Experiments on the blood : with some remarks on its morbid appearances / by William Hewson.

EXPERIMENTS

ON

THE BLOOD,

WITH

SOME REMARKS

ON

ITS MORBID APPEARANCES.

By WILLIAM HEWSON, F.R.S.

Read before the ROYAL SOCIETY, 1770.

LONDON,

Printed by W. BOWYER and J. NICHOLS.

M.DCC.LXXI.

EXPERIMENTS

ON

THE BLOOD,

WITH

SOME REMARKS

ON

ITS MORBID APPEARANCES.

By WILLIAM HEWSON, F.R.S.

Read before the Royal Society, 1770.

LONDON,

Printed by W. Bowyer and J. Nichols.

M.DCC.LXXI.

Experiments on the Blood, with ſome Remarks on its Morbid Appearances.

Read June 14 & 21, 1770.

AS the following Experiments are made on a ſubject generally thought important, and as the inferences which I have ventured to draw from them ſeem to explain ſome appearances in diſeaſes, they will not, I flatter my ſelf, be thought altogether unworthy the attention of this learned Society.

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 being obvious, that till this be done, our inferences from their proportions will be liable to conſiderable fallacies. Two of the lateſt writers on this ſubject agree, that if the blood, after being taken from a vein, be ſet in a cold place, it will not eaſily ſeparate, and that a moderate warmth is neceſſary: this is a fact that is evinced by daily experience. They likewiſe ſay, that the heat ſhould be leſs than that of the animal, or than 98° of Fahrenheit's thermometer; and that if freſh blood be received into a cup, and that cup put into water heated to 98°, it will not ſeparate; nay, they even ſay, that it will not coagulate; but this, I am perſuaded 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

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, 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, that 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 coagulated, but the ſeparation was much advanced before the whole of the blood in the baſon was coagulated. Thence I am led to conclude, that the ſeparation of the blood in a given time, is in proportion as the heat in which it ſtands is nearer to the animal heat, or 98°; or greater in that heat than in any of a leſs degree. And I am confirmed in this inference by experiments hereafter to be related, where the blood in the living animal whilſt at reſt was found both to coagulate and to ſeparate.

It is well known, that the *craſſamentum* conſiſts of two parts, of which one gives it ſolidity, and is by

ſome

ſome 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. Theſe two parts can be ſeparated by waſhing the *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 coagulable lymph 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 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

* 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.

the

the red particles, or globules as they are called, which, ſay they, being in 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 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 venous blood aſſumed a more florid redneſs, where in contact with the air-bubbles, whilſt at 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

opening of living animals *; and as a ſimilar change is produced 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 blood-letting, that the blood which comes firſt out is blackiſh, but afterwards it becomes more florid: in theſe caſes, the arterial blood paſſes into the veins without undergoing that change which is natural to it.

Some of the neutral ſalts have a ſimilar effect on the colour of the blood to what air has, particularly nitre; thence ſome have attributed the difference of colour in the arterial and venous blood to nitre, which they ſuppoſed was abſorbed from the air whilſt in the lungs. But we know that this is a mere ſuppoſition, for air contains no nitre. Indeed nitre is far from

* 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 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, after it is collapſed, this change cannot take place.

being

being the only neutral ſalt which has this effect on the blood, almoſt all the neutral ſalts have the ſame. In making ſome experiments on this ſubject, I have obſerved a much more remarkable effect which neutral ſalts have upon the blood; and that is, 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; and 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 Society with at preſent: it may be ſufficient to obſerve that in general they agree in producing this change *. And it is leſs neceſſary to

* It may be neceſſary to obſerve here, that thoſe made with the volatile alkali, and with the earth of allum, are to be excepted.

be particular in giving a detail of their effects, from our not knowing of any uſe this would be in medicine, becauſe we muſt not conclude that their effects in the body would be the ſame as we ſee they are out the body. Indeed, theſe experiments, as well as many 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 determined to do my utmoſt to throw ſome light on this ſubject. It was with this view that I have made ſome experiments even on living animals, being convinced that ſuch experiments could not otherwiſe be made ſatisfactorily.

