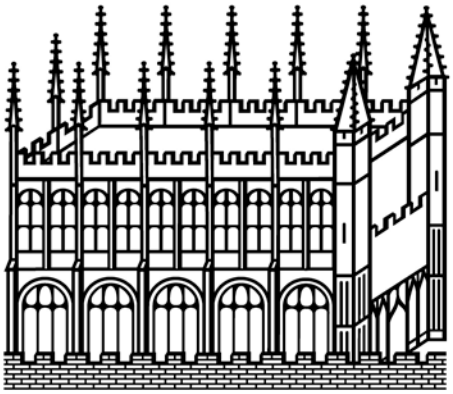

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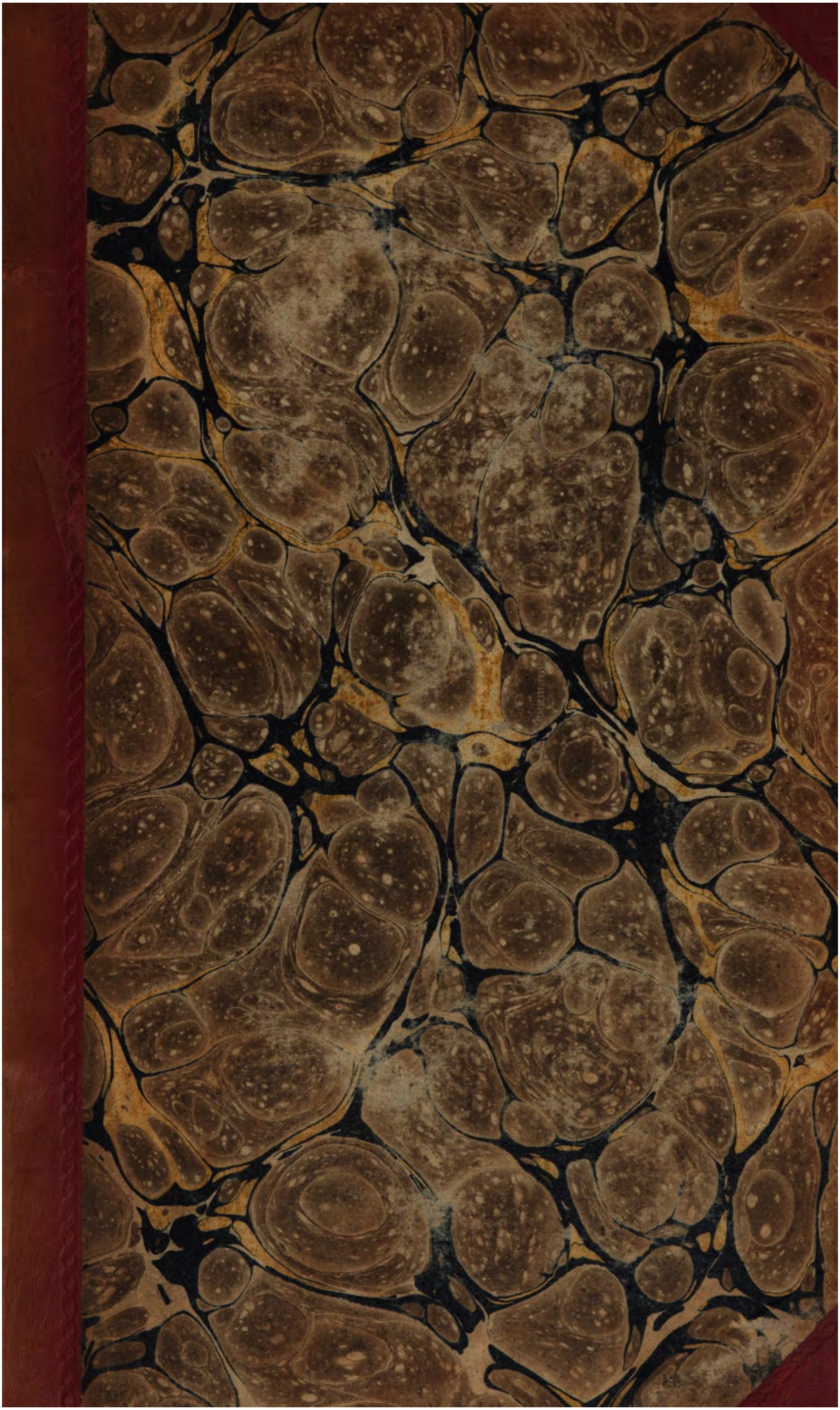

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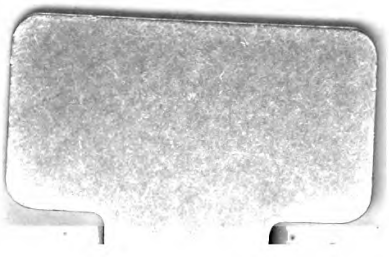


EXPERIMENTAL RESEARCHES

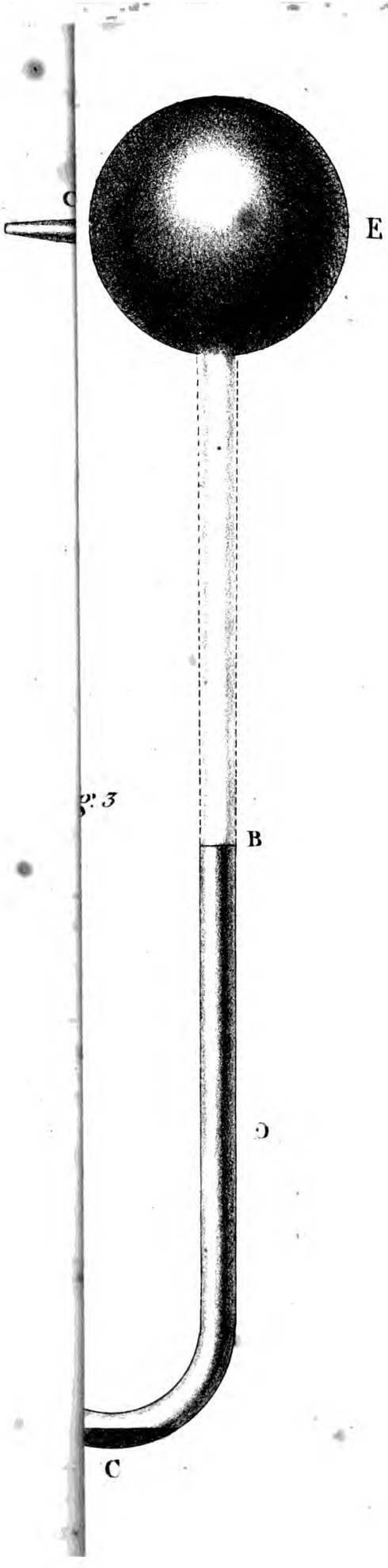
ON THE INFLUENCE OF

ATMOSPHERIC PRESSURE

UPON THE

BLOOD IN THE VEINS,

&c. &c. &c.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EXPERIMENTAL RESEARCHES

ON THE INFLUENCE EXERCISED BY

ATMOSPHERIC PRESSURE

UPON THE

PROGRESSION OF THE BLOOD IN THE VEINS,

UPON THAT FUNCTION CALLED

ABSORPTION,

AND UPON

THE PREVENTION AND CURE OF THE SYMPTOMS CAUSED BY THE BITES

OF

RABID OR VENOMOUS ANIMALS.

(DEDICATED BY PERMISSION TO HIS MAJESTY.)

WITH

AN APPENDIX,

CONTAINING

THE ORIGINAL REPORTS OF BARON CUVIER AND OF PROFESSORS DUMERIL AND LAENNEC, TO THE ROYAL INSTITUTE OF FRANCE, AND TO THE ROYAL ACADEMY OF MEDICINE OF PARIS, &c. &c.

BY

DAVID BARRY, M.D.

KNIGHT OF THE ORDER OF THE TOWER AND SWORD, MEMBER OF THE ROYAL COLLEGE OF PHYSICIANS IN LONDON, FIRST SURGEON TO THE PORTUGUESE ARMY, SURGEON TO THE FORCES, *&c. &c.*

" Habemus etiam et vivaria pro bestiis et avibus omnigenis,—
Experimentum etiam sumimus super illas venenorum omnium
et antidotorum—Ut corpori humano melius caveamus."—
BACON. *Nova Atlantis.*

LONDON:
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DEDICATION.

TO THE KING'S MOST EXCELLENT MAJESTY.

SIRE,

If to the proud consciousness of being one of that people which your Majesty's wisdom has rendered so pre-eminent in Arts, in Arms, and in Commerce; the work now humbly offered to your Most Gracious Majesty's protection, shall entitle its author to add that of having diminished the amount of human evils by increasing the stock of human knowledge, the highest ambition will be gratified, and the utmost labour rewarded of,

Sire,

Your Majesty's

Most humble Servant,

And dutiful Subject,

DAVID BARRY,

Surgeon to Your Majesty's Forces.

PREFACE.

THE first of the two Essays contained in this volume is composed from Notes of a Memoir *On the Motion of the Blood in the Veins*, which I had the honour of reading before the Academy of Sciences at Paris, on the 8th of June, 1825—and to it is annexed a translation of the Report made to the Institute of France, by Baron Cuvier and Professor Dumeril, who were appointed to witness a repetition of the experiments, and to examine the said Memoir.

The Second Essay, a corollary to the first, had for its basis a short note read by me before the French Academy of Medicine, on the subject of EXTERNAL ABSORPTION.

And the Appendix contains copies of certain original documents referred to in the preceding pages.

I feel that it would be quite impossible for me to give an adequate notion of the liberality and disinterested kindness with which the leading men of science in Paris received my communications, or of the facilities which they afforded me not only to bring them forward, but to prosecute

and repeat the inquiries and experiments necessary to their elucidation.

After I had read the first Memoir, Professor Laennec procured for me the permission, as well as the entire means, to repeat my experiments at the School of Medicine. In him I found the assistance of consummate anatomical and physiological science, with the high-minded zeal of liberal friendship.

M. Dumeril also, in the handsomest manner, furnished me with the means of again repeating the experiments in presence of himself, the Baron Cuvier, Professors Laennec, Cruvelhier, Billery, Breschet, Edwards, and many other distinguished persons.

Baron Cuvier, to the honour he conferred upon me by being present at my experiments, generously added that of placing at my disposal every thing necessary to prosecute my investigations at the Royal Garden of Plants, an establishment which has already ensured with posterity immortal honour to him, and to the nation of which he is so distinguished an ornament.

The letter of M. Girard*, director of the Veterinary School at Alfort, will show with what noble zeal the science of physiology is cultivated in France. In England, on the contrary, an outcry has been raised of late, not entirely unsup-

* See Appendix, No II.

ported by some leading professional men, against every thing like inquiry, having for its basis *direct experiment upon living animals.* Yet the little that we know of the laws of life is drawn from this source alone.

The examination of a quiescent machine can only *suggest* the use of its parts when they were all in movement. Well-directed experiment upon these same parts, actively employed in fulfilling their various functions, either confirms the suggestion, giving it the validity of a law, or at once destroys the whole fabric of a baseless theory.

" *Unicum sæpe experimentum, integrorum annorum laboriosa figmenta refutavit* *."

The wisest and the most virtuous men of the ages they lived in spent a large portion of their time in making experiments upon living animals. Those of Harvey were honoured by the presence of his sovereign, who, by that act alone, would have been entitled to a share of the immortality gained by the illustrious discoverer of the circulation †.

Those who have stated that Harvey made but few experiments, and that to these few we owe

* Haller, Tom. i. Præf.

† " In jugulari venâ internâ denudatâ damæ vivæ (coram multis nobilibus, et rege serenissimo domino meo, assistentibus) per medium divisa et abrupta," &c.—HARVEY, *De Circ. Sang.*

but little, should have read his works. In these they would have learned, that an unlimited supply of animals was placed at his disposal, by the enlightened prince to whom he was physician. His own words are singularly applicable to these candidates for unscientific popularity,—"*Qui nihil nisi homines secant.*"

Bacon (Lord Verulam), whose stupendous powers of mind have never, perhaps, been equalled, in drawing a picture of what an institution or university ought to be, in order to afford the fullest opportunities for acquiring useful and exalted knowledge, puts the following words into the mouth of one of the *patres domûs Salamonis,* in his *Nova Atlantis.*

"Habemus etiam septa et vivaria pro bestiis et avibus omnigenis, quibus, non tam propter novitatem et raritatem, quam ad dissectiones et experimenta anatomica utimur; ut ab iis, quid fieri possit circa corpus humanum lucem accipiamus. Veluti vitæ, in iis continuationem, licet nonnullæ partes quas vos pro vitalibus habetis perierint, aut extractæ fuerint. Resuscitationem nonnullorum, quæ specie tenus, mortuæ erant. Experimentum etiam sumimus super illas, venenorum omnium et antidotorum, et aliorum medicamentorum, tam chirurgicorum, quam medicinalium, ut corpori humano melius caveamus."

Haller, who was a senator and a minister in his native country, and not less remarkable for the

benevolence and integrity of his character, than for his profound researches into the laws of life, says, in the preface to his great work on Physiology, " Dissecanda ergo animalia, verum minime sufficerit cadavera dissecuisse, viva incidisse necesse est. A cadavere motus abest, omnem ergo motum in vivo animale speculari oportet; sed in motu animati corporis interno et externo, tota physiologia versatur. Ergo ad sanguinis circuitum, ad ejus subtiliores motus perspiciendos, ad respirationem, ad intestinorum reptatum, ad chyli iter intelligendum, absque vivorum animantium strage, nihil omnino profici potest."

They who inveigh most loudly against experiments upon living animals, and who affect an excess of sensibility, have never made any experiments themselves. They are contented with the exposition of what they, in their wisdom, suppose nature *ought* to do, instead of investigating what she actually does.

Others talk of needless cruelty. If any useful knowledge is to be obtained by an experiment, none of the means necessary to arrive at this knowledge can be needless, and none else can be adopted without defeating the purpose aimed at; therefore, in useful experiments, there never is needless cruelty, or, in other words, unnecessary pain inflicted.

When medical men are praised at public meetings, and their letters there read with applause,

in which they profess the determination, neither to open the living book of animal nature themselves, nor permit it to be opened by the youth committed to their charge, our best feelings are allowed to take a very wrong direction. There are those, however, who have had the candour and the honesty to assert in the face of this vulgar clamour, that we have as good a right to make animal life subservient to the increase of our useful knowledge, as of our bodily strength and amusements. This is plain common sense, and must in the end prevail. One word as to the essays and experiments.

A vague unauthenticated notion, that the return of the black blood to the heart is, in some undefined way, *influenced* by suction, may be traced as far back as the time of Harvey. Haller, and many others also, noticed a marked coincidence between the respiratory movements of the thorax in the warm-blooded mammalia, and the motion of their venous blood. But the mechanism was never pointed out, by which nature, in these animals, applies the mighty agency of atmospheric pressure to the veins, and connects, as cause and effect, the expansion of the chest with the afflux of the centriptal fluids to the heart. The experiments, therefore, that demonstrate this mechanism, and supply these important desiderata in physiology, must be entitled to the meed of novelty, along with whatever other merits they may possess.

The first experiment, p. 10, however nearly it might have been approached, was never made; that upon the pericardium, p. 19, was never even imagined by any man living or dead, before me. Whether my conclusions be just or new must be soon decided—in the mean time, as the most intense power of the reasoning faculties of man can never arrive at a discovery so perfectly original, as to be entirely unconnected with every thing that was known or suggested before, I shall reply to those who deny the originality of my researches in the words of the great Haller.

" Præterea æquo animo oportet expendisse, non eum verum inventorem esse, cui vaga aliqua cogitatio elapsa est, in nullo fundata experimento, sed eum omnino eam laudem mereri, qui verum ex suis fontibus, per sua pericula, suasque meditationes, eruerit, et adeo firmis rationibus stabiliverit, ut veri cupidos convincerit."—*Haller*, tom. i. lib. 3.

D. Barry.

Paris, March 24, 1826.

ERRATA.

Page 19, line 15, *for* *xy*phoid *read* *xi*phoid.
30, „ 11, *for* cav*as* *read* cav*æ*
37, „ 14, *for* subclavi*on* *read* subclavi*an*.
141, „ 8, osse*sus* *read* osse*ous*.

N. B. The details introduced into the text at p. 28, relative to the experiments performed at Professor Coleman's on the 10th of February last, were intended for, and ought of course to have appeared as a *note*, as they formed no part of the original Memoir.

PART I.

MEMOIR ON THE MOTION OF THE BLOOD IN THE VEINS.

Read by the Author, before the Academy of Sciences, on the 8th of June, 1825, at the Institute of France.

OBJECT OF THE MEMOIR.

THE object of the following Memoir, is to demonstrate by proofs, drawn from the anatomical structure of animals and from direct experiment,

First. The powers by which the blood is propelled through the veins to the heart.

Secondly. The comparative velocity with which it is moved through the veins, and through the arteries.

Thirdly. That the constant supply of blood to the heart, cannot depend solely upon the causes to which it has been hitherto ascribed.

What is the amount of all that has been hitherto proved, relative to the Circulation of the Blood?

We owe to the sagacity of the immortal Harvey our knowledge of this incontrovertible fact, that the blood in the living animal is in constant circulation from the ventricles through the arteries and veins, to the heart again, where it is to receive a fresh impulse.

Harvey not having been able, either by dissection or experiment, to discover any other power actively and constantly employed in propelling the blood along this course, assigned the whole task to the heart alone. The reasonings and the experiments which he adduced in illustration of this doctrine, clearly prove that the circulating current takes the direction which he had already pointed out, but certainly do not rigorously demonstrate that the heart is the sole impellent power.

Later physiologists have done but little to shew either the truth or the error of Harvey's assertions. They have merely admitted a few secondary sources of impulse to the blood; such as—

1. The contractile power of the arteries, whether the effect of muscular or elastic fibres. 2. The insensible contraction of the capillaries, supposed to be independent of the heart. 3. The action of the veins themselves upon their contents. 4. The pressure of muscles of voluntary and involuntary motion.

Of these supposed powers, some are so little susceptible of being demonstrated by direct experiment, others must be so uncertain in their operation, and the theories which they have been brought to support are so opposed to each other, that the evidence against is, *à priori*, nearly as strong as that in favour of their existence.

The supposition that the cavities of the heart possess the power of dilating themselves, and therefore of acting alternately as suction and forcing-pumps, although adopted by some existing physiologists, has hitherto derived but little support either from anatomy or experiment. *This opinion was too trite, even in the days of Harvey,

* "Neque verum est similiter quod vulgo creditur, cor, ullo suo motu aut distensione, sanguinem in ventriculis attrahere, dum enim movetur expellit, &c.—HARVEY *de Motu Cordis*, cap. ii.

to merit serious refutation. Neither the auricle nor the ventricle appears to be furnished with any intelligible muscular apparatus, by which either can accomplish its own dilatation. Every thing we find in them seems evidently calculated to favour their contraction.

The doctrine of the active resiliency of the lungs, tending constantly to leave a vacuum between their surface and the parietes of the thorax, and thereby assisting to bring uninterrupted atmospheric pressure upon the blood in the veins, was, I believe, first broached by Dr. Carson, of Liverpool, in 1815. Being, however, purely theoretical, and unsupported by direct experiment, it seems, notwithstanding its ingenuity, to have made but little impression, for although published now ten years, I do not find it alluded to in the lectures or the writings of the French physiologists.

The amount, then, of all that has been hitherto proved, and of which there is any thing like material evidence relative to the circulation of the blood, may be found in this short sentence, written nearly two hundred years ago.

"*Necessarium est concludere, circulari quodam motu, in circuitu agitari in animalibus sanguinem, et esse in perpetuo motu, et hanc esse actionem sive functionem cordis, quam pulsu peragit.*"—HARVEY, De Motu Cordis, cap. xiv.

ATMOSPHERIC PRESSURE.

Arguments drawn from Anatomy.

