and scaly diseases (eczema, lepra, and psoriasis), especially when occurring in cachectic states of the system. By those engaged in the country as parochial surgeons, who have numerous cases of chronic skin diseases in unhealthy children, and who have not already employed this remedy, it is worth a trial, from the facility with which it can be procured by the poor.

It is hardly necessary to add that, if the tongue is loaded, it is advisable to commence with mild mercurials and aperients, before the urtica is used; and that, in all cases, a plain wholesome diet, and frequent washing of the whole skin, are to be recommended. Indeed, to prevent the return of skin diseases which have been of long standing, it is of primary importance that the whole surface of the body should be washed daily with soap and water; for, as eruptions are often the means which nature sets up for relieving a faulty state of the system through the skin, it is rational to suppose that a free and perspirable condition of that extensive surface will assist nature in removing her waste matter, and thus will prevent the necessity of this scaly, or papular, or vesicular action. What the older practitioners attempted by means of issues may be more naturally effected by the daily use of soap and water to the whole cutaneous surface. In one case, this practice alone, steadily persevered in for several months, and continued as a habit, permanently cured psoriasis palmaria of more than twenty years' duration.

Washing the whole surface daily with soap and water will be found to be a very serviceable recommendation, not as a mere matter of cleanliness, but as one of the directions for the general treatment of many chronic diseases, and for the preservation of health in advancing life. Its effect is very different to that of merely washing the body with cold water, or using shower-baths, or bathing followed by dry friction. It does not give the same shock as cold water alone; and it is thus much more easily borne by the delicate, who find that the reaction after cold sponging is exhausting. It more effectually removes the waste epidermis than flesh-brushes or hair-gloves; and suits those whose delicate skins are often irritated into eruptions by these rough appliances. For those who are very subject to colds, and are yet too delicate to bear cold sponging, the addition of soap, by its stimulating property, acts as a tonic to the skin, and thus guards it against the effects of sudden changes of temperature, besides aiding in getting rid of the waste and superfluous matters, which often require the actions which constitute a cold for their discharge from the overloaded system. The yellow soap, from the resin it contains, is an excellent stimulant as well as detergent; and a better still, in many cases, is a soap containing a small quantity of Barbadoes tar, called Hendrie's petrole soap, the daily use of which is peculiarly serviceable in keeping up a smooth and perspirable state of the skin when it is naturally dry, and especially in declining life, when the loss of muscular power prevents exercise carried to perspiration; for, as man is organised to earn his bread by the sweat of his brow, if he cannot do this, he must use some artificial means to produce the same state of his perspiring organ, or he will suffer. A physician, distinguished in his day in a neighbouring county, who now, though verging on 90, retains a freshness and vigour of intellect, as well as a sound condition of health, which enable him to enjoy his existence physically and socially, and to devote a portion of each day to the cultivation of science, informed me that he attributed much of his health and activity to this daily soaping of his skin, which he had persevered in since he was a very young man. And the elderly, who usually have much spare time at their disposal, will find that the employment of a portion of it daily in this way will repay them for the pains. In the management of the disorders of the general health which so often attend the cessation of menstruation, this practice is, for obvious reasons, to be recommended.

Southampton, November 1864.

ON THE COMMUNICATIONS BETWEEN THE LYMPHATIC SYSTEM AND THE VEINS:

PROVISIONAL REPORT FROM THE LONDON COMMITTEE OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, APPOINTED TO MAKE AN EXPERIMENTAL INQUIRY INTO THIS SUBJECT.

By THOMAS HODGKIN, M.D.

[Read to the Medical Section of the British Association, at its Meeting at Bristol, 1836.]

[In physiology, as in medicine, some subjects may be said for a time to be fashionable, or at least to excite a temporary interest, and then to be lost sight of or become obsolete. This is somewhat the case with the function of absorption; nevertheless, I have thought that the following unpublished paper, which was written for the Medical Section of the British Association for the Advancement of Science—a section which the formation of the PROVINCIAL MEDICAL ASSOCIATION has superseded—might not be unworthy of a place in the JOURNAL, in order that some of the facts which it records may be preserved. T. H.]