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 giving us an opportunity of making ſome experiments on the blood, which we could not otherwiſe do, from its coagulating ſo ſoon when taken from the blood-veſſels.

This property of one of the neutral ſalts has been long known, amongſt thoſe who prepare blood for food; for it has long been a practice with ſuch people, to receive blood into a veſſel containing 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 cloth, without

leaving

leaving any *coagulum* behind: by this means they have an opportunity of mixing it with other ſubſtances for the uſes of the kitchen.

Although the coagulable lymph ſo readily becomes ſolid when expoſed to the air, yet whilſt it circulates it is far from being ſolid: 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 deſireable to aſcertain what brings on 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 laid open to the air, to cold, and is at reſt; for whilſt in the body, air is excluded, it is always 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 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 keep 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, after being at reſt for three hours and a quarter, above two thirds of it was 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 baſon, and of that which is merely at reſt, are ſo different, that reſt alone cannot

not

not be ſuppoſed to be the cauſe of the blood's coagulation out of the body.

To ſee the effects of cold on the blood, I made this experiment.

Experiment V.

I killed a rabbit, , and cut out one of its jugular veins immediately, proper ligatures being previouſly made upon it; I then threw the vein into a ſolution of ſal ammoniac and ſnow, in which the mercury ſtood at the 14th degree of Fahrenheit's thermometer. As ſoon as the blood was frozen I took the vein out again, and put it into luke-warm water till it thawed 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 blew in warm air into the empty vein, and put another ligature upon it, and letting

ting 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 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 coagulated by reſt alone, the air was probably the cauſe of its coagulation.

From comparing theſe experiments, may we not now venture to conclude, that the air is a ſtrong coagulant of the blood, and that it is to this its coagulation when taken from the veins is chiefly owing, and not to cold or to reſt?

It may not be improper to obſerve here, that there are none of the above related 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 which I made, 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 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 Society'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 the veins, at the end of ten minutes, the blood was ſtill entirely fluid, or without any appearance of the coagulation's beginning *. If opened at the end of fifteen minutes, at firſt ſight it 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 part of the blood coagulated. 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 congeal, and then weighed, was found to weigh 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

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

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 coagulates nearly at the ſame period in the veins of the human body. 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 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 tis to this

cauſe

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 being at reſt, is probably owing its coagulation in the 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 flowing 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 we ſometimes obſerve to come from this cavity, and which, when they are more condenſed by the ouzing out of the *ſerum*, and of the red particles, 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

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

I have repeated ſeveral times, that I might be ſure of the fact. I have alſo varied the experiment a little, having ſometimes put the vein into a phial of water, and froze 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 put it into oil, and then froze it; but after all theſe trials, the reſult was found to be the ſame. The blood was always evidently fluid on being thawed, and as evidently jellied when expoſed to the air.

Thus far I have related ſuch experiments as I have made, in order to diſcover the cauſes of the coagulation of the blood, out of the body. Next, if agreeable to this learned Society, I ſhall lay before them ſome other experiments that I have made on this fluid.

On the Degree of Heat which coagulates the Lymph, and the Serum of the Blood; with an Enquiry into the Cauſes of the inflammatory Cruſt, or Size, *as it isc alled.*

Read Nov. 15, 1770. IN the preceding paper, beſides mentioning ſome circumſtances which promote the ſeparation of the blood, and which affect its colour, I have enquired into the cauſes to which its coagulation when taken from the veins is owing, and the manner in which it coagulates when at reſt in the body. I ſhall now proceed to lay before this learned Society, an account of ſome other experiments which I have made upon this fluid.

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, I think, been determined. It has been ſuppoſed to require a degree of heat almoſt equal to that which coagulates the *ſerum* *; but a much leſs is neceſſary, as will appear from the following experiments.

* Vide Traité du Cœur. T. ii. p. 93. Schwenk, Hæmatolog. p. 138.

Experiment VII.

Having found, from a number of trials, that blood, kept fluid by being mixed with 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 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 heat of 160°, as will appear hereafter.

