XIV. *An Account of some Experiments on the Blood in connexion with the Theory of Respiration.* *By* JOHN DAVY, *M.D. F.R.S., Assistant Inspector of Army Hospitals.*

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CONNECTED with the theory of respiration and of animal heat there are many questions of interest respecting the blood, about which physiologists differ in opinion, and which consequently are fit subjects for further inquiry.

Some of the more important and fundamental of these questions I have endeavoured to investigate experimentally, and in the present communication I propose to submit to the Royal Society the results which I have obtained, with the hope that they may be considered not unworthy of a place in the Philosophical Transactions.

I. *Is blood capable of absorbing oxygen independent of putrefaction?*

To endeavour to satisfy myself on this point, on which in a former inquiry I had arrived at a negative conclusion in opposition to the commonly received opinion, I have employed two methods of experimenting: one, of agitating blood, recently drawn and rapidly cooled in common air and in oxygen, in a tube of the capacity of two cubic inches, divided into a hundred parts; the other, of agitating it in larger quantities with the same airs, in the very convenient apparatus employed by Dr. CHRISTISON when engaged in a similar inquiry, consisting of a double tubulated bottle of the capacity of thirty-two cubic inches, provided with stop-cocks adapted by grinding, to one of which a moveable bent glass tube was fitted to connect it with a pneumatic trough, and to the other a perpendicular tube surmounted by a funnel.

The blood subjected to experiment in every instance was prepared by the displacement of its fibrin. This was done by agitating it with small pieces of sheet-lead in a bottle filled to overflowing and closed with a cork, enveloped in moist bladder and covered over with the same, tied round the neck of the bottle, so as to exclude atmospheric air, whilst in the act of coagulating and of cooling, and to allow, when cooled, of the withdrawal of the cork.

Prepared thus, and rapidly cooled, I have tried different specimens of blood, venous and arterial, of Man, of the Sheep, Ox, Dog and Cat; and the results, when the blood has been taken from a healthy animal, have been decisive and consistent. In every instance, whether atmospheric air or oxygen was used, after agitation, there was a marked diminution of the volume of air.

In the examples which it may be advisable to bring forward, I shall confine myself to a few of the experiments I have made on the blood of the Sheep.

Using the graduated tube over mercury, sixty-two measures of arterial blood from the carotid artery, agitated with thirty-three measures of common air, produced a diminution of two measures; and sixty-three of the same arterial blood, agitated with thirteen of pure oxygen, a diminution of three; whilst sixty-three of venous blood from the jugular vein of the same animal, agitated with thirty-three of common air, produced a diminution of six; and seventy of this blood, with thirteen of oxygen, a diminution of eight*.

In experiments on a larger scale, using the double-mouthed bottle, in which about ten cubic inches of blood were agitated with about twenty-two cubic inches of air, the results were in accordance with the preceding. Thus when the arterial blood of the Sheep was agitated with common air and with oxygen, on turning the stop-cock of the bent tube there was an absorption in one instance of about ·3 cubic inch, and in the other of about ·4 cubic inch; and, with venous blood, in the instance of common air, of about 1· cubic inch, and in that of oxygen, of about 1·25 cubic inch.

These experiments were all made on the blood of the same animal. In experiments on the blood of different individuals of the same species of animal, and on the blood of animals of different species, the results have varied in regard to the degree of absorption, and remarkably so in the instance of the blood of man.

In every instance the absorption or disappearance of a portion of the air has been attended with some change in the colour of the blood; the venous has invariably acquired the florid vermillion hue of arterial blood, and the arterial has had its florid hue heightened.

The air that has been absorbed or which disappears, when atmospheric air has been used, in accordance with the commonly received opinion and the results of Dr. CHRISTISON's experiments†, has been found to be oxygen.

Relative to the residual air, when pure oxygen has been used, whether on the smaller or larger scale of experiment, no carbonic acid gas has been detected in it in the most carefully conducted trials. When common air has been employed, then a trace of this acid gas has been found in the residual air after agitation with the blood, but not exceeding 1 per cent. at a temperature between 40° and 50°. I shall notice in detail an experiment with each, as the results are of consequence.

