The facts obtained from these researches seem to us to warrant the following conclusions.

1. Both chloroform and ethidene administered to animals have a decided effect in reducing the blood-pressure, while ether has no appreciable effect of this kind.

2. Chloroform reduces the pressure much more rapidly and to a greater extent than ethidene.

3. Chloroform has sometimes an unexpected and apparently capricious effect on the heart's action, the pressure being reduced with great rapidity almost to *nil*, while the pulsations are greatly retarded or even stopped. The occurrence of these sudden and unlooked for effects on the heart's action seems to be a source of serious danger, all the more that in two instances they occurred more than a minute after chloroform had ceased to be administered and after the recovery of the blood-pressure.

4. Ethidene reduces the blood-pressure by regular gradations, and not, so far as observed, by sudden and unexpected depressions.

5. Chloroform may cause death in dogs by primarily paralysing either the heart or the respiration. The variations in this respect seem to depend to some extent on individual peculiarities of the animals; in some, the cardiac centres are more readily affected, in others, the respiratory. But peculiarities in the condition of the same animal very probably have some effect in determining the vulnerability of these two centres respectively, and they may both fail simultaneously.

6. In most cases, respiration stops before the heart's action; but there was one instance in which respiration continued when the heart had stopped, and only failed a considerable number of seconds after the heart had resumed.

7. The use of artificial respiration was very effective in restoring animals in danger of dying from the influence of chloroform. In one instance, its prolonged use produced recovery, even when the heart had ceased beating for a considerable time.

8. Under the use of ethidene, there was on no single occasion an absolute cessation either of the heart's action or of respiration, although they were sometimes very much reduced. It can, therefore, be said that, though not free from danger on the side of the heart and respiration, this agent is in a very high degree safer than chloroform.

9. These results confirm and amplify those stated in a previous report, to the effect that ethidene does not compromise the heart as does chloroform. By the method of experimentation then employed, the effect on the blood-pressure could not be determined; and altogether the results here obtained are more exact and unequivocal.

It may be added that since the last report ethidene has been given to a number of patients of all ages, with results which may be described as satisfactory. Given freely at first, it produced anaesthesia as rapidly as chloroform, and the effect could readily be kept up by comparatively small subsequent doses. The only drawback is that in some cases it produced vomiting; but it has not been determined that it does so more frequently than chloroform, over which it has the further advantage of producing less excitement and being more agreeable to the patients.

Isobutyl chloride was given to three patients, but it produced considerable excitement and proved an imperfect anaesthetic.

In concluding this report, the Committee would express the belief that, so far as purely physiological tests go, ethidene has proved itself as efficient an anaesthetic as chloroform, and a much safer one. It is not asserted that this agent ought at once to replace chloroform in practice; but it is believed that a very strong case has been made out for an extensive trial of it. It is impossible for the Committee, having numerous other engagements, to give the agent that extended trial in actual practice which is desirable; and they would ask those who are engaged in practice to use it in their cases, and to report the results they may have obtained.

It appears that ethidene was first employed as an anaesthetic by Dr. Snow. He administered it in fifteen cases, with good results. (See *Snow on Chloroform*, etc., last paper, published in 1858.) In 1870, it was used by Liebreich and Langenbeck in Berlin. (*Berlin. Klin. Wochenschrift*, Nos. 31 and 33, 1870, p. 401.) In 1871, two papers appeared; one by Sauer, in the *Pharm. Centralblatt*, vol. xiv, p. 140; and the other by Steffen, in *Deutsche Klinik*, No. 44, p. 398. Sauer mentions one case of death in a patient suffering from heart-disease. In thirty-three cases, two vomited, and two suffered from nausea and headache. In 1872, Steffen publishes another paper in the same journal, p. 358, in which he gives details regarding twenty cases; and he states that the results were satisfactory. (See also *Fahresb. der Medicin.*, 1870, 1871, and 1872, where abstracts are given.) Snow states that the difficulty in obtaining ethidene pure may prevent its general use. That difficulty is now removed; and there is little doubt that, if required, ethidene may be made in a state of purity and at a moderate cost.