Experiment VIII.

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

Experiment IX.

I next repeated the experiment, ſo that the heat was never higher than 114°, and was commonly at that

that degree during the eleven minutes, at the end of which time 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 it uſually does. Now as the blood, in the laſt experiment but one, was coagulated, when the heat had never riſen above 120° and an half; and in this experiment was fluid, though it had been expoſed 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 ½ of Fahrenheit's thermometer.

As to the degree of heat at which the lymph in human blood coagulates, I have not yet had an optunity of trying it in a more ſatisfactory way, than with the mixture with Glauber's ſalt, in which ſtate it coagulates at 125°. As we find that the human blood and that of a dog jelly nearly in the ſame time, when expoſed to the air, I think it is probable, that the preciſe degree of heat at which the lymph of the human blood coagulates, is between 114° and 120° ½. I have thought of making the experiment on the umbilical cord of a recent *placenta*, which 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 confounded with the lymph) coagulates, is generally ſaid to be 150°; but from the trials I have made, I am inclined to believe it requires a greater heat to fix it. My experiments were made in the following manner.

Ex-

Experiment X.

I took a wide-mouthed phial, containing *ſerum*, and placed a thermometer in it, and then 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 body 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 blood-veſſels; but there is little foundation for ſuch a ſuppoſition, ſince the animal heat is naturally only 98 or 100°, and in the moſt ardent fever is not raiſed above 112°.

I ſhall next proceed to enquire 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 it ſhould be attributed. But 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

boured 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 extended deeper and deeper, the blood being ſtill fluid. 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 obſerved 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 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 be no doubt that the cruſt was formed 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.

Ex-

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 uppermoſt part, the red particles ſubſiding. I then made a ligature upon one vein, ſo as to divide the tranſparent from the red part of the blood; and, opening the vein, I let out the tranſparent part, which was ſtill fluid, but coagulated ſoon after. On preſſing the *coagulum*, it was found to 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 coagulate. Nay, ſome have gone ſo far as to ſay, that in thoſe diſorders where the inflammatory cruſt is ſeen, the blood is almoſt coagulated

* This is not the only animal that ſeemed to be in health, 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 that die a violent death, though the blood remains longer fluid in ſuch caſes than it does in the baſon where a ſize appears.

even

even before it is let out of the vein. Now I am perſuaded from experiment, that the coutrary 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 it is commonly. To this opinion, I was firſt led by attending to the phthiſical patient's blood above-mentioned; 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 complaints, and no cruſt on their blood, I found that the blood, after being taken from a vein, 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 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 prove this.

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 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 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;

terwards; 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 was begun to be formed near the air-bubbles; in ſeven minutes a tranſparent ſize appeared over a conſiderable part of the ſurface which was quite fluid, whilſt 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 in thoſe caſes 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 was found not to coagulate completely in leſs than an hour and an half?

The effect that inflammation has in leſſening the lymph's diſpoſition to coagulate, is likewiſe plain from the following experiment, where the blood in the heart of a dead animal ſeems to have coagulated 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 teaſpoon, was found to coagulate 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 find it coagulates 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 pag. 14.

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 covered 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 where the cruſt appears. And again, the blood in the heart of an animal that dies a violent

lent 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 diſcover, whether that remarkable appearance, the inflammatory cruſt, could be owing to any other cauſe than to the coagulable lymph's being attenuated, and having its diſpoſition to coagulation leſſened: and as the ſame appearance might be ſuſpected to ariſe from an increaſed ſpecific gravity in the red particles, or from the *ſerum* alone being attenuated, I endeavoured to decide the queſtion by the following experiments.

Experiment XVII.

Into a phial, marked A, I put an ounce of the *ſerum* of the blood of a perſon, whoſe *craſſamentum* had an inflammatory cruſt.

Into another, marked B, I poured an ounce of the *ſerum* of a perſon whoſe blood 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 *buff*. In attending to them, I could not obſerve that the red particles ſubſided at all ſooner in the *ſerum* of the blood

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.

