leſs quantity of ſuet was ſufficient to make up for the waſte of his body, than of any other ſort of ordinary food; and that, when compared with

with the lean part of meat, its nutritive power was, at leaſt, as three to one.

I MAY here add another circumſtance that occurred to me when I firſt thought on this ſubject, which is, that ſince we believe the oil, or animal fat, is re-abſorbed from the adipoſe membrane to ſerve for nouriſhment to the body; and as ſome of the patients (whoſe caſes have been related above) could not take food, the re-abſorp-tion therefore of this oil might not be ſo much the cauſe, as the effects of the diſ-order under which they then laboured: or, in other words, that upon ſome defect in the digeſtive organs, the powers of nature drew from their magazines of oil in the adi-poſe membrane, a ſupply of that fluid then perhaps neceſſary for the uſe of the body. In order to clear up this point, I thought

it

it would be a ſatisfactory experiment, to compare the *ſerum* of the blood of animals at different periods after feeding them. For, if the re-abſorption of the oil was merely to make up for the want of other food, or, if the *ſerum* was white merely from a greater quantity of oil being taken up in order to ſupply the wants of the body, then the *ſerum* ought to be whiteſt in the animals kept longeſt without food, or whoſe body was moſt in want. And as I had found that geeſe had very commonly this white *ſerum*, though their chyle was tranſparent, I choſe to make the experiment on them. I therefore took two of them that were very hungry, and feeding both of them with oats, one I killed four hours after, when I knew a part of the oats were undigeſted; and upon examining the blood, I found the *ſerum* whitiſh, and full of ſmall globules; on its being ſuffered to ſtand a little time, the white part aſcended to the ſurface like a cream. The other was killed forty-eight hours after eating, when its ſtomach was found empty, and the *ſerum* of its blood quite tranſparent, and without any cream riſing to the ſurface, or any appearance of ſmall globules, when examined with the microſcope. Now, this experiment ſeemed to me deciſive, and to point out clearly, that the whiteneſs of the *ſerum* was not occaſioned merely by the body being in want of food, and therefore, drawing the oil from its magazines; becauſe here the animal moſt in want of food had its *ſerum* leaſt white; but was occaſioned by the fat's being re-abſorbed faſter than it was uſed (from its place being ſupplied by the freſh chyle) and thence was accumulated in the blood-

veſſels,

veſſels, ſo as to give whiteneſs to the *ſerum*. And from the ſame obſervation it likewiſe appears probable, that the great re-abſorption, and the accumulation of the fat in the veſſels of the plethoric patients above-mentioned, was the cauſe of their want of appetite, and of their other complaints, and not the effect of them.

May not therefore a too great re-abſorption of the fat, and its accumulation in the blood-veſſels, be now admitted as the cauſe of one ſpecies of a *plethora?*

And may it not likewiſe be uſeful in ſome complaints of the ſtomach, to attend to the whiteneſs of the *ſerum?* For, although fat is a ſubſtance little liable to diſeaſe, yet it may perhaps be ſometimes ſo vitiated, or may ſo incommode nature,

that

that ſhe may be obliged to take it up from her magazines, and to uſe it, or to throw it out of the body. Whilſt this is doing, a ſickneſs of the ſtomach, and want of appetite, may be indications of fulneſs; and therefore, inſtead of wanting remedies to ſtrengthen the ſtomach, may require bleeding, and other evacuations.

APPENDIX.

APPENDIX,

RELATING TO

THE DISCOVERY

OF THE

LYMPHATIC SYSTEM

IN BIRDS, FISH, AND THE ANIMALS CALLED AMPHIBIOUS.

BEING

A Vindication of the AUTHOR's Right to theſe Diſcoveries, in Oppoſition to the Claim of Dr. ALEXANDER MONRO, Profeſſor of Anatomy in the Univerſity of Edinburgh.

APPENDIX.

AN account of the Lymphatic Syſtem in Birds, Fiſh, and Turtle, was given to the public in the Philoſophical Tranſactions, vol. lviii. and lix. for which communications the Royal Society has ſince honoured me with their gold medal. Theſe diſcoveries Profeſſor Monro claimed, in a letter read before that moſt reſpectable body on the 19th of January 1769; and has ſince perſiſted in that claim, in a pamphlet called, A State of Facts, &c. printed at Edinburgh 1770. Now, as both that letter and the pamphlet muſt of courſe have been ſeen by many who know not what can be urged againſt them, I think it but a duty I owe my own character, to lay before the public the proofs I have collected of their inſufficiency to procure Profeſſor Monro the credit of having anticipated me in thoſe diſcoveries; and, I hope, that although in doing this I ſhall treſpaſs on the time and patience of the reader, yet it will be ſome excuſe for me, that I had endeavoured, as much as could be expected on my part, to ſettle the diſpute without troubling the public with it.

As Profeſſor Monro has, in this pamphlet, not only endeavoured to vindicate his claim to theſe diſcoveries, but has likewiſe cenſured me on account of a paper on the *emphyſema*, it is neceſſary, be-

fore

fore I come to the controverſy about the Lymphatics, that I ſhould relate what has paſſed between us on that occaſion.

In the third volume of the Medical Obſervations and Inquiries, is publiſhed a paper on the *emphyſema*, in which I propoſed the operation of the *paracenteſis* of the thorax, to let air out of the cavity of the cheſt; which air I endeavoured to ſhew was the cauſe of the worſt ſymptoms attending that diſeaſe. Not long after this, I was informed that Dr. Monro had declared publicly, he had mentioned that obſervation in his lectures, both at the time, and before I attended them, which was in the winter 1761, and complained, that I had omitted doing him juſtice in this particular.

When I heard this, I made inquiries of ſome of his pupils, who I found had taken notes at his lectures, and by two of theſe gentlemen I was favoured with excerpts from their notes, which convinced me that he had anticipated me in propoſing that improvement. I then determined to let him know, that my omitting to mention his name on that occaſion was entirely owing to my ignorance of his claim. This I was the more deſirous of doing, from having heard that he had exclaimed againſt me with ſome acrimony, on the ſuppoſition that I had got the hint from him, and was conſcious of it; which being far from the truth, I determined to ſhew him in what manner I had really made the obſervation, and thereby ſtop his exclamations. I determined likewiſe to ſhew him

him that I was desirous of giving him the credit of having had the idea before me, and thereby to prevent all dispute about the matter. The following is a copy of the letter which I sent him on that occasion.