DESCRIPTION OF PLATE.

Some of the tracings referred to in the text are here reproduced, it being understood

that in the original the tracings are white on black instead of black on white. The basement line represents the point at which the needle was when the mercury in the two limbs of the manometer was level. According to the pressure, the mercury is pressed down in the one limb and raised in the other, the tracing giving the latter. As the actual pressure equals the weight of the column of mercury raised, this will be represented by the difference in the levels in the two limbs, and will be double the distance from the basement-line to the tracing. The tracings are to be read from left to right.

A. Represents the occurrence mentioned in the text, in which, while a dog was recovering from chloroform, a sudden fall of pressure occurred, with great reduction in the frequency of the heart's contractions. There is a gradual recovery, with striking inequalities of pressure corresponding with respiratory movements.

B. Another sudden fall of pressure and retardation of the heart's contractions occurring seventy seconds after the removal of the chloroform, and when the pressure had nearly returned to the normal.

C. The arterial pressure in a dog under the influence of chloroform. There is a progressive but somewhat irregular reduction of pressure, ending in a stoppage of the heart, while respiration continues. There is an imperfect recovery of the heart, the contractions barely causing the pressure to reach the basement line. Beneath this tracing are copies of the markings made with the chronograph, half seconds being registered. The markings used to indicate the administration or leaving off of an agent are also reproduced.

D. The arterial pressure in a dog under the influence of ethidene. There is a regular and gradual reduction in the pressure, the respiratory variations being preserved.

E. This is a continuation of D, after an interval. Ethidene has been given all the time, and the illustration begins at the point where the pressure is lowest. The pressure is now beginning to rise, the ethidene being still continued.

F. The arterial pressure in a dog under the influence of ether. No effect on the pressure is produced, and the respiratory variations are perfectly preserved.

G and H show the variations in pressure produced in rabbits by the administration of chloroform, the animals being at this time imperfectly under its influence. These are presumed to be reflex.

J and K show the exaggerated respiratory variations produced while artificial respiration was being carried out. It is to be understood that the animals made synchronous respiratory efforts. The arrangement for artificial respiration not only blew air into the lungs, but sucked it out.

AN ADDRESS

ON THE

INFLUENCE OF POSITION UPON LOCAL CIRCULATION.

*Read before the Harveian Society of London.**

By JOSEPH LISTER, F.R.S.,

Professor of Clinical Surgery in King's College, London.

MR. PRESIDENT AND GENTLEMEN,—Having been honoured by a request from the Council that I would make some communication to this Society, I have thought that no subject could be more appropriately brought before an association connected with the name of Harvey, than some observations begun several years ago, but hitherto unpublished, regarding the effects of the position of a part upon the circulation in it.

My attention was first directed to this subject fifteen years since, when I was engaged in endeavouring to devise a satisfactory method of excising the entire articular apparatus of the wrist for the cure of carious disease. In that operation, although no large arterial branches are divided, the very protracted character of the procedure would render the oozing from small vessels a source of serious loss of blood to the patient if it were allowed to go on unchecked. Accordingly, I was led to deviate from what was then the ordinary practice of restricting the use of the tourniquet to amputation, and employed the instrument in the excision referred to. And I found that, when the hand was raised to the utmost degree, and kept so for a few minutes, and then, while the elevated position was still maintained, a common tourniquet was applied to the arm, being screwed up as rapidly as possible, so as to arrest all circulation in the limb and at the same time avoid venous turgescence, I had a practically bloodless part to operate upon, and thus gained the double advantage of avoiding hæmorrhage and inspecting precisely the parts with which I was dealing. And, having found such great benefit from this bloodless method of operating in the instance referred to, I extended it to other operations on the limbs.

