Some observations relating to the powers of circulation and the state of the vessels in an inflamed part / [Alexander Philips Wilson Philip].

SOME OBSERVATIONS

RELATING TO THE

POWERS OF CIRCULATION

AND THE

STATE OF THE VESSELS

IN

AN INFLAMED PART.

By A. P. W. PHILIP, M.D., F.R.S. Ed:, &c.

COMMUNICATED

By Mr. EARLE.

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Read June 17, 1823.

I AM induced to trouble the Society with the following observations relating to the powers of circulation, because opinions respecting them, which appear to be equally inconsistent with our knowledge of the animal economy and the common laws of mechanics, have lately made a considerable impression on some of the medical men of this country. These opinions appear to have been originally suggested by a love of novelty, and to have become current with many for no other reason than that nobody has been at the trouble to exa-

mine the grounds on which they rest. This subject naturally leads to the much-disputed question of the nature of inflammation, and disputed it must ever remain while our opinions respecting the powers of circulation are so vague as they at present appear to be. The reader will judge from the facts which will be laid before him, how far our difference of opinion arises from the real deficiency of our knowledge, or a careless application of it. Such expressions as, that the subject is very obscure, that our knowledge of it is very imperfect, &c., are often employed to save us the trouble of enquiring what the state of our knowledge actually is.

Much has lately been said of the dilating power of the heart, and what has been called the resilience of the lungs. Those powers are not imaginary. Very simple experiments prove their existence; and we are indebted to Dr. Carson for having particularly called our attention to them: but I believe it will be with some surprise that every one will, for the first time, examine the foundation of the inferences drawn from them, when it is considered by what men these inferences have been sanctioned.

An experiment made many years ago by Dr. James Johnson, demonstrates, in a very satisfactory way, the dilating power of the heart. He immersed in water the heart of a turtlenewly killed, and could, from the tinge given by the blood, ob-

serve it continually expelling, in the systole, the water it had drawn in during the diastole. But if we grasp the heart either of a warm or cold blooded animal, we find that, during the diastole, it offers little if any greater resistance to the hand than other muscles in a state of relaxation do. In short, it is quite soft and compressible, till the systole recurs: from which, it is evident, that its dilating power is very small. It is also to be observed that the dilating power seems chiefly to reside in the ventricle, the auricle being much thinner, and more inclined to collapse. Nor will the dilating power of the heart be much aided by what Dr. Carson calls the resilient power of the lungs. This power cannot operate in many of the lower classes of animals, or in the fœtal state in any class; and in the perfect animal of the higher classes, its effect is very inconsiderable. It appears, from an experiment related in the 111th page of Dr. Carson's Treatise, that, even in the bullock, it is not equal to the pressure of a column of water of ten inches. It does not indeed appear, from the experiments of Dr. Carson, how much the effect he observed depends on what he calls the resilience, or merely on the weight of the lungs. It is evident that, as far as this power goes, whether it depends on the weight or resilience of the lungs, it tends to dilate the heart; the atmosphere, through the medium of the blood, pressing as much on the internal surfaces of theheart as on the external surface of the chest.

But, admitting that the dilating power of the heart, and tendency of the lungs to collapse, are sufficient in degree to support the motion of the blood in the veins, another insurmountable and surely most evident objection to the hypothesis, presents itself. What is the effect of an exhausting syringe adapted to a tube filled with a fluid? If the fluid must rise before it can enter the syringe, after the first portion has entered it, either the fluid continues to rise, or the sides of the tube begin to collapse. If the sides of the tube are firm enough to resist a pressure equal to that of the column of fluid to be raised, it rises; if not, the tube collapses. Could the sides of the veins resist a pressure equal to that of the column of fluid to be raised, even if they were not constantly pressed upon by the surrounding parts? Nay, so small is their elastic power, that they collapse by their own weight as soon as emptied. Yet it is through these tubes that a column of blood of many feet is to be raised by suction! Besides, the elastic power of the ventricle can have no operation on the veins, the auricle being interposed between them, and contracting during the diastole of the ventricle.