SIR,

Being informed that you have publicly complained of me, "for having, in a "paper printed in the third volume of the "*Medical Observations* and *Inquiries*, omit-"ted doing you the justice of mentioning "your having proposed the operation "there recommended, in the same circum-"stances, long before:" and as I am confident I deserved not to be complained of on that account, I have taken this opportunity

tunity of ſtating the manner of my making the obſervation, and at the ſame time of letting you know, that ſince I have learnt that you likewiſe had made it, I am willing to do you juſtice. The thought firſt occurred to me in reading Mr. Cheſton's Pathological Obſervations and Inquiries, in which he gives a caſe of the *emphyſema:* this caſe is told in ſuch a manner, that I think it is hardly poſſible any unprejudiced perſon ſhould read it and not be convinced, as I was, that the cauſe of the principal ſymptoms was air in the cavity of the cheſt. Mr. Cheſton himſelf, in relating that caſe, came as near making the obſervation as poſſible, to miſs it, and yet he did miſs it. From this hint I proſecuted the ſubject, as is mentioned in that paper; and before I publiſhed it, I conſulted every author I could

could eaſily procure, who I thought was likely to treat of the ſubject. And I certainly ſhould have done juſtice to any that I found had anticipated me, and ſhould not have avoided the opportunity of doing you the ſame juſtice. But I knew not, at the time of that publication, that you had ever given the leaſt hint on that ſubject. About the middle of laſt ſummer I was told by a gentleman from Edinburgh of your manner of treating me, at which I was not a little ſurpriſed, as I was not conſcious of having given you the leaſt cauſe of complaint. But having ſince learnt, from other gentlemen who attended your lectures before the time of my publiſhing that paper (and who, at my requeſt, conſulted their notes), that you had really mentioned it, I cannot

now

now doubt that you had made the obſervation before me. At the ſame time I muſt aſſure you, that to ſuppoſe I knew it at the time of publiſhing that paper was doing me injuſtice. Your accuſation, I preſume, is founded on the ſuppoſition of my having heard you deliver the obſervation at your lectures, when I had the pleaſure of attending them. But I do aſſure you, that if I ever heard the leaſt hint on that ſubject, either from you, or from any other perſon, I had not any remembrance of it at the time I wrote that paper. You are not, indeed, the only perſon who, as I now find, has anticipated me: the author of the Monthly Review for laſt June * ſays, he had long had the

* See Monthly Review for June 1768, p. 446.

of

ſame idea, and that he mentioned it in his account of Mr. Cheſton's book. But of this too I aſſure you I was ignorant, when I wrote my paper. What muſt give farther conviction to any unprejudiced perſon of my ignorance of your having made the obſervation, is this: I firſt mentioned it in a paper which I read to a private ſociety, in which were preſent many gentlemen that had attended your lectures, and yet all theſe gentlemen expreſſed themſelves pleaſed with the obſervation, as new and intereſting, and not one of them gave the leaſt intimation of their having ever heard it before. And yet thoſe gentlemen are as likely to remember any obſervation which tends to the improvement of phyſic or ſurgery, as any I know. I ſhall mention their names, to juſtify my good opinion of them; Drs. Stark,

Stark, Parſons *, Saunders †, Pepys ‡, and Ruſton §. The obſervation was likewiſe mentioned in another ſociety of young gentlemen, and alſo in a public hoſpital, where many, who had been your pupils, heard it, and yet no perſon ever told me before I publiſhed that paper (which was almoſt a twelvemonth after I had firſt mentioned the ſubject), that you or any other perſon had ever anticipated me. However, this I relate only to ſhew I was ignorant at that time of your having made the obſervation. But now I know that

* Profeſſor of anatomy at Chriſt's-Church, Oxford.

† Phyſician to Guy's hoſpital.

‡ Phyſician to the Middleſex Hoſpital.

§ Some of theſe gentlemen attended Dr. Monro's lectures about the ſame time with myſelf, the others ſince.

you had, I have not the leaſt unwilling-neſs to acknowledge it, and to do you juſtice in any future publication. At the ſame time that I juſtify my own conduct, give me leave to ſay, that your manner of treating me (if fairly repreſented) was not ſo civil as might have been hoped for. When you complained of me, 'tis a pity you had not likewiſe hinted there was a poſſibility of my being ignorant of your having had the idea. You might perhaps too without impropriety have hinted, that ſhould it come to my ears that you had anticipated me, I might poſſibly be capable of ſuch an exertion of candour as to acknowledge it. But, to have done with ſuppoſitions, this at leaſt I am ſure of, that though I may be as covetous of fame as moſt people, yet I am incapable of taking any unjuſtifiable methods of acquiring it.

I am, Sir, &c.

Dec. 31. 1768.

This letter, Profeſſor Monro could not but acknowledge, " ſufficiently ſatis" fied him in having ſecured his title, as " the firſt who had propoſed that im" provement." Yet ſo unfair an account does he give of it in his State of Facts, that he only ſays, I acknowledged, that " I " could not doubt he had made the ob" ſervation before me; but the farther par" ticulars of it (he adds) it is needleſs to " trouble the reader with, ſince as much " as is neceſſary of theſe will be ſufficient" ly underſtood, from his letter in anſwer" to to me, which, ſurely! is not the caſe; for it no-where appears in his letter, that, beſides mentioning my conviction of his having anticipated me, I had likewiſe promiſed to do him juſtice in a future publication. Nor does it appear in his letter, that I had in mine ſhewn how little probability there was of my having got the idea from him. Theſe the reader may perhaps think Profeſſor Monro ought to have declared, in juſtice to me. For what more could be expected of me, ſeeing I had by accident hit upon an obſervation, which, as it happened, he had made before, than to acknowledge the priority of his title, as ſoon as I knew it, and to put that letter into his hands by which he might always be ſure of ſecuring to himſelf what was his due. But Profeſſor Monro ſays, it was unneceſſary to give a fuller account of my letter. But why was it ſo? Not ſurely in juſtice to me, nor for the ſatisfaction of the reader.—— Nay, ſo far is Dr. Monro from doing me juſtice on this occaſion, that he even intimates I rejected tapping the cheſt with a *trocar*, becauſe it happened to be his method, as if the ſame was not the method recommended by many of the writers on the ſubject of the *paracenteſis* of the thorax, for the caſes in which they adviſe that operation, to whoſe method I alluded, and not to his, which I then knew nothing about.

