effects of these agents, that they do not necessarily require to circulate in the blood in order to produce their special effect. In the lower animals, when strychnine or brucine is applied directly to the spinal cord, tetanic effects instantly follow; and in all probability, when they are introduced into the blood, they produce their tetanising consequences by being carried in the current of the circulation to the cord; thus toxicologically influencing it as if they had been primarily applied to it. Centric irritation or centric morbid conditions of the cord (and, it may be, of the cerebro-spinal system) may lead on, according to these experiments, to tetanic disease, independently of any morbid condition of the blood. . . . .

"3. The appropriate and specific affection of the spinal cord, or cerebro-spinal system, constituting traumatic tetanus, would appear to be sometimes, if not always, a condition excited by some influence propagated upwards along the nerves, from the seat of the injury or wound to the central portions of the nervous system. In proof of this, we have the fact that occasionally (as in cases published by Hicks, Murray, Larrey, etc.), but not often, the artificial division of the nervous communication between the seat of the wound and the nervous centres has arrested the disease, when performed in a very early stage of the attack. What the nature of the transmitted influence may be, we have no means at present of judging. But we have analogies for the transmission itself in some of the phenomena of electrical induction and propagation. And let me further add, that the symptoms of tetanus assimilate themselves very greatly to a rapid succession of electrical or galvanic currents, transmitted peripherally from a nervous trunk or centre to the sets of muscles affected.

"At the present time, various physiologists are busy with recondite inquiries into the laws pertaining to the electromotive powers of the nervous and muscular systems of the body. When our knowledge of these laws is more advanced, physicians will perhaps be able to deduce from them a truer and a juster pathology of tetanus and other convulsive diseases."

Which of these hypotheses explains most clearly the case I have just related, it is difficult to decide; but I am inclined to think that the draughts of cold air to which she was exposed may have excited inflammation of the cord and its membranes, and produced the disease, in a constitution highly predisposed both by the puerperal state and the circumstances by which she was surrounded; and this view of the case directed the treatment adopted. Perhaps chloroform ought to have been employed at the commencement; but the want of adequate attendants, and officious interference of a self-willed husband, prevented it.

Reading, January 1855.

ON THE CAUSE OF THE EMPTINESS OF THE ARTERIES AFTER DEATH.

By I. L. W. THUDICHUM, M.D.

[*Read before the Physiological Section of the Medical Society of London, January 9th*, 1855.]

THE remarkable phenomenon of the emptiness of the arteries after death, which retarded the discovery of the circulation, and reserved it for more modern times, was urged by the opponents of Harvey as their principal and insuperable objection to the new doctrine; and this source of difficulty the discoverer naturally was most anxious to overcome, and to make it harmonise with the general principles of the circulation, which he had just promulgated. He devoted to this object the second part of his exposition; and, after having in the first part established *the fact* of the circulation, he entered in the second into an explanation of the *causes* by which the blood is moved in its course; and these causes he vested exclusively in the heart, the propulsive power of which, he maintained, drives the blood through the whole of the arterial and venous canals. But here again arose the conflict with the fact of the vacuity of the arteries after death; and unfortunately, as if the clearness and simplicity of his genius had failed to support him in that contest, he was led, we think, into a labyrinth of elaborate deductions and of ingenious but somewhat evasive arguments, which leaves an unpleasant impression that his attempt to give an explanation of the powers by the operation of which one half of the channels of the blood are emptied of that fluid after death, would ultimately be found to be admissible only so long as investigation remained imperfect. This was tacitly admitted by the followers of Harvey, who brought the arteries to the aid of the heart; and, dividing the labour of the circulation, invested these tubes with an active propulsive force. It is, therefore, not a little surprising, after this admission of the insufficiency of the heart to perform the functions attributed solely to it by Harvey, to find his doctrine advanced again and supported in a paper, recently the subject of an animate discussion before this Society. The fallacy in Harvey explanation, as quoted by Dr. Richardson, so ably eluc dated by many of the earlier writers, so completely pointe out by Carson as early as 1820, and again adverted to in the late discussion before this Society by Drs. Snow and Pavy, is, that the heart is supposed to drive blood through the remote portion of the arterial system without some impinged medium, without pushing something before it, which of course is blood again, and which at last would still have to remain in the arteries. This fallacious conjecture was supported by statements altogether, as I believe, destitute of proof; such as, that in animals which have been killed by processes which make the suspension of the heart's action the proximate cause of death (such as suffocation by drowning, asphyxia by irrespirable gases), the arteries contain blood as well as the veins—a statement which has been described as the exact reverse of the fact.

The followers of Harvey, by investing the arteries with a heart-like force, by no means removed the difficulty. With Carson, we find the difficulty only placed a little further on in the system. The very disciples of Harvey show, by their numerous attempts to strengthen their position, that they are not satisfied with their explanation, which is indee only a combination of their own, based upon too conservative a principle. With all respect due to this feeling of veneration, we regret to see the similarity which the bequests of Harvey have to the relics of Hunter in the College Museum: they must not be touched, even if they could be improved tenfold.