I had long remarked in every thing I heard or read on the circulation of the blood, that the pressure of the atmosphere was either entirely left out in the enumeration of its causes, or considered as merely a secondary agent. This appeared to me the more extraordinary, from the effects of pressure being so striking, when acting upon liquids moving in tubes. Harvey does not even allude to such a cause; and Haller, in speaking of the pulmonary circulation says, that* the pressure of the air may be passed over in silence. It seemed to me, however, impossible, that the alternate expansion and contraction of the thoracic cavities should not affect the contents of the

* Ut pressio aeris pro nulla potest haberi. (Haller, loco citato.)

great veins opening into them, in the same manner as the expansion of a pair of bellows would the contents of flexible tubes, in communication with their cavities; I reasoned thus:—

The right and left cavities of the thorax have within them each a lung or bag, divided into a greater or lesser number of distensible cells, communicating with one another, and with a common tube, the trachea. When the chest is enlarged by the act of *inspiration*, air rushes in through this tube, to distend the air-cells, and force them to occupy that space, in which the expanding parietes of the thorax tend to leave a vacuum. But as it is evident that the air would follow the expanding sides of the chest much more readily, if there were no cells to be distended, and as it is an unalterable law, that all liquids in communication with an enlarging cavity will be pressed towards it, if exposed at the same time to atmospheric influence; it became presumable that blood would be forced into the thorax through the cavæ during inspiration.

Having once caught this view of the part which respiration might probably bear in the circula-

tion, particularly of the venous blood, several known facts presented themselves in support of its correctness, *viz.*, the swelling of the external jugular veins during *expiration*, and their immediate collapse upon *inspiration*. The checking of certain hæmorrhages by forced inspirations; the fatal accidents that have been known to follow the opening or the dividing large veins, and above all, the situation of the heart itself, placed in the centre of the chest in a bag, at all times too large for its volume, and which seems not only protected from direct atmospheric pressure, but is probably enlarged in all its diameters by the act of inspiration.

Upon turning my attention more particularly to the anatomy of the thoracic viscera, I was struck with the analogy which I thought was observable between the mechanism of the heart, pericardium, and mediastinal pleuræ, as resembling a pair of bellows, and that of either lung within its proper cavity, compared to the same instrument.

The situation of the fibrous bag of the pericardium in the human subject, and the covering which its lateral surfaces receive from the medi-

astinal pleuræ, reflected over them from the roots of the lungs behind, and from the sternum and ribs before, are well known to all anatomists. When the lungs are expanded, their surface is necessarily enlarged. When the ribs carry forward the sternum, and when the diaphragm presses down the abdominal viscera, the internal surface of the thorax is *also* enlarged; consequently the pleuræ covering these surfaces is put upon the stretch, and that portion covering the pericardium on either side is pulled upon at its margins on both sides, in the directions best calculated, not only to protect the fibrous bag from pressure, but to enlarge its cavity throughout.

The motion of the sternum during inspiration, tends to bring the anterior surface of the pericardium forwards and upwards. The synchronous movement of the diaphragm tends to enlarge it downwards, and to complete the analogy. As each lung is furnished with a pipe, through which it receives and discharges air, so is the heart, with its receiving pipes (the veins), and its discharging pipes (the arteries), through which it receives and discharges blood.

But as the aorta, the great discharging pipe of

the heart, is equally employed during both stages of respiration in sending blood out of the thorax, it seemed probable (if my reasoning with regard to the effect of inspiration upon the blood of the cavæ were well founded,) that enough of blood should be brought into the chest during its expansion alone, to supply the discharging tubes during a whole act of respiration. Thus the necessity of a reservoir became evident, into which this blood might be drawn by the expansion of the three thoracic bellows. Having, by these arguments and others now unnecessary to be recapitulated, brought my hypotheses thus far, I came to the following presumptive conclusions.

1. That a liquid such as water in an open vessel, being by means of a tube placed in direct communication with the cavity of one of the great veins within the thorax of a living animal, would be forced by atmospheric pressure to rise in the tube, and that the motion of the liquid within the tube would be regulated by the respiratory movements of the animal.

2. That the same phenomena would be exhibited by establishing the same communication between

the liquid and any of the cavities around the vein.

The consideration of the pulmonary venous circulation I deferred altogether, until I should have ascertained by experiment, whether my theory with regard to the effect of atmospheric pressure upon the blood of the cavæ were likely to prove correct. I accordingly planned and executed the following experiment.

First Experiment.

Having first ascertained upon the dead horse, that a tube of proper size and length might be readily introduced down the jugular vein, as far as the anterior cava, I proceeded thus—

On the 16th October, 1824, I selected a horse condemned to be destroyed on account of an incurably diseased hoof, but sound in every other respect. The animal having been thrown upon his right side, I laid bare his left jugular vein, tied it below its middle, and about an inch below the ligature introduced into its cavity, in a direction towards the heart, a large-sized flexible catheter, having a spiral glass tube fitted into its

outer end*. The rounded point of the catheter was cut off above the lateral openings. The diameter of its bore was about $\frac{3}{16}$ of an inch, its length $10\frac{1}{4}$ inches. The diameter of the spiral tube at A. was $\frac{1}{8}$ of an inch, at C. it was something less. The length from B. to C. four inches.

When the horse was thrown, his breathing became almost entirely thoracic; the rising and falling of his ribs could be readily and distinctly counted. The respiration was also audible. The catheter having been pushed towards the heart as far as it would go, a ligature, which had been passed under the vein a little below the opening made to admit the catheter, was firmly knotted round both.

The point C. of the spiral tube, over which I had hitherto held my finger, was now immersed in a cup of water deeply coloured by a solution of common Prussian blue. The moment that I removed my finger, the blue liquid rose through the spiral, and flowed rapidly towards the heart. The sun happening at the moment to shine

* Plate, Fig. 1.

strongly on the tube, I saw, in the most satisfactory manner, the undissolved particles of blue pass up from the cup and round the spiral during *inspiration*, and halt or return slowly towards the cup during expiration. Not a drop of blood was seen to enter the tube, but bubbles of air sometimes appeared upon the surface of the liquid in the cup during expiration. The breathing being audible, allowed me to keep my eye steadily fixed upon the motion of the liquid, and to ascertain, beyond all possibility of deception, that this motion was entirely dependent upon the movements of respiration.

My very ingenious friend, Dr. Macann, to whose suggestions and assistance I am largely indebted, being stationed on the opposite side of the horse's neck, where he had not so good a view of the tube, by placing himself close to me, soon became fully convinced that the blue liquid moved upwards through the spiral in exact correspondence with the *inspirations*, and halted or returned towards the cup with the expirations.

To vary the proofs of this wonderful coincidence between the movements of the blue liquid

in the tube and the respiration of the animal, I withdrew the point C. from the liquid in the cup for a moment during *inspiration*, so as to admit one or two bubbles of air, and returned it again immediately. A space more or less extensive of the tube became thus transparent. Upon the next inspiration these bubbles were forced round the spiral with considerable velocity, and the whole tube again became uniformly blue by the ascent of more liquid from the cup. This part of the experiment, several times repeated, invariably afforded the same results.

A considerable quantity of cold water and also of air had now been forced into the vein and thence to the heart. The animal gave strong indications of suffering, and as the fact that inspiration produces a relative vacuum within or around the anterior cava was considered as fully established, the experiment was discontinued. I forgot to mention that towards the latter part of the experiment, when the animal's respiration became hurried and irregular, blood appeared in the tube on two or three occasions during *expiration*. The next inspiration, however, invariably restored the blue liquid to its place.

During the various trials and repetitions of this experiment which I made upon horses, I had occasion to remark, 1. That when the animal was standing, although the coloured liquid invariably rose in the tube, atmospheric pressure was never so distinctly marked as when he was prostrate. This I proved by experimenting upon the same animal in both positions.

2. That the connexion between the motions of the liquid in the tube and the respiration cannot be satisfactorily observed while the horse is standing, because his breathing when in the erect posture, and at rest, is scarcely, if at all, perceptible.

3. That when the respiration became hurried from whatever cause, or when it was embarrassed by disease, there was frequent regurgitation of blood through the tube, but never once did this occur except at the moment of expiration, and never under any circumstance did the liquid ascend in the tube, except at the moment of inspiration. This experiment, repeated upon the anterior and posterior cavæ of dogs, afforded similar results.

Here it is essential to remark, that if the com-

municating tube be introduced into the femoral vein of a dog or horse, and pushed no further towards the heart, inspiration will produce no effect upon the liquid in the cup, because the relative vacuum of the thorax can be filled up from the other veins of the animal's body, which will require a weight of atmospheric pressure to send forward their contents, *less* than would be necessary to force up the blue liquid, by the sum of all the secondary powers, such as contractility, *vis à tergo, &c.* The influence of the atmosphere invariably moves that first which requires the least pressure.

Considering the correctness of my first presumptive conclusion to be sufficiently established, I proceeded to put the second to the test, by the following experiment, calculated to ascertain the effect which a direct communication with the thoracic cavities around the cavæ would have upon a liquid, circumstanced as in the last experiment.

Second Experiment.

I introduced into the thorax of a dog near the median line and on each side of the posterior extremity of the sternum, a metallic tube, pointed like a writing-pen. The animal being placed upon his back the tubes were directed downwards and forwards parallel to the mediastinal pleuræ, which in the dog, in this position, suspend the pericardium from the sternum. To the external extremity of each tube was attached a small caoutchouc bag filled with a composition of lard and wax, and pierced at its bottom by a small hole.

As soon as the point of the tube had penetrated the pleura, I took a small flexible catheter, having at one end the barrel of a quill, in the side of which I had made a cut to act as a valve, opening readily from within outwards, and shutting in the contrary direction by its natural elasticity. The catheter thus armed, I passed into the hole in the caoutchouc bag, through the metal tube and into the chest. The little bag was attached to the margins of the wound by suture. This being done on both sides of the sternum, I next fitted

to the outer end of each catheter which had been hitherto plugged, a spiral glass-tube*, one end of which was already immersed in a coloured liquid. The communication being thus complete on both sides, the liquid rose rapidly through the spirals and flowed into the chest during inspiration, and remained stationary or fell during expiration. The movements of the liquid in the tubes were so regular, and so completely dependant upon the respiratory movements of the animal, that the one might be counted whilst observing the other. During inspiration I admitted into the glass-tube bubbles of air and small portions of the blue water alternately, so as to make the ascending column resemble a string of coloured beads, which played up and down through the spirals, particularly towards the latter part of the experiment, marking in a beautiful and striking manner the stages of the animal's respiration.

Two other metallic tubes, similar to those already described, were passed into the chest at two distant points, with the intention that the

* See Fig. 1.

openings of the catheters should be placed between the pleura costalis and the lung on either side; but having operated too near the diaphragm, one of the catheters passed between it and the stomach and liver, the other between it and the posterior surface of the lung. No motion whatever was observed in the liquid communicating with the abdomen, whilst that of the liquid communicating with the anterior surface of the diaphragm was precisely similar to the movements noticed in the other tubes.

Before the dog was destroyed, a stop-cock was fitted into his trachea, so as to command his respiration. When the stop-cock was shut, and the animal made powerful efforts to inspire, the blue liquid flowed upwards through the spirals with much greater force and rapidity than when the passage of the air through the windpipe was unobstructed. The ends of the two catheters that had been first introduced were found, when the body was opened, one on each side of the pericardium, between it and the concave surface of the lung, which had not suffered the slightest injury.

Third Experiment.

A similar communication still remained to be established with the bag of the pericardium, but hitherto in all the trials which I had made upon the dog the cavities of the heart had been penetrated, and the results of the experiments thus rendered inexact. The long and delicate connexion between the pericardium and sternum in this animal, added much to the other difficulties. The pericardium of the horse I found to be the most favourably circumstanced for my experiments. In this animal it is attached to the periosteum of the upper surface of the sternum from the fourth rib backwards, extending its adhesion posteriorly to the base of the xyphoid cartilage, from whence it turns sharply upwards and forwards behind the heart to be attached to the lower surface of the posterior pulmonary veins. By dissecting up the point of the xyphoid I was able to pass a pointed tube along its upper surface, through the lower margin of the diaphragm, and into the pericardium at its posterior and in-

ferior angle, without penetrating the peritoneum. The tube was armed with a caoutchouc bag as in the last experiment. Through this bag I passed a flexible catheter into the tube nearly to its point. Thus when the pericardium was penetrated, the catheter could be pushed in immediately, and to any length, so as to prevent the heart from being wounded by beating against the point of the tube.

In all the cases in which I succeeded in establishing a communication between the bag of the pericardium exclusively and a coloured liquid, the fluid rose in the tube as rapidly as in the former experiments, and, in all but one, its motion upwards was governed by the animal's inspirations. In all, however, with the exception of this single case, although the liquid invariably halted or descended during expiration, there was an oscillation of the fluid upwards, which seemed independent of respiration, but could not be observed during inspiration, because then it was confounded with the general motion of the liquid upwards. This third movement was acknowledged by my friend Mr. Bennett, an anato-

mist and physiologist, as distinguished as he is modest.

In the case of exception, the horse was in the last stage of exhaustion. The pulsation of none of his arteries could be felt, and the liquid continued to flow upwards from the beginning to the end of the experiment, without any intermission, and this whether he was placed upon his back or his side.

When either of the ventricles was penetrated—an accident which frequently happened, as long as the blood was allowed to flow through the tube—the animal did not seem likely to perish sooner than he would have done by any other hæmorrhage of the same amount; but when the effusion took place within the pericardium, he invariably died when the bag was filled to its utmost extent. In these cases the heart was found compressed, and smaller than natural, in the midst of an immense coagulum.

Professor Coleman was kind enough to afford me an opportunity of repeating the first and third experiments at the Veterinary College on Friday, the 10th February, 1826. There were present,

besides the Professor and his numerous and respectable class, Mr. Sewel, Dr. Bostock, Mr. Wardrop, Mr. Broughton, Dr. Macann, and many other highly distinguished men.

The subject was a donkey. All expressed their satisfaction at the entire success of the experiments, but particularly that upon the pericardium. The tube was introduced into the cavity of this bag without inflicting the slightest injury upon the heart. The liquid was taken up with wonderful rapidity, and in perfect accordance with the dilatation of the thoracic cavities during inspiration.

Upon opening the animal, the flexible tube was seen projecting some inches into the bag of the pericardium, in the depending portion of which was found a considerable quantity of the liquid used in the experiment.

PULMONARY VENOUS CIRCULATION.

Before I state the inferences which appear to me deducible from the facts already recorded, I shall say a few words on the motion of the blood in the veins of the lungs.

Since it is evident that the blood sent into the aorta cannot arrive through any other channel than the pulmonary veins, it will not be unreasonable to conclude, either that the lungs must be equally pervious to the blood of the right heart during all the stages of respiration, or, that if they are not so, there must be a reservoir from which the left heart can be supplied during the period when they are least pervious.

The lungs themselves are placed within two cavities, which, as we have just seen, are in a state of tendency towards the formation of a vacuum during the act of inspiration, and therefore the pulmonary veins would, at first sight, appear to be all equally exempt from pressure in every part of the thorax, at the moment of its expansion. A more attentive examination, however, will shew, that nature has ensured, by a beautiful and simple mechanism, as constant and as ample a supply to the left heart, as she has to the right, and by the same means; *viz.*, atmospheric pressure. I shall take the thorax of the horse as an example to illustrate the pulmonary venous circulation in the warm-blooded mammalia.

In the horse, the posterior cava quits the spine as soon as it arrives at the crus of the diaphragm ; it then runs along this muscle for a considerable distance, until it arrives opposite the base of the heart, when it passes into the thorax like a rope across a room, unconnected with every thing for five or six inches of its length, except with the thin, gauze-like membrane which extends from the right side of the pericardium to the diaphragm, and which seems to hang from the outer and upper side of the thoracic cava like a curtain. As this membrane conducts the phrenic nerve to its destination, I shall take the liberty of calling it the phrenic curtain, not being aware of any other name by which it may be distinguished.

The two great posterior, or right and left pulmonary veins form, by their early confluence in the right cavity of the thorax, behind the pericardium, a capacious reservoir, which is still further enlarged by the junction to its left side more anteriorly of the two common trunks of the principal middle left pulmonary veins.

There is a deep notch lined by pleura made into the inner face of the great right lung from

before backwards, almost to its root. The irregularly pyramidal slice of lung thus half-detached from, but still adhering by its base to the parent-lobe, is the middle lung of quadrupeds. It is thrust upwards and to the left of the loose posterior cava, but without forming the slightest adhesion to this vessel. In this situation, then, it would hang across the vein, were not a portion of its upper, or rather left, surface pasted up to the floor of the great reservoir just mentioned, and to some inches of the bevelled edge of the left lung, each preserving its proper pleura. The point of this little lung, with all its lower sides and angles, are free. This connexion between the middle lung and the roots of the posterior pulmonary veins is not the only one. Two, three, or more veins coming from the left superior anterior angle of the middle lung open their trumpet-shaped-mouths into the floor of the reservoir precisely at the three points best calculated to pull it downwards and to the right, when the middle lobe, filled by inspiration, is strained towards its parent lung by the pleura lining the notch. The anterior, the largest of these connecting veins,

is inserted into the centre of the common trunk of the two middle veins already mentioned. The second, into the centre of the conflux of this trunk, with the great left posterior vein. The third, (in the lung now before me) to the left of the centre of the conflux of this last with the right posterior vein. Thus, if the middle lung were pulled down from its adhesions to the left of the cava, and at the same time revolved upon its base towards its parent lobe, its veins prolonged would form arcs of that angle, of which the right phrenic curtain and the floor of the reservoir would represent the sides. When the horse's lungs are artificially inflated, the middle lobe makes precisely the movement described.

In this arrangement there are the following remarkable circumstances: 1st. The principal veins of the left lung enter the right thorax. 2d. The veins of the middle lung cross the largest vein of the right lobe, to empty themselves into a particular point of the conflux of the left pulmonary veins. 3d. The veins of the middle lung empty themselves at one of its extremities, instead of at

its root. The purpose of this mechanism I illustrated in the following manner:—

Fourth Experiment.

After having laid bare about half an inch of the lower surface of the left posterior pulmonary vein, I introduced into its cavity, towards the heart, the end A of the tube (fig. 1.), tying the vessel round it. The point C was immersed in a glass of red wine and water. By pulling gently upon the apex of the middle lung, in the direction in which it would move when inflated, the coloured liquid rose with such force, that it flowed abundantly into the reservoir. When I ceased to pull, the liquid ceased to flow. When I pulled the lung horizontally towards the left, the coloured water seemed rather inclined to return towards the glass. When I pulled horizontally towards the right, the liquid rose, but the more the middle lung was lifted from its attachments, the more rapidly the liquid flowed.

The right posterior pulmonary vein, and right side of the great reservoir, have no vein entering

them from the middle lung, because the root of the posterior cava is extensively attached to them a little farther forward.

The diaphragm in its retrograde descent pulls upon the posterior cava in a direction downwards and backwards. The lower floor of the left, and the upper of the right sinus venosi, are thus removed from the axes of their respective cavities. The phrenic curtain pushed to the right, by the expansion of the middle lung, favours this movement of the cava, while it tends to widen its tube.

The cavities to which this distending mechanism is applied during inspiration are exempt from pressure, whilst the pulmonary veins in direct communication with them are exposed to the full pressure of the air rushing in by the trachea to distend the air cells. Besides, the pressure of the atmosphere is exerted upon an extent of surface of the pulmonary veins, holding an inverse ratio of proportion to the capacity of their tubes.

To comprehend the mechanism by which the great pulmonary veins or reservoirs of the left

heart are expanded in man, it is only necessary to observe their connexion with the pericardium. A little tongue appears to be cut in this bag from behind forwards, to allow each vein to pass on to the heart through a kind of square hole. This tongue is then pulled backwards and outwards a little out of the general line of the insertion of the pericardium, and firmly glued to the anterior surface only of the vein. This mechanism is distinctly seen in man, as well on the right side as on the left, within the pericardium. There is a little pouch over each pulmonary vein, having its point directed outwards, whilst in quadrupeds no such contrivance exists. When the pericardium, therefore, in man is brought forward by the elevation of the sternum, and when it is enlarged at its base by the expansion of the lungs, the anterior surfaces of the pulmonary veins, where they enter the left sinus venosus, must be strained forwards, whilst their posterior surfaces are retained in their place.

If any further illustration were necessary of the use of this peculiar attachment of the pericardium to the pulmonary veins in man, we have

only to observe, that if the loose bag be pulled, however forcibly, in the direction which the movements of respiration give it, the strain will be brought on the anterior surfaces of the veins only, never on any portion of the arteries.

This peculiar mode of connexion between the pericardium and pulmonary veins does not exist in quadrupeds, except as far as concerns the anterior veins of the left lung, and even in these in a less remarkable manner; whilst the accretion of the contiguous sides of the cavas and pulmonary veins, so marked and so extensive in quadrupeds, is not found in man, at least not on the outside of the pericardium. In support of the importance of the pericardium in the mechanism of the circulation, it may be remarked, that it is perhaps the only part of the animal which is never found entirely wanting.