Ten cubic inches of venous blood (its fibrin displaced) from the jugular vein of a Sheep, rapidly cooled, were agitated in twenty-two cubic inches of oxygen gas, which had been well washed with lime-water. After an absorption equal to about one cubic inch, some of the gas was expelled and passed into lime-water, the transparency of which it did not in the slightest degree impair.

The same quantity of similar blood was similarly treated with common air, which

* Notwithstanding the frothing attending the agitation of blood in air, the absorption or diminution of volume was ascertained with tolerable accuracy by observing the rise of the mercury in the tube. The experiments were made as nearly as possible under ordinary atmospheric pressure, which was easily effected, as the mercurial pneumatic trough used exceeded in depth the length of the tube.

† Edinburgh Medical and Surgical Journal, vol. xxxv. p. 94.

also had been washed with lime-water. After an absorption equal to about one cubic inch, some of the air was passed into lime-water: it occasioned a just perceptible cloudiness; and eighty measures of it agitated with lime-water were reduced hardly to 79·5.

Taking it for granted that this very minute quantity of carbonic acid derived from the blood existed in the state of gas, and not contained in the aqueous vapour, as is possible, it is matter for consideration from whence it was derived,—whether it was formed at the moment by the action of the air on the blood, or, previously existing in the blood, was now merely expelled. Further on I propose to return to this important question.

When I reflect on the results stated above relative to the absorption or disappearance of oxygen, and compare them with those alluded to formerly obtained, I am not a little surprised at their discrepancy; and I can only account for it by supposing that it may have been owing in part to the difference of season when the two sets of experiments were conducted. The first were made in Malta in 1829, in the hot months of July and August, when the thermometer in the open air was generally above 80° and occasionally above 90°. The last have been made in England, and principally in January of the present year, when the temperature of atmosphere was occasionally low, the greater part of the time below the freezing point, and often as low as 20°. From what I have witnessed, I am induced to infer, that the higher the atmospheric temperature is, and the less necessity there is for the production of animal heat, the less difference there is between venous and arterial blood, and the less power the former has of combining with oxygen and of forming or evolving carbonic acid. In Malta I carefully compared the blood of the jugular vein and carotid artery of a Sheep during the season mentioned; when coagulated and still hot, there was no perceptible difference in their colour; in each it was less florid than the arterial blood of the same animal in England in winter, and less dark than the venous: its hue was as it were a mixture of the two. And in this observation I could not be mistaken; the circumstances under which it was made precluded mistake; vessels of the same size were used; similar quantities of blood were introduced, and they were seen in the same light side by side. And in confirmation of this view I may remark, that during the last winter, when the cold was unusually severe, I found the temperature of deeply seated parts, and especially of the heart and its left ventricle, in the instance of Sheep, unusually high: the mean of nine observations on the temperature of the left ventricle in different animals was 107·5; the lowest was 105·5; the highest 109·; whilst the temperature of the rectum (the mean of the same number of observations) was 104·4.

II. *Does the blood, especially venous blood, contain carbonic acid capable of being expelled by agitation with another gas, as hydrogen or oxygen?*

In a paper published in the Philosophical Transactions for 1832, Dr. STEVENS has answered this question in the affirmative: he maintains that carbonic acid gas exists in venous blood; that it may be expelled by oxygen or by hydrogen, although not by the air-pump; and he supposes that the difference of effect is owing to a peculiar attraction for carbonic acid exercised by these gases.

To endeavour to resolve my doubts on this important point, I have had recourse to the apparatus already mentioned, viz. the graduated tube with the mercurial pneumatic trough, and the double-mouthed bottle furnished with stop-cocks, &c. as being well adapted for simple and decisive experiments.