The causes which retard the improvement of the doctrine of the circulation did certainly not originate with the illustrious discoverer himself. If any man knew that no physiological knowledge could be final for times to come, Harvey did. But we behold in the history of our own science the remarkable fact so often observed in the world's history, that a master has propounded a reformed doctrine, which he himself declared open to improvement in the course of the progress of science; and that his disciples have made the doctrine an orthodox article of faith, declaring it, against the will of its originator, unaccessible to improvement. But surely the days of Dr. Hales have passed; and we no longer think the laws of hydrostatics inapplicable to the circulation—an opinion, however, to which even Hunter inclined, and in which he was joined by a generation adorned with such names as Charles Bell and Lawrence.

In the course of the analysis of the fact to the consideration of which we have devoted these lines, there are several questions to be answered, of which the most important are, Are the arteries, or part of them, found devoid of blood always, or only in a majority of cases; what is their condition if devoid of blood? and what if they contain blood? and under what circumstances do they contain blood?

In cases where they are devoid of blood, they contain—

1. A very small quantity of gas, viz., oxygen, with an exceedingly small proportion of nitrogen and vapour of

water. *They contain air*, though they do not carry it. This is the state in which they are found in the majority of corpses.

It has, however, been stated that the arteries are occasionally met with—

2. Charged with blood like the veins. Valentin* states that this is not unfrequently found, particularly in animals killed by the electric flash, or hunted to death; in individuals struck by lightning, or killed quickly by suffocation. He supposes the same could perhaps be the case in persons affected by nervous or putrid fever, in bleeders, in women who died from uterine hæmorrhage; and we are left to suppose that in these cases the blood had been driven out by the contraction of the arteries, but, not coagulating so quickly, had time left to return to the arteries after the cessation of contraction. Dr. T. Davy, in a letter to Sir James M'Grigor, observes that, in the tropics, directly after somatic death, as soon as the body has lost its warmth and flexibility, the heart, the arteries, and veins, are filled with perfectly fluid blood. But, twelve or sixteen hours later, there is only very little blood left in the large arteries, and the blood in the auricles is found coagulated. This statement is noticed, only because it assumes that there should be time left to see the arteries filled after common kinds of death.

Dr. Richardson is of opinion that, in cases of obstruction in the left side of the heart, the arteries might be found full, though the heart outlived the circulation; and he states, moreover, that, in some descriptions of sudden death, the arteries were found as full as the veins. The same statement is made by Dr. Carpenter,† accompanied by the explanation, that in these cases the vitality of the whole system appeared to be destroyed simultaneously, and the blood remained in the vessels as it was at the moment of decease.

The arteries are further stated to have been observed containing—

3. Cords of coagulated blood, with no surrounding serum. This was observed by Dr. Carson,‡ in the descending aorta, above the bifurcation, of a rabbit which he had killed by admitting air into the cavity of the chest, and causing the lungs to collapse. The larger arteries of a sheep, killed in the same manner, contained a small cylinder of coagulated blood. Dr. Richardson mentioned arteries containing cords of coagulated blood, with no surrounding serum. In these cases, coagulation perhaps began before the cessation of the heart's action, and formed the cord, as it gives rise to the so-called polypus of the heart—the fibrinous coagulum, which is no doubt formed during the last contractions of the heart. I have no doubt that the serum disappears from the arteries in these cases in the same manner as commonly the blood does, and not merely by diffusion through the arterial coats.

The limited number of these statements, and the complete absence of clearly observed and distinctly reported cases of arteries charged with blood after death, show either that the attention of dissectors has not been directed particularly to the subject; or that these cases are so exceedingly rare as to constitute quite an exception. I, for my own part, never met with any blood in any artery, in several hundred dissections, in the five years during which my attention has been directed to the subject.

The absence of blood, therefore, is the rule; and this absence, says the general doctrine of physiology, is caused by the contraction of the arterial system. As a proof that it is the more general doctrine, I may be allowed to quote section 1199 of Valentin's *Handbook of Physiology*, a work which has passed through several editions, and has been recently translated into English.

"When all motion ends, the arteries contract in a high degree. Their *lumen** (calibre) not unfrequently disappears by that entirely. This has the effect of driving their contents to the direction in which there is least resistance. A part comes in this manner in the capillaries and in the more expansible veins. The latter very likely also suffer the effects of the contraction of the minutest nets of blood-vessels.

"If this contraction would end after a few seconds, the arteries by their elasticity would strive to occupy their former space. The blood, still fluid, could return; and the whole phenomenon would have no permanent consequences. But as this state lasts for hours, a great part of the mass of blood coagulates in the meanwhile. When, then, the arteries enlarge again, their interior cannot be filled completely with fluid blood. Extensive parts, therefore, are left empty; and they take up water-vapour, and the gases derived from the blood."

This statement is, I think, the essence of the physiology of the present day. Every sentence of it, however, I consider to be open to grave objections. But in preference to making assertions, let me at once proceed to the proofs on the subject which I have to offer.

The emptying of the arterial system by contraction of its walls could only be effected in three ways.

1. By a general contraction, in which each description of artery must contract in proportion to its *lumen;* so that, at the end of the contraction, there is throughout the whole no *lumen* left.