I shall detail one more experiment, because it affords additional evidence of the effects of atmospheric pressure upon the blood of the veins, which in this case performed the part allotted to the blue liquid in the other experiments.

Fifth Experiment.

On the 30th November, 1824, I took a horse, which had undergone no previous experiment whatever, threw him, secured him, and laid bare his left jugular vein for about eight or ten inches, following the vessel as far towards the chest as I thought safe. I next passed a ligature under it at either extremity of the external incision: these I knotted lightly, each over a small cork. Considerably nearer the chest than the middle of the incision, I made an opening into the vein, and introduced into its canal, towards the heart, the end A of the glass instrument (fig. 2) as far as the globe would permit. The vessel was secured round the tube by two turns of small twine, lightly knotted, above the reverted lip of the lower opening. I next divided the vein behind the globe, and passed the upper end of it over the opening B, securing it as before. This being done, I cut upon the corks, first the lower, then the upper, ligature. The blood now rushed rapidly through the globe. Its motion was at first visible, but,

after a few seconds, could not be perceived from where I sat, the horse lying prostrate under me. The apparatus was well adjusted, and kept its place. The blood, I knew, passed freely into the chest, for there was no enlargement of the vein above the globe.

I now carefully washed the outside of the glass, and placed myself upon my knees, supporting my right hand extended upon the ribs of the prostrate animal. By this arrangement I was able to apply my eyes close to the globe, and at the same time to feel, in the most exact manner, the expansion and collapse of the thorax. The dark blood, which nearly filled the globe, left a small space unoccupied at its upper side. Very little light, however, was reflected from the mass below, and therefore, whilst I observed it in a sitting posture, there appeared to be no motion on the inside. When I applied my eyes closely, I could distinctly perceive the blood rise within the globe, and, as it approached the upper part, assume a lighter red, as if a froth were raised upon it by the rush to pass the lower opening. This appearance regularly accompanied the elevation of the ribs,

over which I held my right hand expanded. Having once caught the proper light, I could perceive distinctly the motion of the blood in the globe, keeping exact time with the inspirations. The horse lay quietly, and breathed tranquilly. The tube kept its place in the most satisfactory manner. There was therefore neither hurry nor confusion. I observed at leisure the perfect coincidence of the passage of the blood through the globe with the inspirations of the horse. This I could not have done so well in any other attitude, as the breathing was not sonorous in this case; for I could not have fixed my eyes on the glass and on the thorax at the same time.

I have said that I observed the blood flowing through the bulb of the tube in exact correspondence with the expansion of the chest. The synchronism was just as well marked as in the experiments with the blue liquid and the spiral, with this exception, however, that in the present case there was no regurgitation, because the breathing was not hurried. My observations were prolonged, repeated, and careful. After watching the globe for some minutes, I resumed my sitting-

posture, returned again to the kneeling position, and observed the same phenomena going on without the slightest alteration.

Three or four times I repeated this proceeding in different lights, and constantly found the same uninterrupted coincidence between the passage of the blood through the globe and the elevation of the ribs.

This experiment appeared to me so conclusive and unequivocal, that I shall never require a repetition of it for my own satisfaction.

I had often tried this experiment before, but without having obtained very satisfactory results. My failure I can now with confidence attribute to the length of the tubes which I had hitherto used, sometimes reaching from the angle of the jaw to the root of the neck. In these cases, as soon as the globe was filled, all movement ceased, owing to the blood being protected from atmospheric pressure through so long a portion of its horizontal course, which also deprived it of the influence of gravitation. My complete success with the short tube justifies this remark.

CONCLUSIONS.

From what has been said, and from what has been observed in the experiments, the two following facts may be considered as proved:—

First,—That the cavities of the great veins within the thorax, and all the thoracic cavities, draw towards them the fluids with which they are placed in direct communication.

Second,—That this attraction, or suction, never takes place but during the expansion of the thorax, that is, during *inspiration.*

From these facts, and from what we have seen in the last experiment, we may conclude,—

1st. That the blood which *runs contrary to its own gravity, arrives at the heart only during inspiration.*

2dly. That the power which impels it at this moment through the veins, is atmospheric pressure.

3dly. That as this power can be applied to the blood of the veins only at the moment of inspiration, this blood must move with a velocity which

is, to that of the blood moving through the arteries, as the time occupied by a whole respiration is to the time occupied by a single inspiration.

4thly. As the blood passes through the greater veins during inspiration only, whilst it is incessantly traversing the arteries, it follows, that an accumulation must take place somewhere between these two orders of vessels, and that the quantity of this accumulation must be to the quantity which passes through the arteries during an entire act of respiration, as the time of one expiration is to that of a whole respiration.

5thly. That, as it makes no difference with regard to the event, whether the accumulation which must be prepared for the expansion of the thorax, be made by two pulsations of the arteries or by ten, it follows that the frequency of the pulse cannot be taken as the measure of the velocity of the blood returning to the heart, because it is the repetition of the inspirations which must regulate this velocity.

6thly. That there are three quantities of blood; one passing through the arteries, one which is

sucked up by each expansion of the thorax, and a third, which is collected during expiration between these two points. When therefore the respiration becomes hurried, this third quantity is diminished, whilst the other two are increased in proportion; but as the heart can admit only a certain quantity, the expanding cavities regurgitate the surplus during their collapse. Hence pathological phenomena, into which I shall not enter for the present.

7thly. That the lymph and chyle must be sucked up towards the chest, through the direct communications which the vessels peculiar to these fluids have with the subclavion and other veins. The question of absorption, therefore, which has hitherto puzzled physiologists so much, may now be considered as decided, for it is clear that the open mouth of a vein, or of any other vessel, having the same kind of communication with the thoracic pumps, must absorb in direct proportion to the sucking power applied to it, and to the pressure exercised upon the matter to be absorbed *.

* See Experiment, No. 1, page 10.

If this last proposition be well founded, so ought to be the following corollary, *viz.:*

That the application of a powerful cupping-glass to a recently-poisoned wound, would prevent the absorption of the poisonous matter.

8thly. It being now evident, from every thing that has been said, that the blood in the veins is placed under the influence of atmospheric pressure, it would be curious to trace the connexion which appears to exist between disease generally, intermittent fever for example, and the daily barometric variations.

9thly. The preceding facts explain also why animal life cannot be maintained beyond a certain degree of atmospheric rarefaction, and why it must cease as soon as the pressure of the surrounding air ceases to be superior to the gravity of the column of blood. Birds are provided with a respiratory mechanism, which, in some measure, exempts them from this inconvenience.

10thly. At the cardiac extremities of the great veins there exists, as we have shewn, a mechanism, which, when called into action by the expansion of the thorax, distends their cavities,

and, consequently, causes the suction of the blood of the veins of the lesser, as well as of the greater, circulation. Now, as this mechanism can act only during inspiration, and as, from its construction, and its position, it must necessarily affect those portions of the auricles within the pericardium, called the sinus venosi, it follows that there can be no alternation of contraction between these parts of the auricles and the ventricles corresponding to the pulse, because the sinus venosi must be in a state of progressive distension from the beginning to the end of *inspiration.*

The influence which this disposition of the parts, as well as the series of facts hitherto noticed, may have upon the motion of the heart, and upon the passage of the blood through this organ, will form the subject of another Memoir.

I shall not now trespass longer on the attention of the Academy, by endeavouring to enumerate all the conclusions deducible from the facts, which, I trust, will be considered as proved by the experiments. In whatever light the results of my researches may be regarded, whether

as merely explanatory of some doubtful points, or as sufficiently novel and important to constitute a discovery, I have brought them as an offering to the Temple of French Science, where, fortunately, Prejudice has not yet stript Physiology of that portion of philosophic honour which is her due.

Paris, June 6, 1825.

SUPPLEMENT.

To illustrate the physical application of the principles hitherto advanced, to the circulation of the blood through the veins, and to shew that the two paramount laws of nature, gravitation and atmospheric pressure, are equally influential with regard to animated as to inert matter, we shall suppose two tubes of equal diameter, each in the form of the letter U *. Let each of the branches of these tubes be fifteen inches in length; one of these tubes shall be formed of a hard substance, such as glass, the other of a yielding distensible material, such as a vein.

(1.) Let mercury be injected into the branch A of the first or hard tube, it will mount in the branch B, until both are full; and if the injection be continued, the mercury will flow out at B in

* See plate, fig. 3.

jets equal to and synchronous with the injections at A.

(2.) If the same operation be performed on the second or flexible tube, the same quantity of mercury will not be sufficient to fill it, because the lower parts of the tube will be distended by the pressure of thirty inches of mercury, that is, of fifteen inches on either side. None, therefore, will flow out at B, until a much greater quantity than that employed in the first operation shall have been injected at A. Even after the mercury shall have been raised to B in the flexible tube, it will not flow out in jets either equal to or synchronous with those injected at A, because a part of the injecting force and of the mercury injected, will be employed in producing fresh distension. The slightest alteration, therefore, in the distensibility of the tube will be felt at B, whatever be the amount of the injecting power at A.

(3.) Let the branch B of the inflexible tube be prolonged to thirty inches, and let a vacuum be established in the reservoir E, with which this branch communicates: the mercury in the branches A and B will be forced by atmo-

spheric pressure up to E. If the mercury can be removed from E according as it arrives, without destroying the vacuum, all that is injected at A will flow into the reservoir E.

(4.) Let us suppose the second or compressible tube, under the circumstances just described, the portion B E being empty, its walls will be pressed flat by the weight of the atmosphere, because the resistance which they offer is much less than that of the gravitation of the mercury, which, in this case, will not rise towards E, unless the tube be rendered incompressible by the introduction of another tube capable of resisting the pressure of the air, as in the first experiment.

(5.) If at the moment that the vacuum is formed, the flexible tube be full as far as E, of a liquid ten or twelve times lighter than mercury, and if it be divided at short distances by valves, each forming a base to the column above it, and if the injections be continued not only at A, but at many other points between C and E, and lastly, if the sides of the flexible tube be attached to the parts about it whose natural position tends to keep these sides asunder, then the tube B E will

not be pressed flat, and the vacuum at E will act upon the contents of the whole tube, as soon as the injecting power shall have placed them within the sphere of its attraction.

(6.) Thus it is evident, that in the case of the inflexible tube the injecting power alone applied at A will discharge at B the precise quantity injected, and that atmospheric pressure alone will force the mercury to rise from C to the vacuum E, without the assistance of the injecting force.

(7.) In the case of the compressible tube, the injecting power alone, however great we may suppose it at A, can never produce a flow of liquid from B precisely equal to the quantity injected, whatever qualities we may endow the tube with, provided that it retain its *distensibility*. (2.)

(8.) If the mechanism by which the mercury is removed from the reservoir E, be made to inject it into A, then as long as the vacuum can be maintained, and the tube remains entire, the circulation of the mercury will continue, provided, with regard to the incompressible tube, that the quantity of liquid remain unaltered; whereas, this

condition is not at all indispensable to the persistence of the circulation through the compressible tube, because its own distensibility on one hand, and the pressure of the air on the other, will always accommodate its capacity to the volume of its contents.

(9.) If the extent of the vacuum be diminished, the quantity of liquid being the same, there will be accumulation in the lower parts of the flexible tube.

(10.) If the liquid in the branch C E of the flexible tube, be forced by mechanical pressure towards the part which offers least resistance, viz., the vacuum, then the reservoir will be compelled to dilate itself more rapidly, the instrument which empties it must increase its action, and produce increased velocity in the contents of the branch A C.

(11.) If the distension of the reservoir E brings up more liquid than is emptied into A, then its contraction will force a part of its contents to regurgitate towards B.

(12.) If the injection of the liquid into A should cease altogether, or become very trifling, then

the continuity of the column will be lost in the branch B by the weight of the atmosphere pressing its sides together, and the expansion of the reservoir must be diminished or cease altogether. In this case, incline the tube towards E, the gravity of the liquid will favour the expansion of the reservoir, which will be renewed without any difficulty, because it is no longer opposed by the gravitation of the liquid.

(13.) If an opening be made at D in the branch C E of the inflexible tube, air or any other fluid will enter by this opening, will force the mercury up to E, and occupy its place; but in the flexible tube, the weight of the atmosphere will press the walls of the tube together above the opening, and the vacuum will cease to act upon the portion below it. In this case a part of the liquid injected at A will flow out at D, and the rest will be employed in distending the lower portion of the tube.

(14.) If a vacuum be applied over the opening D—first, neither air, nor any other fluid can be forced into the opening; secondly, a portion of the liquid which otherwise would have been com-

pelled to rise to E, will be recalled towards D, and *à fortiori*, a much larger portion of the liquid contained in A C.

(15.) It is now evident, that the liquid contained in the flexible tube can mount to E only, at the moment when the reservoir is expanding, and that at the instant when the tendency to a vacuum ceases in the reservoir, the liquid will obey the law of gravitation, and will distend the lower parts of the tube. (2.)

(16.) It is also easy to conceive that the motion of the liquid in the branch A will be in direct proportion to the injecting power, as the velocity of the contents of the branch B will be to the expansion of the reservoir E, and that a mutual influence will be felt in both branches through their communication at C, whether by one or more canals.

(17.) Let the reservoir E be *now* the most dependent part of the tube. The gravitation of the liquid will be opposed to the injecting power in the branch A, whilst it will favour the influence of the vacuum in the branch B. But as atmospheric pressure is everywhere equal, if the contracting

force of the reservoir E be able to resist the gravitation of the liquid in B, then that which is injected at A can be received into E, only at the moment that the tendency to a vacuum takes place.

(18.) If the tube be supposed incompressible from A to B *only*, then the liquid will fall from B in jets equal to and synchronous with those injected at A, and its gravitation towards the reservoir will only operate from B to E, where it will accumulate, producing distension proportioned to its quantity, and to the interval between the expansions of the reservoir. The dilatation of the reservoir will affect the contents of the tube B only as far as it is compressible.

(19.) If a portion of the tube A B be rendered compressible at C, (E being still the most dependent point,) a *depression* and *elevation* will be perceptible at C, corresponding to the *expansion* and *contraction* of the reservoir E. If this compressible portion be again rendered *incompressible*, the depression and elevation must cease at C, because atmospheric pressure can no longer affect it.

(20.) Let us suppose that the portion C be again rendered flexible, and that it is the lowest point of the tube. In this case the force of gravitation in both branches, and the injecting force in the branch A, will direct the liquid towards C, and the *depression* and *elevation* will not be perceptible, but the portion C will be permanently distended. (2.)

(21.) Let us now apply these data to the living animal, to man, for example. The aorta and lower cava represent the flexible tube. The thoracic cavities, but particularly the pericardium, are the reservoirs in which the tendency to a vacuum takes place during *inspiration*. The heart is the instrument by which the contents of the reservoir are injected into the branch A, without destroying the vacuum. It is not difficult to perceive that every thing we have said relative to the flexible tube is perfectly applicable to the parts below the heart.

(22.) The application of the data resulting from the case in which the tube is supposed to be *partly incompressible*, and where the reservoir is

the most depending point, merits more particular attention.

(23.) This tube is represented in man by the carotid arteries on one side, and the jugular veins and upper cava on the other; the incompressible portion of the tube by the cranium. As the form of the adult head is incapable of alteration, so must the volume of its contents remain unaltered, however they may be modified in point of density. The veins which run between the two tables of the flat bones of the skull, and in the substance of the vertebræ and other bones, present striking examples of the inflexible tube *.

(24.) The pulsation observed in the jugular veins, synchronous with that of the arteries, proves that the blood which runs in the veins of the cranium is subject to the general laws of gravitation. The same experiments performed upon the jugular veins of animals in the horizontal and vertical positions afford different results.

* M. Brechet was kind enough to give me an opportunity of examining his splendid preparations and plates of these veins.

Sixth Experiment.

I fixed the glass tube (fig. 2), in the jugular vein of a horse while he was standing, in such a way that the current of the blood must pass through the globe, and I observed, 1st, that the jets which fell from the end next the head were not synchronous with inspiration; 2dly, that they were more frequent than the pulse, but that the beat of the artery had a marked influence over them. Thus the jets falling into the globe became much less distinct when the carotid was even lightly pressed, than when it was free; but the influence of gravitation was constantly sending some drops into the globe. When the horse was thrown, and placed horizontally, the movements of the blood through the globe were perfectly synchronous with those of inspiration. When the horse was again placed on his legs, the vein above the upper end of the tube having become straightened by exposure to the air, a pulsation was observed at that point, the acme of whose swell was precisely synchronous with that of the carotid. (18.)

This experiment accounts for the difference hitherto observable in the opinions of physiologists, as to the cause of the pulsation of the jugular veins; some having made their observations upon man in the vertical position, others upon animals placed horizontally.

There is at this moment, in one of Professor Laennec's clinical wards, a patient, aged sixty-eight, in whose external jugular veins, and upper portion of the superficial thoracic veins, a regular pulsation has been observed for a considerable time. It has sometimes extended to the veins of the upper arm. The jugulars begin to swell before the artery, which beats forty-eight in the minute, but they arrive at their acme at the moment that the artery strikes the finger. The bleedings ordered for his disease, (hypertrophia cordis,) and a diarrhœa, which sometimes occurs, diminish the venous pulsation, but have never removed it. When this man is made to recite any thing, the pulsation ceases entirely, but the veins continue to increase in size. (18.) When he inspires, in order to continue the recitation, the veins collapse immediately, and so on as long as he con-

tinues to speak. When the head of this patient is placed lower than his thorax, the veins of the neck swell prodigiously, and their pulsation ceases. (17.)

This man lately became dropsical, his abdomen rapidly increased in volume, with oppressed respiration. The pulsation of the jugulars however, still continued. He was tapped some days ago, and from twenty-five to thirty pints of hydropic water were drawn off. At the morning visit, after this operation, there was no pulsation observable in the veins of the neck or chest. The jugulars remained perfectly collapsed, and became prominent only when he coughed or expired strongly. (17—18.)

The accumulation of blood which, before the tapping, used to extend in the jugulars almost to the angle of the jaw, now takes place below the clavicle, and the jets which fall from the head, pass through these veins without being per ceived.

Professor Laennec, besides permitting me to note these observations in his hospital, condescended to verify their exactness with me at the

bedside of the patient *. To this learned physician, all foreigners, attending his practice and his most interesting pathological lectures, are largely indebted, for the kind assiduity with which he directs their research.

(25.) With regard to the elevation and depression of the brain and its membranes, which, under certain circumstances, are observed to take place in living animals, I shall take the liberty of quoting the words of Haller.

" Ergo si vivo animali non nimis debilitato, cranium aperueris, aut trepano aut unco, duramque matrem detexeris, et digito à cranio depresseris, videbis ad singulas inspirationes subsidere cerebrum, aut solum aut cum suis involucris; vicissim idem cerebrum surgere, cranio se admovere, digitum impositum repellere. Et vix respirationis in cerebrum effectus demonstrari potest, nisi duram matrem à cranio depresseris †."

* I hazarded an opinion that the jugular pulsation would return again with the return of the dropsical effusion and consequent swelling of the abdomen. This prognostic was fully verified by the event.

† Tom. ii. lib. 4.

The causes of these phænomena, according to Haller, are, first, The greater facility afforded by the expansion of the lungs during inspiration, to the passage of the blood through these organs. "Nascitur ergo derivatio, et sanguis venosus undique ad eam sedem confluit." Secondly, The obstacles opposed to this same blood during expiration. "In expiratione thorax contrahitur, comprimuntur pulmones, auriculæ, venæ cavæ, fit refluxus in venas cerebri. Hinc presso, ut ego presseram thorace, elisâque venâ cavâ, perinde cerebrum elevatur *."

Here it is important to remark, that even after

* Since the printing of my Memoir sur les Causes du Mouvement du Sang dans les Veines, the following passage has been pointed out to me in the Philosophical Transactions of 1810, (Part I.) as in a great measure anticipating my views, with regard to the influence of the expansion of the thorax upon the return of the blood through the veins.

"At the instant that the chest is dilated for the reception of air, its vessels become also more open for the reception of blood, so that the return of the blood from the head is more free than at any other period of complete respiration. On the contrary, by the act of expelling air from the lungs, the ingress of the blood is so far obtructed, that when the surface of the brain is exposed by a trepan, a successive turgescence and subsidence of the brain is seen in alternate motion with

a portion of the dura mater has been laid bare, the *elevation* and *depression* of the brain do not take place, so long as the membrane, by its adherence to the margins of the opening in the bone is able to resist atmospheric pressure; but as soon as a part of the tube becomes compressible, by the separation of the dura mater from the cranium, these phænomena become apparent, unless the head of the animal be placed in the most depending position. In this case, they cannot take place, for the reasons already stated. (20.) This fact I have proved by direct experiment upon the living animal.