THE Medical Section of the British Association for the Promotion of Science was induced at its meeting in Edinburgh to name a commission to make an experimental inquiry into the mutual relation existing between the venous, lymphatic, and lacteal systems; but the principal object of inquiry was stated to be to settle the question as to the existence or absence of the numerous communications between the two systems, occasioned by the lacteals and lymphatics in different parts of the body emptying themselves into the veins by a shorter course than those of the thoracic duct and right trunk; the affirmative of this having been stated by Fohmann and Louth, and more strongly insisted upon by Lippi; and the negative being affirmed with equal confidence by Panizza and other able anatomists. To facilitate the operations of the commission, it was composed of two sections; the one meeting in Edinburgh, the other in London. Amongst the Edinburgh Section were two gentlemen justly esteemed as able practical anatomists and physiologists, Drs. Sharpey and Allen Thomson, who had already been engaged in this inquiry, and have not failed to continue the research, in compliance with the wishes of the Association. I am uncertain whether any report was made by them to the meeting in Dublin; but their views at least have been given to the public in an excellent paper from the pen of one of these gentlemen, Allen Thomson, which may be found in No. cxxv of the *Edinburgh and Medical and Surgical Journal*.

With respect to the London Section, which consisted of Dr. Roget, Dr. Clark of Cambridge, W. Clift, Bracy Clark, Richard Owen, and myself, I regret to say that, though two years have nearly passed, we have done very little towards the completion of the task assigned to us. This must be in part ascribed to the difficulty which we have found in combining for the purpose of making researches requiring so much continued and undivided attention, and so much practical skill in manipulation, in conjunction with favourable opportunities neither to be commanded nor anticipated.

As we have not been altogether inactive, we have thought it right to make a provisional report of progress, and at the same time to request permission to act for another year. I have therefore drawn up the following communication.

It soon appeared to us that the Anatomical School at Guy's Hospital was likely to furnish the greatest facilities for the practical part of this inquiry; and, as a further assistance, we have associated with us Francis Sibson, jun. (now Dr. Sibson) and (the late) Thomas King, both justly esteemed as able practical anatomists and zealous physiological inquirers. It is to their assistance that I am indebted for the examination by means of mercurial injection of several subjects, in whom the size and visibility of the lymphatic system promised facilities for the inquiry. To

the co-operation of the latter gentleman, I am also indebted for the results which we obtained in inferior animals.

Those who are at all practically acquainted with the minute injection of the lymphatic vessels, must be fully aware that it is a process requiring not only great delicacy of touch, but also a good eye, familiarised with the appearance of the most minute arteries, veins, and absorbents. The importance of this fact made me unwilling to trust to our own hands and eyes only; I therefore wrote to Professor Lippi, whose results it has been the most difficult to verify, and requested that he would favour us with a few wet preparations, capable of affording ocular demonstration of the existence of those communications which he has described. With this request he has obligingly promised to comply; and I have been hoping to be able to place before you the conclusive evidence of his injections. In this, however, I am disappointed, having at present only received his reply to the observations of Panizza. I have likewise endeavoured to learn from Professors Fohmann and Louth whether the researches of Panizza are regarded by them as sufficiently conclusive to induce them to abandon or modify their former opinions; but on this point I am still in expectation. I shall therefore proceed to state the facts which we have as yet been able to observe.