2. By a vermicular contraction or peristaltic motion, beginning at the valves of the aorta, and so intense as to leave no *lumen*. Ascending the arch, it would go to the carotids and subclavians on the one hand; and descending the aorta, it would proceed to all the organs and the extremities, with the utmost care, over all the obstacles presented by the branching points and bifurcations, on to the capillaries; or

3. By a combination of both the above mentioned modes.

We do not leave out of sight the contraction in length, by means of the longitudinal fibres; but the principal work, according to the theory of the dependence of the emptiness of the arteries on their contraction, devolves upon the transverse fibrous coat. In all three cases, this must contract, either at once or successively, so that there may be no *lumen* left.

Let us recollect that none of these modes of contraction has ever been seen in the living, dying, or dead subject. Not the contraction, but the contractility of the arteries, has been urged as a proof of their contraction being the cause of their emptiness. Under what circumstances has this contractility been examined?

1. *By Experiments on the Living Subject.* These have been very numerous. Contractions of single arteries upon mechanical irritation, have been observed by Verschuir,† Hastings,‡ Jones;§ and in arteries simply laid bare, by Hunter,‖ Fowler,¶ Parry,** Tiedemann,†† and Hastings.‡‡ In all these experiments, the contraction was limited to the single spot of the denuded artery. The frog's web under the microscope, and the arteries of the same animal under the naked eye, show the rule to be one and the same for capillaries and for large arteries—that they contract locally upon local irritation, and that the local irritation has no influence beyond the spot directly irritated.§§

The contraction of arteries on exposure to the air is not

* Handbuch der Physiologie. Ed. II; vol. i, p. 509; vol. ii a, p. 115.
† Handbook of Comparative Physiology. Fourth edition, § 248.
‡ On the Cause of the Emptiness of the Arteries after Death. Medico-Chirurgical Transactions, vol. xi, part i, p. 105.

* *Lumen* is a term of frequent use in German medical and physiological writings. It is most nearly expressed in English by the word *calibre*.
† De Arteriar. et Venar. Vi Irritabil. Exp. 5, 7, 8, 13, 14, 17.
‡ Inflammation of the Mucous Membrane of the Lungs. London: 1820. pp. 24 et seq.
§ Treatise on the Process employed by Nature in Suppressing Hæmorrhage from Divided and Punctured Arteries, and On the Use of the Ligature, etc. London: 1805.
‖ Works of John Hunter, F.R.S., with Notes. Edited by J. F. Palmer. London: 1837. Vol. iii.
¶ Dissertatio Inauguralis de Inflammatione. Vide Hastings, loc. cit.
** Experimental Inquiry into the Nature, Causes, and Varieties of the Arterial Pulse. London and Bath: 1816.
†† Oppenheim, Experimenta circa vitam Arteriarum. Mannheim: 1822. Exp. 1, 9, 12.
‡‡ Loc. cit., p. 31.
§§ Houle, Allgemeine Anatomie, 1841, p. 514.

even uniform in the part so exposed. Hastings saw an artery, which was smooth on being laid bare, become uneven, and, like the trachea, constricted in rings. Any stimulus, let it be a mechanical or dynamical one, pressure or electricity, will make an artery contract. But this does not prove that an artery contracts, to which this stimulus has not been applied. For example, Dr. Pavy, in tying the carotid of a sheep, applied to it three stimuli: the very powerful one of the exposure to the influence of the air, two ligatures, and a wound. All these stimuli must be kept away, if we want to have a pure observation.

It is exceedingly doubtful whether an artery has ever been observed to contract so that there was no *lumen* left, even on application of the most powerful stimuli, such as galvanism or laceration. Mr. Paget, in a conversation I had the honour of having with him about a year ago, stated to me that he had frequently seen the renal artery of a dog contract under the influence of galvanism, so that there was scarcely any *lumen* left. But he did not think the same could be produced or take place in the aorta. In the experiment of Hunter, the posterior tibial artery of a dog had so contracted after having been laid bare a short time, that, on dividing it, the blood only oozed away through the opening. There was, therefore, a *lumen* left in this frequently quoted experiment.

2. The arteries have been examined on the dying subject with regard to their contractility. But these experiments only relate to animals bled to death, and therefore the same cautions must be observed as we have already stated to be necessary in experiments on the living. The part of the arterial system, on which we have inflicted the wound, does not show anything at all; for the stimulus of the cut is so strong, that, as we all can daily observe, it diminishes the *lumen* of the artery considerably, not only because the pressure of the blood is removed, but because there is an active contractile power excited by the wound, and by exposure to the air. If the loss of blood were the cause of contraction, there could be no expansion; or, as I had better express it, relaxation of the contraction, no appearance of elasticity after death, as was observed by Verschuir, Dr. John Thomson,* Parry and Schwann ;† and the vessel could not contract to a smaller calibre than even it has after death, as it did in the quoted experiment of Hunter. All the objections enumerated above apply to the relative observations and experiments of John Hunter and others. Parry, in his work *On the Arterial Pulse*, states that he bled a sheep to death through the carotid, which he had laid bare. During the bleeding, the circumference of this artery contracted from 320-400ths of an inch to 160-400ths of an inch; and, after death, when contraction ceased, but not the elasticity, it gained a circumference again of 234-400ths of an inch, which figure we must consider to represent the normal width of this vessel at the time when it was neither forcibly expanded nor actively contracted. I must here point out that Parry did not see the *lumen* disappear entirely, there was a circumference of 160-400ths of an inch left, below which the artery did not contract. It cannot cause surprise that this artery did contract under the influence of three stimuli—the dissecting out, exposure to the air, and division by the knife.