In 1873, I was one day illustrating this subject to my clinical class in Edinburgh by raising one of my hands to the utmost while the other was kept dependent, in order to exhibit the contrast between them in redness, when a sensation of chilliness coming on in the hand that was raised made me feel, and at once express, the conviction that something more was occurring than could be explained by the mere mechanical effect of the position of the part upon the blood in the vessels, and that the diminution of pressure upon their walls resulting from the action of gravity upon their contained blood must operate as a stimulus to the vaso-motor nervous apparatus of the limb, so as to induce reflex contraction of its arteries.

* The paper, as now published, embraces some considerations on which I did not enter at the time of its delivery, and also some fact subsequently ascertained.

I will now ask this man (with his arms bare) to raise one hand high into the air, while the other hangs beside him. You observe at once the striking contrast between the two. In the one elevated, not only have the veins entirely collapsed, but the colour is almost that of the limb of a corpse. So white is the hand, as to imply that the minute arteries must surely be in the same state of extreme constriction as occurs during syncope.

I will now apply to the arm close to the axilla a bandage of elastic webbing; putting on the first few turns with firmness and rapidity, so as to avoid any intermediate condition of engorgement through obstruction of the return by the veins before the arteries are completely compressed; while the later turns may be put on, if we please, more leisurely and with less firmness, to ensure, by the accumulation of the elastic force, complete maintenance of such constriction of the limb as prevents all circulation. The elastic band having been fixed with a pin, the limb is lowered and the man will stand aside for a few minutes.

I may state that, when Esmarch published his method of bloodless operating—consisting, as my hearers are all aware, of first expelling the blood from the limb by means of an elastic bandage wound firmly round it from the distal extremity upwards, and then applying another elastic constricting band in the manner you have witnessed just above the first, so as to prevent recurrence of circulation on the removal of the latter—I thankfully adopted the use of the upper constricting band in preference to the common tourniquet. I did so because the elastic band has the great superiority over the inelastic strap of the tourniquet, that it follows up any yielding that may occur in the soft parts subsequently to its application, and thus prevents the necessity which we often used to find with the common tourniquet, especially if the limb was swollen through inflammatory or oedematous effusion, of tightening up the instrument repeatedly in consequence of recurrence of bleeding, attended on each occasion with venous engorgement. But, while I gratefully accepted this part of Professor Esmarch's procedure, I did not see sufficient reason for substituting his mode of emptying the limb of blood by elastic bandaging for our former practice of trusting to the elevated position. I shall recur to this point in the sequel.

Being desirous of testing with greater precision than was possible upon the human subject the correctness of the view which I had been led to entertain of the cause of the paleness of an elevated limb, I performed on November 29th, 1873, the following experiment upon a horse. An arrangement having been made by means of ropes and pulleys, one rope being connected with a broad sling beneath the abdomen and others with the feet, so that the animal could be either raised into the air with the feet dependent, or laid on its side on the ground with the legs extended horizontally, or again placed on its back with the feet drawn vertically upwards, chloroform being administered, I exposed, at the lower part of one of the fore-legs, an artery about as large as the human vertebral, situated along the outer aspect of the metacarpal bone.

In the elevated position of the limb, the wound proved almost absolutely bloodless, closely resembling one in a dead animal; and the artery was straight and pallid, and no pulsation could be perceived in it. When the animal was turned round so that the feet were dependent, the artery became much increased in size, tortuous, red, and pulsating powerfully, and blood oozed freely from the surface of the wound; and when the limbs were placed horizontally, an intermediate condition took place, both as regards the artery and the hæmorrhage. By means of suitable callipers, careful measurements were taken repeatedly of the external diameter of the vessel in the different positions of the animal, which, having had no chloroform administered after the conclusion of the cutting operation, appeared to be in a normal state as regards the force of the circulation. The accompanying diagram exhibits the results obtained, and also the section of the artery after removal from the body, when it was found contracted to almost complete obliteration of calibre.