In injecting the inferent lacteals in the mesentery, the gentlemen who have assisted me have repeatedly found the metal pass as readily into the veins as into the efferent lacteals. In this respect, our experience perfectly coincides with that of most others who have examined this part of the system. Both Lippi and Panizza are agreed on this point. The elder Meckel pointed out this communication long ago. Lindner confirmed his observation. Fohmann not only observed the same thing, but found the mercury pass so readily into the veins proceeding from some absorbent glands, that he was unable to make it enter the efferent vessels, or even to perceive the existence of such vessels; by which he was temporarily led into the error of believing that in these instances the veins were the sole canals for carrying off the chyle or lymph. Louth and Breschet, Müller, and Drs. Sharpey and Allen Thomson, may also be signalled amongst the practised and trustworthy investigators of this system, who have confirmed the fact of mercury injected by the inferent lacteals passing off by the veins. These investigators are not, however, agreed as to the mode in which in these instances the mercury has passed from one system to the other. Drs. Sharpey and Thomson inclined to the explanation long since adopted by Hewson, Haller, and Cruikshank, that the communication is to be attributed to the rupture of the vessels; while Panizza, Louth, and Breschet, as well as Lippi, are disposed to admit the existence of a natural communication in these structures. It has likewise been supposed that, without the existence of any direct communication by inoculation, distinct apertures, or connecting vessels, the injection may have passed from one set to the other by a sort of transudation through the sides of the vessels generally. Until we can succeed in continuing our researches with sufficient minuteness and accuracy into the interior of the gland to be able confidently to assert the existence or absence of distinctly organised communication, we must be satisfied to render our conjectures more or less probable by means of the attendant circumstances. In the injections made at Guy's Hospital, where very recent bodies could easily be employed, the mercury passed so readily from the glands into the veins, as to form one of the principal obstacles to a successful and extensive injection of the lacteals. There was no reason to suspect rupture; and I do not remember that any appearance of extravasation in the gland was ever observed. In this our experience is confirmed by that of other examiners, who have found it necessary to tie the veins before they could fill the efferent lacteals. Similar observations have been made upon the absorbent glands in the axilla. It would seem next to impossible that so uniform a result could depend on the accidental circumstance of two corresponding ruptures taking

place in contiguous branches of the venous and lymphatic systems. I should indeed almost be tempted to deny the possibility of such an occurrence even as an occasional and accidental event, had not Dr. Sharpey assured me that he had actually known it to have taken place. The idea of transudation through the sides of the vessels must, I think, be regarded as inadmissible in the case of mercurial injections, although it might be more difficult to reject it in those of an aqueous character.

These examples of lacteals communicating with veins fell under our notice, whilst seeking to verify Lippi's observations respecting the direct communication of lacteal vessels of considerable size with the vena cava, vena portæ, or other large veins. In this endeavour, we have hitherto been by no means successful, although repeated attempts have been made by mercurial injection with the lacteals and the absorbents of the liver. In one instance only, my friends Francis Sibson, jun., and John Blackburn, when injecting some of the absorbent vessels in the abdomen, found the mercury passing through a gland into the vena cava by means of a vessel about as large as an eye-probe, and about an inch in length, which had so much the appearance of a lymphatic vessel that several well-practised anatomists were unable to decide that it was not so. I am nevertheless disposed to doubt whether it may not have been a delicate vein; I must therefore allow this instance to be placed with some others of a similar character adduced by Lippi, against which a similar doubt has been raised. I may observe, however, that, at the Anatomical School of the London Hospital, in injecting the absorbents in the abdomen of an ass, the mercury took the same course through a gland to the cava, by what appeared to be a lymphatic trunk. The preparation has been preserved dry, in which state no decided opinion can be formed respecting the nature of the vessel in question. Bracy Clark, whilst investigating some points of anatomy in the horse very early in the present century, found the receptaculum chyli, or the duct, soon after leaving it, emptying itself into a lumbar vein. Opportunities have repeatedly occurred in which the lacteals were rendered distinct by the quantity of chyle they contained, and one in which a peculiar morbid secretion rendered them in various parts not only conspicuous, but firm and knotty; but in none of these instances could any lacteal be traced to a vein in the abdomen. The lacteals in a dog were carefully examined, with the same result, by my friend T. King, who had previously tied the thoracic duct, for the purpose of rendering them turgid, and which had the desired effect.

Hitherto we have made no researches for the express purpose of investigating the peripheric origin or extremities of the lacteals or lymphatics; but we have seen minute lacteals in the villi of the small intestine in one or more individuals who had died whilst digestion was in process.