By means of the graduated tube I have agitated venous blood in hydrogen over mercury, as about a cubic inch of each, and other proportions, and have left the blood exposed to the influence of the gas for several hours; and I have made similar trials with it, using larger quantities in the double-mouthed bottle, as sixteen cubic inches of each, and also other proportions; the results have been either of the same negative character, or, if different, indicating only the disengagement of carbonic acid gas in an extremely minute quantity. In all the experiments with the graduated tube in which fresh blood was used, whether of Man or of the Sheep, the fibrin displaced out of the contact of the air, on agitation with hydrogen, there was no sensible increase of the volume of the gas, and no diminution of it when it was transferred to, and shaken with, lime-water. And in the best experiments, on a larger scale, with the double-mouthed bottle, when most attention was paid to all the circumstances likely to insure accuracy, as in the first instance the exclusion of air from the blood, and in the second the having it of the temperature of the bottle and of the room, the results have been similar, and of a negative character. I shall describe a small number of experiments, those, the results of which appeared least ambiguous.

Seven cubic inches of Sheep's blood from the jugular vein, its fibrin broken up by agitation with lead whilst coagulating, out of contact with air, and cooled under water, were agitated with hydrogen (twenty-five cubic inches), previously well washed with lime-water, and which, tested by lime-water, after this precaution were found perfectly free from carbonic acid. On turning the stop-cock of the bent tube connected with distilled water, no change of volume was indicated, and the blood was agitated again with the same result. By means of the perpendicular tube distilled water was admitted, and some of the gas expelled; first a cubic inch into a graduated tube filled with lime-water, and next about four cubic inches into a vial filled with distilled water, and in which afterwards a little lime-water was added to the gas; in neither could any traces of carbonic acid be detected; the lime-water remained transparent.

Ten cubic inches of venous blood, taken by a large orifice from the arm of a young Man threatened with hæmoptysis, the fibrin broken up in the same manner as the last, and rapidly cooled under water in a running stream to the temperature of the room, 51°, were similarly treated with hydrogen, and with precisely the same result; after having been twice well shaken, on turning the stop-cock, there was no change of volume. The blood was kept in contact with the hydrogen over night, the stop-cocks closed. The night, that of the 23rd of January, was severely cold; at 11 o'clock the following morning the temperature of the room was only 45°; now on turning the stop-cock of the bent tube the water rose in it to the extent of about one-eighth of a cubic inch.

Twenty-four cubic inches of the mixed arterial and venous blood of the Sheep, collected and prepared with similar precautions, were divided into two portions of about twelve cubic inches; one was agitated in the double-mouthed bottle with hydrogen after the introduction of a little milk of lime, the other without this addition. The result in each instance was the same; on opening the stop-cock, after the agitation, the water rose a very little in the bent tube, about one-twentieth of a cubic inch.

The results of some of the trials already described on the action of oxygen on venous blood, both pure and mixed with azote, in the form of common air, are very consistent with those just detailed on hydrogen. Previously to stating some other results in quest of fresh evidence on the same subject, it may be advisable to notice particularly the power which blood possesses of absorbing carbonic acid.

From experiments which I made on blood and serum in 1824 and 1828*, I inferred that each is capable of absorbing about an equal volume of this acid gas. I now find that when pure carbonic acid gas is brought in contact with blood or serum over mercury, and moderately agitated under ordinary atmospheric pressure, that the absorption of gas exceeds the volume of the fluid, both in the instance of blood and serum. The results of some experiments are exhibited in the following Table. The majority of them were obtained between a temperature of 40° and 45°; the three last at about 51°. When the venous and arterial blood, and the serum tried, were from the same animal, the numbers expressing the results are entered in the same line.

Volume of carbonic acid gas absorbed by 100 parts of blood and serum.

No.	Animal.	Venous blood.	Venous serum.	Arterial blood.	Arterial serum.
1	Sheep......	160	......	167	
2	Sheep......	155			
3	Sheep......	......	......	133	
4	Sheep......	142			
5	Sheep......	......	......	150	
6	Sheep......	166			
7	Sheep......	......	......	......	120
8	Ox........	194			
9	Ox........	181			
10	Man	118			
11	Heifer......	120	......	......	117
12	Sheep......	148	125	141	125
13	Sheep......	......	118	......	118
14	Man	153			
15	Sheep......	160			
16	Sheep......	......	......	159	

* Philosophical Transactions for 1824. Edinburgh Medical and Surgical Journal, vol. xxx.