You observe that, in the elevated position of the limb, the vessel was nearly as small as it was in that state of extreme constriction. On the other hand, in the horizontal, and still more in the dependent posture, the external diameter became considerably increased. But, in order to judge of the augmentation of the capacity of the tube for transmitting the blood, we must look to the area of the internal calibre, which, having measured the thickness of the wall of the artery after removal from the body, we have the means of estimating for the various positions of the limb. For, the substance composing the arterial wall being of course a constant quantity, the ring constituted by its transverse section has always the same area, though varying in form, being thinned out as the vessel expands. The area of the ring is calculated from the dimensions obtained after removal of the artery from the body, viz., the external diameter and the thickness of the wall; and the area of the internal calibre for any other condition of external diameter is simply the area

of the corresponding circle minus the area of the ring.* The numbers in the diagram are hundredths of an inch, and the drawings are accurate to scale, though magnified for convenience of representation; and from these as well as from the calculated numbers, it will be seen that, when the limb was horizontal, the area of the internal calibre was more than three times as great as in the elevated position, and that, when it was dependent, the capacity of the tube was increased about sevenfold.

METACARPAL ARTERY OF HORSE			
SECTION AFTER REMOVAL	LIMB ELEVATED	LIMB HORIZONTAL	LIMB DEPENDENT
EXT. DIAMETER	EXT. DIAMETER	EXT. DIAMETER	EXT. DIAMETER
17	18	21	25

AREA OF CALIBRE	AREA OF CALIBRE	AREA OF CALIBRE	AREA OF CALIBRE
12.5	40	132	276

The more we consider these facts, the more clear is it that they cannot be accounted for as merely mechanical results of diminution and increase of the pressure of the blood upon the arterial walls, in consequence of the different effects of gravity upon the fluid in the tubes in different positions. The arteries, in any given state of contraction of their transversely arranged muscular fibres, are by no means disposed to yield readily in the lateral direction to increase of pressure from within. This is evident from the fact that they are not increased in diameter by the successive strokes of the powerful muscular pump, the heart. The surgeon, when tying a large arterial trunk in its continuity, does not find, on clearing the vessel of its sheath with the point of his knife, that he is dealing with a body that swells at every pulse, but with one of unvarying dimensions. And, in the experiment on the metacarpal artery of the horse above referred to, no changes in the transverse measurements were noticed so long as the limb was maintained in any one position.† If any increase do occur in the diameter of an artery in systole, it is inappreciable by ordinary methods of measurement.

Yet, the increase produced in the pressure of the blood upon the arterial walls by the ventricular contractions is certainly very great.

* In truth, the increase of internal calibre in the dependent position is thus rather underestimated; because the tortuous form which the vessel then assumed implied a certain amount of increase in its length; so that the material composing its wall was thinned out, not only in consequence of lateral expansion, but also to some slight degree through longitudinal stretching.

† Professor A. V. Volkmann, in his valuable work, *Die Hæmodynamik*, relates experiments which he made by forcibly injecting water into portions of arteries removed from the body, proving that they yielded in the transverse direction even more than in the longitudinal to pressure from within. But Volkmann wrote before the discovery of the vaso-motor nervous system (he published in 1850), otherwise he would have been well aware that his experiments on dead animals left out of account altogether the most important element in this question, viz., the muscular contraction of the vessels as distinguished from mere elasticity. Volkmann admits that, when an artery is laid bare in the living animal, the only perceptible evidence of expansion of the vessel during pulsation is the tortuous form assumed by it at every pulse in consequence of longitudinal stretching, and that the transverse distension which he supposes to take place on theoretical grounds is inappreciable. Even in the dead vessel, he could not obtain evidence of transverse yielding by measurement with the compasses, but only by calculation from the observed increase in length which the vessel experienced when over-distended with liquid, together with the increase in the volume of the tube, as indicated by the additional weight of water which it admitted, *vide op. cit.*, pages 407, 422, and 423. The impressions conveyed to the finger in feeling the pulse, and also the indications of the sphygmograph, are no evidence of expansion in the cylindrical tube. In both cases, pressure is made upon it, producing a more or less oval condition of the transverse section of the vessel; and the impulse experienced is essentially the result of a tendency to restoration to the circular form as the blood-waves pass through the constricted part. A precisely similar pulse is produced in a cylindrical tube of inelastic but flexible material, through which a fluid is forcibly driven in a jerking stream. In an actual experiment made to illustrate this point, the inelastic tube was formed of a strip of thin mackintosh-cloth, with its edges stitched closely together. One end of this tube was connected with a piece of caoutchouc tubing, through which water was driven by a force-pump, while the other end of the inelastic tube was continued in another piece of caoutchouc tubing, the distal end of which had a somewhat narrow glass tube tied into it to simulate the resistance in the capillaries, and ensure a continuous though jerking stream by bringing into play the elasticity of the caoutchouc and of the air in the air-chamber of the pump. At every stroke of the pump, the finger, placed on the inelastic tube of mackintosh, experienced a sensation exactly similar to that in feeling the pulse. When the pulse is visible in the human subject, the appearance is unquestionably caused by the movements of the vessel, as it becomes alternately curved and straightened in systole and diastole.