(26.) From what we have seen in the Memoir, and from what has been said in the Supplement,

the different states of the chest."—*Croonian Lecture, by* W. H. Wollaston, M.D.)

The illustrious author, for whose transcendent talents I have ever entertained the most profound veneration, will not, I trust, be offended at my observing, that though he has not quoted the name of Haller, the whole passage is a close translation of the extracts from that great physiologist, which I had placed in my text, without having seen the very interesting and ingenious lecture just quoted. This passage, therefore, cannot be supposed to have advanced our knowledge of the causes of the progression of the blood in the veins one point beyond where Haller left it.

it is evident that fluids, whether moving through living, or through inert tubes, obey the laws of pressure and of gravitation; and that in the quiescent living animal, the only demonstrable active powers employed by nature to propel the contents of the veins towards the heart, are—

First, The impulse given by the pressure of the heart itself, continued through, and propagated by the arteries. By this power the blood is sent into the situation where it can be most favourably acted upon, by

Secondly, Atmospheric pressure, diminished or entirely taken off around the cardiac ends of the venous tubes during the expansion of the chest, but unaltered and entire around every other part of their surface, opposed only by the gravity of the fluid acted upon.

Thirdly, Gravitation, when the heart is relatively the most depending point, or when this power is acting with the pressure of the heart's contraction upon the base of the venous column.

Of these powers the pressure of the atmosphere is by far, the most intense in its degree, the most constant in its influence, and the most

unvarying in its amount. It is that without which the circulation could not be maintained beyond a few moments.

Hence it must now be needless to repeat that the constant supply of blood to the heart cannot depend solely upon the causes to which it has been hitherto ascribed, as already stated at the commencement of the memoir.

(27.) The following phænomena, amongst others connected with the venous circulation in man, afford still further proofs of the identity of the laws which preside over the motion of fluids, whether through organized or in lifeless tubes.

1. The swelling of the lower extremities in habitual dyspnœa. (9.)

2. The effect of violent bodily exercise. (10.)

3. Pulsation of the veins synchronous with respiration. (11.)

4. Fainting from loss of blood, and the best mode of relieving it, by placing the head and heart lower than the rest of the body. (12.)

5. The effect of opening a vein at a distance from the heart. (13.)

6. The effect of a vacuum established over this opening. (14.)

7. The circulation within the cranium, between its tables, and in the substance of other bones. (18.)

8. The swelling of the jugulars during expiration. (18.)

9. The pulsation of the jugulars corresponding to that of the arteries. (18.)

10. The elevation and depression of the brain and its membranes in infants, before the closing of the fontanelles, and in adult animals when a portion of the cranium is removed.

(Signed) DAVID BARRY.

INSTITUTE OF FRANCE.

Royal Academy of Sciences,
Paris, August 30, 1825.

The perpetual Secretary of the Academy for the Natural Sciences certifies, that the following is extracted from the Notes of the Proceedings of the Sitting of Monday, the 29th of August, 1825.

REPORT UPON DR. BARRY'S MEMOIR, ON THE MOTION OF THE BLOOD IN THE VEINS.

THE circulation in the vertebrated animals is one of the parts of physiology upon which we have acquired the most positive degree of knowledge. Our more exact notions however, do not date farther back than the beginning of the sixteenth century, the epoch when Harvey demonstrated the true mechanism which gives motion to the blood, and which favours its continual transport through the system.

Every one knows that the tubes which go out from the heart, and through which the blood is propelled, and directed to all the parts of the

body, are called arteries; and that the tubes which conduct the blood, the chyle, and the lymph, to the heart, have received the name of veins. In short, that the heart itself, the organ which, to a certain degree, determines the mode of the circulation, varies as to its position, its structure, and many other appreciable circumstances, whilst its essential mechanism, by which its functions are executed, remains nearly the same.

The direction in which the venous blood is constantly carried towards the heart was noticed by Michael Servet, more than fifty years before Harvey made those direct experiments by which he ascertained the true mechanism of the circulation. Notwithstanding this important and memorable discovery, many discussions have since arisen as to the true causes of the *progression of the blood in the veins*. It is of importance to the question which we are about to examine, to give a brief account of the leading opinions which have been started upon this subject, without, however, entering into a chronological history of them.

We shall place at the head, the impellant action of the heart and arteries, which was supposed to be continued through the venous capillaries by the pressure exercised upon them at their anastomoses with the arteries. This was the opinion of Harvey. According to Bichât, the absorbing power of the venous capillary system is sufficient to originate, and afterwards to keep up the progression of the blood through the veins, assisted by the action of the coats of these vessels themselves. In fine, according to the opinions of various authors, a great number of secondary causes facilitate this action of the veins; such as the motion of the great arterial trunks, generally placed between two veins; the pressure exercised both externally and internally on all the organs by the skin, by the muscles, by the viscera, which collapse, and are distended alternately. But the action of respiration was more particularly noticed, from its evident connexion with the mechanical return of the blood by the veins. To explain this phænomenon, some supposed that the blood was brought up with greater or less velocity, according as the lungs

were more or less empty (RUDIGER). Or as a deeper and more rapid inspiration allowed a freer course to the blood in the lungs (SANTORINI). HALLER, tom. ii., of his Physiology, page 333, quotes a great number of experiments, which he repeated upon living animals. In those of Valsalva and Morgagni he observed, when he laid bare the great veins, such as the anterior and posterior cavæ, the jugulars, the subclavians, that it was at the instant when the animal made a deep inspiration, that the venous blood arrived at the heart; that it was at this moment that all the veins unloaded themselves, grew pale, collapsed, and emptied themselves of the blood which they contained; and that during the expiration which immediately followed, the same veins swelled, became livid, round; and that the more distinctly the two periods of respiration were marked, the more apparent these phænomena became.

Morgagni had already stated (*De causis et sedibus morborum, lib.* 19, art. 33 et 34), that, by attentively observing the jugular vein of a living dog, whilst he held his hand upon the abdomen

of the animal, he had clearly ascertained, that at each time the belly was elevated by the act of inspiration, at that very moment the vein collapsed, to swell again as soon as the parietes of the abdomen fell during the act of expiration.

A great number of authors since this period, particularly our able associate, M. Majendie (*Physiologie*, 2d edition, page 418), have corroborated these circumstances, and have brought in proof of the connexion between inspiration and the quickening of the motion of the blood in the larger venous trunks, new and ingenious experiments, which have confirmed the constant occurrence of this phenomenon. But at the same time, considering it to be merely an auxiliary mean of facilitating the arrival of the venous blood. In fine, although the greatest number of physiologists attributed the progression of the venous blood towards the heart to a vacuum formed in this organ, Bichat (*Anatomie Générale*, tom. i, page 429), very properly observed, that the motion of the blood in the veins still needs much elucidation; for, adds he, notwith-

standing all that authors have written upon this subject, it still presents much obscurity, in which but few gleams of light are perceptible.

We have thought it our duty to enter into these details, in order that the Academy might be able to judge of the Memoir, for the examination of which, M. the Baron Cuvier and I have had the honour of being appointed Commissioners.

In this work Dr. Barry states his peculiar views on the subject of the motion of the blood in the veins. He details minutely the proceedings which he has contrived, we can say, with sagacity; which he has executed upon living animals, with address; and which he was kind enough to repeat several times under the eyes of your Commissioners.

His Memoir presents three principal points of inquiry.

1. To determine by positive experiments, what the power is which forces the venous blood to direct its course, from the most minute ramifications where it has its source, towards the heart, where it empties itself.

2. To appreciate, and to compare, the velocity

with which the blood is moved in the veins, and in the arteries.

3. To prove that the never-failing supply of venous blood to the heart, cannot be solely owing to the causes to which it has been hitherto attributed.

Under the first head Dr. Barry, in studying the phenomena of the venous circulation, has been led to observe that, by the act of inspiration a vacuum is formed within the chest when it tends to dilate its capacity, and that all liquids in communication with the interior of the thorax must be attracted thither, being forced towards it by atmospheric pressure.

All the facts with which we are acquainted, *it must be confessed,* find their explanation in this physical effect. Such are, for example, the swelling of the jugular veins during expiration; their collapse at the moment of inspiration; the cessation of certain hemorrhages by means of forced inspirations; the absorption of air by the veins, and the accidents which have resulted from it, when any of these vessels near the heart have been opened or divided.

The author, not satisfied with bringing these facts as evidence to support his opinion, resolved to strengthen it by direct experiments, of which the following are the principal:—

Having fixed into one of the great veins, such as the jugular of a living animal, one end of a tube, armed with a stop-cock, whilst the other end was plunged in a coloured liquid, he observed, upon opening the stop-cock, that when the animal *inspired*, the liquid was forcibly drawn up; and that during expiration, on the contrary, the liquid remained stationary, if it did not return towards the vessel. We are able to announce moreover to the Academy, that whenever the experimenter introduced the same tube, which was contrived with much ingenuity, into either of the thoracic cavities, or even into the pericardium, the same phenomena were reproduced.

Dr. Barry made use of spiral glass tubes, in order that by increasing the distance which the liquid had to pass over, its motion might be rendered more apparent. He also either mixed with the coloured liquid some drops of oil, or allowed some bubbles of air to enter the tube,

that the ascent of the liquid might be more strikingly perceptible.

In all these experiments, executed with the greatest address, and with such satisfactory precautions, as would obviate all objections which might be opposed to them, the author of the Memoir, the result of which we are anxious to lay before you, fully ascertained, that the sucking action of the great veins was precisely coincident with the instant when the animal endeavoured to form the vacuum in his chest; that the black blood passed through the veins only during the act and the time of inspiration; and that this venous movement was always placed under the influence of the action of atmospheric pressure.

M. Barry is so convinced of the action of the atmosphere upon venous absorption, that he considers the application of a cupping-glass to a recent wound, into the interior of which any deleterious matter may have been introduced, as a certain mean of preventing the absorption of the poisonous matter.

M. Barry attributes also to atmospheric pressure the absorbent action of the pulmonary venoso-arterial system, or of the lesser circulation.

But here the author offers reasonings based upon anatomical structure, rather than upon positive observation; and some facts of comparative anatomy might be successfully opposed to this opinion, which the author has not brought forward with such conclusive experiments as those upon which he has based his demonstration of the action of atmospheric pressure upon the greater venous circulation.

As to the appreciation of the comparative velocity of the blood in the two orders of vessels which it traverses, the author founds it upon the notion that the pressure of the atmosphere is the principal power which impels the venous blood to the heart during inspiration. This blood, therefore, must move with a rapidity which is to that of the arterial blood, as the time employed in one entire respiration, is to the time of a single inspiration. Thus the frequency of the pulse cannot be taken as the measure of the velocity of the blood returning to the heart, because according to the first hypothesis, it would be the repetition of the movements of inspiration, which would regulate this velocity.

This part of the Memoir is entirely founded on

reasoning, and is not supported by such proofs and observations, as would permit us to pronounce an opinion upon this particular point.

With regard to the last consequence, which the author deduces from his Memoir, *viz.*, that the supply of venous blood to the heart cannot be attributed solely to the causes hitherto pointed out; we must declare, that the mere idea of the pressure of the atmosphere being the principal cause, was not first taken up by him. Many others had pointed out this even before Dr. Zugenbhuler, who has thought proper to address a claim of priority to the academy, putting in at the same time a dissertation, *De Motu Sanguinis per Venas*, published in 1815. This author however, although he recognises the action of the pressure of the atmosphere, considers the heart as the first cause of the vacuum which is formed in the system. But M. Barry attributes the dilatation of the heart itself, and of its auricles, to the tendency to a vacuum which takes place in all the cavities of the chest, during inspiration; demonstrating this action by positive experiments, whilst M. Zugenbhuler offers argument only in support of his opinion.

In concluding this report upon M. Barry's interesting Memoir, we feel it our duty to declare, that the experiments described with much detail by the author, have been performed and repeated more than twenty times upon dogs, upon sheep, upon horses; that they have constantly succeeded whenever he was able to bring fairly into operation the ingenious apparatus which he had contrived for the purpose; and that these experimental researches took place under our eyes, at the School of Medicine, at the King's Garden, at the School of Alfort in presence of Mr. Girard, and at the Abbattoirs of Monttaucon.

Your commissioners consider these researches as made in the very best spirit, and as eminently calculated to elucidate the physiological history of the venous circulation in the mammalia.

Under this impression they have the honour to propose to the Academy—that the author be invited to continue his investigations relative to the causes of Absorption, a subject which presents much interest, and the most useful applications to the animal economy; and that M. Barry's Memoir be inserted amongst those of learned strangers.

Your commissioners, however, must not conceal

that in their particular opinion the act of inspiration which appears to produce a vacuum within the thoracic cavities of animals having lungs, such as the mammalia and birds and consequently the attraction of the venous blood towards these cavities, is not sufficient to explain the motion of the blood in the veins of fishes, and of some reptiles, in which the mode of respiration is different. The same coincidence of action not being observed between inspiration (which in these animals is a species of deglutition), and the arrival of the venous blood at the cavity of their hearts.

(Signed) BARON CUVIER,
DUMERIL, *Reporter.*

The Academy adopts the conclusions of this report.

Certified to be according to the original.

Perpetual Secretary, Councillor of State, Commander of the Royal order of the Legion of Honour,

BARON CUVIER.

PART II.

ON ABSORPTION.

CHAPTER I.

Short History of the Ancient and Modern Theories of External Absorption—IMBIBITION—Comparison of the Ancient and Modern Modes of treating Poisoned Wounds. Influence of these Theories upon Practice.

THE progress of our knowledge in the physiology of absorption as exercised by abraded surfaces, is traced in the history of poisoning through superficial wounds. How or when man first became acquainted with this baleful art is hidden from us in the most remote antiquity. It had attained to a degree of perfection, and certainly of effect, long before the date of the very earliest records that have reached us, equal to, if not surpassing what is known to the most enlightened nations of the present day.

The story of the arrows of Hercules dipped in the venom of the Lernæan Hydra—the circumstantial accounts of the sufferings and death of Chiron, Nessus, and Hercules himself, leave no room for doubt upon this subject. They furnish the details of so many direct experiments, proving that men in those remote times knew that certain poisons deposited in wounds were carried into and mixed with the general mass of blood*.

Those to whom we are indebted for these details have unfortunately not recorded any opinion as to the manner in which the mixture of the poison and the blood was thought to be effected. Mankind seem to have been satisfied with the knowledge of the fact, and the practical application of it to the purposes of war and the chase. If any inquiry were made as to the mechanism by which the deleterious substance was removed from the surface towards the centre, no satisfactory account is given of it previously to the times of Celsus and Galen.

* Posse mori cupias tum cum cruciabere diræ
Sanguine serpentis per saucia membra recepto.
Ovid. *Met.* lib. 2.

The advances made in anatomy by these great physicians enabled them to perceive that the veins were the fittest organs through which the matter from abroad could pass into the general system; and as they found these tubes leading directly towards the centre, they recommended that a ligature should be placed above the poisoned wound, if on a limb.

This more enlightened view of external absorption continued to prevail amongst physiologists for seventeen centuries, as we learn from Redi, who wrote in 1664. "Ex consilio Galeni fiat stricta ligatura non procul à vulnere in parte superiori, videlicet, ne per sanguinis circulationem, venenum ad cor feratur, totaque sanguinea massa inficiatur*."

As the ancients did not distinguish the arteries, particularly the smaller ones, from the veins, it is probable that all the vessels carrying blood were promiscuously considered as absorbing organs.

The discovery of the true mechanism of the circulation by Harvey, (although it reflected but

* Redi, *de Viperis.*

little additional light upon absorption,) by distinguishing the centripetal from the centrifugal current, must necessarily have excluded the arteries from any share in this function.

Redi must have been acquainted with the theory of the circulation as taught by Harvey, and with the existence of the lymphatic vessels, discovered fourteen years before he wrote; yet his opinions with regard to external absorption appear to have been exactly those of Celsus and Galen.

It was only about the middle of the eighteenth century that the duties of absorption were first *exclusively* attributed to the lymphatics. The high authority of the Hunters, who taught that these vessels were the only organs employed in conveying matter from without into the system of the living animal, overturned the opinions which had prevailed upon this subject, without having been once questioned for nearly two thousand years.

M. Majendie deserves infinite praise for the able manner in which he demonstrated the error of considering the lymphatics as the sole

absorbents, and the necessity of returning to the sounder doctrine of venous absorption, held by Celsus, Galen, Redi, Ruysch, *&c.* His experiments, without proving that the lymphatics are not absorbents, leave not the slightest doubt that the veins do absorb.

These experiments however notwithstanding their ingenuity, do no more than bring back the question of absorption to the stage of advancement in which Ruysch and Boerhaave had left it, with this difference, that instead of assertion we have direct proof that this function may be carried on by the veins. But the causes which induce or compel the matter deposited on a wounded surface to enter the cavities of the veins, and to mix itself with the passing current of the blood, still continue to be desiderata. Now that tangible facts alone can be received as demonstrations in physiology, the notions of a peculiar unintelligible vital power of discernment and appropriation existing at the ends of the absorbing radicules, cannot even be alluded to.

M. Majendie aware of this, and of the little that his experiments had added to our stock of

knowledge on the subject alluded to, proposed *imbibition**, as sufficient to account for the transfer of matter from the surface of a wound to the current of the venous blood. According to this doctrine the matter placed in contact with a wound, if solid, is first dissolved in the fluids of the part, and when the coats of the vessels are soaked in the solution, that part of it which penetrates to their inside is washed off and carried forward by the current of the circulation.

This would render absorption a very tedious and uncertain process indeed, as we shall see by the conditions required to effect it.

1. There must be a current flowing in the vein through the coats of which the imbibition takes place, else the imbibed matter cannot be washed off and carried forward.

2. If the vein does contain a fluid, the imbibition or passive soaking of its coats may take place at least *as readily* from within outwards as in the opposite direction.

3. The open mouth of a divided or wounded vein

* *Physiologie*, 2 Ed. *Absorp. Veineuse.*

cannot become the subject of imbibition under any circumstances, and if the vessel be collapsed and empty imbibition will take place to no purpose, there being no current to carry forward the imbibed matter.

4. In all wounds minute arterial and lymphatic branches must be divided and laid bare as well as veins, and as there can be no very great difference in the density of their coats, imbibition may take place through the sides of all, and consequently absorption if there be a current flowing through their tubes, but not otherwise.

Thus according to M. Majendie's own shewing, in order that matter shall be conveyed from the surface into the circulation, it is necessary that it be placed in contact with the *outside* of a vein through which a current is actually flowing, and that the coats of this vein shall be incapable of being soaked from within by the contained liquid, while they are ready to be soaked in the same liquid from without, holding the matter to be imbibed in solution.

This last condition of soaking or imbibition of a liquid in one direction only, though applied to

both sides of the same substance, is rather difficult to be comprehended; for if both sides of a sponge be placed in equal contact with water, imbibition will go on towards the centre equally from both surfaces, and will cease when the sponge is saturated. If this simple fact were to be verified in the case of the vein, the poison would never reach the current on its inside. But M. Majendie has most fully and satisfactorily proved that it does reach the current. There must then be some agent beyond mere passive imbibition, to give this unvarying direction from without inwards, to a liquid which, *à priori*, should rather pass in the opposite direction.

Such is the present state of our knowledge of absorption. Some still hold that the lymphatics are the sole absorbents—some that the sanguiferous veins alone perform this function—some that both are concerned in it—all know that absorption does take place. This was known two thousand years ago—Celsus and Galen pointed out the veins as the proper organs—moderns have added or substituted lymphatics.

How much useful knowledge then have we

gained upon this subject in three thousand years? Let us examine the results of the application to practice of the different theories, and see where the advantage lies.

In the very early ages, there appears to have existed no theory on the subject of poisoning from the surface. Men were satisfied with the existence of the fact, and busied themselves only in seeking for a mode of cure.

Philoctetes was restored to health by the skill of Machaon after having been wounded by one of the poisoned arrows of Hercules*; yet Chiron, though himself a teacher of medicine, fell a victim to a similar wound†. The arrow by which Nessus was killed required no poison to effect its purpose, having passed through the centre of his thorax, nor could any antidote have saved him‡.

* Prop. 2—1, 59.

† In the fourth book of the Iliad, Machaon is made to suck the wound of Menelaus. This is certainly the earliest record of a vacuum having been applied to a wound, whether poisoned or supposed to be so.