Next, as to the diſcovery of the Lacteals and Lymphatics in Birds, Fiſh, and the animals called Amphibious; of theſe an account was laid before the Royal Society on December the 8th, 1768. I was preſent when it was read, and had afterwards

terwards some conversation on the subject with Dr. Donald Monro, who, as appears by the sequel, informed his brother, the Professor, of what I had done. Not long after this, I again saw Dr. Donald at St. George's Hospital, and he then told me, that the *Lymphatics* and *Lacteals* in those animals had been discovered by his brother eight years ago, as he now learnt by a letter from Edinburgh, a part of which letter he was to shew to every body, and which was already given to be read before the Royal Society. When I was informed of this, I was astonished, as I remembered to have heard the Professor, since that time, declare that they were not discovered. Besides, I had a note taken from his lectures within two years of his making this claim, in which was a similar acknowledgment *. I was convinced there-

* That note is now printed below, p. 156.

ſore that he had no title to theſe diſcoveries. Upon which I laid before the Royal Society my reaſons for that conviction, in a letter to one of their ſecretaries. Of this letter I ſhall give the reader an account, but ſhall firſt lay before him a literal copy of Dr. Monro's claim.

Copy of Dr. Alexander Monro's *claim, &c. read before the Royal Society, Jan* 19, 1769.

" Above four years ago (ſays he) I in-
" jected the lacteal veſſels of a turtle, or
" ſea-tortoiſe, with quick ſilver, after in-
" jecting the artery and vein with wax,
" and have ſhewn this inſtance of the veſ-
" ſels in the oviparous animals every year
" in my college, and had a drawing made
" of

"of it two years ago by Dr. Palmer, a "copy of which I have ſent incloſed, en- "graved by Donaldſon.

"I LIKEWISE, eight years ago, men- "tioned thoſe veſſels in Fowls and Fiſhes, "which I had ſeen, but not injected."

HERE then is an aſſertion about the veſſels which I had diſcovered, that is far from being equivocal. For here he affirms, that he really had ſeen them eight years ago, nay, that he had even mentioned them to others. This letter too was ſent imme- diately after he heard that I had laid be- fore the Society an account of thoſe veſ- ſels in birds and fiſh *. It could not there- fore be meant merely to inform the Socie-

* As he acknowledges, State of Facts, p. 4.

ty that, now ſeeing Mr. H. had diſcovered the Lacteals and Lymphatics in Birds and Fiſh, he likewiſe had the pleaſure of ſhewing them that he (Dr. Monro) had diſcovered them in the turtle. This I ſay, could not be his meaning; for if it was, why did he ſend his letter ſo precipitately? Why did he not ſend a deſcription of thoſe veſſels in the turtle, in order to make his letter worthy their notice? and why did he ſay he had ſeen them in Birds and Fiſh? The Society, he knew, wanted not his teſtimony to prove that Birds and Fiſh had them. What then could he mean by it, but to claim the diſcovery?

As there could be no doubt that Profeſſor Monro meant this letter as a claim to theſe diſcoveries; neither had I any doubt but I ſhould, for the reaſons above-mentioned, prove that he had no right to them. In order therefore to prevent the prejudices that might ariſe againſt my papers, from his being believed to have anticipated me in theſe diſcoveries, I wrote a letter to one of the ſecretaries of the Royal Society, in which I firſt ſhewed, that I had ſeen the Lacteals of the turtle about a year before him, and then, when I came to ſpeak of thoſe veſſels in Birds and Fiſh, againſt the probability of Dr. Monro's having anticipated me in theſe diſcoveries, I made uſe of the following arguments.

1*ſt*, His not having, by his own confeſſion *, injected them, which he certain-

* In his claim; ſee above, p. 151.

ly

ly would have done, in order to complete the difcovery. To which I obferved he had the ftrongeft motive, both from his knowing the importance of the fubject, and from his having unfortunately declared, in the 57th page of his Anatomical and Phyfiological Obfervations, printed at Edinburgh, 1758, "that, after a con-"fiderable number of experiments which "he had made, he was convinced, that "neither Birds, Fifhes, nor oviparous "animals in general had either Lacteals "or Lymphatic Veffels." After which declaration, I conceived it improbable he fhould patiently wait eight years without injecting them, efpecially as I had found it an eafy matter to inject them, when once they were difcovered. And I added, the probability was, that if he had *feen* thofe veffels, he would have haftened

to

to inject them, and to complete the discovery, were it only to prevent another person's doing it, and thereby acquiring the reputation of having done what he himself had *in vain* attempted, by such a considerable number of experiments as *were sufficient to convince him*, that such vessels existed not in those animals.

2*dly*, I SAID, his claim to the discovery of those vessels, by affirming he had seen them eight years ago, was contradicted by public declarations made after that time; for he had, since, acknowledged in his lectures, *that he had sought for them in vain by a variety of experiments*. And even so late as within these two years *, he declared likewise in his public lectures, " that

* My letter being dated Jan. 10, 1769.

"the Lymphatic Syftem was fuppofed to "take place only in men and viviparous "animals, and by analogy in thofe fifhes "placed by Linnæus amongft the *mam-* "*malia*, and how far was their juft ex- "tent (he faid) was not certain, but that "he had found them in fome amphi- "bious animals, as in the turtle." Thefe declarations, I obferved, were inconfiftent with his claim to the difcovery.

Besides ufing thefe arguments, I promifed the Society I would hereafter produce unqueftionable proofs of the invalidity of his claim, having by this time found, that the Doctor, fortunately for me, had exprefsly acknowledged in his lectures, *that he had fought for them in vain*, almoft every year fince the time that he now pretends to have feen them.

Dr. Monro being informed of theſe proceedings, ſent me his letter, dated June the 8th, which he has ſince printed in his *State of Facts*. But that letter appeared ſo confuſed, that I knew not what to make of it. Sometimes I thought it was meant to prove that he had diſcovered thoſe veſſels, agreeably to his aſſertion read before the Royal Society: but this I ſoon after ſuſpected could not be the caſe, becauſe, after relating all his facts and experiments, he concludes, *not* that he had diſcovered them, *but only* "that he "had ſeen what he ſtrongly *ſuſpected* to "be lacteals in thoſe animals" (*viz.* Birds *)—And, "that (from preceding "experiments) he was *perſuaded* that Birds "were provided with lacteal veſſels, and

* State of Facts, p. 21.