When the coronary artery of the lip is divided in a child in the operation for hare-lip, the little fountain of blood that springs from the cut vessel may be sometimes seen to rise about twice as high in systole as in diastole, implying that even in so small a branch, in spite of the equalising tendency of the elasticity of the tubes of transmission, the systolic pressure of the blood is double the diastolic.

The tracings given by the recording hæmodynamometer, and reproduced in modern physiological works to illustrate the variations of the blood-pressure, though very interesting in some respects, are entirely untrustworthy as indications of the relative pressures of systole and diastole. For, in all such instruments, the apparatus opposes more or less resistance to the altering pressure; and time is required to overcome that resistance; so that rapid variations, such as those of the cardiac pulsations, are most inadequately represented.

With reference to the present inquiry, I was anxious to obtain definite information on this important point; and it occurred to me that this might be simply and surely done by making the blood write its own record, by means of a stream issuing from a minute orifice in a tube tied into an artery, the projected blood being allowed to fall upon a horizontal sheet of paper drawn smoothly past the animal. By such an arrangement, the effects of the varying degrees of pressure of the blood would be observed untrammelled by any resisting apparatus; and, as the range of projection is directly proportioned to the projecting force, a comparison of the distance to which the blood was thrown in systole with the distance in diastole, as indicated by the curve formed by the drops sprinkled on the paper, would afford perfectly trustworthy evidence of the relative amounts of systolic and diastolic pressure. In an experiment of this kind performed upon a dog under chloroform, but with the circulation very active, a curved glass-tube being tied into the external carotid, with an orifice only .0075 of an inch, so small in proportion to the calibre of the common carotid that the outlet could not materially affect the blood-pressure, the stream which issued from the tube, inclined at an angle of about 45 deg., was projected twice as far in systole as in diastole. And very similar results were obtained with a tube tied into the common carotid of a rabbit operated on without anaesthesia.* We are, therefore, not far wrong in considering the pressure of the blood upon the walls of a considerable artery as doubled, in normal circulation, through the contraction of the ventricle.†

Now, we have no reason to suppose that the elevation of the leg of a horse from the horizontal to the vertical position, or *vice versa*, would produce a greater difference than this in the pressure of the blood upon the arterial walls. The original manometrical experiment of Stephen Hales, though performed with comparatively rude apparatus, is strikingly illustrative of this point. Having tied into the femoral artery of a horse a brass tube adapted to a long one of glass held in the vertical position, he found that the blood rose in the glass tube to the height of 9 ft. 8 in. above the level of the left ventricle,‡ whereas the distance from the level of the heart to that of the metacarpal artery, where I operated, is only about four feet. That is to say, the column of blood from the level of the heart to that of the artery, when the limb was raised, had not half the height of that which the force of the heart was able to sustain in one of the main arterial trunks. Now we know that