3. The arteries have been examined on the *somatically dead* subject; that is, on the subject in which the action of the heart and lungs had just ceased. These experiments have not been very numerous as it seems. Hewson, in his *Experimental Inquiries*, states that he bled a donkey to death; the renal arteries were contracted like ropes, but we miss the statement that there was no *lumen* left.

The arteries of human subjects have been more frequently examined in executed criminals. They gave not unfrequently a negative result from one cause or other, so that Henle‡ was of opinion they did not contract upon galvanic irritation. But the newest experiments of Kölliker§ give evidence to the positive. A man was beheaded at quarter past nine A.M., and was placed on the dissecting table at forty-eight minutes past nine A.M. The temperature of the room was 16.8 Reaumur (70° Fahr.). Kölliker examined the abdominal aorta directly on opening the abdomen with regard to the question of contractility. The galvanic stimulus effected a contraction from sixteen *millimètres* (three-fifths of an inch) to one *millimètre* (one-twenty-fifth of an inch),* a very good result in favour of the advocates of contraction, if there is no misprint.† With regret we hear that no other arteries were examined.

No experiment shows contractions of such a nature or degree as we must suppose to be indispensable for the object of emptying the arteries. The theory, therefore, is devoid of every direct proof whatsover. Yet it might be correct, if there were not certain anatomical facts in all individuals, and pathological facts in many individuals, which render it highly improbable, nay, impossible, that contraction can be the cause of the emptiness of arteries.

I. *Anatomical Facts.* The vertebral artery, in entering the foramen transversale of the sixth cervical vertebra, becomes closely attached to the sides of this foramen, and of all the others through which it passes on its way to the brain. This attachment is so close, that under no circumstances could a contraction of the artery take place sufficient to exclude the *lumen*, without the artery being detached from the bony walls. This circumstance it is which makes wounds of the vertebral artery always fatal.

It is the same with the internal carotid artery on its way through the carotid canal, where it is not only closely attached to the bone by a very small quantity of uniting tissue, but also by several branches which it sends to the bone, such as the branch sent to the tympanum. That the artery in this canal can never contract so as to leave no *lumen*, is evident to every one who examines this part with regard to the question at issue.

Similar circumstances are presented by all those arteries which pass through bony parts, through foramina of the skull, and most prominently by the middle meningeal artery, the branches of which are, more or less, imbedded in the meningeal sulci, but in many instances are totally surrounded by the bone.

II. *Pathological Facts.* How are the rigid ossified arteries to be emptied, where the idea of a contractive power cannot be entertained for a moment? In this condition, and in atheromatous disease, the circular fibrous coat, or the muscular coat, is degenerated. It is always well to rest in the shade of an authority, or two; let me, therefore, quote Professors Rokitansky and Paget. They affirm that, in the diseases adverted to, the muscular fibres are not merely obscured by the abnormal deposits, but that they waste and entirely disappear.‡ The muscular or transversely fibrous coat is not merely involved in the degenerative process, but is the principal and first seat of the deposits. In the large arteries with four coats, the coats wholly waste, and their remains appear united in a single pellucid layer, of which the whole thickness may be occupied by the deposit, consisting of fat or oily particles.§ And yet these arteries, which cannot possibly contract a hairbreadth of their circumference, are found as empty as any artery of the most healthy construction.

Again, there are the *large aneurisms* in the continuity of the arteries, which are always found empty after death, provided there have existed no obstruction to the flow of blood through these sacs, such as mortification of the parts underneath the aneurism. I have seen large aneurisms of the aorta as devoid of blood as if they had been carefully washed out; they could not possibly have been contracted to any extent, as neither their tissue, nor their attachment to the spine on one side, and to the sternum on the other, admitted of it.

* Lectures on Inflammation. Edinburgh: 1813.
† Berlin Encyclop. Art. *Gefässe*, p. 229.
‡ Allgemeine Anatomie, p. 519.
§ Verhandlungen der Würzburg. Physical. Mediz. Gesellschaft, 1854, p. 13.

* We are left to suppose that the figures indicate diameters.
† The reporter in Schmidt's Jahrbücher shares my doubt, and supposes that *eleven millimètres* are intended where *one* is mentioned.
‡ Surgical Pathology, i, p. 140.
§ Gulliver, Medico-Chirurgical Transactions, vol, xxvi, p. 86.

How are the arteries, to which reference has been made, to get rid of their contents, as we find they do, when contraction is rendered impossible by attachment to solid walls, or by their being stiff and incontractible themselves? That is the question at issue.