What purpose then is served by the dilating power of the ventricle increased by the tendency of the lungs to collapse? It favours the entrance of the blood suddenly propelled into it by the contraction of the auricle; and the degree of dilating power is well proportioned to this office. Without

this dilating power, the tendency of the ventricle would be to remain in a state of collapse after the systole, and part of the power of the auricle would be expended in dilating the ventricle. Here, as in many other instances, both in man and the inferior animals, we see nature saving the muscular, by the substitution of the elastic power. The latter is the simplest, and its action tends little to exhaust it. In like manner the arteries are rendered elastic, to favour the ingress of the blood on the contraction of the ventricle. It is evident that a greater power must have been bestowed on the ventricle had the arteries been wholly inelastic. Their elasticity by resisting the pressure of the surrounding parts, and thus tending to preserve a uniformity of caliber, facilitates both the ingress and passage of the blood suddenly thrown into them. Had the blood entered by a continued stream, and been carried on merely by the power of the vessels themselves, this elasticity would not only have been of little use, but evidently injurious, as far as it tended to impede the muscular, or by whatever other name we choose to call that power by which the blood is propelled by the vessels. Thus it is that little elastic power is bestowed on the veins, which we shall find are unassisted in the propulsion of their contents.

But as wonderful ~~powers~~ effects have been ascribed to the muscular, as to the elastic power of the ventricle, it has been seriously maintained, and that by authors of respectability, that the circulation is supported by the muscular power of this organ alone.

Have those who maintain this position made even the rudest calculation of the degree of resistance to be overcome in driving the blood through two capillary systems* at such a rate, that, in any given time, the same quantity shall be delivered by the veins, which is thrown into the arteries? Have they made any estimate of the strength necessary in the different sets of vessels, and, particularly, in the larger arteries, to sustain a power capable of overcoming this resistance? Let them give to the heart what imaginary power they will, they cannot make this power greater than the coats of the vessels will bear without rupture †.

Let us turn from hypothesis to simple matter of fact. If the motion of the blood be maintained by the power of the heart alone, it will, of course, cease when this power is destroyed.

A ligature was thrown round the vessels attached to the heart of a frog, and the heart was then cut out. On bringing the web of one of the hind legs before the microscope, I found the circulation in it vigorous. I could not distinguish it from that in the

* It is to be recollected that, in an important part of the body, the blood is, for the second time, distributed through the capillaries, before it returns to the heart.

† Much has been said of the incompressible nature of the blood, and the consequent effect of each additional quantity thrown into the arteries; but were the blood absolutely incompressible, which we now know not to be the case, what has been said on this head could only be correct on the supposition of the vessels also being absolutely unyielding.

web of a healthy frog. It continued in this state for many minutes, and, at length, gradually became more languid *.

A rabbit, about two months old, was killed by a blow on the occiput. The chest was then laid open, and a ligature thrown round the aorta. Part of the mesentery was now brought before the microscope, and both the gentleman who assisted me, Mr. Sheppard, and myself, saw the blood moving in it with great velocity. I had many times, with the assistance of the microscope, seen the circulation in the healthy rabbit, and could not perceive that the loss of the power of the heart at all affected it for the space of many minutes †.

It appears from these experiments, that the motion of the blood in the capillaries, that is, those vessels which are too small to be distinguished by the naked eye, has no direct dependance on the action of the heart. Does it depend on a power remaining in the larger vessels, or on the power of the capillaries themselves? This point also may easily be determined by direct experiment. If it depend on the former, it will be uninfluenced by stimulants and sedatives acting on the capillaries; if on the latter, the velocity of the blood will be greater or less according as they are more or less excited.

* Experimental Inquiry, 2d Edit., p. 91.

† Experimental Inquiry, 2d Edit., pp. 194 and 195.

I found from many experiments to which I shall again have occasion to refer that the velocity of the blood in the capillaries is immediately influenced by the state of these vessels. When they were stimulated by the concentrated rays of the sun, the application of spirits of wine, or gentle friction, the velocity of the blood in them was immediately, and, by the two first, to a great degree increased *. When the power of the capillaries was destroyed, even in the perfect animal, by the direct application of an infusion of opium or tobacco to them, the motion of the blood through them immediately ceased †, so little influence has the action of the heart and larger arteries on the circulation in these vessels: and in many experiments, where the motion of the blood in the capillaries had become very languid, it was accelerated, and even renewed when it had ceased, by stimuli applied to them ‡.