To ſee whether the ſpecific gravity of the red globules was increaſed, I tried 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 blood which had no cruſt. In viewing theſe, I could not obſerve that the globules of the blood which had an inflammatory cruſt ſubſided at all ſooner than thoſe of the blood which had none: thence I am inclined to conclude, that the ſpecific gravity of the red particles, or globules as they are called, is not increaſed 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 is probably intirely owing to 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 when the cruſt appears, they ſubſide from the ſurface of the whole maſs of blood in half that time; ſo that the

whol

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, now conclude, that in thoſe caſes where the inflammatory cruſt appears, the coagulable lymph becomes thinner, and its diſpoſition to coagulation is leſſened; both of which circumſtances contribute to the ſubſiding of the red globules before the ſurface of the blood is coagulated, and thence give riſe to this appearance, called the inflammatory cruſt or ſize *?

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 appearance occurs; and 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 is a truth: 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 inflamma-

* 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.

tion;

tion; it is often met with where there ſeems to
no ſuch diſeaſe, in particular in the blood of pr
nant women. And we may likewiſe obſerve w
Sydenham, that even in thoſe caſes where the bl
has a diſpoſition to form a white cruſt, yet i
trickle ſlowly from the vein, this *ſize* will not
pear; for in ſuch caſes, probably, the blood be
to coagulate before the whole has done flowing
that the agitation prevents the red particles fr
ſubſiding from the ſurface. There are therefore
veral circumſtances to be taken into the account,
fore we judge, from the preſence or want of
ſize, whether there is, or is not, an inflammation

The whitiſh cruſt differs much in denſity in
ferent caſes; in ſome it is extremely denſe, in oth
it is ſpongy or cellular, and contains a quantity
ſerum in its cells.

XXXIV. *Furt*

Further Remarks on the Properties of the Coagulable Lymph; on the ſtopping of Hæmorrhages; and on the Effects of Cold upon the Blood.

Read Nov. 22, and Dec. 6, 1770.

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 appearance. One might ſuppoſe that it was owing to a difference in ſome circumſtance in the operation, ſuch as in the velocity with which the blood flowed into each cup, or by the latter 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 have therefore ſuſpected 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

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 her pulſe bore 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 they were ſet down immediately when filled on the ſame window, yet there was no inflammatory cruſt on that in the cups, though a very denſe one on that in the baſon. And again, although that in the baſon was 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 her 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 operation; 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

rom his arm. This quantity was divided into our portions; the firſt was received in a cup, and vas in meaſure little more than an ounce; the econd, in a baſon, to the quantity of two ounces; he third, in a cup, which held one ounce; and the ourth, in a baſon, to the quantity of three ounces. Each veſſel was placed immediately upon the vindow; and it was obſerved that the blood in the irſt was lateſt in coagulating, and had a cruſt over he whole ſurface; that in the ſecond, had a cruſt nly upon a part of its ſurface; but that in the third nd fourth had no cruſt, and manifeſtly coagulated efore either of the other two.

From theſe experiments it is evident, that the roperties of the coagulable lymph can be ſoon hanged; ſince, in the ſpace of three or four mi- utes, the diſpoſition to coagulate was increaſed, and robably too the lymph was thickened. It might ndeed, at firſt ſight, ſeem poſſible, that blood-letting ad only let out the vitiated part; but this is highly nprobable; for, ſuppoſe a part only of the blood vas vitiated, that part muſt have been equally diffuſed hrough the whole maſs, and there is no likelihood f its getting out of the veſſels before the reſt of the lood; and conſequently it ought to have appeared in he laſt equally as in the firſt cup, but it did not. Bleed- ng, therefore, in thoſe caſes alters the nature of the lood, not by removing the vitiated part, and giving oom for new blood to be formed, as has been ſpected; but probably by changing that ſtate of he blood-veſſels on which the thinneſs, and leſſened endency of the lymph to coagulation, depends; vhich ſurely is a very curious circumſtance. This

fact is the more remarkable, ſince it ſhews the probability of the opinions of thoſe who main that this vitiated blood is the cauſe of the diſe ſince the diſeaſe remained, though the properties the blood were changed *.