Let me here yet point out that the followers of the theory of the evacuation of the arteries by contraction entirely disregard the fact, that the veins are endowed with a similar or like contractility upon the application of the stimuli enumerated above as causing contraction of the arteries. We have direct observations of the contractions of veins on the application of stimuli, by Verschuir,* Hastings,† Marx,‡ and Bruns.§ The latest ones are by Virchow,‖ made on the beheaded subject already mentioned. One hour after the separation of the head, a mesenteric vein on the ileum contracted at least one-fourth of its diameter on the application of galvanism. A piece of the vena cava inferior, behind the liver, contracted, on application of the poles, from thirty-nine *millimètres* (one inch and a half) in length, to thirty-three *millimètres* (one inch and three-tenths), and showed a great many wrinkles. After a longer action of the galvanism [twenty-two minutes], the length was only twenty-five *millimètres* (nearly one inch), and consequently assumed an appearance like that of the arteries. Two hours after death, the vena axillaris contracted to one-half of its diameter, from ten *millimètres* (two-fifths of an inch) to five *millimètres* (one-fifth of an inch).

The thoracic duct contracted better than any artery or vein, even to the perfect occlusion of its *lumen*. But the contraction was permanent, and not at all progressive or peristaltic. This circumstance shows that the contractility manifested on the application of galvanic or other irritation, is no proof of the existence of contractions during or after death; the veins and the most contractile thoracic duct invariably being found full of blood and lymph, a circumstance incompatible with the assumption of a tonic contraction.

The heart cannot propel at a distance; the arteries cannot all contract; and, in some cases, none of the arteries can contract. Where, then, must we look to find the mystic power which empties the arterial vessels? The direction has been pointed out by Carson; but he passed its seat, and went beyond it. The seat was pointed to by Dr. Snow, in the late discussion, and by some others before him. The power itself has been defined, and its existence supported by facts, by Dr. Carpenter, in his *Comparative Physiology*. It is the diffused forces of the capillary system.

Some positive facts have been collected by others and by myself, proving beyond doubt the existence of these forces of the capillary system. A great part of these facts have been perspicuously represented by Dr. Carpenter; who, however, yet admits an active influence of the arteries in the process of circulation, and attributes a supposed additional impulse of the blood to the contraction of the muscular walls of the arteries. The facts, which I beg to submit to examination, are derived from three branches of medical science; from comparative physiology, from human physiology, and from pathology or pathological physiology.

I. *Facts derived from comparative physiology* are always to be used with caution in drawing inferences from them; because the reliance upon analogies very often leads to aberration. Single facts, therefore, are better proved in themselves, without adverting to analogies. But the principles of the general working of nature are best understood by reference to comparative science.

The laticiferous circulation in plants we know to be entirely capillary. The single vegetable errant cell, and the largest Indian fig-tree, grow and live alike, with only capillary circulation. The sap traverses a distance of several hundred feet in length, with no propulsive power but the capillary.

In the lower order of animals there is, according to Dr. Carpenter, the holothuria, which has no central contractile organ which seems powerful enough to propel the blood through all the minute ramifications of its vascular system. The same author states that even in the higher articulata, and in all the mollusca, save the cephalopods, so large a part of the systemic circulation is "lacunar", that it seems impossible to imagine that the action of the heart can urge the blood through the branchial vessels.

We see, then, that there may be a circulation without a heart, or contractile propulsive power; and, as the next stage, a circulation with the aid of a heart, which, however, evidently could not by itself maintain the circulation.

It has been adduced in favour of the views here put forth, that the embryo of the chick possesses a circulation before the existence of pulsating vessels. This circumstance would no doubt go far as an argument, but I regret to say that the observations of Wolf,* Pander,† and Von Baer,‡ still admit so many doubts, that after the critique they have received from Bischoff,§ I cannot consider them sufficiently conclusive, and, therefore, do not feel satisfied in quoting such doubtful facts in support of my views.

II. *Facts derived from Human Physiology*. Some of these have already been adduced by Dr. Carpenter, such as the development of the uterus during pregnancy. I cannot do better than quote his words.‖ "The unusual activity of the nutritive operations of the uterus induces an increased demand for blood in its capillary circulation, which is supplied by an increase of the diameter of the trunks that transmit fluid to the organ. And this is entirely independent of the heart's action. This may be said of the development of the mammæ for the secretion of milk, of the rush of the blood through these organs during the act of suckling. All these facts seem to indicate, without much doubt, that the quantity of blood sent to individual organs, and the force with which it is transmitted through them, are augmented with any increase of energy in the vital processes taking place in them, the *vis a tergo* derived from the impulsive power of the heart remaining the same."

I arrive now at a fact which strikes me most of all. Physiologists have seen the difficulty attending an explanation of the circulation through the liver. They found that the blood, after it had traversed the capillary system, had lost nine-tenths of its original pressure before it came to the right auricle. Magendie¶ found one-tenth of the pressure of the carotid left in the corresponding part of the jugular; Valentin,** only one-eleventh or one-twelfth; and Ludwig and Mogk†† even considerably less than that.