Thus, it appears, that the propulsion of the blood through the capillaries, is the function of these vessels themselves, and has no further dependance on the heart, than that a due supply of blood to them cannot long be obtained after the action of

* Introduction to my Treatise on Symptomatic Fevers, 4th Edit., pp. 15 and 16.

† Experimental Inquiry, 2d Edit., p. 133.

‡ Introduction to a Treatise on Symptomatic Fevers, 4th Edit., pp. 15 and 16. The motion of the blood in the capillaries is quite uniform. I could never observe that the contractions of the heart in the least degree influenced it.

this organ has ceased. The motion of the blood in the capillaries does not wholly cease, in internal parts, for several hours after the power of the heart is destroyed*. It arises from this cause, that the larger arteries of animals which have been dead for some time, are found empty. That the continued action of the capillaries must readily empty them, will be evident, when we recollect how much the sum of the areas of the branches of an artery, exceeds the area of its trunk †.

The effect of the larger vessels in supporting the motion of the blood, cannot be so easily demonstrated as that of the capillaries; but the reader will find, from many experiments, detailed in various works, and particularly in the introduction to Dr. Hastings's work on the Inflammation of the Mucus Membrane of the Lungs, that they possess a contractile power, which obeys both chemical and mechanical stimulants, and for what purpose they are endowed with such power, if not for the propulsion of the blood, it would be difficult to understand. Whatever may be said of the larger arteries, as the motion of the blood in the capillaries is independent of the power of the heart, that in the larger

* Experimental Inquiry, 2d Edit., p. 196.

† The instantaneous destruction of the nervous power immediately destroys the power of the capillaries (Exper. Inq. 2d. Ed. Exp. 28 and 29). The arteries are not found empty after death by lightning, although their elasticity is unimpaired. See a paper on the vacuity of the arteries after death by Dr. Fennel in the Philadelphia Journal, No. 2.

veins must either depend on their own power, or on the impulse given to the blood by the capillaries, a position which nobody will maintain. If then the blood is carried on in the capillaries and larger veins by the power of these vessels themselves, and we find, from direct experiment, that the arteries possess a similar power, we can hardly conceive that this power is not employed for the same purpose, in aid of the impulse given by the heart. It is surely more consistent with every thing we know of the animal economy, to suppose that the vessels should assist in the propulsion of their contents, than that these should be driven through them as through inanimate tubes.

Whether the power of the vessels be a muscular power or not, a question of subordinate consequence in a pathological point of view, must chiefly rest on analogy; but there can hardly be a stronger analogy than exists in favour of this opinion, to say nothing of the fibrous appearance observed in some of the larger vessels. The power of the capillaries obeys the same laws with that of the heart. The capillaries and the heart are excited by the same stimulant, and for the same purpose—the propulsion of the blood. These vessels are affected in the same way as the heart, by other agents, directly applied to them, whether stimulant or sedative*. They bear the same relation to the nervous system, their function being like that of the heart

* Treatise on Symptomatic Fevers, 4th Edit., pp. 15 and 16, and Experimental Inquiry, 2d Edit., p. 133.

independent of that system*, but capable of being increased or diminished, or even destroyed through it†.

The direct influence of the nervous system on the capillaries, explains many of the phenomena both of health and disease. Hence the flushing and paleness of particular parts, especially of the face, produced by affections of the mind, which cannot depend on the general state of the sanguiferous system. Hence the influence of different states of the nervous system on the various secreting organs, &c.

There is no disease in which the influence of the nervous system on the capillary vessels is so striking as in inflammation; we see it made to recede from one part, and attack another, and modified in all its stages by causes whose operation is confined to the nervous system alone.