‡ ——Et missâ fugientia terga sagittâ
Trajecit. Extabat ferrum de pectore aduncum.
—— Sanguis per utrumque foramen
Emicuit, *mistus* Lernæi tabe Veneni. Ovid *Met.* lib. ix.

As to Hercules, he appears to have been destroyed by a corrosive poison*, and no treatment was had recourse to. Of the two treated, one recovered.

Hippocrates, as far as I can recollect, says nothing of poisoned wounds, although he would seem to make allusion to them in the following passage †.

"*Cucurbitulæ, quæ eum in usum fabricatæ sunt ut ex carne attrahant et avellant.*" In this sentence the first mention of cupping-instruments occurs. If they were used in the cure or prevention of traumatic poisoning, there can be very little doubt that they were supposed to act merely by extracting the deleterious matter *(e carne)* from the wound. The pressure of the atmosphere being then entirely unknown, no theory connected with this agent could have existed.

When the blood-vessels were pointed out as the channels through which the poison passed into the system, the ligature above the wound was naturally thought of, and as the *cucurbitulæ* at-

* ——— "Letiferam conatus scindere Vestem;
"Quâ trahitur, trahit illa cutem——" Ovid *loco citato.*

† Hipp. Sect. I. De Medico.

tracted towards them the contents of these vessels, their utility was rather confirmed than otherwise, for it was evident that the blood flowing from the infected surface would carry with it some portion of the poison which had been deposited there.

Accordingly Celsus, with his usual eloquence and perspicuity, places the cucurbitulæ unequivocally at the head of all preventative and remedial agents in cases of recently-poisoned wounds *.

Talking of the bites of animals, and after remarking that all such wounds are more or less envenomed, he says †,—"*Utique autem, si rabiosus canis fuit, cucurbitula virus ejus extrahendum est; deinde, si locus neque nervosus, neque musculosus est, vulnus id adurendum est.*"

For the bite of the viper, he recommends that a ligature should be immediately placed above the wound. "*Dein venenum extrahendum est.—Id cucurbitula optime facit.*" If, he adds, there should happen to be no cupping-instrument at hand, a circumstance which can scarcely be supposed as likely to occur, "*Homo adhibendus est, qui vulnus exsugat.*"

* Celsus, lib. 5, cap. xxvii. † Loco citato.

These passages, and many others to be found in the same author, fully prove—

1. That the *cucurbitulæ* were the chief, if not in his opinion, the only effectual means to be resorted to for the extraction of poison from wounds.

2. That these instruments were so universally applied to this purpose at the time he wrote, that they were always to be found at hand.

3. That direct suction by the mouth was next to cupping the best preventative, and that either of them was sufficient in cases of viper-bites; for in his directions upon this subject the cautery is not mentioned.

After this the question of priority in the application of a vacuum to wounds inflicted by the bites of rabid and venomous animals, for the purpose of extracting the poison, can be entertained only by the antiquarian, and no man more modern than Celsus can be at all contemplated in the discussion of it.

Strabo, Pliny the elder, Galen, Plutarch, all mention the Psylli, the Marsi, and the Ophigines, as having acquired the reputation of being born with the hereditary power of curing the bites of

venomous serpents. The Psylli, as we learn from Celsus, always sucked the wound. *Ergo quisquis exemplum Psylli secutus id vulnus exsuxerit, et ipse tutus erit, et tutum hominem præstabit.*

Plutarch tells us that when Cato commanded an army in Africa, finding he lost more men by the bites of venomous reptiles than by the arms of the enemy, he hired and attached to his camp a certain number of the Psylli and Marsi, who treated their patients by sucking the bitten part until it swelled. *Et ita, fere semper sanabatur Æger, certo periturus si hac ope abesset* *.

Suetonius informs us that when Augustus saw the body of Cleopatra, who had but just expired from the bite of a serpent, he ordered the Psylli and the Marsi to suck her wounds, hoping that the fair victim might still be restored to life through their exertions.

Redi in his treatment of the bite of a viper follows Celsus to the letter †.

Boerhaave under the head *Antidota* observes, that poison may be removed from the body by

* Boerhaave, Antidota.

† Redi, De Viperis.

various means. Formerly, he says, that which was deposited in wounds was sucked out by the Psylli and the Marsi. In our days, '*hodie per cucurbitulas magnas, validas, sæpe renovatas.*' He was one of the last of the mechanical physiologists, and looked upon many of the phænomena of organized matter as more immediately dependant upon physical causes.

The knowledge acquired about this time of the structure and course of the lymphatics; the opposition set up against the mathematical and mechanical physicians by the supporters of *vital action;* but above all, the absence of direct experiment upon the living animal, produced a total change in the doctrines of external absorption. The lymphatics were now denominated the absorbents *exclusively*, whilst the sanguiferous veins were refused all participation in this function.

The consequent revolution which the treatment of poisoned wounds underwent was equally striking. The cupping-glass was laid aside as too mechanical, or if employed, was considered merely as a counter-irritant. The lymphatics of the part had taken up the poison by a peculiar

vital principle inherent in them. Their action must, therefore, be modified. Stimulants must be given to induce the exhalants to throw off the morbific matter. Irritants must be applied to the wound. That unlucky medical adage, *ubi stimulus ibi fluxus*, was found peculiarly applicable. The discharge was to be kept up by every possible means, whilst the vitality of the absorbents was to be destroyed by caustics. The knife and the heated iron were sometimes used, but more frequently by the unlettered cow-leech than by the learned physician.

Messrs. Vellermé and Trolliet, in a long article on *Rage* in the *Dictionnaire des Sciences Medicales* which is highly creditable to the talents and research of these physicians, do not give a single case in which cupping was tried, although they quote this plan of cure from the ancients. In short, from the days of Celsus to the present, I have not been able to meet with any record of a fair trial having been given to the application of the vacuum, either to the bite of the rabid dog or venomous snake, although every author who

has alluded to either of these subjects invariably mentions *cupping*, but merely as a secondary remedy.

M. Orfila, whose profound researches in toxicology justly entitle him to be considered as the highest modern authority in this department of medical science, in enumerating the preventative measures proper to be adopted in the treatment of the bite of a mad dog, recommends cupping the part, for the purpose of promoting a discharge of blood. In his directions for the treatment of a recent viper-bite, the cupping-glass is not mentioned.

Neither M. Majendie nor his followers appear to have founded any new mode of treatment upon the doctrine of imbibition, as applicable to the prevention or cure of traumatic poisoning. How far the injection of tepid water into the veins of animals labouring under hydrophobia may be conducive to their recovery, or whether this practice be connected with the physiology of living *imbibition*, I confess myself unable to declare.

In this rapid and imperfect sketch of the history of external absorption and traumatic poisoning, there are three epochs.

The first extending from the times of Machaon to those of Celsus. The second from Celsus to Boerhaave. The third from Boerhaave to the present time.

In the records of the first period we find but few and imperfect traces of any theory of absorption, while the treatment of poisoned wounds was hidden and disfigured by the religious absurdities of the day.

The second period is marked at its commencement by sounder physiological views, as to the manner in which the poison deposited in a wound was carried into the system. The blood-vessels were considered the channels through which this transport from the surface to the centre took place. To these vessels, therefore, the curative and preventive treatment were chiefly directed.

Some of the most futile and pernicious administrations however still clung to the practice even of the wisest physicians of these times

such as the re-application of the poisoned weapon to the wound which it had already inflicted*. Yet, notwithstanding this, and many other modes of treatment equally inefficient and absurd, the plan of cure pursued by Celsus in cases of wounds inflicted by poisoned weapons, or by rabid or venomous animals, was beyond all comparison more successful than the mode of treatment adopted by the best physicians of the present day.

A failure in preventing the ill effects of the bite of a venomous serpent when suction had been continuously employed was considered so remarkable, that Ælianus, who wrote in the time of Adrian, took the trouble to record, that a mountebank was bitten in the arm by a serpent (*aspide,*) which he was exhibiting in the Forum during the ædileship of Pompeius Rufus, and that *though he sucked the wound himself,* he died in three days, his gums and palate having first mortified. It was not the death of the man, but

* Vulneri cuspis quod intulit hoc prodest; veneno cuspis illita prodest quibus serpens venenum intulit.—GALEN.

the failure of suction in preventing it, that rendered the event remarkable *.

As to hydrophobia Celsus expressly states, that it only occurs when the wound inflicted by the dog has not been attended to; *ubi parum occursum est.*

† The following passage from Celsus will account perhaps for the poor mountebank's misfortune:—" Illud quoque ne interimat ante debebit attendere, ne quod in gingivis palatove aliave parte oris, ulcus habeat.—Lib. 5, cap. ii.

Chapter II.

Can Absorption, strictly speaking, be called a Vital Function?—Definition of Absorption—Why it cannot take place in Vacuo—Its Causes—Proofs that Absorption of Poisons does not take place in Vacuo.

Two of the most powerful and most general agents of nature are gravitation and pressure. Their influence is never for a moment suspended either with regard to living or inert matter: we can conceive no state of organization capable of maintaining an existence independent of their power.

Motion is the effect which renders their operation as a cause perceptible to us. Inert matter moves in obedience to the impulse communicated by them, without offering any resistance of its own by which this impulse can be directed or modified.

Living matter is also moved, but under certain

circumstances it possesses the faculty of modifying the impulse of either or both of these agents, according to the organization peculiar to its mode of existence. The business then of organs as far as relates to these powers, appears to be, to favour one or other of them, to combine, to divide, to oppose them to each other, in short, to modify their operation.

Each organ finds in one or both of these agents, an assistant or antagonist according to the necessity of the action to be performed. Thus the true antagonist to the soaring eagle's wing is gravitation. The fulcrum upon which the wing acts is atmospheric pressure. When the bird stoops upon his prey gravitation is no longer an antagonist, but a powerful assistant to his descent.

When a liquid flows from a compressible tube, or from one open at both ends, if the tube be perpendicularly placed both pressure and gravitation will favour the discharge of the fluid, whilst pressure alone will oppose it; but as the favouring and opposing pressures are equal gravitation will be unresisted.

If pressure be removed from the upper end of this tube, then the gravitation alone of the liquid will be opposed by pressure alone at the lower or discharging end. But as the pressure of the atmosphere is nearly the same at all times, whilst the gravitation of the liquid varies in direct proportion to its specific gravity and the height of its own column, if the sum of these be less than that of the opposing pressure, then the liquid will flow out at the upper opening of the tube, where as pressure has ceased to exist, gravitation alone can offer resistance to the pressure from below.

It is evident that the liquid would have continued to flow out at the lower opening of the tube, if gravitation and pressure had been allowed to remain in their natural relations towards each other; and that to alter these relations in the manner described some third power must have been put in operation. But as inert matter does not *per se* possess this power, although it is capable with its assistance of exhibiting the phænomena just mentioned, it follows that the peculiar and distinguishing privilege of organized

matter, as far as regards these two great agents, does not consist in the phenomena resulting from their modification, but in the self-moved action of the organs by which this modification is produced.

If this view be correct, neither the flowing of the blood through the veins towards the thorax, in direct violation of the immutable law of gravitation, nor the transport of matter by means of this fluid, from the surface to the centre of the living animal, can, strictly speaking, be called a *vital function,* because both are the effects of the modification of pressure, an agent common to all matter. It is that action, or set of actions, by which the modification is produced, to which the epithet *vital* should be attached; because this action is peculiar to living matter possessing an organization such as we at present contemplate.

Thus the word *absorption* representing, in the language of Physiology, the transport of matter from the surface to the centre of a living animal, must be admitted with the same limitations as the word *suction,* conveying, in the language of Physics, the idea of a liquid forced by atmosphe-

ric pressure into a cavity, where, by expansion or otherwise, a tendency to a relative vacuum had been established. Both these terms having been applied to the phenomena connected with them, long before the pressure of the air was known to be the cause of these phenomena, must, in the present state of our knowledge, be considered as equally wanting in philosophical precision, and equally imperfect representatives of the ideas intended to be expressed.

Absorption then, as exercised by living animals, in its physical acceptation, and with reference to matter external to these animals, is the transport of that matter from their surface towards their centre.

According to this definition, when a liquid, such as coloured water placed in an open vessel, mounts against its own gravity through a glass tube having one end immersed in the liquid, and the other inserted into the cavity of one of the great veins within the thorax, the ascent of the liquid, and its flowing into the animal's heart, is a true and genuine act of *absorption*, rendered visible by means of the glass tube, the outer end

of which represents the open mouths of the absorbing veins.

This ascent, or absorption, of the liquid being placed under the influence of atmospheric pressure exclusively, as has been already proved by the experiments detailed in the Memoir on the progression of the blood in the veins, it is evident that, if the liquid were placed under a vacuum, instead of being exposed to the air, it would not flow upwards in the tube, but, on the contrary, would return, provided that the pressure around the extremity in contact with the liquid were rendered less than that around the extremity inserted in the cavity of the vein within the thorax.

Thus the immediate causes or circumstances indispensable to the accomplishment of absorption are reduced to two, *viz.*

1. A free communication between the matter to be absorbed and the thoracic cavities.

2. Atmospheric pressure, modified by the expansion of these cavities around one end of the communicating tubes, while the same pressure is free and undisturbed around the other end.

With these data, and taking for granted that

the sanguiferous or lymphatic veins, or both, are the organs of absorption, their communication with the thorax being exactly the same as that of the tube in the experiment just alluded to, it was natural to presume that the absorption, or transport of any substance, (a poison, for example, deposited in a wound of a living animal,) could not take place if the points of contact of the absorbing surface and of the matter to be absorbed, were placed under the influence of a vacuum.

To prove the truth or error of this induction, I procured different kinds of poison, the fatal activity of which had been well ascertained; such as prussic acid concentrated, pure strychnine, upas tieuté, white oxyde of arsenic, &c. I satisfied myself by repeated trials, that six drops of the acid introduced into the cellular tissue of the thigh of an adult rabbit, would kill him in two minutes—that a grain of pure strychnine deposited in a recent wound of the same animal will produce death in from five to seven minutes, and that a grain of *upas tieuté* will destroy him in ten or twelve minutes.

I experimented with these and other poisons

upon rabbits and dogs, having almost always two animals placed under exactly the same circumstances, except that the piston cupping-glass was applied to one, whilst the other was abandoned to his fate. The animal abandoned invariably perished within the periods stated. The animal, to which the vacuum was applied, never shewed the slightest symptom of poisoning, although the deleterious matter remained in contact with the wounded surface during the space of an hour, two hours, and even so long as five hours consecutively.

When the poison was conveyed by means of a tube under the integuments to some distance from the opening by which it had been introduced, if the cupping-glass was applied to the sound skin, corresponding to the spot where the poison had been deposited (the wound being without the bounds of the vacuum), not only was there no indication that any portion of the poison had been absorbed during the application of the glass, but even after it was taken off the animal continued for one or even two hours to carry imbedded in his cellular tissue a dose which would infallibly

have destroyed him in a few minutes had the cupping-glass not been previously applied.

In these cases, when I waited for the appearance of the tetanic convulsions, the reapplication of the glass immediately suspended them, and the removal of the poison through an incision in the integuments saved the animal.

When I applied the cupping-glass over the opening made in the integuments for the purpose of introducing the tube, leaving the poison under the skin outside the bounds of the vacuum, no absorption took place during half or three-quarters of an hour, but as soon as the glass was removed absorption began.

If, during the application of the glass, I made an incision between its edge and the point where the poison was placed under the integuments, absorption went on as if no vacuum were applied.

Chapter III.

Experiments upon External Absorption—Remarks by M. Andral—Conclusions of M. Laennec's Report.

First Experiment.

On the 12th of August, 1825, at nine o'clock in the morning, in presence of the Rev. Mr. Langley, one of the censors of the University of Oxford, of Dr. Wilson, of the same university, and of M. Miriadeck Laennec, M. D. of Paris, I took two adult rabbits of the same size, and equally healthy. A small wound was made in the skin and cellular tissue of the outside of the left thigh of each. These were filled with precisely equal quantities of impure strychnine; one immediately, the other after an interval of one minute.

After waiting forty-five minutes the rabbits exhibited no other signs of poisoning than some convulsive movements of the muscles of the jaws.

The little wounds were therefore enlarged, and additional portions of strychnine were introduced. Fifteen minutes after the second application, the two rabbits were seized, at the same moment, with convulsions of the most decided tetanic character, which threw their whole frames into the most violent agitation. The spasms lasted some seconds, and returned almost immediately in the rabbit that had been first poisoned, but not so soon in the other.

The piston cupping-glass was now fixed over the wound of the rabbit that had suffered the two convulsions. The other was abandoned to his fate, and died in fifty-five minutes after the second application of the strychnine, having suffered repeated attacks of tetanic spasm and opisthotonos, each exceeding the last in violence and duration.

The rabbit, upon whose wound the cupping-glass had been applied, being placed upon his side, made from time to time some slight struggles, but owing to the forced position in which he was necessarily held, we could not decide whether these movements were convulsive, or merely voluntary.

When the glass, after having been kept on for three-quarters of an hour was removed, and after the wound had been washed, and the rabbit set at liberty, he was seized with a violent attack of true opisthotonos: this lasted about a minute and a half. We all thought him dead, but he recovered with great rapidity, rose upon his legs, and after three-quarters of an hour ate and ran about as if nothing had happened. On the 15th he was again exhibited to the same gentlemen in perfect health, and without having suffered any other attack that I am aware of.

Having read before the Academy of Medicine of Paris a short note containing the details of the above, and some other experiments of a similar nature, that learned body did me the honour to appoint a committee from amongst its members to witness and report upon the repetition of them, and also upon the view I had taken of their physiology.

The committee consisted of the professors Laennec and Orfila, with M. Adelon, secretary to the

Section of Medicine, and author of the work entitled *Physiologie de l'Homme.*

I met these gentlemen at the hospital of *La Charité,* in M. Laennec's amphitheatre, on the 17th of August, 1825, and performed the following experiments. There were present, besides the members of the committee, the celebrated chemists M. Pelletier, Robinet, and Petroz, M. Billery, professor of medicine at Grenoble, and many other physicians and pupils, foreign and French.

Second Experiment.

Assisted by M. Petroz, to whose talents and address I am largely indebted, I took three adult rabbits, and introduced into a wound made in the thigh of each a grain of pure strychnine, brought to the meeting by M. Pelletier, and prepared in his laboratory.

The first rabbit was dead between the fourth and fifth minute. The second rabbit had the cupping-glass applied immediately after the introduction of the poison — the third rabbit at the end of the fourth minute from the deposition of the strychnine in his wound, and after

he had already suffered two attacks of tetanic spasm.

When the glasses were removed after half an hour's application to each, the animals seemed perfectly free from all effects of the poison. The wounds were dressed with sticking-plaster after the poison had been carefully washed off.

Two hours precisely after the removal of the cupping-glass from the wound of the third rabbit, he was seized with convulsions. They yielded immediately to the reapplication of the glass, which was left on for twenty minutes. Neither of the rabbits suffered any other attack, and continued in apparent good health.

Third Experiment.

At the suggestion of M. Orfila (who seemed to think that the salutary effects of the vacuum might be owing to its removing the poison from the surface of the wound), eight grains of the white oxyde of arsenic were introduced deeply under the skin, and into the cellular substance of the thigh of a middle-sized dog. The edges of the

wound were firmly united by suture over the arsenic. The same operation was performed upon another dog of the same size, and with the same precautions. For my own satisfaction, I placed the same quantity of arsenic *superficially* in a wound made at the same point in the thigh of a third dog of equal weight: no suture was used.

Three-quarters of an hour after the insertion of the poison into the thigh of the first dog, the piston cupping-glass was applied. The other two dogs were left to nature.

The vacuum over the wound of the first dog was kept up for five successive hours, during which time the only symptom he shewed of having absorbed any portion of the arsenic was a discharge of saliva rather more copious than natural during the first hour. When the glass was removed, and the stitches cut, the poison was found at the bottom of the wound. The loose skin was cut away, the parts were carefully washed, and the dog set at liberty. He was in perfect health, and continued so for three days, when he was turned into the street.

The increased discharge of saliva was noticed

in both the other dogs. The second whined, and became very uneasy at the end of the first hour after the introduction of the arsenic. Nausea, vomiting, and purging, with tenesmus, came on at the beginning of the third hour. Spasms, convulsions, paralysis of the hinder legs supervened; in short, when we removed the glass from the first dog his case was hopeless: he died in the night.