From this obſervation we may be led to thi that it may be uſeful to receive the blood more f quently into ſmall cups, inſtead of a baſon, and attend more carefully to the alteration produc upon it by bleeding; as we may by that means p haps learn to determine better, what quantit ſhould be taken away in particular caſes. For would ſeem probable that the operation is likely have the moſt effect on the diſeaſe, in thoſe ca where the greateſt change is produced by its mea on the diſpoſition of the blood to coagulate; and that change, we can judge, by comparing the blo in the firſt cup, with that in the laſt; for the fi cup will nearly ſhew the ſtate of the blood at the b ginning; and the laſt cup the ſtate of the blood the latter part of the evacuation.

It frequently happens, that inſtead of an inflam matory cruſt over the whole ſurface of the *craſſamen tum*, there is only a partial one, which appears in larg ſpots or ſtreaks. In ſuch caſes I have obſerved, tha

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

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 related experiments.

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, it occurred to me, 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 an obſervation of Dr. Hunter's, which is "that the faintneſs which comes "on after hæmorrhages, inſtead of alarming the by-"ſtanders, and making them ſupport the patient by "ſtimulating medicines, as ſpirits of hartſhorn and "cordials, ſhould be looked upon as ſalutary; as it

"ſeems

" ſeems to be the method nature takes to give " the blood time to coagulate!" As this obſervation 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 two minutes in beginning to coagulate; and that the blood taken later, or as the animal became weaker, coagulated in leſs and leſs time; till at laſt, when the animal became very weak, the blood, though quite fluid as it came from the veſſels, yet had hardly been received into the cup before it congealed. I have alſo varied the experiment, by receiving blood into different cups at different times, whilſt 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 obſerved, on comparing the cups, that the blood which iſſued laſt coagulated firſt. I have obſerved likewiſe, that the blood coagulates with a different appearance in proportion as the animal becomes weaker; that which follows the knife begins to coagulate in about two minutes; it firſt forms a film or pellicle on the ſurface, which extends gradually through the whole

blood,

blood, yet ſo ſlowly that its progreſs may be obſerved, eſpecially if the pellicle be moved from time to time. But the blood that flows from the fainting animal is coagulated in an inſtant, after it once begins. From this obſervation, 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 rouze the patient by ſtimulating medicines, or 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, and promote 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

* 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 ruptured on their ſides and not entirely divided, makes me believe that contracting the bleeding orifice is not the only method nature takes to ſtop an

circumſtances that ſeem to concur in cloſing the bleeding orifices.

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 not improbable that in ſome caſes where the hæmorrhage is no 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 produced 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 is ſo commonly ſeen in inflammatory diſorders, and has ſo very morbid an appearance, 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 improperly: for, to ſay nothing of pregnancy, in

hæmorrhage. Her reſources indeed are great, and ſhe has often more methods than one of producing the ſame effect.

which

which the appearance is almoſt conſtant, there are few phyſicians that have not ſeen patients, who, even in ſuch circumſtances, have been 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 34, who, in leſs than twenty-four hours after bleeding, had ſymptoms of great weakneſs.

As it appears from the above related experiments, that the diſpoſition of the blood to coagulate is increaſed by bleeding, it may perhaps be uſeful to attend 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 in rheumatic, and likewiſe 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 attending to the laſt cup, if it contain 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 26, 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 coagulating than common. I ſuſpected what remained in the veſſels had the ſame

 diſpoſition;

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 very early, and therefore before the red particles had time to ſubſide, from that part of the lymph which had its diſpoſition to coagulation leſſened.