"confirmed in this *opinion*, by having "injected them in one of the ſame ovipa- "rous claſs, the turtle *."———Or, in other words, Dr. Monro claimed a diſcovery, by telling the Royal Society that he had *ſeen* thoſe veſſels in Birds eight years ago, which he now proves, by ſhewing he had only *ſuſpicions* about them, and an *opinion* that Birds had them, becauſe Turtles had them.

At other times, I thought that poſſibly, after finding what his pupils teſtified, he might now be convinced he had imprudently claimed thoſe diſcoveries, and might intend this as a ſort of an acknowledgment (tho' an aukward one) of my being the firſt who had ſeen thoſe veſſels. But the vindictive ſtile of his letter convinced me it

* State of Facts, p. 23.

was not meant as an apology *, as likewiſe did the ſtile of a ſhort note written on the cover of that letter, of which note the following is a copy.

To Mr. Hewson, &c.

SIR,

When you have read the incloſed, you are very welcome to write ſuch remarks on it, as to you, or to your friend Dr. H——r, or to any of his friends, ſuch as Dr. ——, &c. may ſeem proper; only when you have done, I think you ought to ſhew it to all thoſe ſocieties, phyſicians, and ſtudents to whom you have made free with

* As for inſtance, where he talks "of (his) "making all the allowance I require for my na- "tural, or (ſays he) I ſhould rather call it, unna- "tural imbecility of memory." This paſſage is altered in his *State of Facts*, p. 22.

my name; or, if this taſk ſhould not ſuit your diſpoſition, or be irkſome to you, after the great fatigue you have taken about me already, pleaſe to let me know this, and I ſhall take that trouble on myſelf.

I am, Sir, &c.

(Signed) ALEX. MONRO.

Edinburgh, June 24.
1769.

THIS ſeemed clearly not to be the ſtile of one who was ſenſible of his error, and was apologizing for it; and convinced me, that Profeſſor Monro intended that letter as a proof of his right to thoſe diſcoveries. However, not to be poſitive that I had hit upon his meaning, I determined, before I laid any thing before the public,

public, to aſk an explanation of that letter. For this purpoſe I wrote to him on the 15th of July, and deſired him to tell me "whether he meant it as a proof of "his right to thoſe diſcoveries"——or "whether he meant by it to give up to "me the right to them." And as I had found him in that letter wandering from the ſubject, and inſtead of concluding that he *really* had ſeen thoſe veſſels, concluding *only*, that he had ſeen what he *ſuſpected* to be the Lacteals in Birds——And again, that he was *perſuaded* Birds were provided with thoſe veſſels—but no where ſaying, that he had ſeen what he *knew* or *could prove* to be their Lacteals, which alone could give him a right to the diſcovery: I therefore told him, "that to "avoid for the future all wandering from "the ſubject, I ſhould ſtate the diſpute

" as it appeared to me;" and then I ſaid, that " it was he who began it, for, on " hearing that I had diſcovered the " Lymphatic Syſtem in the three claſſes " of oviparous animals, he had ſent a let- " ter, which was read before the Royal " Society, and was to be ſhewn to every " body. In this letter he aſſerted, that he " had diſcovered the Lacteals in a turtle " about four years ago, and in Birds and " Fiſh eight years ago; and that he " even mentioned theſe diſcoveries to " others." Theſe aſſertions (I added) were conſtrued a claim to the diſcoveries I had made. With this letter I likewiſe ſent him a copy of mine, which had been read before the Royal Society. By theſe means I thought I ſhould either keep him to the ſubject in queſtion, or, if he ſhould again wander, the reader would be con- vinced

vinced it was not my fault, but his own, that he *now* knew not what he had *then* aſſerted.

This letter, however, had no effect. I therefore wrote to him again, hoping he might now be convinced that his claim was ill-founded, and might therefore be induced to retract it, inſtead of obliging me to prove to the world its invalidity. The following is a copy of the letter which I ſent him on that occaſion:

SIR,

It is now above ſix weeks ſince I wrote to you, deſiring an explanation of your letter of the 8th of June. As you have not

not given me that explanation, I have now taken up the pen to inform you, that agreeably to your own defire, and in order to juftify my conduct towards you, I am commenting upon that letter which you fent me. My comment would be more to the purpofe, were I always fure I underftood you, but if that fatisfaction fhould ftill be denied me, I muft proceed as well as I can, and I muft fay, that if I fhould miftake your meaning, it will not be wilfully, fince you might, by an anfwer, have cleared up all ambiguity. I cannot help regretting, that this difpute fhould fubfift between us, both on my own account, as I think it hard to have the trouble of proving my right to difcoveries which are certainly my own, particularly as it takes up that time which I hoped to employ to a better purpofe; and I likewiſe regret it on yours, ſince, in order to maintain my right, I ſhall be under the neceſſity of producing ſome facts and teſtimonies, which, in my opinion, cannot but lead to concluſions very unfavourable to your reputation. And as I ſhould be ſorry that one of my firſt attempts to lay the foundation of my own character, ſhould be attended with circumſtances which may hurt yours, and really wiſh to avoid it; I therefore ſtill hope that this diſpute may be ſettled in a more eaſy manner. You muſt, I think, be now convinced, that in claiming theſe diſcoveries you have injured me, and cannot be at a loſs to know what might be expected from you on ſuch an occaſion. But if, inſtead of doing me that juſtice which might be expected from a man of candour, you treat this letter likewiſe with ſilence, then juſtice to myſelf requires that I ſhould no longer delay producing ſuch proofs as I poſſeſs of your having no right to theſe diſcoveries, and ſhewing them to the very reſpectable Society, to which I have promiſed them; or to ſuch *phyſicians*, *ſtudents*, *&c.* as may have heard of your claim; without regretting much that thoſe meaſures which I take to maintain my right, may perhaps affect ſenſibly the character of a man, who having firſt injured me and afterwards had his error pointed out to him, was incapable of candidly acknowledging it.

I am, Sir, &c.

(Dated)
Sept. 9. 1769.