The symptoms in the third dog came on much earlier after the poisoning, were more intense, and succeeded each other with greater rapidity. According to the accounts of the persons left in charge, he died some hours before the second dog.

The following experiment was instituted with a view to observe the effects of the vacuum not only in preventing absorption, but in mitigating or arresting the symptoms peculiar to the poison applied.

Fourth Experiment.

First rabbit.—Six drops of hydrocyanic acid were poured into a small wound in the integu-

ments of the thigh. At the end of the second minute the animal was dead.

Second rabbit.—Six drops of the same acid were poured into a wound exactly similar to the last. The piston cupping-glass was applied over the wound forthwith. At the end of eleven minutes the rabbit having manifested no symptom of poisoning, the glass was removed in order to observe what might happen. In one minute after this the animal was seized with opisthotonos of so decided a character, accompanied by total cessation of the respiratory movements, that the word *mort* was already written down by M. Adelon; when, as he states in his notes of the experiment, "M. Barry reapplied the piston cupping-glass. In proportion as the sucking effects of the vacuum became more decided, the respiration which had ceased returned, the tetanic spasm became less intense, and more distant in its attacks. At the end of four minutes the rabbit appeared to be perfectly free from the effects of the poison."

Sixteen minutes after this the cupping-glass was again removed. Two minutes after its removal opisthotonos supervened. The glass was a third

time applied, when the spasm immediately subsided. In twelve minutes the glass had fallen off. The convulsions did not return, and the animal continued in perfect health for many days, until he became the subject of another experiment.

Fifth Experiment.

One grain of upas tieuté was introduced through the barrel of a quill, to the distance of about an inch, between the skin and muscles of the thigh of an adult rabbit, where it was deposited, without its having touched the sides of the wound. The little incision through which the quill entered was stitched up, and the cupping-glass was applied upon the sound skin over the poison.

No symptom appeared during two hours that the glass remained fixed, nor for two hours after it had been removed. The rabbit ran about, fed, and appeared in perfect health. At the expiration of this time he was seized with tetanos. The glass was immediately reapplied: the convulsions ceased instantly. After a few minutes application the glass was removed, the upas taken out through an incision made for the purpose, and the parts stained by the solution of the poison were

cut away. The wound was washed and sewed up: the rabbit lived, and did well.

This experiment was repeated with this difference, that the cupping-glass was applied over the external wound, leaving the upas under the skin outside its boundary. No symptom occurred during three-quarters of an hour that the glass remained on, but the moment it was removed the animal was seized with convulsions. These, however, were arrested, and the animal was saved as before.

A third rabbit, poisoned exactly as the two former, and for which nothing whatever was done, died within the eleventh minute after the insertion of the upas.

M. Petroz, with his accustomed ingenuity, who repeated the second variation of this experiment, using hydrocyanic acid instead of upas, reports, that he saved the animal without reopening or washing the part where the poison had been deposited; and that notwithstanding the tetanic convulsions had come on before he could apply

the cupping-glass, he succeeded in saving the animal, by frequently working the piston, volatilizing the acid, and expelling the whole of it through the upper opening of the exhausting syringe, where its characteristic odour was very marked during the operation.

Professor Laennec, who witnessed the whole of these proceedings, drew up a report, in which, after recapitulating the principal experiments, he comes to the following conclusions * :—

" 1st. Your committee is therefore of opinion that Dr. Barry's experiments (being the continuation of those by which he has already endeavoured to prove that the venous circulation is carried on principally under the influence of atmospheric pressure) establish, in the most incontestible manner, the influence of this agent on the circulation of the absorbent vessels, the proposition which the author sought to demonstrate.

" 2ndly. That the knowledge of this important fact may be considered as a *real discovery*, not-

* Vide Appendix, No. 4.

withstanding the theoretical views and vague ideas entertained by some authors, and the empirical administration of suction to poisoned wounds, a practice more common with half-civilized people than more polished nations.

" 3rdly. Your committee proposes that the thanks of the academy be addressed to Dr. Barry, that he be invited to repeat his experiments upon the venom of the viper, that his memoir be inserted amongst those of the academy, and that his name be added to the list of its foreign members.

(Signed) LAENNEC, D. M.

This report having being read at the academy by M. Adelon, it was proposed, that as Messrs. Orfila and Laennec were then both absent from Paris, some new members should be added to the committee, and that further experiments should be instituted for the purpose of ascertaining, if possible, the following points, *viz.*:

1st. Whether the cupping-glass placed elsewhere than over the poisoned wound, or its imme-

diate neighbourhood, would, by acting as a *counter-irritant,* prevent absorption, or relieve the symptoms caused by it.

2ndly. Whether the cupping-glass acts upon the symptoms by recalling to the surface any portion of the matter already absorbed.

3rdly. How long its application may be delayed after the insertion of a given poison, and yet prevent the appearance of the symptoms.

Accordingly, M. Pariset, perpetual secretary to the academy, M. Andral, Jun., and M. Segalas, with M. Adelon, were named as a new committee. In their presence, and at their suggestion, the following experiments were performed.

The details are literally translated from the notes taken by M. Andral on the spot.

Sixth Experiment.

" One grain of upas tieuté was introduced into the subcutaneous cellular tissue of the thigh of a rabbit, the wound was closed by a suture. Tetanos came on at the eleventh minute; at the end of the twelfth minute death."

Seventh Experiment.

" One grain of upas tieuté was introduced into the thigh of a rabbit as above. The cupping-glass was applied one minute after, and left on twenty-four minutes. About two hours after the glass had been removed symptoms of tetanos came on. Reapplication of the glass for ten minutes—immediate cessation of the convulsions —poison removed from the wound—parts washed —animal restored to health."

Eighth Experiment.

" Introduction of a grain of upas into the thigh of an adult rabbit as above. *Three* minutes after the glass was applied, and left on twenty-four minutes. Poison removed, wound carefully washed: no symptoms."

Ninth Experiment

" One grain of upas introduced as before into the thigh of a full-grown rabbit. *Six* minutes

after the cupping-glass was applied, and left on twenty-four minutes. Wound treated as in last experiment: no symptoms."

Tenth Experiment.

"The last experiment repeated upon another rabbit. Glass applied *ten* minutes after the introduction of the poison, that is, less than one minute before the period when the symptoms of poisoning began to appear in the first rabbit. The glass was left on twenty-four minutes. No symptoms: wound treated as before."

Eleventh Experiment.

"Injection of six drops of hydrocyanic acid *(au quart)* into the cellular tissue of the thigh of an adult rabbit. In one minute convulsions, in two death."

Twelfth Experiment.

"Similar injection in another rabbit. Convulsions rather before the end of the first minute.

Application of the glass: immediate cessation of the spasms, and permanent restoration to health, as in the other experiments."

Thirteenth Experiment.

" Introduction of *four* grains of upas tieuté into the thigh of a small dog. The piston cupping-glass was applied at the same time to a similar wound on the corresponding part of the opposite thigh. Symptoms of poisoning at the end of eight minutes: these soon acquired such a degree of intensity that the animal was upon the very point of expiring. In this state of extreme suffering the cupping-glass was removed to the poisoned wound, and the vacuum established. Instantly the symptoms were alleviated. The animal was truly recalled to life; but from time to time he still suffered slight attacks of tetanos. After a quarter of an hour's application the glass was removed, and the animal appeared restored to health *."

* This animal was found dead some hours after the glass had been removed.

Remarks by M. Andral.

" In this case the cupping-glass appears to have moderated the symptoms by arresting all further absorption of the poison; but that which was already in the circulation does not seem to have been recalled to the surface of the wound, because the symptoms continued, although mitigated; unless we choose to suppose that the continuation of the convulsions was owing to the impression already made upon the nervous system. On the other hand, the animal economy does not rid itself of deleterious substances so promptly as is generally thought: this the following experiment would seem to prove."

Fourteenth Experiment.

" One quarter of a grain of pure strychnine dissolved in two ounces of distilled water was injected into the trachea of a middle-sized dog. For several hours after the animal showed by the stiffness of his limbs, and by a convulsive agi-

tation from time to time, that he was still under the influence of the poison."

Fifteenth Experiment.

"With a view to observe whether the cupping-glass acted by bringing back to the surface any portion of a substance introduced into the cellular tissue by injection, we injected into the subcutaneous tissue of the inside of the thigh of a dog about two drachms of a saturated solution of the *sulfate of soda.* The wound was carefully wiped, and the glass applied. After working the piston a few times, we found the salt, by means of a proper test, in the reddish liquid which the pressure of the air had forced into the glass."

The above and many other analogous experiments were repeated and varied before many French and foreign physicians, but never exhibited the slightest anomaly.

Chapter IV.

Experiments upon the Bite of the Viper.

For the purpose of giving a more useful application to this method of preventing poisoning by external absorption, I had several dogs and rabbits bitten by vipers, of which I had procured a considerable number from Grenoble and Fontainbleau. To the bites of some I applied the cupping-glass, to the bites of others nothing; and although the animals abandoned did not ultimately perish, the results obtained by the comparison were precisely analogous, as far as regards the symptoms, to those observed in the preceding experiments, that is, the animals bitten by one, two, and sometimes three vipers, when the cupping-glass was applied for half an hour, suffered no symptom whatever of constitutional poisoning; whilst those that were left to nature

were invariably attacked with convulsions, stupor, and the dogs by vomiting.

Pigeons invariably perished from one bite of the ordinary viper of Fontainbleau, exhibiting, when left to nature, the commencement of the fatal symptoms before the fifth minute; but when the cupping-glass was applied immediately after the bite, they not only showed no signs of having absorbed the venom while the glass remained on, but eventually escaped when the treatment to be noticed hereafter was adopted.

The local action of the viper's venom, mentioned by Fontana, so marked and so rapid in its effects, seems to be concentrated by the cupping-glass within its own bounds, particularly in dogs, but is entirely prevented in rabbits. This difference is owing to the different density of the skins of these animals. The vacuum sucks a reddish serum in considerable quantity through the skin of the latter, whilst very little or nothing is forced through the skin of the former.

Experiments made with living Vipers upon Dogs, Rabbits, and Pigeons.

Sixteenth Experiment.

On the 29th of September, 1825, in Baron Cuvier's anatomical laboratory, where, with his usual condescension, he was kind enough to permit me to avail myself of the talents and dexterity of M. Rousseau, Jun., one of his principal preparators, a large viper was applied * to the thigh of a half-grown weakly rabbit. The reptile bit twice: a minute drop of blood marked each puncture made by the fangs. One minute after the bites the piston-glass was applied upon the bitten part. M. Rousseau, who held his eye close to the glass whilst I worked the piston, observed a drop of transparent amber-coloured liquid issue from each of the punctures. This was followed by a considerable quantity of

* M. Rousseau applied the vipers by seizing them with a long forceps behind the posterior projecting angles of the head, and placing their nose in contact with the part intended to be bitten: they never failed to bite as often as we wished.

reddish serum, which rose into a thin froth, and in fifteen minutes nearly filled the glass with its large transparent bubbles. The vacuum was kept up for thirty-five minutes. When the rabbit was set at liberty he appeared to suffer no inconvenience: the little wounds presented nothing remarkable.

One hour after this rabbit had been bitten the same viper was presented to the thigh of another, which he bit twice also, drawing blood as before. The second rabbit was larger and much stronger than the first. A pale yellow spot was noticed almost immediately around each puncture made by the fangs. When the animal was set at liberty the bitten leg appeared slightly paralyzed. Ten minutes after the bite, the whole integuments of the bitten part appeared livid. Half an hour after, the lividity was intense, and had extended to the circumference of half a crown.

The next day an open gangrenous ulcer occupied the whole of the livid circle, discharging a fetid sanies. The leg and thigh were swelled. Forty-eight hours after the bite, the ulcer was

still open, but not so fetid. Seventy-two hours after the bites, the ulcer looking healthy, the limb reduced.

During all this time, the rabbit first bitten never showed the slightest symptom of either local or general poisoning. The second rabbit refused his usual food during the first thirty hours after he had been bitten, and looked dull.

Seventeenth Experiment.

On the 13th October M. Rousseau, with his accustomed dexterity, applied two large fresh vipers to the thigh of a middle-sized dog. The part had been previously shaved. Each viper bit twice with eagerness. Two minutes after the first bite, a cupping-glass was applied over the punctures. Dr. Edwards, who honoured this experiment by his presence and assistance, observed several drops of a yellowish-red fluid, oozing from the little wounds inflicted by the viper's teeth.

The glass remained fixed thirty minutes, and was then removed. Some very slight scratches,

which did not go through the skin, having been made with a razor, the cupping-glass was again fixed on, but the quantity of blood extracted did not exceed a drachm and a half.

At the end of forty minutes from the commencement, the glass was finally taken off, and the part washed. Large livid spots were distinctly perceived around the wounds inflicted by the fangs.

The dog did not appear to have suffered the slightest inconvenience from having been bitten. He ate and drank. Twenty-four hours after the bite there was still no symptom either local or general. On the second morning a gangrenous eschar was found to occupy the whole of the integuments which had been included in the cupping-glass. The leg and thigh were swelled, but the general health of the dog seemed unimpaired. His lameness was scarcely perceptible; in short, the eschar was thrown off in a few days, leaving a clean sore, which healed soon after; and the animal recovered without any other symptom than those mentioned.

Eighteenth Experiment.

To ascertain whether the vipers used in this experiment were really venemous, one of them was presented to the breast of a young pigeon, and suffered to bite *once*. Although this was the third bite made by the reptile within an hour, the bird showed symptoms of being affected by the poison at the third minute, fell on his side at the fifth, and died at the end of the twentieth minute after he had been bitten.

Nineteenth Experiment.

Another dog of the same size as the subject of Experiment No. 17, was also bitten by two large vipers, and exactly in the same manner. He showed strong symptoms of suffering about the eighth minute after the bites, uttered plaintive sharp cries, and became excessively restless. At the fifteenth minute made violent efforts to vomit; vomited abundantly at the twentieth; then lay down upon his side at

full length in a kind of stupor. In this state he continued the whole of that day, refusing food and drink.

Next morning the bitten leg was much swelled. The parts livid; ulceration already commencing. The animal dull, dejected, and difficult to be roused. After extensive gangrenous ulceration he recovered, but very slowly, and was much emaciated.

Twentieth Experiment.

On the 24th October two adult rabbits were bitten, each by three vipers, and by each viper three times. To one of these rabbits I applied the cupping-glass, which was left on thirty minutes. In this as in No. 16, I observed a considerable quantity of serous fluid ooze through the skin, and afterwards expand into thin froth with very large bubbles, filling the glass. I now dissected out the skin and cellular substance which had been included under the glass, applying the vacuum again for ten minutes; after which the

wound was washed and the lips of it brought together by suture. The rabbit when set at liberty appeared to be in perfect health.

The other rabbit had been left to his fate. On the 25th, at five in the afternoon, the cupped rabbit was as well as if nothing had happened to him: the wound in the thigh looking exactly as if it had never been touched by a viper's tooth, and inclining to heal.

The rabbit that had been left to nature hung his ears, and looked very dull: the bitten thigh was much swelled, whilst a large gangrenous livid phlyctena, filled with a thin sanies, occupied the whole of the bitten part.

On the 27th, the cupped rabbit in excellent health: the wound healing without any appearance of gangrene. The phlyctena in the other rabbit had degenerated into an extensive fetid ulcer. This animal after much suffering finally recovered.

Twenty-first Experiment.

In presence of M. Dumeril, professor of physiology, a young pigeon was bitten twice over the

pectoral muscle by a very large viper. The cupping-glass was applied immediately after the second bite, and left on *eight minutes only*. Nothing else was done. No symptoms of poisoning occurred for fifteen minutes after the removal of the glass, when the bird began to stagger. In a few minutes he fell upon his side, his respiration becoming remarkably slow. This pigeon was dead at the expiration of an hour and sixteen minutes after the second bite. About fifteen minutes before his death, the cupping-glass was again applied, but produced no visible effect.

A second pigeon had been bitten by a very small viper twice, exactly in the same place as the first. Five minutes after the first bite he showed the usual symptoms of poisoning, such as inability to stand, falling on the bitten side, slight convulsions. He died at the end of the fifty-fifth minute from the first bite.

Dissection.—Upon examining the bitten parts of both pigeons, the whole of the great, and a large portion of the lesser pectoral muscles were livid, tender, and almost decomposed in the pigeon that had not been cupped. The correspond-

ing parts in the cupped pigeon were perfectly natural, with the exception of two livid spots which we traced around two distinct veins, into the thorax. The intestines of both pigeons presented traces of recent and active inflammation with livid vascularity.

Fontana lays it down as a law, in poisoning by the viper's venom, *that the longer the animal has survived the fatal bite, the more intense are the lividity and decomposition of the bitten parts.* In this experiment the reverse was strikingly manifest.

Twenty-second Experiment.

On the 5th November, in Baron Cuvier's laboratory, and in presence of Messrs. Rousseau, father and son, a small-sized old dog was bitten in the thigh by three vipers, and by each viper three times. The reptiles had been previously much excited. Three minutes after the first bite the piston-cupping-glass was applied and kept attached for fifteen minutes. It was then removed, and the whole of the skin and cellular substance, down to the muscle, which had been included, within the vacuum, was removed by the knife. The

glass was again immediately applied over this fresh wound, and kept on for fifteen minutes longer. The parts were now carefully washed. Some little bits of livid cellular substance were removed. The lips of the wound were brought together by suture, and the dog set at liberty. Not the slightest symptom of poisoning appeared about the animal. Two hours after his wounds had been dressed, he escaped from the servant, and ran with such vigour as to leave his pursuers no chance of coming up with him.

Twenty-third Experiment.

On the same day M. Rousseau, junior, presented a very large viper, which had been particularly excited, to the bare breast of a young pigeon, three-quarters grown. The viper bit deeply and eagerly *once*. Both the little punctures made by the fangs were marked, each by a small bloody stain. The piston-cupping-glass was applied forthwith. Two amber-coloured drops were now seen to issue from the little wounds already noticed, and were very soon followed by minute quantities of very dark-coloured blood. The glass was kept

on fifteen minutes. The livid parts around the little punctures were now dissected out. A gangrenous phlyctena had already formed, containing a thin ichor. After the infected parts had been removed, the glass was again put on for ten minutes. Again the glass was removed, and a small portion of muscle, or rather of a livid vein running into the muscular flesh, was dissected out. Not the slightest symptom of poisoning appeared. The pigeon walked upright and seemed in perfect health.

9th November.—The pigeon has continued to enjoy good health, and was this day shown to M. Rousseau. The following is his note:—

"J'ai vu le pigeon que nous avons fait mordre le samedi cinq de ce mois. Ce même pigeon est très bien portant le neuf. Au Jardin du Roi, le 9 Novembre, 1825."

Fontana states that amputation of the pigeon's leg *three or four seconds* after it has been bitten by a viper, saves the animal; but if the operation be delayed *one minute*, however high above the bite it may be performed, instead of saving the animal, it hastens his death.

CONCLUSIONS.

From these experiments, and from the uniformity of their results, we may consider the following facts as proved :—

First.—That neither sound nor wounded parts of the surface of a living animal can absorb when placed under a vacuum.

Second.—That the application of the vacuum by means of a piston-cupping-glass placed over the points of contact of the absorbing surface, and the poison which is in the act of being absorbed, arrests or mitigates the symptoms caused by the poison *.

Third.—That the application of a cupping-glass for half an hour deprives the vessels of the part over which it had been applied of their absorbing faculty, during the hour or two immediately succeeding the removal of the glass †.

Fourth.—That the pressure of the air forces into the vacuum, even through the skin, a portion of the matter introduced into the cellular tissue

* Vide Exp. No. 4. † Vide Exp. No. 5.

by injection; that is, if the skin of the animal be not too dense, as in the dog. (Exps. 16, 20.)

From these facts we again arrive at the conclusions already established by the experiments detailed in Part I., viz.,—

1st. That *the taking up* of matter from the surface by the sanguiferous and lymphatic veins, and the progression towards the heart of the contents of these vessels, are placed under the influence of atmospheric pressure, in all animals possessing thoracic cavities, and exercising over them the power of alternate contraction and dilatation around that point to which the centripetal current of their circulation is directed.

2d. That, as the veins and lymphatics communicate with the thoracic cavities nearly in the same manner, the cardiac ends of both must be exempt from atmospheric pressure when the thorax is expanded, and therefore the pressure on the surface and extremities of these vessels being unresisted at this moment, except by gravitation, must not only press their contents upwards, but also force matter from abroad into their open mouths, or

porous sides, when stript of their more dense coverings.