If we take as the standard of the pressure of the blood in the carotid 150.0 *millimètres* of mercury, the following figures for the jugular were found by experiments:—

Magendie...	Dog ...	15.0 *mm.*	= 1-10th original pressure
Valentin ...	Dog ...	13.0 *mm.*	= 1-11th to 1-12th
Ludwig	Mean of	2.0 *mm.*	= 1-75th
Mogk	7 dogs.	13.0 *mm.*	= 1-12th

Similar circumstances we find in all the veins of the body; and in no vein does the pressure of the blood exceed 23.7 *millimètres;* that is, a little more than one-sixth of its original pressure. Now, if we make the application to the vena portæ, and admit the highest pressure ever found in a vein to be present in it, viz. 23.7 *millimètres*, the blood in traversing the stomach, the small and large intestines, the

* Loc. cit. Exper. 10, 17, 18.
† Loc. cit., p. 61 et seq.
‡ Distribe de Structurâ et Vitâ Venarum, p. 71 et seq.
§ Allgemeine Anatomie, p. 93.
‖ Verhandlungen der Würzburg. Physical. Medicinischen Gesellschaft.

* Bischoff, T. L. W., Entwickelungsgeschichte der Säugethiere und des Menschen. Leipzig: 1842.
† Vide Bischoff, op. cit
‡ Entwickelungsgeschichte der Thiere. Königsberg: 1837. Part II, p. 126.
§ Op. cit., p. 495. The latest observations of Baer, J. Müller, Valentin, Reichert, and others, concur in shewing that there never is any movement of the blood without a movement of the heart.
‖ Op. cit., sect. 479.
¶ Léçons sur les Phénomènes Physiques de la Vie. Tome iii. Paris: 1837. 8vo., p. 151.
** Handbuch, etc., t. i, p. 488, § 1120.
†† De Vi Flumiuis Sanguinis in Venarum Cavarum Systemate. Marburgi: 1843. 8vo.; and in Henle and Pfeuffer, Zeitschrift, f. d. rat. Med. Vol. iii, 1845. 8vo., p. 73.

rectum, spleen, pancreas, and gall-bladder, has lost at least five-sixths of its original arterial pressure. And if, according to the same authorities, the capillaries in the organs just enumerated offer by their size the least difficulty to the passage of the blood, and if, notwithstanding that advantage, the blood loses five-sixths of its original pressure, how is the blood to pass through the network of the capillaries of the liver, with only one-sixth of its original pressure? How can it pass through a gland, where, from the size of the capillaries, it not only encounters the double difficulty it found in the intestines, but also *is retarded* (which seems most likely, notwithstanding all that has been said to the contrary) by the accession of more blood under a higher pressure out of the hepatic artery.

The explanation of the progress of blood through the vena portæ is, as it stands, most unsatisfactory, without the assumption of a particular capillary power. The only advantage the blood in the vena portæ has, seems to be a slight increase of tension, in consequence of this vein alone containing the blood of several arteries, which is the reverse to what occurs in other veins, several of which contain the blood of only one artery.

But this advantage will never account for the circumstance of the transmission of blood under one-sixth of a pressure through capillaries which offer more resistance than others, in which five times the force is spent for transmission, not to forget the disadvantage of the portal vein (which, by the bye, is only two and a half inches long) in having no valves.

The writers on circulation, whether they admit contractility of the arteries or not, invariably bring forward as the chief proof of the circulation being a mere pumping process, one experiment, which for example, we find quoted in Kirkes' *Physiology*. It is, that in a newly killed animal, where the action of the heart and lungs has just ceased, bullock's blood can be transmitted through the arteries to the veins by the sheer force of a syringe. If there is no doubt about the fact, the explanation of it would not appear to be quite so simple and satisfactory. One is led to suppose, that if blood is transmitted by mere mechanical power, other fluids, such as water, oil, salt-water, mercury, must be equally easy of transmission by the same mechanical power, and during any length of time from the somatic death to the beginning putrefaction. This would perhaps be the case, if blood did not coagulate in the capillaries and veins soon after death. Attempts have, therefore, been been made to prevent coagulation by injecting water directly after the cessation of somatic life, and to wash the vessels out by the mechanical power. But it was found that water could not be transmitted, notwithstanding the application of the mechanical power. This difficulty has been left without sufficient explanation; and we may, therefore, be allowed to offer some hints. Physiologists, in injecting bullock's blood after somatic death, make use perhaps of the capillary power, the extent of which is not yet recognised by them. They can drive mercury through capillaries, and can fill capillaries with many injections; but such an amount of force must be applied for that purpose by the hand of the experimentalist, that, if we would measure it according to the estimated power of the heart, it would enormously surpass the stroke of this organ; and one is inclined to think that, if there were no other force promoting circulation than the heart, the heart of a whale would be required in a human chest, to effect even a very slow and languid circulation.

An experiment has been brought forward to prove that the pressure of the blood is sufficient to overcome the resistance which it encounters in the kidney. Mogk* inserted a tube of two *mètres* length (4 feet 10⅜ inches) in the renal artery, and kept it constantly filled with warm blood, which traversed the kidney and left the vein in a continued stream. This experiment shows what capillary force will do in the kidney, as the vena portæ proves the effects of the same power in the liver.

* Op. cit., p. 73.