* Dr. Burdon Sanderson has pointed out to me that I have not been the first to make the blood-stream write its own record upon paper. In 1874, which is before the date of the experiments mentioned in the text, Dr. Leonard Landois of Greifswald published, in Pfleger's *Archives for Physiology*, an account of what he terms *Hämatographie*, and gave photographic representations of tracings obtained by drawing a piece of paper past an animal in which he had opened an artery, so that the stream of blood was received upon the paper. These tracings are very beautiful, especially from the light they throw upon the phenomenon of dicrotism of the pulse and the respiratory curve, but they were not designed to estimate the relative force of systolic and diastolic pressure, and no inference can be drawn from them with reference to that question. Instead of having a tube tied into the artery with an orifice so small as to have no appreciable effect upon the tension of the blood in the vessel, as in my experiments, the stream was allowed to flow from the divided artery either directly or through a tube of uniform calibre inserted into it to prevent closure of the orifice; and instead of having the paper placed horizontally at a given distance from the artery, to show the range of projection, he held it perpendicularly to the stream, and no mention is made of its distance from the vessel.

† It may perhaps be objected, that the results of the experiments described in the text cannot be taken as a fair indication of the increased pressure upon the arterial walls in systole, because, in the actual circulation, motion of the blood is produced as well as tension, whereas these two effects are confused together in the experiments, inasmuch as the stream which issues from a tube tied into an artery is the result of the entire force of the heart. This would be a valid objection if the actual amount of the tension upon the vascular parietes were the object of inquiry, but it has no force whatever against an estimate of the relative amounts of the systolic and diastolic pressure; or, to speak more strictly, the increase of tension in systole is underestimated by the method of experimenting; because, when fluid is injected through a tube offering a certain amount of resistance to its passage (as is the case in the vascular system), an increase of the force with which the liquid is injected produces less proportionate increase of motion than it does of tension; and, therefore, a method of experimenting which represents the effects of the entire force of the circulation, without distinguishing between motion and tension, underrates the increase of tension at the higher pressures.

‡ See *Statistical Essays*. By Stephen Hales, D.D., F.R.S. 1769.

the pressure of the blood is not materially less in arteries of secondary dimensions, like the metacarpal, than in the main trunks.* And therefore the raising of the limb into the vertical position could not, as the mere result of the action of gravity upon the blood, diminish the pressure of the fluid upon the arterial wall by as much as one half; or, conversely, the putting the limb down again could not do so much as double the pressure of the blood upon the vessel.

Seeing, then, that the doubling of the pressure which results from the contraction of the ventricle has no appreciable effect upon the diameter of an artery, it appears clear that the great alterations in the size of the metacarpal artery of the horse which resulted from varying the positions of the limb cannot be explained on mere hydrostatic principles, and that, in order to account for them, we must admit contractions and relaxations of the muscular coat of the arteries in obedience to nervous action.

That the force of the heart is amply adequate in the human subject to drive the blood freely through the vessels of the distal parts of a limb, in spite of the elevated position, provided that the arteries are relaxed, I shall now have the means of plainly demonstrating. The constricting band has been on this man's arm for eight minutes; yet, thanks to its efficacy as a tourniquet, the limb remains as pale and corpse-like as when the application was made, although he has kept his hand down in the interval. I shall now ask him to raise the hand again to the utmost degree; and, while it is so placed, I shall remove the elastic bandage. This having been done, you observe that the skin of the hand is beginning to show patches of redness; and now, a few seconds more having elapsed, even the finger-tips, as well as the rest of the limb, are of florid red hue. The veins, you see, remain collapsed, the blood being continually drained out of them by the action of gravity; but the arteries, in spite of that action, instead of being empty or nearly so, as they were when the limb was previously elevated, are distended even to the remotest capillaries. He will now raise the other hand, and you observe the extraordinary contrast between the two limbs, both in the elevated position; the hand last raised becoming as pallid as the other did before the elastic band was applied.