3rd. That as the height of the column of lymph exceeds that of the column of blood in the lower cava, by the distance from the lower point of the right auricle to the upper part of the subclavian vein in man, and as the course of the lymph is more tortuous and indirect (from passing through glands) than the course of the venous blood; it follows, that the velocity of the transport of matter from the surface to the centre, must be less in the lymphatic, than the sanguiferous veins, and that the comparative quantity transported by the two sets of vessels must be influenced by the circumstances already noted, and by the relative capacity of the vessels themselves. The difference in the specific gravities of blood and lymph should, perhaps, be also taken into calculation.

4th. That as *imbibition, transudation, or passive soaking* of a part in a liquid may take place *in vacuo*, neither can be the agent which induces or compels matter deposited on the surface to penetrate into the cavities of the veins; for although the

cupping-glass may arrest the current of the circulation in the smaller vessels during the period of its application, and even for some time after its removal, yet if imbibition could force the poison, which had been lying in the wound for hours, into their tubes, the washing of the part after taking off the glass would not save the animal from the effects of a substance which with the simple contact of the atmosphere would have killed him in a few minutes.

Chapter V.

Comparative Absorbing Powers of the Tissues.—Morbid Poisons.—Contagion and Infection.

Seeing, then, that atmospheric pressure favourably modified, and a free communication with the thoracic cavities, are the two indispensable requisites to enable any part to accomplish the function of absorption, we might *à priori* conclude, that the absorbing powers of the different tissues stand in direct proportion :—

1st. To the pressure to which their veins are exposed.

2nd. To the freedom of communication with the thoracic cavities.

3rd. To the permeability of the mouths and coats of the veins.

4th. To the number of the veins.

Accordingly we find that the membrane or tissue in which the air-cells of the lungs are formed absorbs with the greatest rapidity, because it unites in the most perfect degree the above con-

ditions. Its veins are the most numerous. Their communication with the central cavity of the thorax is the shortest and most direct. Their coats are the most pervious ; whilst their contents are forced forward by the whole pressure of the air rushing down the trachea during inspiration, increased by rarefaction and the resistance of the bronchiæ and air-cells.

Twenty-fourth Experiment.

One grain of alcoholic extract of nux vomica, dissolved in two ounces of distilled water, and injected through the trachea into the lungs of a dog, produced tetanic spasm of the limbs and opisthotonos within the tenth *second* from the commencement of the injection, and death in less than two minutes. He breathed freely after the syringe was removed.

A similar quantity of the same liquid was injected through a stop-cock, which had been previously fitted into the trachea of another dog, and the stopper was turned the moment the

injection was completed. The symptoms came on some seconds latter. Opening the stop-cock, and allowing the animal to breathe, did not protract his existence.

Twenty-fifth Experiment.

One ounce of alcohol was injected into the jugular vein, towards the heart, of a full-grown fox-hound. In a few minutes he appeared to be profoundly intoxicated. Half an hour after this operation, when the animal began to recover, but whilst he was still breathing slowly, as if apoplectic, *four* grains of spirituous extract of nux vomica, dissolved in six ounces of distilled water, were injected into his lungs, through an opening made in the trachea. Ten seconds after the completion of the injection, he was seized with strong tetanic convulsions. At the end of the third minute he appeared to be quite dead; at the fifth minute respiration returned, and with it the convulsions. Each convulsion pulled back his head, stretched out all his limbs stiff and separate, and lasted exactly during the act of inspiration. In expiration

the spasm relaxed, but invariably returned with each inspiration. This coincidence of the tetanic spasms with inspiration continued six minutes, the spasms becoming more and more distant as the respiration became slower, until death closed the scene.

At the opposite extremity of the scale of absorbing tissues stand the ossesus, the fibro-cartilaginous, the epidermoid. In these there is no absorption, although there may be *imbibition.*

Fontana could never succeed in producing any effect by the application of poisons to the bare insulated nerves of living animals.

Between the extremes of the scale are ranged the subcutaneous cellular tissue, the visceral mucous*, the serous, and other tissues lining cavities.

The conjunctiva absorbs freely, because its vessels are numerous, their coats thin, and exposed to full pressure.

* The peculiarities attached to the absorbing powers of this tissue are reserved for a separate paper.

Pliny the Elder has recorded a very curious observation with regard to the membrane lining the female parts of generation*, and although it is calculated to throw the most important light upon a very interesting point of legal medicine, I am not aware that it has been noticed by any writer upon that subject.

These experiments account for the communication of disease without contact. The infective matter of small-pox is more abundantly and more fatally taken into the system by breathing the atmosphere of the variolous, than by inoculation —the plague, by inhaling the effluvia of the pest-house. In short, whatever poison is capable of being suspended or dissolved in the air as a menstruum, must inevitably pass into the blood of those who respire this air thus infected. "Qui

* Cum constat omnium venenorum ocyssimum esse aconitum[a]. Tactis quoque genitalibus fœminini sexus animalium, eodem die inferre mørtem. Hoc fuit venenum, quo interemptas dormientes, a Calpurnio Bestia, uxores, Marcus Cæcilius accusator objecit. Hinc illa atrox peroratio ejus in digitum.— *Plinius Secundus, Hist. Nat.* lib. xxvii.

[a] Aconitum, supposed to be a compound similar to the hunting-poison of the Gauls.

cum non respirare non possunt, contagium miseri, evadere nequeunt*."

Certain states of the atmosphere connected with heat, moisture, agitation, electricity, &c., may enable it to hold in suspension a greater or a less proportion of morbific matter. The quantity absorbed by those who respire it must stand in direct relation to the quantity thus applied to the mucous surface of their lungs.

1st. Some poisons are incapable of being dissolved in the atmosphere, at least in sufficient abundance to produce their usual effects upon man. Such are the vaccine virus, and, generally, all those peculiar to the brute creation.

2dly. Some poisons cannot be sufficiently concentrated to affect the system through any other surface than that of the air-cells of the lungs. Such are the deleterious gases and effluvia.

3dly. Some are capable of infecting through all vascular tissues, but most fatally through the lungs, owing, perhaps, to the greater extent of surface to which they are there applied, and to

* Galen, 5ª. 96 G.

the other circumstances just noticed. Such are the virus of small-pox and plague.

The specific morbid poisons of the first and second classes are limited in their effects to certain surfaces. Those of the third class are unlimited. None, however, can communicate disease, even supposing all other things favourable to its developement, without the existence of one condition, indispensable alike in all cases, *viz.*, the contact of the poison with the surface through which it is to pass into the circulation.

But to bring about this contact between the lungs and the virus of small-pox or plague, a certain approach must be made towards the source of infection; for it is only around this source that the atmosphere can be so charged with the infective matter as to afford sufficient for respiratory absorption.

If the air around an infected individual, or bale of goods, could be so impregnated with the emanation of variolous or plague virus, or with the germs of any other disease whatever, as the distilled water was with nux-vomica in the 24th and 25th experiments, there cannot exist even a sha-

dow of a doubt that a sound individual respiring that air, would be more rapidly and more abundantly poisoned than he could be by inoculation.

If one infected individual cannot furnish enough of virus to charge the atmosphere around him with the seeds of his disease, we know that a greater number can; and if the air be not disposed at one time to hold these germs suspended, we know that at other times it is so disposed. Therefore, whilst men have lungs constructed as these organs are at present—whilst the mucous surface of these lungs *are* exposed to the contact of every thing the atmosphere holds in solution—and whilst it is certain that the most fatal poisons may be thus deposited on the most rapidly-absorbing tissue of the whole frame, the healthy should be carefully and distantly separated from the infected; nor should they ever, under any circumstances, respire the air which the emanations from the latter may have poisoned.

From what has been said on the subject of specific morbid poisons, may be seen the incorrectness, nay, even the dangerous tendency of the

distinction lately attempted to be established by some writers, between *contagion* and *infection.*

If contagion be considered as having reference only to the necessity of *contact* between any of the specific poisons and an absorbing surface, then all the diseases communicated by morbific matter, whether solid or gaseous, must be ranged under the head of *contagious.* But if it refer to the presumed necessity of contact between sound and infected individuals, then none of the diseases alluded to can be called contagious, because this kind of contact is in no case necessary to their being communicated.

The word *infect* and its derivatives clearly convey the idea of something noxious introduced into the system. They admit of no quibbling ambiguity, and should, in sanatary logic, universally supersede the use of the word contagion and its adjectives.

Chapter VI.

Application of the foregoing Principles and Experiments to Practice in the Treatment of Poisoned Wounds.

In applying the principles developed in the preceding reasonings and experiments, to the prevention and cure of the symptoms usually produced by the absorption of deleterious matter deposited in a wound, or on an abraded surface,—I shall consider, first, those cases in which the poison is simply placed in the wound, and does not exercise any local action on the tissues of the part.

Secondly, Those cases in which the poison is injected into the parenchyma, or vessels of the part, and when some local action is visible.

Thirdly, That unique variety of poisoning produced by the bite of the rabid dog.

My observations as to curative and preventive

measures shall be confined to such as are entirely physical and external.

These are, 1st, The ligature between the poisoned wound and the heart. 2d, The cupping-glass, or vacuum. 3d, Excision and scarification. 4th, The actual cautery. 5th, Protection from atmospheric pressure.

1. In all cases of superficial poisoning, when the deleterious matter is simply deposited in the wound, the application of the cupping-glass over the point of contact will save the individual, provided it be made with the precautions to be noticed hereafter, and before a dose sufficient to cause death shall have been absorbed.

2. In cases where the poison has been injected, as, for instance, by the hollow fang of a viper or rattlesnake, though the cupping-glass may have been applied, yet as the local action of the venom goes on in vacuo, the parts acted upon should be cut out after the venom has been concentrated and partly extracted by the cupping-glass, which should be immediately reapplied over the wound made by the knife, for the purpose of extracting the contents of the newly-divided vessels from a

greater distance than could be done before the operation. After this the actual cautery may be administered, if thought necessary; but never under any circumstances before the second application of the cupping-glass, for this reason,—that when the mouths of the vessels are hermetically sealed by the hot iron, they can give out nothing to the vacuum.

3. The poisoning that results from the bite of a mad dog, so far as regards the simple deposition of the deleterious matter in the wound, and the total absence of local action upon the wounded tissues, comes strictly under the first, or least complicated class of cases. But the tardiness with which the poison is absorbed, or if absorbed, with which it produces its peculiar effects, entitles it to be considered as a species *sui generis*.

Fortunately this anomaly does not alter the preventative indications. These are purely physical, and as such must be ever unvaried. The first thing, then, to be done in treating the recent bite of a rabid dog, is to apply a powerful cupping-glass over the wound. This measure supersedes at once the ligature, ablution, exci-

sion, *&c.*, during the period of its application, and for a certain time after its removal *. 2. After the cupping-glass has been applied for an hour *at least*, the whole of the parts wounded or abraded by the bite should be freely dissected out. 3. The cupping-glass should then be reapplied immediately for the reasons already stated. 4. The wound should next be hermetically sealed by the actual cautery. 5. The part should be as little exposed to the contact of the air after the slough comes away, and as soon healed up, as possible.

If the first application of the cupping-glass shall have so concentrated the poison, as that the excision of the part will remove it, or if the second application of the glass shall have recalled such particles of it as may have been forced into the wounded vessels too far to be reached by the knife, but not beyond the limits of the influence of the vacuum, the individual will be as secure against hydrophobia as if he had never been bitten. But if the poison has already been transported into the circulation, there to undergo its

* Experiments 5—7.

incubation, it is evident that no external measures can be of use.

The notion that the hydrophobic poison is absorbed after the manner of other substances similarly circumstanced, but that it does not produce its peculiar effects, until it has wandered through the *penetralia* of the animal for forty days or longer, is in direct opposition to all analogy.

The experiments which we have witnessed with the vegetable, mineral, and reptile poisons, applied to animals externally, prove that the commencement of the symptoms is synchronous with the consummation of the absorption, and that their repetition is dependant upon its renewal.

When the hydrophobic wound has been cicatrized, previously to the appearance of the symptoms, we almost always find that it either opens again by ulceration, or that a painful line is felt extending from it towards the thorax. Indeed, both these circumstances are often observed. Hence arises a strong presumption that it is only at this moment the fatal absorption commences, and that, as we have seen in experimental poisoning, the completion of the first act of absorp-

tion is soon followed by the appearance of the disease peculiar to this species of infection.

In order that specific constitutional disease should be produced by the application of an animal poison to a wound, it is necessary, 1st, That the quantity of the poison be increased by the assimilation, to a certain extent, of the matter with which it is placed in contact. 2d, That this augmented, or assimilated virus should be carried into, and mixed with the blood, and that the whole mass of the circulating fluids should be thereby contaminated.

The first of these conditions is observed when syphilitic, variolous, vaccine, or glanders-poison is applied to an absorbed surface.

The second is proved beyond all question, by the admirable experiment lately made by Professor Coleman. He transfused some of the blood of a glandered horse into the veins of a sound horse, and thus communicated the disease.

This experiment alone would entitle Mr. Coleman to hold that high rank amongst the physiologists of Europe, which he so eminently occupies amongst those who know him, or have

enjoyed the opportunity of hearing his highly-interesting lectures on the physiology and pathology of the horse.

Under the presumptive impression, then, that in hydrophobic, as well as in all other species of poisoning, the transport of the deleterious matter from the wound into the system, and the appearance of the symptoms peculiar to the poison, follow each other as cause and effect—as soon as the cicatrix begins to feel at all tender, or when there is sufficient evidence that the animal which inflicted the bite was rabid, we should immediately apply the cupping-glass, and proceed exactly as in the case of a recent bite; nor should the actual presence of hydrophobia deter us from this proceeding, any more than the presence of tetanic spasm in repeating the Fourth Experiment.

It may here be asked, how is it that the cupping-glass should now rank so low as a measure of prevention or cure in poisoned wounds, whilst its character remains unimpeached from the time of Celsus, the day of its supremacy, up to the present hour?

The answer is, that as the true mechanism of absorption was never understood, nor ever thought to be connected with atmospheric pressure, the *ratio medendi* of the vacuum must have been but imperfectly comprehended, and therefore the circumstances which might promote its success, or contribute to its failure, could not have been duly appreciated.

As the laws presiding over physical causes and effects must ever have been, and must ever remain the same, the failure of the cupping-glass when it ought to have saved the individual, can only be attributable to improper interference with the poisoned wound previously to its application. This interference usually consisted; 1. In scarifications, which might or might not extend beyond the area to be covered by the mouth of the glass. 2. In the actual or potential cautery. 3. In free exposure to the air.

Celsus recommends surrounding the wound with incisions or scarifications before the cucurbitula is placed over it. "*Neque alienum est ante scalpello circa vulnus incidere.*"

Galen to this preparatory measure adds the ac-

tual cautery. " *Scalpello circumcide vel igne amputa, ventosam post hæc ni locus vetat infer* *."

Here are two records fully proving that the vacuum could not always have succeeded even in the hands of Galen, although applied with due promptitude after the insertion of the poison.

If it be an object to impress a retrograde direction upon the fluids connected with the wound, and thereby recall to the surface any particles of the poison which may have already entered the mouths or pores of the divided vessels, it is evident that the more exclusively the pressure is directed to the wounded surface, and to the little vessels connected with it, the greater will be the probability of their contents being squeezed out into the vacuum: it being an invariable law, that of many things equally pressed, that which requires least pressure to be moved will yield first, and move in the direction where there is least resistance.

Now, when the soft parts about a wound, however minute, are forced into the vacuum of a cupping-glass, the point which offers the least resistance

* Galen, (5 ª, 96 G.)

to the exit of the fluids contained in these parts is the little wound itself. But if scarifications have been made around it, it is no longer so. Therefore the balance between the vacuum within the glass, and the pressure without, will tend to be established by a discharge from the scarifications, and not from the original wound. Hence the probability of the poison being forced out of the wound and the vessels around it, will be diminished in proportion to the magnitude of the scarifications.

If these scarifications extend beyond the area of the vacuum, the contents of the vessels thus divided will cease to be influenced by it, and therefore whatever portion of the poison may have passed beyond the point of division, will be carried to the heart, as if no vacuum had been applied.

If actual or potential cauteries shall have been used, and if any portion of the poison remain beyond the depth to which their action may have extended, the application of the vacuum will be perfectly useless, because the openings through which the poison might be pressed out are sealed up.

The adoption, then, of any preparatory measure previously to the application of the vacuum to poisoned wounds, must deduct from the probability of success, as well by the countervailing effects of the measures themselves, as by the loss of time they occasion. The ligature, recommended by Celsus to be placed between the wound and the heart, but not so tightly as to deprive the limb of sensation, should, with simple ablution of the part, and protecting it from the contact of the air, be the only remedial measures ever suffered to precede the application of the vacuum; and even these, only when a cupping-glass or suction by the mouth cannot be immediately commanded.

Excision and cautery can be of use only in proportion to their extent. If they reach beyond the poison they will certainly save, but not otherwise. The particles which had been already forced further than the boundary of the excised wound, will be sent to the heart with greater rapidity after the operation than they otherwise would have been*, owing to the wider mouths of

* Fontana, Experiments upon Pigeons.

the vessels being now fully exposed, and open to receive the atmosphere within their cavities.

When the cupping-glass has been applied for an hour to the poisoned part, previously to removing it with the knife, the contents of all the vessels will have acquired a retrograde direction, and from not being permitted to flow freely into the vacuum, a perfect stasis of the fluids is established; hence the loss of the absorbing faculty of the cupped surface already noticed. (Experiments 5—7.)

Thus by allowing the first cupping to precede the excision of the part, not only is there a greater quantity of the poison removed, but the danger of a more rapid absorption is avoided, whilst the certainty of extracting a still further portion, or, perhaps, the whole of what may have remained, constitutes an additional and important advantage to be obtained by the second cupping *.

The advantage to be derived from the actual cautery, after the excision and second cupping, is also of a strictly physical nature. The

* Experiments 22, 23.

burning of the little vessel hermetically closes its mouth, and renders its tube incompressible for a certain extent. Its absorbing powers, therefore, are suspended, because the pressure of the atmosphere can neither force any thing into it, nor compress it upon its own contents, so as to force them forward towards the heart.

APPENDIX.

APPENDIX.

No. I.

M. Legallois's Report of the Experiments on the Venous Circulation, made at the Faculté de Médecine.

EXPERIENCES FAITES A LA FACULTE DE MEDECINE PAR M LE Dr. BARRY.

N. B. Les rapports indiqués supposent l'animal debout, dans une position naturelle.

Première Expérience.

Le 14 Juin, en présence de MM. Laennec professeur à la Faculté, Breschet, chef des travaux anatomiques, Billery de Grenoble, Bennet, chirurgien du Collège de Londres, et de plusieurs élèves, M. Barry a répété l'expérience suivante, déjà consignée dans le mémoire qu'il a eu l'honneur de lire à l'Académie des Sciences dans la séance du 8 de ce mois.

La jugulaire interne fut mise à découvert sur un chien de petite taille. Une incision ayant été pratiquée aux parois de cette veine, une sonde de gomme élastique fut introduite dans sa cavité, et dirigée vers le cœur. Au bout

extérieur de cette sonde était fixé un robinet, dans l'autre extrémité duquel on introduisit un tube de verre coudé à angle droit et en partie contourné en spirale.

L'appareil ainsi disposé, le tube de verre fut mis en communication avec un vase rempli d'une teinture d'indigo; puis le robinet ayant été ouvert, on vit qu'à chaque dilatation inspiratoire du thorax, le liquide bleu passait dans le cavité du tube, et s'y élevait à une hauteur d'autant plus considérable que l'inspiration était plus prononcée. Dans l'expiration, le liquide restait en place, ou rétrogradait un peu vers le vase. A la fin de l'expérience seulement, le sang veineux refluait quelquefois vers le tube lorsque l'animal expirait.

Deuxième Expérience.

La même expérience fut répété sur un cheval, le 10 Juin, devant MM. Laennec, Cruveilhier, professeur d'anatomie à l'Ecole de Médecine; Breschet, Bogros, prosecteur de la même faculté; Bennet, et de beaucoup d'élèves. Les résultats furent les mêmes, à cette différence près, que pendant l'expiration on n'observa aucune régurgitation du sang veineux dans le tube. Pendant l'inspiration, le liquide affluait en abondance vers le cœur, et bientôt il ne'en resta plus dans le vase, qu'on fut obligé de remplir une seconde fois.