In anſwer to this letter, he ſent me one dated Sept. 30, in which, inſtead of anſwering my queſtions, he evades them, concludes as vaguely as in his former; and when he ſpeaks of his aſſertion read before the Royal Society, alters its ſenſe, qualifying the alteration with "*to the beſt of his re-*" "*collection*" denies he was miſtaken in claiming theſe diſcoveries. And, what is ſtill more remarkable, accuſes me of having made concluſions injurious to him, "*by arguments, weak, inconcluſive,*" "*not real, but feigned.*"

It was evident, from ſuch a letter, that he would not embrace the opportunity I offered him, and avoid a diſpute, by acknowledging his miſtake, and retracting his claim. I therefore no longer heſitated to print the proofs I had collected of his not having anticipated me; and though I had once intended to make ſome remarks on his letter of the 8th of June, as is mentioned above, yet I afterwards determined to omit theſe, ſince the teſtimonies of his pupils alone ſufficiently proved that he had no right to thoſe diſcoveries. By theſe means I reduced the publication to half a ſheet of paper: in which I firſt gave an account of his claim made, by ſaying "*he had ſeen thoſe veſſels* EIGHT *years* "*ago*;" then I mentioned, as arguments againſt its *validity*, that I had myſelf heard him ſince that time declare, "*he could not* "*find thoſe veſſels*," and that, beſides, I had a note taken at his lectures by a gentleman within two years of his making it, which contained a ſimilar declara-

tion

tion *, and afterwards I faid that I had written to fuch gentlemen as I knew had, as well as myfelf, attended his lectures within thefe eight years, defiring them to confult their notes, and to let me know what Dr. Monro had faid as to the exiftence,

* Dr. Monro has mifinterpreted this paffage. He fuppofes I meant that this note was taken two years before 1762, whereas I meant two years before 1768, the time when he claimed thefe difcoveries, and when I wrote my letter to the fecretary of the Royal Society. The note itfelf is printed above, page 156.

Dr. Monro has taken notice of another inaccuracy, that is, where I had faid, " He afferted that " he had anticipated me in thefe difcoveries ;" inftead of faying " that he claimed them by afferting " he had injected, &c." The former Dr. Monro feems to allow to be what he meant, but not *exactly* what he faid. It was therefore a fmall inaccuracy. But his claim is now printed *verbatim*. See above, p. 150.

or non-exiſtence, of the lacteals and lymphatics in theſe animals; without mentioning the diſpute between us, or any opinion I had formed, that they might be unprejudiced to either party. And that ſuch of theſe gentlemen as had taken notes ſent me excerpts from them, which, as I had ſuſpected, agreed with what I had myſelf heard the Profeſſor ſay upon that ſubject.

THAT Dr. Haygarth, phyſician at Cheſter, had ſent me the following paſſage from the notes which he had taken in 1764.

"FOWLS, and ſome fiſh (ſays Dr. Monro) have not lacteal veſſels that we can "ſee; they have no conglobate glands in "the

"the mesentery; perhaps they (the lac-"teals) don't run into each other, but in-"to red veins, and hence never are so large "as to become visible."

"THIS note," adds Dr. Haygarth, "was taken in 1764, and if Dr. Monro "had changed his sentiments on this sub-"ject in the year 1765, I should certain-"ly have taken notice of so remarkable a "circumstance."

THAT, from Mr. Orred, surgeon at Chester, who attended Dr. Monro's lectures during the winter 1765-6, I had learnt, that Dr. Monro, when he spoke of the anatomy of a cock, declared: "he "never saw, or observed any *glandulæ* "*vagæ*, or lacteals, but had seen lympha-

"tics in the neck, ending in the jugu-"lar *."

THAT, from ſome notes, ſaid to be copied from thoſe taken by Dr. Taylor of Reading, in the winter 1765-6, I had procured the following exerpt; and Dr. Taylor, on being requeſted to conſult his own copy, had acknowledged it was a juſt one.

"THE lymphatic ſyſtem (ſays Dr. Mon-"ro) is ſaid to take place only in men, "and viviparous animals, and from ana-"logy in thoſe fiſhes placed by Linnæus,

* It may be neceſſary to mention here, that the diſpute between Dr. Monro and me is, *who firſt diſcovered the lacteals of birds*; for as to the *lymphatics* in their necks, (mentioned in this gentleman's note) theſe, we both allow, were diſcovered by Mr. John Hunter about ten years ago.

"under

"under the claſs of *Mammalia* *: how far
"is their juſt extent is not certain, but
"we have found them in ſome amphibi-
"ous animals, as in the turtle †.

"It is ſaid that this ſyſtem is wanting
"in oviparous animals; but this is not
"univerſally true; for we mentioned,
"that we found them in a turtle, and
"they would probably be found in other
"orders and *genera*, if properly examin-

* In the excerpt it is *amphibia*; but it is evident from the ſenſe, and from comparing it with the other notes, that it ſhould be *mammalia*.

† As to the *lacteals* of the turtle, there is no doubt but that Profeſſor Monro and myſelf hav both diſcovered them. He in the ſummer 1765; I before that time, viz. in the autumn 1763; when I took a ſhort deſcription of thoſe veſſels, which is publiſhed with my paper on the lymphatic ſyſtem in birds, in the 58th volume of the Philoſophical Tranſactions.

"ed. But admitting that they are not "demonstrable there, it doth not follow "that they are wanting, for, perhaps, "they may run only a little way, and ter-"minate in red veins."

THAT Dr. Maddocks, Physician to the London Hospital, had favoured me with the following excerpt from notes which he took at Dr. Monro's lectures, in the winter 1765-6.

"LYMPHATICS are found in viviparous "animals, and, therefore, I presume, in "the whale, which is of this kind. They "are not to be found in oviparous ones, "fishes, nor the *amphibia*: this is the "common doctrine. *I will not say how* "*far they may be found in some birds*, but I "have found them in some of the amphi-

"bious

"bious animals, as in the turtle, run-
"ning along the root of the mefentery."

THAT Mr. Hull, furgeon at Stevenage, had fent me the following excerpt from his notes, taken about four years ago.

"I never could, to this day (fays Dr.
"Monro) find a fingle branch of a lacteal
"in the abdomen of fowls, nor any lac-
"teals, or glands of the conglobate kind
"in the mefentery, notwithftanding I have
"made experiments with that view very
"often. I kept fowls twenty-four hours
"without food, then fed them with bread
"foaked in milk, and tinged it by turns
"with blue, madder, and faffron, and
"afterwards opened them at feveral dif-
"ferent times, in order to difcover the
"lacteals, but all without fuccefs. Yet,

"perhaps, the lacteals may be in fowls, "tho' not demonſtrable." This, adds Mr. Hull, I will anſwer for being *verbatim*, or nearly ſo, as Dr. Monro delivered it in the anatomical hall, at Edinburgh, on the 13th of February, 1765 *.