The effect of the absorption of the gas to perfect saturation was on the arterial and venous blood the same; it rendered both very dark; the serum it rendered more liquid, which was well marked by diminished tendency to froth on agitation.

I shall now proceed to notice the trials which I have instituted of agitating blood and serum, to which a known quantity of carbonic acid had been added, with one or more of the gases considered by Dr. STEVENS as exerting an attraction on carbonic acid, and by that means expelling it.

From the experiments which I have made on serum, it appears in its healthy state incapable of absorbing oxygen, or of immediately furnishing carbon to form carbonic acid; and in no instance in which I have agitated it with common air or with hydrogen, when obtained from the blood of a healthy animal, has there been any indication of the disengagement of gas; it therefore is peculiarly well fitted for the trial in question.

To nine cubic inches of serum from the mixed blood of the Sheep one cubic inch more of serum was added, containing a cubic inch of carbonic acid, with which it had been impregnated over mercury. The mixture of the two was introduced into the double-mouthed bottle without delay, and well agitated with twenty-two cubic inches of common air. On turning the stop-cock there was no change of volume. The serum was transferred, and there was added to it, with as little motion as possible, another cubic inch of serum, containing the same quantity of carbonic acid. Now poured back into the bottle and agitated, on opening the stop-cock a little air was disengaged; it was collected and found equal to $\frac{4}{100}$ths cubic inch. The serum was left exposed to the action of the air in the bottle over night, the stop-cocks closed; the following morning on opening the stop-cock of the bent tube no air was expelled; on the contrary, there was a just perceptible rise of water in it. The experiment was carried further: the serum was transferred to a vial and closed, and the double-mouthed bottle was filled with hydrogen. The serum was returned and well agitated with the hydrogen. On turning the stop-cock $\frac{14}{100}$ths of a cubic inch of air was expelled. It was agitated a second time without further expulsion of air, and left in contact with hydrogen for more than twelve hours without any further effect. Thus it appears that of the two cubic inches of carbonic acid gas introduced into the serum, only one-fifth of a cubic inch was expelled by successive agitation with atmospheric air and with hydrogen.

One cubic inch of venous blood of a Man which had absorbed 1·2 cubic inch of carbonic acid, was mixed with twelve cubic inches of similar blood, and agitated with hydrogen in the double-mouthed bottle. A very little air only was expelled, viz. $\frac{4}{100}$ths cubic inch.

To fifty-five measures (1·1 cubic inch) of venous blood of a Sheep, twenty measures of gas were added over mercury, composed of about equal parts of oxygen and carbonic acid. After agitation about seventeen measures were absorbed, and the blood had acquired a florid hue; ten measures more of oxygen were added; there was no further absorption. The tube was transferred to lime-water and agitated; the residual air was not diminished; it amounted to thirteen or fourteen measures (the froth prevented precision in marking the quantity), which possessed the properties of oxygen, as tested by the taper.

To twenty-eight measures of venous blood of a Sheep, which had absorbed twenty-six of carbonic acid, forty-nine of oxygen were added in the graduated tube over mercury: after agitation the blood had acquired the florid arterial hue, and there appeared to be an expansion of one measure. Transferred to lime-water there was an absorption of one or two measures, and no more.

I shall notice one experiment more, in which blood nearly saturated with carbonic acid was exposed to oxygen, a membrane intervening. Forty-seven measures of venous blood, which had absorbed thirty-three of carbonic acid, were introduced into a glass tube half an inch in diameter, closed at one end with gold-beater's skin, and when filled with blood at the other end also, it was placed over mercury in a small receiver, and thirty-seven measures of oxygen were added to it, under a diminished pressure of about one inch. After twenty-four hours there was no change of volume; the blood in the tube had acquired throughout the arterial hue; the gas, thirty-eight measures, transferred to lime-water and agitated, diminished to thirty-two.