Ces préparatifs terminés, il devint facile d'établir par l'intermédiaire de la sonde une communication entre la cavité du péricarde et un vase rempli d'une liqueur colorée en bleu. Alors il fut sensible pour tous les yeux qu'à chaque mouvement d'inspiration la liqueur s'élevait dans le tube, qu'elle redescendait dans l'expiration, et que ces mouvemens étaient pour la vitesse et pour l'étendue précisément en raison directe de ceux du thorax; de telle sorte que quand la respiration était profonde, le liquide s'élevait très-haut, et

pénétrait même dans le péricarde, tandis qu'il semblait agité d'oscillations courtes et rapides quand les mouvemens de la poitrine étaient faibles, mais rapproches*.

Signé : EUG. LEGALLOIS.
Aide de clinique à la Charité.

* Le procès-verbal de ces expériences fut redigé sous les yeux de M. le professeur Laennec, par son élève, M. Legallois, jeune, homme de talent, et fils du célèbre physiologiste de ce nom.

No. II.

Letter addressed to Dr. BARRY *by* M. GIRARD, *Director of the Veterinary School at Alfort.*

Alfort, ce 6 *Juillet*, 1825.

(Ministère de l'Interieur.)

Monsieur le Docteur.

J'ai l'honneur de vous annoncer que j'ai pris des mesures pour que plusieurs chevaux soient soumis à vos expériences vendredi prochain. Si vos occupations vous permettent de vous rendre à l'école ce jour la, je serai flatté de vous recevoir, et de vous donner toutes les facilités que vous pourrez désirer.

Agréez, Monsieur le Docteur, l'assurance de la considération distinguée avec laquelle j'ai l'honneur d'être

Votre très-humble et très-obéissant Serviteur,
le Directeur de l'Ecole,
GIRARD.

M. Le Docteur Barry, rue de la Paix, No. 12, *bis.*

No. III.

Report made to the Institute of France by Baron CUVIER *and Professor* DUMERIL, *upon the Memoir on the Venous Circulation.*

INSTITUT DE FRANCE.

ACADEMIE ROYALE DES SCIENCES.

Le Secrétaire Perpétuel de l'Académie pour les Sciences Naturelles certifie que ce qui suit est extrait du procès-verbal de la séance du lundi 29 Août 1825.

La circulation dans les animaux à vertèbres est l'une des parties de la physiologie sur laquelle nous avons acquis le plus de connaissances positives. Ces notions exactes ne datent cependant que du commencement du xvi^e^ siècle, époque à laquelle Harvey démontra le véritable mécanisme, qui met en mouvement et qui favorise le transport continuel du sang. On sait que les canaux qui partent du cœur, et par lesquels le sang est poussé, dirigé vers toutes les parties du corps, sont les *artères;* et que ceux qui conduisent le sang, le chyle ou la lymphe au cœur, ont reçu le nom de *veines;* enfin que le cœur, ou l'organe qui détermine jusqu'à un certain point, le mode de circulation, varie par sa position, par sa structure, suivant beaucoup de circonstances qu'on est parvenu à apprécier, quoique le véritable mécanisme par lequel son action s'exécute reste à peu près le même.

La direction suivant laquelle le sang veineux est constamment entraîné vers le cœur avait été reconnue par Michel Servet plus de cinquante ans avant les expériences positives qui firent découvrir à Harvey le véritable mécanisme de la

circulation. Cependant, depuis cette importante et mémorable découverte, il s'est élevé un grand nombre de discussions sur les véritables causes de la progression du sang dans les veines.

Sans présenter ici une histoire chronologique des diverses opinions émises à ce sujet, il est important pour la question que nous allons avoir à examiner, de rapporter brièvement les principales. Nous mettons au premier rang l'action impulsive du cœur et des artères qui se continuerait par la pression qu'elle est censée exercer sur les radicules des veines, avec lesquelles les artères s'abouchent dans leur transmission. Telle était l'idée de Harvey. Suivant Bichat, la puissance absorbante du système capillaire veineux suffirait pour faire commencer d'abord, et continuer ensuite, cette progression à l'aide de l'action des parois des veines elles-mêmes. Enfin, suivant l'opinion de divers auteurs, un grand nombre de causes accessoires faciliteraient cette action des veines; telles sont: le mouvement des gros troncs artériels, placés le plus souvent entre deux veines, la pression exercée à l'extérieur et au-dedans de tous les organes, par la peau, par les muscles, par les viscères qui s'affaissent alternativement après avoir été distendus. Mais c'est surtout l'action de la respiration dont la coïncidence a été observée d'une manière très-évidente, comme corréspondante au retour mécanique du sang par les veines. Pour expliquer cet effet, les uns ont supposé que le sang était appelé avec d'autant plus de vitesse, que les poumons étaient plus vides (Rudiger), ou qu'une inspiration plus forte et plus rapide permettait au sang un cours plus libre dans les poumons (Santorini). Haller (tome 2 de sa *Physiologie*, page 333) cite un grand nombre d'expériences qu'il a répétées sur les animaux vivans, d'après celles de Valsalva et de Morgagni, par lesquelles il a reconnu qu'en mettant à nu

les grosses veines, telles que les caves supérieures et inférieures, les jugulaires, les sous-clavières, c'était au moment où l'animal faisait une forte inspiration que le sang veineux parvenait au cœur; que, dans cet instant, toutes ces veines se désemplissaient, pâlissaient et s'aplatissaient, se vidaient du sang qu'elles contenaient; que, dans l'expiration qui suivait immédiatement, les mêmes veines se gonflaient, devenaient bleues, cylindriques; et que, plus les deux temps de la respiration étaient marqués, plus ces phénomènes devenaient apparens.

Morgagni avait même dit (de *Causis et Sedibus Morborum*, lib. 19, art. 33 et 34), qu'en considérant attentivement la veine jugulaire mise à découvert sur un chien vivant, et en appuyant la main sur l'abdomen de l'animal, il avait évidemment reconnu que, toutes les fois que, par l'acte de l'inspiration, le ventre s'élevait, dans le même moment la veine s'affaissait pour se regonfler aussitôt que, par l'acte de l'expiration, les parois de l'abdomen retombaient sur elles-mêmes.

Depuis, un grand nombre d'auteurs, en particulier notre habile confrère M. MAGENDIE (*Physiologie*, 2nde édition, page 418), ont vérifié ces circonstances, et ont apporté en preuve de cette concordance de l'inspiration avec l'accélération du mouvement dans les gros troncs veineux, des expériences nouvelles et ingénieuses qui ont confirmé la réalité constante de ce phénomène, mais en la regardant comme un moyen accessoire qui facilite l'abord du sang veineux.

Enfin, quoique la plupart des physiologistes aient attribué uniquement au vide qui s'opère dans le cœur la progression du sang veineux dans cet organe, Bichat (*Anatomie générale*, tome i., page 429) a dit avec raison que ce mouvement éprouvé par le sang dans les veines exigeait encore beaucoup de recherches; car, ajoute-t-il, malgré tout ce qu'ont

écrit les auteurs sur cette question, elle offre une obscurité où on n'entrevoit encore que quelques traits de lumière.

Nous avons cru devoir entrer dans ces détails pour mettre l'Académie dans le cas de juger le mémoire pour l'examen duquel M. le Baron Cuvier et moi avons l'honneur d'être désignés commissaires.

Dans ce travail, M. le docteur Barry expose ses idées particulières sur le mouvement du sang dans les veines; il décrit avec beaucoup de détails les procédés qu'il a imaginés, nous pouvons le dire, avec sagacité; qu'il a exécutés très-adroitement sur les animaux, et qu'il a répétés avec la plus grande complaisance et à plusieurs reprises sous les yeux de vos commissaires.

Son mémoire présente trois objets de recherches principaux.

1. De déterminer par des expériences positives quelle est la puissance qui force le sang veineux de se diriger des plus petites ramifications où il est puisé jusqu'au cœur, où il aboutit.

2. D'apprécier et de comparer la vitesse avec laquelle le sang se meut dans les veines et dans les artères.

3. D'établir que l'abord continuel du sang veineux ne peut être assigné *uniquement* aux causes auxquelles il a été attribué jusqu'à présent.

Sous le premier point de vue, en étudiant le phénomène de la circulation veineuse, M. Barry a été conduit à reconnaître que, par l'acte de l'inspiration, il se fait un vide dans la cavité de la poitrine, laquelle tend à se dilater, et que tout le liquide en communication avec l'intérieur du thorax devait y être attiré comme forcé par la pression atmosphérique. Tous les faits connus trouvent, il faut l'avouer, leur explication dans cet effet physique; tels sont en particulier le gonflement des veines jugulaires dans l'expiration, et leur

affaissement dans le mouvement inverse; la cessation de certaines hémorrhagies par des inspirations forcées; l'absorption de l'air par les veines et les accidens qui en ont été la suite lors de l'ouverture ou de la section de quelques-uns de ces grands canaux voisins du cœur.

L'auteur ne s'est pas contenté de rapprocher ces faits, qui viennent à l'appui de son opinion, il a voulu la corroborer par des expériences directes, dont voici les principales.

Ayant ajusté sur l'une des grosses veines, comme sur la jugulaire d'un animal vivant, le bout d'un tube de verre garni d'un robinet, et ayant placé l'autre extrémité libre de ce tube dans une liqueur colorée, il a reconnu, après avoir ouvert le robinet, que, toutes les fois que l'animal faisait une forte inspiration, le liquide était vivement absorbé, et que dans l'expiration, au contraire, il restait stationnaire, s'il ne refluait pas.

Nous pouvons annoncer de suite que le même phénomène se reproduisait toutes les fois que l'expérimentateur avait introduit le même tube disposé très-artistement dans un des cavités du thorax, et même du péricarde.

Afin de rendre ce mouvement du liquide absorbé par le tube plus sensible à la vue, M. Barry s'est servi de canaux contournés en spirale, afin que, l'espace à parcourir étant plus long, le mouvement devînt plus evident; et, pour rendre leur ascension plus distincte, il a mêlé ou introduit dans les liquides colorés quelques gouttes d'huile ou des bulles d'air, qui servaient à faire mieux distinguer leur progression.

Dans toutes ces expériences, exécutées avec la plus grande adresse et avec des précautions bien satisfaisantes contre toutes les objections qu'on pourrait leur opposer, l'auteur du mémoire, dont nous désirons faire connaître les conséquences, s'est assuré que le mouvement aspirateur de la grosse veine

était coïncident avec l'instant où l'animal tendait à opérer le vide dans la poitrine ; que le sang noir ne traverse les veines que pendant l'acte et le temps de l'inspiration, et que ce mouvement veineux est toujours placé sous l'influence de l'action et de la pression atmosphérique.

M. Barry est tellement convaincu de cette action de l'atmosphère sur l'absorption veineuse, qu'il regarde comme un moyen assuré d'empêcher l'absorption d'une matière vénéneuse, l'application d'une ventouse sur une plaie récemment empoisonnée, ou dans l'intérieur de laquelle on aurait introduit une substance délétère.

M. le docteur Barry attribue également à la pression atmosphérique l'action absorbante du système pulmonaire vénoso-artériel, ou de la petite circulation. Mais ici l'auteur offre plutôt des raisonnemens établis sur des dispositions anatomiques que sur des observations positives, et quelques faits d'anatomie comparée pourraient être objectés avec succès à cette opinion, que l'auteur n'a pas présentée avec des expériences aussi concluantes que celles dont il s'est appuyé pour démontrer l'action de la pression de l'atmosphère sur la grande circulation veineuse.

Quant à l'appréciation de la vitesse comparée du sang dans les deux ordres de vaisseaux qu'il parcourt, l'auteur l'a faite d'après l'idée que la pression de l'atmosphère est la principale puissance qui pousse le sang veineux dans le cœur pendant l'inspiration. Ce sang doit nécessairement se mouvoir avec une rapidité qui est à celle du sang artériel comme le temps employé à une respiration entière est à celui d'une seule et unique inspiration, et que la fréquence du pouls ne peut être prise comme la mesure de la vélocité du sang qui revient au cœur, puisque, dans la première hypothèse, ce serait la répétition du mouvement inspirateur qui réglerait cette vélocité. Cette partie du mémoire est

entièrement fondée sur le raisonnement, et n'est pas appuyée de preuves et d'observations qui nous permettent de manifester une opinion sur ce sujet.

Enfin, quant à la dernière conséquence que l'auteur paraît devoir tirer de son mémoire, que l'abord du sang veineux au cœur ne peut être uniquement attribué aux causes indiquées jusqu'à présent, nous avouerons que cette idée de la pression de l'atmosphère comme cause principale n'a pas été primitivement reconnue par lui ; plusieurs autres l'avaient indiqué, même avant le docteur Zugenbuhler, qui a cru devoir faire une réclamation à l'académie, en lui envoyant une dissertation *de Motu Sanguinis per Venas*, publiée en 1815.

Mais l'auteur, tout en reconnaissant l'action très-évidente de la pression de l'atmosphère, regarde le cœur comme la cause première du vide qui s'opère dans le système, tandis que M. Barry attribue la dilatation du cœur lui-même et de ses oreillettes à la tendance au vide qui s'opère dans toute la cavité de la poitrine dans l'acte de l'inspiration, en démontrant cette action par des expériences positives, tandis que M. Zugenbuhler ne présente que des raisonnemens à l'appui de son opinion.

En terminant ce rapport sur le mémoire intéressant de M. Barry, dans lequel nous nous faisons un devoir de déclarer que les expériences décrites avec beaucoup de détails par l'auteur ont été faites et répétées plus de vingt fois sur des chiens, sur des brebis, sur des chevaux ; qu'elles ont constamment réussi toutes les fois qu'il a pu exécuter, comme il le désirait, les procédés ingénieux qu'il a imaginés dans ce but, et que ces recherches expérimentales ont eu lieu sous nos yeux à la Faculté e Médecine, au Jardin du Roi, à l'Ecole d'Alfort, devant M. Girard, et aux abattoirs de Mont-faucon.

Vos commissaires jugent ces recherches faites dans un très-bon esprit et très-propres à éclairer l'histoire physiologique de la circulation veineuse dans les mammifères. Sous ce rapport, ils ont l'honneur de proposer à l'Académie d'inviter l'auteur à poursuivre ses recherches sur les causes de l'absorption, recherches qui peuvent offrir un grand intérêt et des applications très-utiles à l'économie animale ; de décider que le mémoire de M. Barry sera inséré parmi ceux des savans étrangers. Cependant ils ne doivent pas laisser ignorer que, dans leur opinion particulière, l'acte de l'inspiration qui peut produire le vide, et par suite l'appel du sang veineux dans la cavité du thorax chez les animaux à poumons, tels que les mammifères et les oiseaux, ne suffit pas pour expliquer le mouvement du sang dans les veines chez quelques reptiles et chez les poissons, qui ont un autre mode de réspiration, la même coïncidence d'action ne pouvant se trouver entre l'inspiration qui s'opère chez ces animaux par une véritable déglutition et l'abord du sang veineux dans la cavité de leur cœur.

(Signé.) Le Baron CUVIER.
DUMERIL, Rapporteur.

L'Académie adopte les conclusions du rapport.

Certifié conforme.

Le Secrétaire perpétuel, Conseiller-d'Etat, Commandeur de l'ordre royal de la Légion-d'Honneur.

(Signé) Baron CUVIER.

No. IV.

Extract from the Report presented to the Académie de Médecine, by Professor Laennec, upon the Experiments connected with External Absorption and Traumatic Poisoning.

" L'Académie nous a chargé, Messieurs Adelon, Orfila et Moi, de lui rendre compte d'expériences faites par M. Le Dr. Barry qui lui ont été communiquées par l'auteur dans l'une de ses dernières séances, et qui tendent à prouver, que la pression atmosphérique est la principale cause de l'absorption lymphatique et veineuse. Ces expériences consistent en ce qui suit, &c.

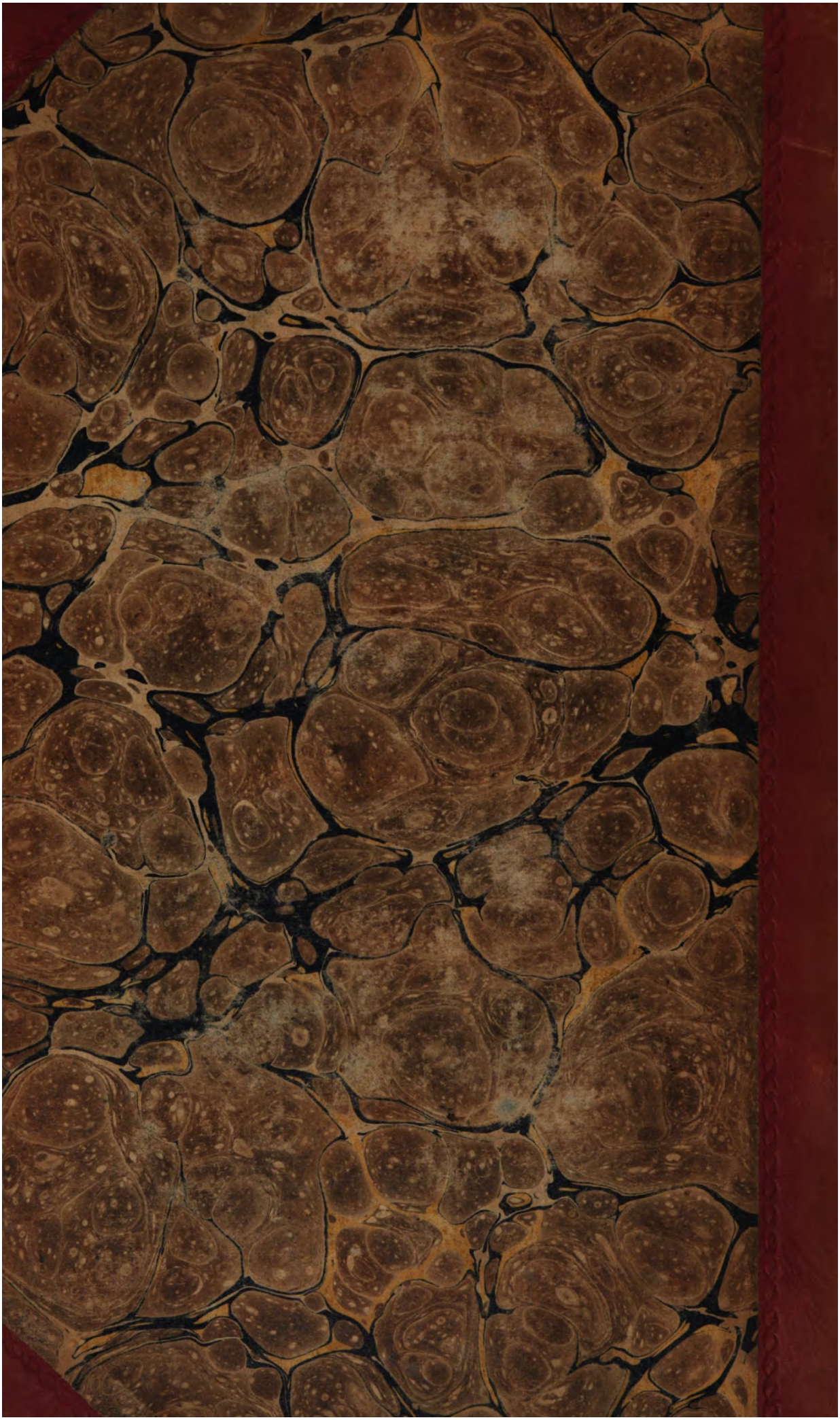
Vos Commissaires pensent en conséquence,

" 1°. Que les expériences de M. Le Dr. Barry, qui sont une suite de celles par lesquelles il a déjà cherché à démontrer que la circulation veineuse se fait principalement, sous l'influence de la pression atmosphérique, établissent d'une manière incontestable cette influence pour la circulation dans les vaisseaux absorbans ; ce que l'auteur se proposoit de démontrer.

" 2°. Que la connaissance de ce fait important peut être regardée comme *une véritable découverte*, non obstant les apperçues et les idées vagues émises par quelques auteurs sur l'attraction du sang vers le cœur, et la pratique empirique de la succion dans les plaies empoisonnées, plus usitée chez les peuples à demi-civilisées que chez les nations polies.

" 3°. Vos Commissaires vous proposent d'addresser des remerciemens à M. Le Dr. Barry, de l'engager à répéter

ses expériences sur le virus de la vipère, d'insérer son mémoire parmi ceux de l'académie, et de mettre son nom sur la liste des candidats aux places d'associés étrangers de l'académie.

(Signé.) M. LAENNEC, D.M.

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