THESE paſſages, I added, were ſufficient to ſhew how little right Dr. Monro had to theſe diſcoveries. Beſides, I ſaid it was a ſtrong argument againſt him, that in the letter I had received (which he has printed in his State of Facts, p. 8.) he could not, after relating all his experiments and obſervations, conclude he had

* If the reader will take the trouble of comparing this note with Dr. Monro's own account of his experiments (State of Facts, p. 12.) he will be convinced how accurately this gentleman muſt have taken notes.

really

really seen those vessels as he had told the Royal Society; but in one place he says, "that, from the preceding experiments, "&c. it is evident that he had seen what "he *suspected* to be the lacteals in birds." And in another, "that he was himself "*persuaded* that birds were provided with "lacteal vessels, and confirmed in this "*opinion*, by having now injected them in "one of the same oviparous class, the "turtle *."

Such conclusions, I said, appeared merely evasive, and never could be considered as proofs of his having discovered

* This argument was repeated in a note, to prevent Dr. Monro's writing; as if the dispute between us was, *who first had suspicions about those vessels*, instead of *who first discovered them.*

those vessels before I did, agreeably to his assertion read before the Royal Society, and since repeated in his State of Facts.

The half-sheet of paper, containing these arguments and testimonies against Dr. Monro, was printed Dec. 1. 1769, and was given to such gentlemen of my acquaintance as had heard of his claim, and a copy of it was sent to the Doctor. Upon receiving which, he published his State of Facts; but, what is singular, he has attempted his justification without taking proper notice of these testimonies against him; as if he could be justified whilst they remain unanswered. And in this State of Facts, in spite of those testimonies, he repeats to us, "that long before 1762, he observed blueish vessels in "the

"the mesentery (of birds) which he *judg-*
"*ed* to be lacteals, and *had mentioned as*
"*such* in his lectures *." And again,
"that about the years 1759-60, he had
"seen collapsed blueish vessels, which he
"*concluded* lacteals, &c. †." What shall
we say of this?

Nay, Dr. Monro has, upon this occasion, even ventured another assertion, viz. "that the notes of his pupils taken for "three years before 1762, will be found to "prove, that he then taught the direct con-"trary" of what I have brought these testimonies to prove he has since taught ‡. Now, surely this is very improbable, and Dr. Monro

* State of Facts, p. 4.

† Ibid. p. 27.

‡ Ibid. p. 26, in the note.

Monro ſhould have adduced ſome teſtimonies to prove it. But ſuppoſing it were true, it would lead to a concluſion unfavourable to him. It would ſhew, that he muſt have miſled either the one ſet of gentlemen or the other;—for he ſays he told the firſt he had ſeen the lacteals—the laſt prove he has ſince taught them that he *never could ſee* thoſe veſſels.

The reader, I fancy, by this time thinks with me, that Profeſſor Monro's claim deſerves no more of our attention. But, as he has printed ſome excerpts from his OWN book of notes, with the parade of having them authenticated, as if they contained the diſcovery, notwithſtanding the above-mentioned proofs of his having acknowledged repeatedly ſince he wrote them,

them, that he never could find thoſe veſſels. I ſhall next, therefore, make ſome remarks upon his notes.

To begin with thoſe relating to the turtle. He diſcovered its lacteals in the ſummer 1765. I had ſeen them before that time, viz. in the autumn 1763. Beſides, I have ſince injected and traced out the whole ſyſtem *; he does not even pretend to have done ſo: it is, therefore, not difficult to determine, who was the firſt diſcoverer, and who has carried the diſcovery fartheſt.

Next, as to the lacteals in fiſh. To prove that he had found thoſe veſſels eight years ago, he tells us, that in a note taken

* See Philoſoph. Tranſact. vol. lix.

from the diſſection of a ſkate on April 24, 1760, he has ſaid, "He had diſco-"vered a whole ſyſtem of lacteals and "lymphatic veſſels running towards the "heart, on the left of, and above the *vena* "*portarum*, and from theſe the auricle of "the heart was blown up. They are pro-"portionally larger, but have fewer valves "than in man *." Now, I will take up-on me to ſay, there is nothing in this note which proves whether he had inflated a lacteal or a vein. For what he ſays of the ſituation of the veſſels, and of his blowing up the heart, is equivocal: the only part of the note which ap-pears to characteriſe the lacteals, is in reality a miſtake; that is, where he ſays *they have valves*. But the lacteals on the

* See State of Facts, p. 12.

mesentery

mesentery of a skate have no valves, and injections pass readily from the large to the smaller branches. And what is even more to the purpose, although it appears, from his calling what he saw *lacteals* and *lymphatics*, that he had at that time some suspicions about them. Yet I am persuaded he has since changed his opinion; and this I think is evident, even from the manner in which he speaks of his experiments made the year after. For, says he, "I have dissected this year (1761, in summer) eight skates, and about a like "number of cods and codlings, but without being able to observe by dissection, "or to inflate any like to lacteal or lymphatic *glands*—I find indeed (he adds), "that blowing backwards in the meseraic "veins, the intestines and the cellular substance between their coats are inflated;

"but this is no direct proof of branches "of red veins absorbing, as these veins "may be burst, or the air may have first "entered the arteries *." Now, this surely is not the language of a man who had seen the lacteals, but of one that was seeking for them. Had he found them, he certainly would have mentioned it in this note, but he avoids the subject entirely, and only says he could not find the *glands*, thereby leaving us to suppose that these dissections were made for the *glands only*, after having discovered the *vessels*: which is highly improbable, since, by his own confession †, he did not inject the vessels, which he knows well enough is the best way of determining whether the glands exist or not; and *one* experiment in

* State of Facts, p. 12.

† See his claim above, p. 151.

this

this way would have been more ſatisfactory than his *eight*, or than eight hundred made by diſſection only *. Add to this, he would not, I think, if he had now ſeen the lacteals, have taken up his time with trying whether the red veins did the office

* If the reader ſhould happen not to be well-acquainted with this part of anatomy, he may not ſee all the force of this argument, which will be ſatisfactory to anatomiſts; for it is a fact admitted amongſt them, that the meſenteric glands are placed only in the courſe of the lacteals, ſo that the lacteals muſt paſs through theſe glands in their way to the heart. The readieſt method therefore of diſcovering the glands, after having ſeen the veſſels, is by injecting thoſe veſſels; for the injection, in its way to the heart, muſt diſtend the glands, and make even the ſmalleſt of them viſible. The veſſels ſeen by the Doctor ſeem to have been very large; can it be ſuppoſed then, if he had been convinced they were lacteals, that he would not have injected them, and thus have determined whether there were glands or not, by one experiment, inſtead of tediouſly diſſecting ſixteen Fiſh?

of abſorption for them, as he ſeems to have done by blowing into theſe veins. Nay, I will go farther, and will take upon me to ſay, that it is probable he was in theſe laſt diſſections convinced he had been miſtaken in what he took the year before for lacteals and lymphatics. This I think evident, both from the notes above-mentioned, and from his manner of treating the ſubject ſince that time. For, if he thought he had ſeen thoſe veſſels, he would doubtleſs have uſed this diſcovery as an argument againſt abſorption by the common veins, as he has ſince uſed that in the turtle. But it appears, from the notes of his pupils *, and even from his own account of thoſe arguments †, that

* See his pupils notes above, p. 170, &c.