I shall employ the remaining part of this paper in an attempt to ascertain how far a review of the facts we possess enables us to advance in explaining the phenomena of this disease; which, both from the nature of the disease itself, and its intimate connection with nine-tenths of all the diseases we are subject to, may be regarded as the most important object of pathological research: and yet it is that on which, as far as I am capable of judging,

* Experimental Inquiry, 2d Edit., p. 77.

† *Ibid*, p. 92, *et seq.*

there has been a greater display of fallacious reasoning, and a greater disregard of facts, than on any other subject with which I am acquainted.

There is no difficulty, with the aid of the microscope, in perceiving the first step towards a state of inflammation. It is well known, that exposure to the air alone, is sufficient to produce inflammation in the internal membranes of warm-blooded animals. This is also the case in the fin of some kinds of fish. The lampern was the fish I employed, and in the warm-blooded animal I employed the mesentery of the rabbit.

On bringing either of these membranes before the microscope, we see a network of vessels, many capable of transmitting the globules of blood only one by one where they follow each other in rapid succession. After the part has remained exposed to the air for some time, the globules begin to move through these vessels with less rapidity, and in proportion as this happens, we perceive the diameter of the vessels enlarging, till that which could admit of only one globule now admits of several. As the motion of the globules languishes, and their number increases, their colour becomes conspicuous, which it is not while they pass in smaller number and with greater rapidity. At the same time that these changes take place we find the number of vessels, capable of transmitting red globules, greatly increased, so that the vessels which, in the healthy state, transmitted only the colourless, are now so

much distended as to admit the grosser parts of the blood. From these two causes the part assumes a redder appearance than natural, and also acquires a greater bulk; and the latter seems further increased by the distension of vessels still too small to transmit the red globules; for the interstices of the red vessels are now more opake than before the morbid distension took place, without the appearance of extravasation of any kind.

While these changes, which may be distinctly seen with the assistance of the microscope, are going on, the part to the naked eye becomes inflamed, more opake, and thicker.

Such then are the changes which take place in the commencement and progress of inflammation. The blood in the capillaries begins to move more slowly; these vessels in the same proportion suffering a degree of morbid distension: and this often goes on till they, by many times, exceed the healthy size, and the blood in the most distended vessels ceases to move altogether.

The motion of the blood in the capillaries we have just seen proved, by direct experiment, to depend on the action of these vessels themselves. When it fails therefore we necessarily infer that their action is failing in the same proportion; and this inference is confirmed by their suffering themselves to be morbidly distended by the *vis a tergo*, an effect which equally proves their loss of power.

It signifies not by what means the power of the capillaries is impaired*, whether by mechanical or chemical injury, whether by a cause operating slowly or suddenly. Any cause impairing their power produces the same effects.

During the foregoing changes, the larger vessels of the part, which are too opake to permit the motion of the blood to be seen in them, suffer no change that can be detected by the microscope, except that, after the distension of the capillaries has become very great, the vessels immediately preceding them in the course of circulation begin to partake of the distension. Thus when the fins of the lampern were first exposed to the air, the inflammation assumed the appearance of a slight blush, in which it was difficult, with the naked eye, to discover any vessels; but, after some time, vessels of a considerable size were seen creeping through the inflamed parts. Before this change is observed in the larger vessels, the capillaries are distended to many times their natural size, and the blood in those most distended, has, generally, ceased to move. This, it is evident, cannot go very far, without the latter vessels wholly losing their vitality, and gangrene ensuing.

The state of the larger vessels of an inflamed part, with the exception just mentioned, is very different from that of the capillaries, and may be as-

* Both in Dr. Hastings's and my own experiments, the means employed were various, but the effects always the same.

certained without the aid of the microscope. The increased pulsation of the larger arteries supplying an inflamed part, sufficiently evinces their increased action: nor is there any difficulty in detecting this increased action. I have often, in inflammatory affections of the jaws, applied the finger to the external maxillary artery, both where it passes over the bone, and after it assumes the name of labialis, and, in rheumatic affections of the head, to the temporal arteries, and perceived them beating with unusual force. On this increased action of the larger arteries of an inflamed part, the throbbing and general appearance of activity in the part depends, and on it is founded the popular opinion that inflammation consists in an increased action of all the vessels of the inflamed part, an opinion adopted without a moment's reflection on what must necessarily be the consequence of such an increased action. We shall, in the conclusion of this paper, see this generally increased action, and its consequences, exhibited by a very simple experiment. The difference between what is called active and passive inflammation, depends on the degree in which the arteries supplying the *vis a tergo* to the debilitated vessels, are excited.