It is worth while to consider shortly how it is that the constricting bandage gives rise to arterial relaxation. It is well known that troublesome after-bleeding not unfrequently follows the application of Esmarch's bloodless method. This is commonly attributed to a temporary paralysis of the vaso-motor nerves, in consequence of the pressure to which they have been subjected. Now, in the experiment which you have just witnessed, one of the two factors in Esmarch's method which might be supposed to have such an effect—viz., the tight elastic bandage, applied from the distal extremity upwards, and temporarily compressing the branches as well as the trunks of the nerves—is absent; so that we have merely to consider the effect of the constricting band upon the nervous trunks at the root of the limb. It is no doubt true that in the arm, where the soft parts are in small proportion to the cylindrical bone, if, instead of an elastic bandage, an India-rubber tube be employed as the constricting agent, in accordance with Esmarch's original proposal, the concentrated pressure so exerted may, in case of a protracted operation, lead to paralysis of a very troublesome if not serious character, both of motion and sensation. But if we follow Esmarch's more recent advice, and diffuse the pressure by using the broad bandage, no such effects are observed. I have tried the experiment upon myself, and I have found that the bandage, applied sufficiently firmly to arrest all circulation and sufficiently long to produce the after-blush in the raised limb, did not affect in the slightest degree either the sensation of my hand or the motor power of the forearm. And as there is no reason whatever for believing that the vaso-motor fibres in the trunks of the nerves are more likely to suffer from compression of those trunks than the sensory and voluntary motor fibres, the theory of paralysis from compression falls to the ground. That which seems to me to be probably the true explanation is, that when a part has been deprived for a while of circulation, the want of the vital fluid creates in the tissues a demand for a supply of it, and that this demand operates upon the vaso-motor nervous apparatus of the limb as a stimulus inducing arterial relaxation, in a manner perhaps analogous to that in which the "besoin de respirer," as the

* Poiseuille, the inventor of the mercurial hæmodynamometer, believed, as the result of his experiments, that there was no appreciable difference in blood-pressure between vessels of much greater divergence in size. Thus, in one of his experiments (see Poiseuille, *Recherches sur la Force du Cœur*, etc., 1828, p. 36), he found that the mercury rose to the same height in the hæmodynamometer in the carotid of a horse as in a muscular branch of the femoral only two millimètres in diameter, whereas the metacarpal artery on which I operated had fully two and a half times that dimension. Marey, indeed, states (*Circulation du Sang*, 1863, p. 150) that the mean tension of the arteries decreases as the vessel is situated further from the heart, and as its calibre diminishes. But, though this is no doubt strictly true, yet it is allowed by physiologists that "the fall" in pressure "is a very gradual one until the smallest arteries are reached."—(Michael Foster, *A Text-book of Physiology*, second edition, 1878, p. 102.)

French have termed it, produces a stimulus to the respiratory nervous system. We know that in the case of the arteries different stimuli produce different effects; cold inducing reflex arterial contraction, while heat occasions, equally through the nervous system, a relaxation. According to this view, then, the necessity for circulation, if I may so express myself, is a stimulus to dilatation which, when sufficiently urgent, overpowers the stimulus to contraction occasioned by the diminution of pressure upon the vessels in the elevated position. And, as a matter of fact, we find that the after-blush is greater the longer the time during which circulation has been arrested, although without anything to indicate nervous paralysis. In complete harmony with this view are the phenomena observed in a limb after ligation of its main artery. When the femoral is tied for popliteal aneurism, the first effect upon the foot is pallor and coolness; but, after the lapse of some hours, the converse condition of abnormal redness and heat supervenes. Here there is certainly no interference with the nerves; but the usual supply of blood is in the first instance notably diminished; and, as a consequence of this, long before there has been any possibility of organic increase in the vessels, the anastomosing branches become so much dilated as to more than compensate for the obstruction of the principal source of supply. This effect can only be brought about through the nervous system; and the most natural explanation seems to be that deficient circulation in a part continued for a considerable time comes to operate as a stimulus to arterial relaxation.