In the elaborate thesis published by G. Breschet, on the occasion of his concours, I observe that he, after careful investigation, is rather inclined to adopt the opinion that in the villi of the intestinal canal the lacteals really commence by very minute pores; but with respect to every other part he unites with Panizza, whose investigations he has repeated, in believing that they commence in a complicated and freely anastomosing plexus of capillaries, which are of larger size than those which are interposed between the arteries and veins, and having no direct communication with them. On some surfaces, he considers that these plexus form a double layer; in fact, he appears almost to have adopted the opinion of Mascagni, that a minute tubular structure, forming a part of the lymphatic system, enters largely into the composition of animal bodies; 'on which account, almost every fluid by which the cellular membrane may be infiltrated, readily makes its way into the lymphatic system. Without going so far as to admit that the cellular structure is principally made up of lymphatic vessels, I cannot refrain from stating, that I believe that, in some way or other, the origins of the lymphatic vessels are intimately connected with the cellular membrane, an idea long since expressed by Blainville, who has not attempted to explain

more minutely this connexion than I can do. In support of this connexion I may mention a fact repeatedly observed by experimenters, and with which our experiments at Guy's Hospital have perfectly accorded; viz., that when a thin fluid, water for example, is thrown into the arteries, and, when the absence of valves will allow it, into the veins also, it is found almost immediately to fill the lymphatic vessels, but the cellular membrane is also at the same time abundantly infiltrated with it; and when anasarca has rendered the cellular membrane oedematous, we have repeatedly seen the lymphatics of the part abundantly injected: a fact which is already notorious, if we may judge by the preference habitually given to anasarca subjects when it is wished to make injections of the lymphatic system.

It may be supposed that these observations go far to prove the direct communication between the origin of the lymphatics and the capillary system intermediate to the arteries and veins. This, however, I am not disposed to consider as proved; since a trifling rupture, or even transudation, might be adequate to account for the phenomenon, whilst the communication has been expressly denied, except where manifest rupture has taken place. Whatever may in general be the mutual connexion between these minute vessels, we have on many occasions met with facts, which it is not easy to account for without admitting the existence of occasional communications of this kind: I allude to the detection of blood intermixed with the lymph in the thoracic duct, or in the larger branches of the lymphatic system. Such admixture has been observed in animals nearly or quite alive. I have seen deeply sanguinolent lymph flowing alternately with that which was nearly colourless from the thoracic duct of a young pig, whose thorax was opened immediately after the spinal marrow had been divided below the occiput, and only gentle pressure was applied to the abdomen to promote the passage of the lymph. I have repeatedly noticed sanguinolent serum in the thoracic duct in man when inspection has been made a short time after death. Yet it must be confessed that such an occurrence has been most marked when there has been either disease in the lymphatic glands or sanguineous extravasation or congestion. If the direct communication between the lymphatics and the arteries and veins is either to be wholly rejected, or only admitted as a partial occurrence, there is perhaps more difficulty with respect to their communication with some other cavities. Lippi has asserted their communication with the excretory ducts of glands, and Meckel long since thought that he had demonstrated their communication with the vesiculae seminales; but Panizza denies any natural communication of this kind, and Drs. Sharpey and Allen Thomson support these views by their own observations.

In connexion with this part of the subject, I cannot omit to mention a fact observed by my friend Thomas King, although I confess I feel some delicacy in anticipating him in announcing a part of the results of a laborious inquiry in which he has for some time past been engaged. The thyroid gland, the structure and conditions of which he has been examining, is known to possess small cells which contain a secretion, the distinguishing peculiarities of which it is needless for me here to state. They are sufficiently characteristic to distinguish it from other animal fluids. Now, the circumstance to which I wish to invite attention is the fact that the lymphatic vessels proceeding from the thyroid gland have been found filled with a fluid precisely identical with that in its cells; and, as it is next to impossible that any rupture or extravasation could have taken place to account for this, it seems to be almost demonstrated that there is a direct communication between the internal surface of these cells and the lymphatics of the organ.