We should, at first view, be inclined to ascribe the increased action of the larger arteries to the impediment opposed to the free transmission of the blood through the debilitated capillaries; but the following facts point out that it depends little, if at

all, on this cause. The anastomoses of the vessels are so numerous and free, that, as we shall presently see determined by direct experiment, if the passage of the blood is opposed through one channel, it immediately finds another, without occasioning any apparent change in the state of the vessels concerned. The degree in which the larger vessels are excited is rather proportioned to the nervous irritation occasioned by the state of the distended capillaries, than to the degree and extent of the inflammation, for a slight internal inflammation excites the whole sanguiferous system, while a more severe external one has little of this effect; and in habitual inflammation, when the vessels have yielded slowly, and, consequently, without much nervous irritation, there is comparatively little increased excitement of the larger vessels of the part, and often, even in internal parts, none at all of the whole system. From these observations it would appear, that it is to the nervous irritation occasioned by the morbid distension of the capillaries, that we are to ascribe the increased action of the larger arteries of the part. We have just seen how much the action of the vessels is under the influence of the nervous system. The final cause of this increased action is evidently to support the circulation in the debilitated vessels, and excite them to a more vigorous action*.

* See the Introduction to the above-mentioned Treatise on Symptomatic Fevers, 4th Edit., pp. 24 and 25.

If the inflammation depend on a debilitated state of the capillaries, it follows, that whatever increases the action of these vessels, should relieve the inflammatory symptoms. This may be regarded as an *experimentum crucis* on the subject, for if exciting the capillaries of an inflamed part does not relieve the symptoms, whatever share the debility of these vessels may have in producing the disease, the co-operation of some other cause must be necessary. If, on the contrary, we find that as, on the one hand, whatever debilitates the capillaries, produces inflammation, so, on the other, whatever gives greater activity to them, relieves it, nothing more is required to prove that on their inactivity the disease depends.

I wetted the inflamed web of a frog's foot with distilled spirits, at the same time throwing upon it the concentrated rays of the sun, from the reflector of the microscope. The blood in all the vessels, except in those of the most inflamed part, began to move with greater velocity, and, in proportion as this happened, their diameters were diminished, their interstices became less opaque, and the redness of the part was lessened. This experiment was repeated on the lampern, with the same result. By gentle friction, and applying distilled spirits, the motion of the blood in the inflamed part was repeatedly accelerated, and in proportion as this happened the vessels became paler, the deeper red returning as the circulation again became more languid.

Dr. Hastings, in like manner, excited the inflamed capillaries in a frog's foot, by oil of turpentine, and observed the inflammatory symptoms abate in proportion as the capillary vessels lost their increased size, and the motion of the blood was accelerated in them; and in one instance, of which he gives an account in the 90th page, this process was continued till the inflammation subsided. Excessive heat and cold, in Dr. Hastings's experiments, produced languid motion of the blood, and dilatation of the capillary vessels, exactly in the same proportion as the part became inflamed. When the inflammation was caused by cold, he saw it cured by a moderate and continued application of heat, by which the motion of the blood in these vessels was accelerated, and they were made to resume their natural dimensions. When the inflammation arose from the excessive application of heat, cold produced the same effects. These facts, while they, in a striking manner, confirm the result of the experiments just related, illustrate positions which I endeavoured to establish in a paper which the editor of the Annals of Philosophy did me the honour to publish in the XIIth volume of that work, that cold, although only the absence of heat, is as positive an agent, with respect to the animal body, as heat itself; and that both cold and heat, the temperature of that body being the mean, like all other agents, act on it, either as a stimulant or sedative, according to the degree in which they are applied, in a certain degree all acting as a sti-

mulant, in a greater degree as a sedative; the difference between what is called a stimulant and sedative consisting, in the former, distilled spirits or heat for example, being more inclined to act as a stimulant, and the latter, tobacco and cold for example, as a sedative: but there is a quantity of tobacco, and a degree of cold, so small as to act as a stimulant, and a quantity of distilled spirits and a degree of heat, so great as to act as a sedative.