The tube was now placed under a receiver, and the air exhausted by the air-pump; a good deal of air was disengaged in the form of froth, and the gold-beater's skin was so distended that it appeared ready to burst; after three or four minutes air was re-admitted; a notable portion of gas was found free between the membrane and blood; thus showing that in oxygen gas carbonic acid gas is less freely exhaled through a membrane than in vacuo.

The results of this second set of experiments are in accordance with those of the first. The inference I am induced to draw from both is rather of a negative kind, and unfavourable to the conclusion of Dr. Stevens already referred to, at least in a strict and general sense. I think it right to express myself thus reservedly, reflecting on some of the experiments in which a very little carbonic acid gas appeared to be extricated on agitating blood with hydrogen; and believing that Dr. Stevens, and other able inquirers, could not have been misled on a point so little exposed to fallacy.

Relative to the effects which Dr. Stevens refers to the attraction of one gas for another, they appear to me, from what I have witnessed in carrying on the inquiry, to admit of explanation on Dr. Dalton's theory of mixed gases, and that in no instance is the effect of disengagement of air from a fluid, agitated with another kind of air, greater than were it agitated in a vacuum.

III. *What is the condition of the alkali in the blood in relation to carbonic acid?*

On this point much difference of opinion exists amongst inquirers; some believing that the alkali, or at least a portion of it, is uncombined, or combined merely with water, or with water and albumen; some that it is united to carbonic acid in the state of carbonate; others that it is saturated with this gas, and in the state of bicarbonate.

The subject, it must be confessed, is one of great difficulty, and very perplexing; partly from the nature of the blood, liable to great variations during life, and to rapid change after death; and partly also from the nature of the alkaline carbonates, hardly less disposed to change than the blood itself, from variation of circumstances, and to pass from one degree of combination into another.

The bicarbonate of soda, I believe, like the bicarbonate of ammonia, can only exist in perfection in the solid state. In dissolving, I find, when exposed to the atmosphere, it gives off a part of its acid, and still more when it is agitated with common air, and more still with hydrogen, and in a greater degree the higher the temperature. This is not favourable to the idea that it exists in the blood, especially when it is considered that this fluid may be exposed to a temperature of 212° without disengaging carbonic acid, of which I have had proof in several trials.

Sulphuretted hydrogen does not expel carbonic acid from the alkalies in solution in water. Bicarbonate of soda dissolved to saturation in distilled water absorbs, I find, 143 per cent. of its volume of this gas; whilst the serum of blood (it was Sheep's that was tried) dissolved 207 per cent., arterial blood 235, and venous 290. This, too, is unfavourable to the same idea; as is also the large proportion of carbonic acid which blood, it has been shown, is capable of absorbing.

Supertartrate of potash occasions an effervescence, when mixed in substance with a solution of the sesquicarbonate, but not of the carbonate of soda; and the effect is similar, whether the mixture be made over mercury, air excluded, or in an open vessel exposed to the atmosphere. The supertartrate of potash also, I find, mixed with blood and agitated with common air, acts as with the sesquicarbonate, and occasions a disengagement of air, and both from arterial and venous blood, and from serum; and the air I have ascertained is carbonic acid.

From these facts, may it not be inferred that the alkali in the blood, in its normal or healthiest condition, is neither in the state of carbonate nor of bicarbonate, but of sesquicarbonate? The power of the blood to absorb carbonic acid and sulphuretted hydrogen accords best with this view, and some other important properties of the fluid are, I believe, in harmony with it.