† See State of Facts, p. 16.

he

he has not done ſo. And again, had he thought he had diſcovered thoſe veſſels, he would not have acknowledged in his lectures ſince that time, that they were not yet known to exiſt *. He has therefore aggravated the impropriety of his conduct in claiming theſe diſcoveries, by the diſingenuity of ſending ſuch notes as proofs of his claim.

AND laſtly, as to the lacteals in *birds*, he tells us, " that, in 1758, he remarked " a veſſel making an arch on the meſen" tery of a cock, which at firſt he believ" ed to be a trunk receiving the lacteals, " but not being able to inject it on trial, " he conjectured to be rather a nerve." And afterwards, in April 1759, " he ob" ſerved in a cock what *looked like* lac-

* See the notes of his pupils above, p. 170.

" teal

" teal veſſels collapſed, of a blueiſh colour, " which ſeemed to terminate at the back- " bone, &c.—Theſe he ſhewed to the ſtu- " dents." And again, after relating the manner of making his experiments upon no leſs than twelve cocks *, he tells us, that " in 1761 he obſerved, in the interſtices " of the great arches of the red meſenteric " veſſels, a pellucid net-work, ſome part " of which ſeems to be compoſed of " branches ſent from a large nerve, run- " ning parallel with the inteſtines, and " nearer to them than where the trunk of " the meſenteric artery ſends off its large " branches; but although (ſays he) *I* " *ſuſpect ſtrongly* there are here too nume- " rous lacteals, and I even obſerve very " ſmall knots, which I take to be analo-

* The experiments were made by feeding theſe birds with oatmeal and madder, oatmeal and rhubarb, &c. See State of Facts, p. 12.

" gous

"gous to our mesenteric glands, yet I "have not observed the above-mentioned "kinds of food to make any odds in their "appearance *, &c." And again, after a variety of other experiments, he says, "he "could not observe more than above-"described." Now, what is there in these notes that can entitle him to the discovery of the lacteals in birds?—Can his seeing a blue plexus on the mesentery, which at first indeed he suspected to be lymphatic, but afterwards to be nervous, and a part of which, he acknowledges, was in his last experiments found to be made of nerves, entitle him to it? or can his discovery of these small knots, which he takes to be analogous to our glands, entitle him to it? Certainly not. Birds have no lymphatic glands on their mesenteries, as I have shewn †. Is

* State of Facts, p. 12.

† Philosoph. Transact. vol. lviii. art. 34.

it not therefore plain, from theſe notes themſelves, that he had not diſcovered the lacteals in birds? Has he not repeatedly ſince that time acknowledged this in his lectures *? What ſhall we ſay then to his aſſerting that he had ſeen them eight years ago, and his laying before a reſpectable Society, and deſiring his brother to propagate ſuch an aſſertion? Or what ſhall we ſay to his perſiſting in it, or above all to his telling us, in his laſt publication (p. 26.) "that in theſe notes the "reader will find the appearance of theſe "veſſels after death really deſcribed †?"

AFTER

* See the notes of his ſtudents above, p. 170.

† Let me beg of the reader again to examine theſe notes, and then judge of the propriety of Dr. Monro's affirming they really contain a deſcription

of

After these notes follow some others, to prove that he had *argued* in favour of the probability of the existence of those vessels in Birds and Fish; and a conclusion that he had *supposed* frogs might have them; and his *suspicions* that Birds might have them—And his *persuasions* that they must have them (*not because he had seen them, but*) because turtles had them. Which are nothing to the purpose, and ought never to have induced him to claim the discovery, or to say he (actually) had seen them. I cannot therefore think it worth while to take any farther notice of these conclusions.

It is indeed remarkable, that Dr. Monro could persuade himself he had any original

of those vessels, when he has himself put it in the power of the reader to observe the contrary.

ginal merit *even* in entertaining ſuch *ſuſpicions* and *opinions*. More than one writer had ſuſpected thoſe animals had them, and that they themſelves had ſeen ſomething like them; for a proof of which the reader need look no farther than Dr. Haller's *Elem. Phyſ.* * But, as thoſe writers had given no proofs of their having diſcovered them, their *ſuſpicions* and *opinions* paſt for nothing.

Professor Monro, not ſatisfied with claiming theſe diſcoveries, has even gone farther; he has intimated, in ſome parts of his book, that I might have learnt them, or a part of them, from him. As in page 4, where he ſpeaks of my "giving "an account of theſe veſſels entirely as

* Lib. ii. Sect. 3. & Lib. xxiv. Sect. 2.

" my own diſcovery," this in page 6. he calls " broaching a new ſubject with him;" and complains of me " for paſſing in ſi-" lence what I might have heard him ob-" ſerve concerning it when I attended his " lectures."—How Profeſſor Monro could pretend that I had learnt any thing from him on this ſubject, that ought not *for his ſake* to be paſſed in ſilence, is aſtoniſhing. What could I learn from one who has repeatedly ſince that time acknowledged *he never ſaw* theſe veſſels; *that they might be too ſhort to become viſible*; and who, at the time I attended his lectures, ſaid, he could not find them, as I have already declared. But, as my teſtimony will have more weight with the reader, when corroborated with that of a gentleman unconcerned in the diſpute, I ſhall next add a copy of ſome notes taken by Dr. Morgan, now professor of medicine in Philadelphia, who attended Dr. Monro's lectures at the same time with myself, and who, at my request, sent me the following excerpt, taken at his lecture upon the question, Whether the common veins absorb or not.