Although we can very rarely inject the lymphatics by forcing the valves in an adult animal, it appeared desirable to ascertain if this could be effected in the foetal state. Having, therefore, obtained some foetal pigs which had probably not passed more than one-third of the period of gestation, we endeavoured to inject the lacteals from the thoracic duct, but found a complete impediment in the

valves. It was not until the tube was introduced into the lower part of the duct, that the mercury passed into the receptaculum chyli. I was greatly surprised at the large comparative size of this receptacle, and for some time was disposed to think that the mercury must have been extravasated, but on careful examination I did not find that this had been the case. I do not know whether this form of the receptaculum chyli is common in the foetal state of other mammalia, or is peculiar to the pig. I mention the fact as an incentive to the farther examination of the lymphatic system in the foetal state; it seems to be a point of embryology which has not hitherto obtained much attention.

There is another observation connected with the lymphatic system which though not directly bearing upon the question proposed by the Association, I may perhaps be permitted to mention in this place. Some years ago, my friend Edward Cock produced a paper on the subject of the foetal circulation in which he showed from the structure of the heart that the blood arriving by the umbilical vein was principally directed to the head and upper part of the foetus, whilst that received by the descending cava finds its way to the lower part of the body and placenta. This arrangement had previously been pointed out by Meckel; but of this neither the author of the paper nor myself were aware at the time. Immediately the idea was presented to me, it suggested a probable object for the lymph and the chyle of the lower part of the body being made to travel so far before it is poured into the current of the venous blood. Had it entered by a shorter course into the vena cava, it would be sent with the purified blood from the placenta to the brain and other important parts of the foetus; whereas, by entering the heart from the superior cava it is sent more immediately to undergo the process performed in the placenta.* This induced me to conceive it possible that as the young marsupial animals pass the greater part of that period of their existence, which is all but foetal, without a placental circulation, the chyle and lymph from the posterior part of the body might be mixed with the venous blood by a shorter course than that which they take in the other mammalia. I soon had an opportunity of examining this point in a kangaroo, but I did not find my idea confirmed; I observed, however, that in this animal there is a thoracic duct on each side, affording another feature of resemblance between the circulation in these animals and that of birds.

The facts which I have next to mention, though they have not been noticed by the London members of the Commission, are confirmed by several observers; viz. Panizza, Müller, and Drs. Sharpey and Thomson, and Professor Breschet. They relate to some of the inferior animals, and appear to me to diminish the amount of difference between the assertions of Panizza and those of Lippi. Some, if not all, of these observers, have seen the thoracic duct (or lymphatic vessels answering the same purpose) pouring the chyle and lymph into the venous system through the vena azygos.

The most remarkable observations, however, have been made with inferior classes of animals. In birds, fishes, and reptiles, the lymphatics do not merely empty themselves into the veins by more than one or two ducts in the thoracic part of the animal, but they also have similar communications in the pelvic portion. In batrachian reptiles, there appear to be habitually four thoracic communications; namely, two in the usual situation, and two about the scapulae. A very remarkable peculiarity has been noticed by Müller in the lymphatic system of reptiles and fishes. It consists of one or more dilatations, endowed with a distinct contractile power, and occasioning pulsations somewhat analogous to those of the heart. These dilatations are generally situated at or near the tail, or in some part remote from the thorax. It is more than probable that it was a pulsating dilatation of this kind, belonging to the

* This direction of the placental blood to the foetal head appears to me likewise to account for the generally fatal result of breech presentations, and to explain and justify the important observations and directions of my friend Dr. R. King, in relation to these cases. (See his valuable manual on this subject.)

lymphatic rather than to the arterial system, which was seen and described by Dr. Marshall Hall, and considered by him as a sort of caudal heart. Richard Owen had discovered that this organ belonged to the lymphatic rather than the arterial system, before he was aware of the discoveries of Professor Müller respecting it. A similar organ has been supposed to exist in birds; but it appears to be admitted at present, that the lymphatic dilatations which are found in birds are not endowed with the power of pulsation.

I shall now trouble the section with a few considerations, which, though they will not solve the question submitted to us for examination, may nevertheless lead the way to our doing so.