III. *The facts derived from pathology* are as yet very few in number, but will no doubt become more numerous every day. Of them, the most remarkable one is the circulation in the acephalous, or as they were synonymously called by Elben* *acardiac* or heartless fœtuses. Since the time it first attracted the attention of anatomists, this fact has puzzled the ingenuity even of such men as Méry, Le Cat, Sömmering, Astley Cooper, Munro, Clarke, Brechet, Lobstein, Tiedeman, Müller, Brodie, Marshall Hall, and a great many others of scarcely less authority.† But all theories advanced became every day more incompatible with the accumulating facts, till Dr. Houston‡ gave a more satisfactory turn to the question. Though I cannot agree with him as regards the direction of the course of the blood in the acardiac fœtus, from facts which I shall have to discuss at another time, yet this minor question does not interfere with my acknowledgment of the validity of Dr. Houston's opinion, that the circulation is carried on by the vital powers of attraction inherent in the vessels of the acephalus itself. This progress has been attempted to be converted into what I am disposed to consider a retrograde movement, by a paper in the *Monthly Journal of Medical Science* for December 1854, by Dr. A. Mercer Adam, who, though he thinks Dr. Houston's views to be the most correct hitherto advanced in explanation of this difficult question, yet feels himself moved to add his own conjectures to the views of Dr. Houston, and to endow rather equivocally the arterial vessels of the acardiac fœtus with an "inherent muscular contractility, which serves to propel the blood to the placenta". Only the return of the blood from the placenta to the acardiac fœtus is, according to this gentleman, probably effected by a vital attractive force inherent in the capillary blood-vessels. It is clear to me that Dr. Mercer Adam is strongly in favour of mechanical circulation, and substitutes it where it cannot be proved; but veins do not admit so easily of a muscular contractility being demonstrated in them; and, therefore, he must admit that the blood in them moves by a vital attractive force, inherent in the capillary blood-vessels.

The following fact has been adduced by Dr. Carpenter.§ "When the normal changes to which the capillary circulation administers are suspended, as in asphyxia or suffocation, the heart's action is entirely insufficient to maintain the current of blood." I opened the chest of a dog, to witness the action of the heart during suffocation, which I induced by admitting air to the pleural cavities. I was astonished to see the frantic efforts of the right ventricle, which resembled so many shocks of electricity rather than the common quiet contraction of the heart. The right auricle was distended to its utmost size. The left side of the heart never showed any diastole, and the pressure of the blood in the aorta sank nine-tenths of its strength immediately. Notwithstanding this interruption of the *vis a tergo*, the venous blood kept on accumulating, and its pressure attained a height which confirmed me in the impression, that death in asphyxia is caused by apoplexy, sooner and more often than by the insufficiency or absence of the arterial supply. There was no *vis a tergo* in the arteries; and still the blood was forced on by the capillaries; but there was a *vis a tergo* exerted towards the lungs, and still capillaries of those organs effectually obstructed the circulation. One principal factor in their sum of forces—the interchange of air and blood—was suspended by the destruction of the mechanism of the respiration.

This experiment gave two facts in favour of the qualities ascribed to the capillary system, the former of which presents great similarity to the observation of Dr. Bennett Dowler, that, in the bodies of individuals who have died of yellow fever, the external veins frequently become so dis-

* Dissertatio de Acephalis, seu Monstris Corde carentibus. Berolini: 1821.
† Vide Elben, and Dr. M. Adam's Essay, to be quoted presently.
‡ British and Foreign Medical Review, vol. ii, p. 596. Dublin Medical Transactions, 1837.
§ Op. cit., sect. 479. See his Human Physiology, sect. 575; and his article on "Asphyxia", in the Library of Practical Medicine. Also see Dr. John Reid, Edin. Med. and Surg. Journ., April 1841; and Physiol., Pathol., and Anatom. Researches, chap. II.

tended with blood *within a few minutes* after the cessation of the heart's action, that, when they are opened, the blood flows in a full stream as in ordinary blood letting.

Two more facts have been brought forward by Dr. Carpenter. There are cases of spontaneous gangrene of the lower extremities, in which all the blood-vessels are found pervious throughout; but whether these cases really admit of the explanation given to them, I am as yet unprepared to say. Gangrene may occur from narrowness of the aortic orifice of the heart, from a contaminated state of the blood, as in ergotism; and, before we ascribe gangrene to the cessation of the nutrient actions of the capillary system, we must naturally exclude all other causes. If this has been done in the cases alluded to (which I regret has not been stated expressly), they certainly are striking illustrations of the capillary power, and the consequences of its cessation.

The other fact is the curious retardation or suspension of the circulation in so-called dead fingers. Dr. Carpenter relates a case, recorded by Dr. Graves,* in which the whole of one leg was thus affected with remarkable periodicity for about twelve hours out of twenty-four; whilst in the interval the circulation was unusually active, the action of the heart being quite natural throughout, and the circulation in the rest of the body not in the least affected.