I have now to mention an experiment which anyone may easily perform upon himself, but which, though extremely simple, is not on that account the less instructive. But first let me state the considerations that led me to it. If the contraction of the arteries of an elevated limb were really the result of an action of a particular part of the nervous system, it might be expected that, on the cessation of the stimulus that evoked it, an unusual relaxation would ensue, corresponding with a period of repose of the nervous apparatus concerned; and that this would be more marked the greater and more protracted had been the effort. Supposing, then, that the hand were raised after the circulation had been brought into full activity by brisk exercise, with the heart working powerfully and the arteries generally in a state of considerable dilatation, if, in spite of these unusual obstacles to arterial contraction, pallor of the limb should result from the elevated position, it might be anticipated that, when the hand was again lowered, it would not only resume its former redness, but acquire for a while a deeper tint than the other, which had been kept dependent throughout. My first trial of this kind was made just after I had been walking with great haste to catch a train, when my heart was beating with unusual vigour, and my hands of florid colour. Having raised my left hand, I saw it become, within half a minute, very pale, and on putting it down after it had been a minute in that position, I observed it grow, within a quarter of a minute, much deeper in arterially red tint than the right, a difference which gradually passed off, so that, in the course of one minute and three-quarters, the hands were again of equal colour. Two minutes later, I repeated the experiment, and this time kept the left hand raised for two minutes, and then, on lowering it, found it to become in ten seconds much redder than the other, which had been suspended the whole time; and, just as might have been expected after the more protracted action of the nervous apparatus, the repose was longer in duration, so that, even after two minutes and twenty seconds, when I was obliged to start for the train, the left hand was still slightly the redder of the two. Now such a result as this was entirely contrary to what could be explained as a consequence of mere hydrostatic laws. If the arteries had been simply emptied in the elevated position by the force of gravity, all that could have resulted on restoring the limb to the dependent posture would have been a return more or less rapidly to the previous condition of vascular fulness. And it is an interesting fact that the veins, though comparatively thin-walled, and much more readily distensible than the arteries, do not at once recover their former size when the elevated limb is lowered, but remain for a while markedly less turgid than those of the other hand, even when, through arterial dilatation, the colour of the skin is not only more florid but manifestly darker. Hence this apparently trivial experiment, if duly considered, seems to me of itself sufficient to prove the truth of the doctrine for which I am contending.

But what may perhaps be regarded as the most conclusive piece of evidence in its favour remains to be related.

[To be concluded.]

PRESENTATION.—Mr. R. Richardson has been presented with a handsome silver salver and tea and coffee service, upon the twenty-fifth anniversary of his residence in Rhayader, in recognition of his public and professional services.

REMARKS

ON FORCIPRESSURE AND THE USE OF PRESSURE- FORCEPS IN SURGERY.

By T. SPENCER WELLS, F.R.C.S.,

Surgeon to the Queen's Household; Consulting Surgeon to the Samaritan Hospital.

IN the BRITISH MEDICAL JOURNAL of January 10th, 1874, among valuable reports from metropolitan hospitals on the use of torsion in surgical operations, the following passage may be found, reprinted exactly as it stands at page 47, except that I have underlined three sentences, namely, "temporary stoppage of bleeding vessels during operations"; "the bleeding vessel is forcibly compressed, and its coats squeezed or almost crushed together"; and "simple compression with the torsion-forceps is sufficient".

"Mr. Spencer Wells (Samaritan Hospital) has introduced a kind of artery and torsion forceps, which very conveniently replace the old spring artery-forceps of Liston, and the bull-dogs used for the temporary stoppage of bleeding vessels during operations, while they are the most readily applied of any of the varieties of torsion-forceps met with in the shops. They are represented in this sketch, and were first

made for Mr. Wells by Krohne and Sesemann. The grasping and holding extremity is roughened by rather deeply cut transverse teeth, so that the bleeding vessel is forcibly compressed, and its coats squeezed or almost crushed together. This is alone often sufficient to stop the bleeding without any torsion, especially if the instrument be left on the vessel for a minute or more. But if the vessel be large, then two or more rotations may be added. Instead of the spring-catch, the fasten-