The great difficulty which has been found by many anatomists, as well as ourselves, in verifying the observations of Lippi, may fairly be admitted as proofs that the distribution of the lymphatics which he has described is by no means common; in short, that it is not the normal anatomy of this system. But, on the other hand, it is insufficient to set aside his asserted facts, and induce us to reject all communications between the veins and absorbents except by the thoracic duct and right trunk; and I confess I am inclined to adopt the idea that such communications do exist as varieties, like those which we find in the arterial and venous system, and which, though not to be found at will, occasionally present themselves to constant observers. Such an observer Professor Lippi has been with respect to the lymphatic system; and, in the conversation which I had with the professor when he visited this country, he informed me that he frequently spent many hours in injecting absorbents, and that when he found a case of communication, he employed an artist to make a drawing of it. He would scarcely have done so, could he have found these communications as uniformly as we are accustomed to find the principal divisions of the arterial system. We might certainly be led *à priori* to look for variations of this kind in the lymphatic system, by analogous varieties in the other vascular systems. It often happens that a particular distribution of arteries, which is quite normal in some inferior animal, is met with as a variety in man; we might therefore expect varieties in human lymphatics, producing a resemblance to those of birds, reptiles, or fishes. Various authorities might be adduced, from anatomists who have preceded Lippi, to prove that variations of this very description have occurred in the lymphatic system of man. Cloquet has noticed several communications in the neck, which afford an analogy with birds or reptiles; and I have myself seen a large absorbent from a lung entering the vena azygos.

Seeing that all the terminations of the lymphatic trunks, whether those universally acknowledged, or those admitted by a few, and absolutely rejected by the majority, agree in this, that they pour their chyle or lymph into the venous blood, and this for the most part very near its arrival at the heart, we may be pretty safely led to the conclusion that the important object of these two sets of vessels must be some function connected with their peripheric extremities. I shall not enter fully into this subject, which I have not merely investigated as the subject of a thesis written several years ago, but have followed up in a supplement, in which I have analysed the experiments of several subsequent labourers, who, without exception, tend to confirm the view which I have taken—namely, that neither the veins nor lymphatics are the sole absorbents; that they do not receive all matters indiscriminately; that the lymphatics do not exclusively act on nutrient substances, as Majendie supposes, but that the principal of selection depends on the lymphatic system taking up those substances which are alkaline, or which stand related as such to the fluids received into the veins which are of the acid class.

I must refer those who are willing to enter further into the consideration of this question, to the Appendix to the translation of Dr. Edwards's work on the *Influence of Physical Agents on Life*; but I may add two recent facts to those which I have there related. A young pig took freely

of milk which had become sour by keeping, and which was rendered further acid by the addition of a little muriatic acid. When the animal was killed, a very few hours after, the contents of the thoracic duct were still decidedly alkaline. The blood from the mesenteric vein was scarcely collected in sufficient quantity for examination; but it was evident that it was nearly or quite neutral, and consequently less alkaline than blood from other veins.

The peculiar secretion which I have stated to have been observed by T. King to fill the cells of the thyroid gland, and thence to pass into the lymphatics, has been proved to possess alkaline properties. Some salts of a neutral character are received both by veins and absorbents. Very shortly after death, the power of selection appears to be lost, and the lymphatics receive indiscriminately the fluids with which the cellular membrane may be infiltrated.

In reflecting on some of the peculiarities of the three divisions of the vascular system, I have been induced to form something like a theory respecting the mode of their formation, and the cause of their peculiarities; which, though founded on observation, is in great measure conjectural, and therefore is only offered as one of those speculations which may give direction to research. In some of the lowest forms of organised beings, in which the motion of a nutrient fluid can be detected, we see this fluid advance and recede in the same track, but in others we observe a circulation in which the moving particles take one direction on one side of a channel, and the opposite direction on the other side of it, without the intervention of any septum to divide the two currents. This form of circulation, which is seen in the chæra, appears not only to be the rudimentary type of an artery and vein, but also to throw some light upon the mode in which, in some situations, these two sets of vessels are produced. Few persons can have minutely examined any vascular anatomical preparation, without being struck with the perfect correspondence and absolute adaptation of one set of these vessels to the other; an adaptation which we could scarcely expect or believe, if the arterial blood, having reached the capillaries, were left to find its own route by which to return.