The learned profeſſor de Haen has taken notice of a curious appearance of the blood, which he could not account for; but which, I preſume, may be explained from ſome of the above experiments. His obſervation is, "That, having bled a perſon in a fever, "the blood was covered with an inflammatory cruſt, "and upon examining the *craſſamentum* in one of "the cups, it formed a ſort of ſack containing a "clear fluid: this fluid being let out, and the whole "covered and ſet by, on examining it next morning, "a very firm cruſt covered the whole again, and "extended to the bottom of the cup*." I once met with a caſe ſimilar to this; for, having bled a perſon into four cups at ten o'clock in the morning, on looking at the blood afterwards, at five in the afternoon, I found the *ſerum* had not ſeparated from the *craſſamentum* in the firſt cup; but the

* Vide Rat. Medendi, cap. vi.

craſſa-

craſſamentum felt as if it contained a fluid in a bag, as profeſſor de Haen has deſcribed it. Upon preſſing it, the fluid guſhed out, which, in a few minutes after being expoſed to the air, coagulated: there was however this difference in the two caſes, that in mine the fluid was red, ſo that it formed a red cruſt over the firſt, which was white. Now this ſeems to have been owing to the blood's having firſt coagulated, where it was in contact with the air and with the ſides of the cup; and the fluid which guſhed out was the *ſerum*, with a part of the coagulable lymph, which yet remained fluid; but, when expoſed to the air, jellied or coagulated, as it naturally does. That one part of the lymph can remain fluid after the other is coagulated, is proved by ſome of the preceding experiments; and I have more than once ſeen blood, which appeared perfectly jellied ſoon after bleeding; yet, on cutting into the *coagulum*, a tranſparent fluid has ouzed out, which afterwards jellied. And ſo ſlowly does this coagulation proceed in ſome caſes, 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 likewiſe diſtinctly obſerved, that in ſome caſes where the diſpoſition to coagulate was much leſſened during the evacuation, the blood at the bottom of the cup has jellied, whilſt the greateſt part of the *ſize* at 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 obſerved in ſome experiments, where I had occaſion

to throw the blood into water, and oil, during the winter ſeaſon whilſt the heat of the water and oil was no higher 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 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 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.

Experi-

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 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, on being let out into a cup, was found to be 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 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 vien 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 in great measure settled to one side.

The

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 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 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 vein. The cup into which it was received was placed upon the window of a moderately warm room, and was examined carefully from time to time; but the blood never had any appearance of coagulation, but remained fluid till it was dried by the evaporation of its water, which happened by the next day. In this experiment the cold ſeemed intirely to have prevented the coagulation of the lymph: ſo ill-founded is the common opinion that cold coagulates the blood.

As the lymph can be deprived of its power of coagulating, on being expoſed to the air, by ſo ſlight a circumſtance, as it would ſeem, as the cooling is ſuddenly; it is the leſs to be wondered at, that ſuch a change ſhould ſometimes take place in the blood

veſſel

veſſels of a living body; an inſtance of which, I have been told, was obſerved by the learned profeſſor Cullen, who having ordered an epileptic patient to be bled, the blood was found not to coagulate; but, on bleeding him the day following, the blood coagulated, as uſual. A ſimilar inſtance, I ſaw lately, by the favour of the medical gentlemen of the Britiſh Lying-in-Hoſpital, who, having bled a woman in a fever that came on ſoon after delivery, the blood was found not to coagulate on being expoſed to the air, but appeared like a mixture of the red particles and ſerum only; the particles having ſubſided to the bottom, in the form of a powder. She died three days afterwards; and, upon opening her, we found the blood had coagulated in her veſſels after death; and that a denſe white *polypus* was formed in each auricle of her heart; one of which, I have now by me. I examined the blood taken away before ſhe died, and found on expoſing it to heat, that it did not coagulate ſooner than *ſerum* commonly does, or under 160°: ſo that it is probable, that at the time ſhe was bled, her blood either was without the coagulable lymph, or that its properties were changed.

After a blow or contuſion, the blood now and then burſts from the blood-veſſels into the cellular membrane, ſometimes forming an *ecchymoſis*, and ſometimes a tumor; and it is a queſtion with ſome, whether that blood coagulates or not; but that it coagulates in moſt of theſe caſes, is proved by the opening of ſuch tumors. Yet it has likewiſe been obſerved, that now and then theſe tumors have been attended with a fluctuation, and after ſome time their contents have been abſorbed: and it has alſo been found upon opening

opening ſome of them, even ſeveral weeks after the accident, that 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.

THE END.