Here the facts derived from pathology end. I leave the subject without entering into the microscopic appearances shown by capillaries in their wonderfully varied constructions and adaptations to the objects of the different organs; I leave it without an attempt even at a definition of the power of the capillaries. One positive experiment has established the certainty of its existence to my mind, and I hope on a future occasion to make it plain also to the mind of others who may favour me with their attention, on which indeed I am afraid of having trespassed already more than perhaps may be justified by the magnitude of the subject under consideration.

34, Keppel Street, Russell Square, January 1855.

EFFECTS OF SUBMARINE DESCENT.

By THOMAS LITTLETON, M.B., F.R.C.S.

In the progress of constructing an iron bridge across the river Tamar at this place, by Mr. Brunel, my attention has been called to certain injuries, sustained by some of the workmen engaged therein during the endeavour, which has now proved successful, to procure a foundation for the central pier. The absence of any sufficient mention of the kind of accidents which attend upon those conducting submarine occupations, in the notices to which I have been able to gain access, induces me to attempt to supply some of these omissions; and I trust that the explanation of the effects produced, though far from satisfactory, may be suggestive to those better able to deal with the question, and may by such cooperation ultimately conduce to the welfare of those to whom society owes a debt which it has so long neglected, and which remains for the Committee on Industrial Pathology of the Society of Arts to discharge. And to do this fully, what better channel of communication and for assistance can be obtained than that of this Journal?

There are accounts given of several of the inconveniences which attend on the descent in a diving bell, at p. 492, No. 349, and p. 177, No. 368, of the *Philosophical Transactions*, by Dr. Edm. Halley; at p. 377, No. 444, by Mr. Martin Triewald; and in Dr. Olinthus Gregory's translation of Abbé Haüy's *Natural Philosophy*, vol. i, p. 224. Such are the following: a painful sensation of pressure on the membrana tympani, which soon subsides, the ready communication by the Eustachian tube establishing an equilibrium of pressure on that part; spitting of blood; bleeding at the nose and ears; blood-shotten state of the eyes; and the oppressive sensation attendant on a confined atmosphere, by which, if the means of constant renewal are not applied, life would be speedily destroyed.

But no allusion is made to one source of danger, that has demanded most attention here; and which, from the suddenness of the attack, and apprehensions of fatal results attending it, more especially deserves notice.

Some reason for this omission is supplied by the difference which the apparatus used here presents from a diving bell, and the less liability to danger which exists in the latter, from the *gradual* manner in which it is lowered and raised, and the consequently slow increase and diminution of pressure to which its inmates are subjected. Were this, which is so rightly insisted on by Dr. Halley, not observed, the same consequences would follow the rapid drawing up of the bell to the surface of the water as attend the working in this cylinder, and from the same cause, *the sudden removal of pressure*.

Considering the effects produced on some few by this change, from a pressure of three and a half atmospheres (the depth at high spring tides being eighty-five feet) to the normal pressure of fifteen pounds, it is a matter of surprise that more do not suffer them. There have not occurred, so far as I am aware, more than half a dozen severe cases, in a work which has occupied daily twenty-five men over a period of many months.

In the severe forms of the attack, the man is taken, within a few minutes after coming out of the cylinder, somewhat as in an apoplectic seizure, with a loss of power, preceded by pains, in the lower limbs (paraplegia), as I have seen in two cases, or of one-half of the body (hemiplegia); another, the only one I have seen so affected, was wholly unconscious, remaining in that state many hours. In those who escape with less injury, their sufferings are in some instances very severe, from pains in the limbs and joints; and few, if any, have wholly escaped these effects at some time or other during the progress of the work.

These are evidently the results of pressure on the cerebro-spinal nervous system; but what are the different links in the chain of causes producing it? It may be, that the respiratory movements are not accompanied by the alternate falling and rising of the brain attending the acts of inspiration and expiration, which undoubtedly constitute important conditions of the brain's action; and the impediments occasioned in this respect alone prove obstructive of its functions. I name this as possibly the case, because it is obvious the first few inspiratory efforts, on coming out from the pressure named, cannot be attended by their ordinary effects, the due influx of venous blood and of atmospheric air into the chest; for the *condensed air* at that time present in the lungs, and continually brought by the blood of the body, until the whole of it has circulated through the lungs, would expand itself, and thus occupy the space, which under normal conditions the venous blood and atmospheric air would enter on the production of the vacuum by inspiration.

But more than this must have occurred to bring about such a continuance for weeks of the loss of voluntary power as exists in some of these cases. There might have been an extravasation of serum into the ventricles, etc., a rupture of small blood-vessels, or possibly, under such physical conditions, there may occur an *extrication of air*, occasioning pressure on the brain. Under a like series of conditions, Boyle, in his experiments on animals breathing a rarefied atmosphere, notices to have manifested itself in the case of a viper,—"a conspicuous bubble of air moving to and fro in the waterish humour of one of its eyes." (*Phil. Trans.*, abridged, vol. i, p. 499.)

The precautions needed to obviate these several inconveniences, and which, if adopted, will prevent the occurrence of any ill consequences, are these. That the pressure be laid on gradually by not more than five pounds (Dr. Halley directs twelve feet of water) at a time, stopping at each increment of pressure during the time which is required for

* Lectures on Clinical Medicine. Second edition. Vol. i, p. 73.