It seems very evident that, in the earliest period of the existence of an embryo, the material of which it is composed possesses in the highest degree that property which may be termed plasticity; by which I do not mean that altogether mysterious influence which gives form to the various parts of the little creature, but merely that physical condition by which a more or less concrete substance is separated from the fluid in which it was dissolved. This coagulation is prone to take place at the surface of the fluid, whether it exist in larger or smaller quantities. The membranes of the ovum are formed as minute vesicles by this coagulation, at the circumference of a single drop; at least, I see that I am supported in this idea by one of the German embryologists. The fluid from which are formed adventitious structures, superadded by disease to fully formed animals, exhibits the same property of plasticity, which we may observe both in the formation of adventitious cellular membranes, as adhesions upon serous membranes, and in the formation of those adventitious structures which may be referred to the type of reflected membranes, which I have elsewhere endeavoured to describe. It seems to be almost demonstrated by the inspection of some very recently formed growths of this kind, that the membrane composing the single or numerous cysts, attached to the interior of a containing cyst, are formed by the coagulation of one fluid in contact with another. The most conclusive evidence, however, of the rapid formation of membranous films by the process of coagulation, is that which may sometimes be seen out of the body, and after death, in some cases in which serum is taken from the pericardium or peritoneum of a very recently dead subject. In the course of a very few minutes, coagulation will sometimes form films capable of retaining the fluid from which they are separated, and appearing to be absolutely identical with the most recently formed false membrane on the surface of an inflamed serous membrane.

These illustrations will, I trust, make evident what I mean by the term plasticity. Keeping this principle in view, let us consider what takes place in the production of vascularity.

At first, it is admitted that these streamlets of blood pass through mere channels in the scarcely concrete substance: there is no distinct vessel, but this in all probability is soon formed, in virtue of the same principle of coagulation at the surface. The surface of the little current becomes a tube. Where opposite currents are established in such a tube, there must be between the two currents a portion of fluid at rest, which must be favourable to the same principle of coagulation, which, by forming a septum between the two fluids, will give to each its own distinct tube, the one corresponding to the other with the most perfect accordance, precisely as we see to be the case in the most delicate membranes, when furnished with two sets of vessels. It will equally apply where one artery may have two veins.* I have entertained this speculation for some years, without being able to see any reasons tending either to refute or more fully to confirm it. Nevertheless, the existence of valves in one set of vessels and not in the other seemed to form so striking a difference between the two, that I confess that it appeared to require a more distinct mode of formation than I had been contemplating. Since my attention has been redirected to the venous and lymphatic systems, I have been led to reflect on the valves which they both possess, and to imagine that I see some little confirmation of my previous views, in the operation of the same principle as the cause of the production of valves. The valves are placed in those vessels in which the *vis a tergo* is neither sufficiently strong nor uniform to afford a guarantee against the chance of regurgitation; whilst they are absent in the arteries, to the extreme branches of which propulsion is communicated at every contraction of the ventricle, however slight. In those vessels which require valves, the very inequality of the current seems to be the cause which produces the valves which are provided to obviate any inconvenience from it. The most transient suspension of the course of the circulating fluid will not only give rise to slight differences in the quality of the fluid in different parts, but also favour a tendency to coagulation. The portion of fluid tending to advance, but not actually proceeding in its canal, must present a convexity towards the portion which it is about to push forward; and at this point coagulation, if it occurred at all, would take place, and the membranes so produced would be a sort of septum convex towards the heart; and, though it would be obliged to give way and break before the advancing fluid, the edges of the flaps would be turned towards the heart by the course of the current, and becoming a permanent structure, they would seem to form a part of the lining membrane. The diagram which I place before you will, I trust, render my meaning more intelligible than I have been able to make it verbally.