"Most authors (says Dr. Monro) concurring in opinion, that fowls were destitute of lymphatics, and not being able to discover them myself, I was led to be of their opinion. I have already observed, that where conglobate glands are found, there are lymphatics, and the converse of this proposition, namely, where there is no conglobate gland, there are no lymphatics. And there being no conglobate glands to be seen in the mesentery of fowls, nor in fishes, I judged these animals to be destitute of

"lym-

"lymphatics; but Mr. John Hunter hav-"ing discovered conglobate glands in the "neck of a swan, put me on further search, "and I then found them plainly in com-"mon fowls, *but never could find any lac-"teals* in their mesentery, though experi-"ments were tried by means of coloured "tinctures of various sorts, as of rhu-"barb, &c."

From this excerpt it is evident, that Professor Monro, when I attended his lectures, taught, as he has since done, that what he knew of the lymphatics he learned from Mr. Hunter, and as to the lacteals *he could not find them*; and this was in the spring 1762, the very year after the time when, according to his letter read before the Royal Society, he should have seen these vessels, and mentioned them in his lectures: and finally, to complete the whole, he now complains of me for passing in silence what I might then have heard him observe concerning them.

Thus have I endeavoured to obviate the arguments in Dr. Monro's publication, and the reader must now, I think, see clearly, not only the impropriety of the Doctor's asserting his right to these discoveries, but the still greater impropriety of his persisting in that assertion.

Besides claiming these discoveries, Dr. Monro has, in his letters on the subject, treated me in a manner which I cannot pass unnoticed—Thus, he first gives the name of misinterpretation to my concluding from the notes of his pupils, that he had

not

not ſeen " what he believed" the lacteals, and then adds:

" SHOULD we even ſuppoſe the above " miſinterpretation venial, what muſt the " reader think, when he is told, you was " informed that a gentleman, who had " attended my lectures two years at leaſt " before I injected the lacteals of a turtle, " that is, nearly about the time you did, " declared he heard me then ſpeak of hav- " ing *ſeen the lacteals in fowls*; and yet " that you continued to vent this injuri- " ous ſuppoſition? That is, you muſt " have ſunk this material information, " ſince it overturned the whole purport of " your ſtory *."

* See his State of Facts, p. 23.

Now, here is an accuſation, which, were it true, would fall heavy upon me. But the caſe is this; I had indeed heard that a gentleman, who attended Profeſſor Monro's lectures about the time I did, had declared he then underſtood the Profeſſor had ſeen the *lymphatics* in Birds. And Dr. Donald Monro, when I ſaw him at St. George's Hoſpital, aſked me, if I had not heard that this gentleman (mentioning his name, viz. Dr. James Blair, then in London, now in Virginia) had ſaid, that the Profeſſor had then mentioned his having ſeen the *lacteals* in Birds. I anſwered Dr. Donald, that I had heard ſomething of the matter, but could conclude nothing from it (or to that effect). The reaſon was this; I knew from the teſtimony of my own memory, that the Profeſſor had then acknowledged the contrary of his ever having ſeen the lacteals *. I knew the ſame from the teſtimony of gentlemen who had attended his lectures ſince. I therefore concluded that this gentleman had confounded his ſaying he had ſeen the *lymphatics*, with his ſaying he had ſeen the *lacteals*, which I thought might eaſily happen, as I never knew him take any notes. And upon receiving the Profeſſor's letter, I wrote to Dr. Blair, and in his anſwer he acknowledged: "that "although he had, indeed, for ſeveral "years, been under a general perſuaſion "that Dr. Monro had ſeen the *lacteals* or "*lymphatics* in fowls, yet he had no note "on the ſubject, and a very confuſed re- "membrance of what he had heard."

* Dr. Morgan's note proves he did ſo, ſee above, page 194.

SIMILAR

SIMILAR to this accuſation is the greater part of a letter which I received from Dr. Monro, in anſwer to two of mine. This letter is dated Sept. 30. 1769. The Doctor has not printed it, but I beg leave to take a little notice of it.

HE begins it by altering the ſenſe of his aſſertion read before the Royal Society, by the introducing the word *believed*, making it rather a doubtful than a poſitive aſſertion. He has done the ſame in the beginning of his State of Facts, qualifying the alteration, by adding, " *to the beſt of his* " *recollection, and that he had not kept a* " *copy of his letter, not ſuppoſing it material* " *to do ſo* *." But ſurely this was not ſufficiently qualifying it. If he did not

* See State of Facts, p. 5.

know

know exactly what he had then asserted, why, before he defended it, did he not ask a copy from his brother, who, most probably, would keep it, *in order to shew it to every body*; or solicit that favour from the secretaries of the Royal Society where it was read, who, he might be sure, had preserved it, as they do every paper that is laid before the Society. And again, if he was not sure of its contents, how could he now venture, in his State of Facts, *positively* to insist, in opposition to what I had declared, "that his first and last assertions were "exactly the same *." This at least was inexcusable.

Next, he repeats his vague inferences as in his letter of the 8th of June, "that "he had seen what he *suspected* to be those

* State of Facts, p. 26, in the note.

"vessels,"

"vessels, &c." and afterwards, when he comes to speak of the conclusions concerning his claim, which I made in my letter read before the Royal Society, he says, "That he was almost ashamed, on my account, to add a plain corollary, that I "must or might have been conscious, "that the injurious conclusion with respect to him, which I was labouring to "impress on the members of a respectable "Society, was drawn from arguments "that were weak, inconclusive, not real, "but feigned *." Afterwards he tells me, "that he is glad, *on my account as well "as his own*, that I am at last really "ashamed of my letter." And he then finishes with the following passage: "An-

* The conclusions alluded to in these passages are printed above, p. 153, and 154.

"other

"other unhappy miſtake of yours (ſays "he) is, that you ſhould not have known, "or rather perhaps misfortune of yours, "ſince you don't ſeem to have known ſo "much, that you ſhould not have been "told, that your preſuming to draw the "above concluſion concerning any per- "ſon who had the ſmalleſt pretence to "character, without producing proof "and abſolute certainty of its being true, "was what you never could be able to juſ- "tify to any *gentleman*."

Now, when it is conſidered that Dr. Monro obliged me to act in the manner I have done, in order to ſecure my right, do not theſe paſſages appear very extraordinary?——But the reader, I believe, will excuſe my not dwelling upon them. I ſhall therefore only add, that the

the *proofs* on which my *conclusions* were founded, being now laid before the public, to their judgment I willingly submit them, and that, with respect to Dr. Monro, I have nothing more to say, than that I hope, *for his sake, as well as my own*, to see no more of his *claims*, his *assertions*, and his *conclusions*.

THE END.

ERRATUM.

The reader is requested to correct the following :——P. 149, l. 7. for *those animals*, read *birds and fish*.