It is evident that the blood cannot be long retained in the debilitated capillaries, and thus, as it were, thrown out of the circulation, without some morbid changes taking place in it. Its vitality must soon cease after its motion is wholly suspended, and the changes, to which dead blood is liable, begin to take place in it. Dr. Hastings observed, that when the debilitated capillaries were stimulated, the blood which passed from them, often contained irregular flocculi, instead of globules, which he compares to the ragged portions separated from the coagulum of arterial blood*.

For the manner in which the various symptoms of inflammation, and means of cure, support the view of the disease, afforded by these experiments, the reader is referred to the Introduction to the Treatise on Symptomatic Fevers above mentioned.

Nothing can be more simple than the *modus*

* Dr. Hastings' Treatise on Inflammation of the Mucus Membrane of the Lungs, p. 97.

operandi of the means of cure in inflammation according to that view of it. All the local measures are such as either relieve the vessels from part of the fluid which distends them beyond their natural capacity, or more directly excite them to a more vigorous action. All the general means are such as influence the *vis a tergo*, either reducing it where it is so powerful as still further to distend the debilitated vessels, or increasing it, when it becomes too languid to afford the aid necessary for supporting some motion of the blood in these vessels, and thus preventing gangrene, the effect of its total stoppage.

It appeared to me that it would tend to throw additional light on what has been said, to subject to the test of direct experiment the principal opinions which prevailed respecting the nature of inflammation previous to that which referred it to a debility of the capillary vessels*. Four only deserve attention: the opinion which supposes this disease to arise from a morbid lentor of the blood clogging the minute vessels; that which ascribes it to what has been termed *error loci*, the grosser parts of the blood getting into vessels too small to transmit them; that which supposes a spasm of the extreme vessels to be its cause; and, lastly, that which refers it to a morbidly increased action of the vessels of the inflamed part.

* For the origin of this opinion, see Dr. Hastings's work, and my Treatise on Symptomatic Fevers.

The reader will readily perceive that the principle of the three first doctrines is the same. In all, obstruction in some of the minute vessels is regarded as the cause of inflammation. It is surprising, therefore, that none of the supporters of these opinions thought of trying whether or not obstruction is capable of producing it. Admitting that the vessels are obstructed, it does not follow that an accumulation of blood will take place in the part. The blood may pass off by anastomosing branches, or the vessels may resist the distending force.

A hot wire was passed through the web of a frog's foot, by which the skin about the hole was shrivelled, and the vessels obstructed, no fluid of any kind being discharged. Here an obstruction was produced surely more than equal to what takes place in many inflammations of small extent, and yet no symptom of inflammation ensued, every part of the web remaining as pale as before the experiment *.

In order to ascertain whether inflammation arises from an increased action of the vessels of the part, it is only necessary to induce such an action, and observe its effects. Having brought the web of a frog's foot before the microscope, I now and then, during some minutes, observed the velocity of the blood, which continued, as far as I could judge, the

* Introduction to a Treatise on Symptomatic Fevers.

same. The foot was then wetted with distilled spirits, and, in a few seconds, the blood in all the vessels was moved with a greatly increased velocity, which, as the web was constantly kept wet with the distilled spirits, continued as long as I observed it, ten minutes or a quarter of an hour. But during no part of the time could I perceive the slightest symptom of inflammation, either with or without the microscope. The vessels, instead of appearing redder, and more turgid, were evidently paler and smaller than before the application of the distilled spirits. The velocity of the circulation was further increased by throwing on the web, the concentrated rays of the sun, from the reflector of the microscope, but still with the same effects*.

The result of these experiments has since been confirmed by many made by Dr. Hastings, of which an account is given in a work to which I have several times had occasion to refer.

* Introduction to a Treatise on Symptomatic Fevers.

THE END.

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