I am anxious to know the comparative condition of the venous circulation in the earliest period of foetal life, in those parts in which the veins are furnished with valves, and those in which, as in the portal system, they are not so; but this is an inquiry which seems nearly or quite impracticable. The comparison of the veins with the lymphatics is rather favourable to my view; since the valves are by far more frequent in the latter, in which the circulation is manifestly the most inconstant. It is also remarkable that in reptiles and fishes, in which the lymphatic system is nearly or quite without valves, there are special organs of propulsion to effect the flow of lymph.

35, Bedford Square.

* The nucleated cell theory, which has met with universal acceptance and application, was either not in existence or not generally known when this paper was written and read. Although the formation of the vascular system has been explained by this theory, I am still disposed to adhere to views which I had previously taken; and the microscopic examination of what appeared to be vessels in process of formation in an adventitious growth has strengthened my confidence in it.

METEOROLOGICAL OBSERVATIONS ON CHOLERA.

By J. A. HINGESTON, Esq.

NO. V.

IN the present paper, I shall make use of the Meteorological Observations taken at the Royal Observatory, Greenwich, and quoted weekly in the Registrar-General's Return of Births and Deaths in London, rather than my own private diary, which of course is not so trustworthy as a public record of the same kind. Occasionally, I shall avail myself of Mr. Glaisher's observations, at Lewisham, and sometimes of information derived from the daily papers. My remarks extend from the 19th of August to the 28th of October, inclusive.

The calm that prevailed throughout the worst period of the disease is denoted by the figures in columns nineteen to twenty-two in the Meteorological Observations of the Royal Observatory; on reference to which, it will be seen, that in the second week of September, when cholera was at the worst, it was represented by 195; whereas, in the last week of August, it had been 835; and in the fourth week of September, when "cholera was slowly retreating from London", it rose to 1,045. The calm was the greatest when the disease was the worst; and the disease began to decline as the wind rose. If we examine the columns denoting the pressure of wind in lbs. on the square foot, we shall find that, between August 19th and October 21st, it was, with very few exceptions, at zero. In the same space of time there were thirty-two days marked down as *calm*, which means that it was absolutely so; but if, according to my own careful observations, we were to double that number, we should scarcely be wrong in the general acceptance of the word; that is to say, nearly the whole period was a calm, or at the most only a light breeze. The exceptions to this oppressive state of the atmosphere were on Oct. 20th, 19th, and 18th, the 8th, 7th, 5th, and 4th; Sept. 23rd, when the cholera began to decline, the 20th, 19th, and 16, when there were some stiff breezes for a short time from the south-west. Except these days, amounting in all to not more than ten, all the rest of the time has been a calm, frequently a dead calm, with what sailors call *cat's paws* along the surface of the stagnant ocean.

On reference to the *General Remarks* from the Royal Observatory, we find that September was ushered in with "a dense blue mist", "a thick haze", and "clouds prevalent about noon". The second, or fatal week of that month, was remarkable for "a thick fog", "a fog that prevailed during the day, and also during the night", and "fog" more or less throughout the seven days. From the 16th of the month of September to the 14th of October, we find the words "fog", "misty rain", "a thin misty rain", "fog and misty rain". According to my own observations, the distant country was, during all this time, dim and misty; small black flies were frequent; and the cocks seldom crew. Occasionally, the atmosphere was translucent, the Isle of Wight was visible from the heights, and ships upon the horizon were distorted by the mirage. The robin redbreast returned early, and the swallows were less vivacious than usual.

The aspect of the country is most peculiar during the cholera period, and cannot be forgotten by those who have had the leisure to observe it accurately. The sunshine is pale and watery, and of a sickly brightness; there is a sticky moisture pervading everything; meat putrifies quickly; and the evenings are chilly. The smell of the country has lost its freshness. The same kind of weather seems to have prevailed throughout the world. A writer in the *Times* (Aug. 8th) from the Baltic, mentions the mist incidentally, and then goes on to say that five fatal cases had happened on board the *St. Jean d'Acre*; and another writer, from Varna (Aug. 26th), says that, while the cholera was raging there, the weather was fine but overcast. That there was a great calm at the same time, there seems no reason to doubt; indeed, it is implied by the information of every corre-