The sesquicarbonate, I may add, seems to be the state of rest of the alkali in combination with carbonic acid, under ordinary circumstances of exposure to the atmosphere. Thus the native compound is the sesquicarbonate, as is also, I believe, the effloresced salt*. And I find that although a solution of the bicarbonate may be brought by the air-pump to the state of sesquicarbonate, it cannot be reduced to that of the carbonate: after it has ceased to give off any air in vacuo, it effervesces

* On exposing carbonate of soda in excess to carbonic acid gas over mercury, the gas is rapidly absorbed with the expulsion of part of the water of crystallization, so as to produce an appearance of deliquescence, and the sesquicarbonate is formed.

with the supertartrate of potash; and if evaporated to dryness over sulphuric acid under an exhausted receiver, on being subjected to heat, it disengages carbonic acid gas.

IV. *Does the blood contain any gas capable of being extricated by the air-pump?*

On this subject also there has been much difference of opinion. Our distinguished countryman MAYOW, more than a century ago, stated that blood, especially arterial blood, effervesces in vacuo, which he attributed to the disengagement of air, his Spiritus Nitro-aëreus*. Sir EVERARD HOME, on the authority of Mr. BRANDE, in 1818, asserted that blood, both venous and arterial, under the exhausted receiver evolves a large quantity of carbonic acid gas, an ounce of blood as much as two cubic inches of gas†. I repeated the experiment shortly after, but without confirming the result; neither by the air-pump, nor by heat applied even to coagulation of blood and serum in close vessels, did I succeed in demonstrating the extrication of this acid‡. Since that time the experiment of the air-pump on the blood has been frequently made, and by observers of great accuracy, as by Drs. DUNCAN and CHRISTISON in this country, by MM. TIEDEMAN, GMELIN, MITSCHENLICH and MÜLLER on the continent, and recently by MM. BISCHOFF and MAGNUS. With the exception of the last-mentioned inquirers, the results have been negative. MM. BISCHOFF and MAGNUS, on the contrary, state that by careful exhaustion they have obtained gas from the blood; the former a small quantity of carbonic acid gas, the latter a notable quantity, and not only of carbonic acid gas, but also of oxygen and azote§.

M. MAGNUS attributes the failures of former experimenters to their having used pumps of imperfect exhausting power, or to their not having carried the exhaustion sufficiently far. In my early and first trial I employed the air pump belonging to the laboratory of the Royal Institution, which was an excellent one, and which I then believed was in good order; but I might have been mistaken.

To endeavour to satisfy myself on this point, I have had an air-pump constructed for the purpose, under the direction of an able artist, Mr. Ross, of 33 Regent Street, already distinguished for his excellent achromatic microscopes, and it has answered perfectly. When the exhaustion has been carried as far as possible, the difference of level of the mercury in the siphon-gauge has not exceeded a quarter of an inch, and over water has not exceeded half an inch.

Experimenting with this machine on blood, collected with such precautions as I believe to have been adequate to insure accuracy of results, in a majority of instances the disengagement of gas has been rendered manifest, and both from arterial and venous blood.

* JOHANNIS MAYOW Opera omnia. Hagæ Com. 1681. p. 133.

† Philosophical Transactions for 1818, p. 181.

‡ Ibid. 1823, p. 516.

§ Annales des Sciences Nat. tom. viii. p. 79. *et seq.* contain a translation of M. MAGNUS' paper, with a figure of the apparatus, &c. used; the original appeared in POGGENDORF's Journal, vol. xl. part 3.

I shall briefly mention the trials which I consider most conclusive, and which satisfied me in spite of an opposite pre-existing bias.

Vials provided with well-ground glass stoppers were filled with distilled water, deprived as much as possible of air by the air-pump; they were then placed under the receiver and kept in vacuo until all adhering air was removed; the stoppers were now introduced, all air being excluded, and they were instantly immersed in distilled water, which had been well boiled. Thus prepared they were taken to an adjoining slaughter-house, where they were filled with Sheep's blood in the following manner, without its coming in contact with the air. For venous blood the jugular vein was exposed; two ligatures were applied to it; the vein was divided between the two, and the upper part, slightly detached, was introduced into a prepared vial under water the instant the stopper was withdrawn, and laid open. The heavier blood proceeding from the vessel of course expelled the water; and when it was supposed to be all expelled the stopper was restored, and the vial was replaced in the water. In the instance of arterial blood it was collected in the same manner from the carotid artery. In some trials the blood was allowed to coagulate undisturbed; in others the fibrin was detached, and the liquidity of the blood preserved by agitating it, the instant the stopper was replaced, with some mercury, introduced with the distilled water, and equally deprived of adhering air.

In about half an hour from the abstraction of the blood, in every instance, it was subjected to the air-pump. The instant the stopper was withdrawn the vial was placed in a small receiver on the plate of the pump, and covered with a little larger receiver, and the air as soon as possible exhausted. No appearance of disengagement of gas was perceptible until the exhaustion was nearly complete; then it was sudden, sometimes considerable, even to overflowing, in the form of bubbles, and it continued some time. The results were not distinctly different, that I could perceive, whether venous or arterial blood was used; I am disposed to think, on the whole, that less air was disengaged from the arterial blood than from the venous.

When blood allowed to coagulate in the vessels was tried, the results varied a little, and appeared to me instructive. At first, on exhaustion, only a few particles of air were disengaged; no more, it might be supposed, than were derived from the contact of the end of the stopper. In two experiments such was the appearance for at least five minutes, conveying the idea that no air was extricated; then abruptly a bubble or film burst with some force, as was denoted by the scattering of the particles of blood; and a bubbling commenced and continued, rendering the indications of extrication of gas unquestionable. And in conformity with this result I may remark, I have never succeeded in obtaining indications of air in the blood in operating on it by the air-pump, if confined in a detached portion of vein, or in the heart of animals, the great vessels, previous to excision, having been tightly secured by ligature. In no instance of this kind have I witnessed any distention, such as occurs if air be admitted previous to the application of the ligatures: clearly indicative, it appears to me, that a very slight compressing force is sufficient to confine the air in the blood, or rather, I should say, prevent its substance assuming the elastic state; and further, the probability, that the quantity of air so condensed is small.

I have stated that in the majority of instances the indications of the disengagement of air from blood in vacuo have been manifest. Exceptions, however, have occurred, and those clear and decisive; inducing me to believe that the quantity of air condensed in the blood is variable; that there are times when it is in excess, and times when in deficiency, and when totally wanting, connected with regularly changing states of the functional system. I hope on another occasion to be able to recur to this part of the subject, on which at present I have collected but a few facts. I may add, that such facts tend in part to reconcile some of the discrepancies referred to in the beginning of this section.

V. *Of the air or gases contained in the blood capable of being extricated by the air-pump.*

As already mentioned, M. Magnus has stated that these gases are carbonic acid, oxygen and azote in notable quantities. Taking the mean of ten of his experiments on the blood of the Horse and Calf (five on arterial and five on venous), the total quantity of the mixed gases he obtained was, in the instance of arterial blood, 10·4 per cent. per volume, and in that of venous 7·6 per cent.; the former consisting of about

6·5 carbonic acid,
2·4 oxygen,
1·5 azote;

the latter of

5·5 carbonic acid,
1·1 oxygen,
1·0 azote.

On a subject of so much importance, it is very desirable that the experiments of M. Magnus should be repeated and verified; for until this is done, considering the physiological history of the blood, it will be difficult to avoid doubt, and to depend on them with that degree of confidence which is justly due only to well-authenticated facts.

The ingenious apparatus employed by M. Magnus being difficult of construction, and not easily used excepting in a well-appointed laboratory, I must leave the repetition of his highly interesting experiments to those inquirers who are more happily situated than myself for engaging in them. On the present occasion I shall limit myself to the detail of some experiments instituted with a view of testing M. Magnus' general results.

As a solution of potassa has the property of absorbing carbonic acid gas, it follows that if mixed with the blood previous to being subjected to the air-pump, it will prove in some measure a test of the kind of air which the blood is capable of affording.

With this intent two vials were prepared, the same as before used, one filled with distilled water, the other with a weak aqueous solution of caustic potash, both carefully deprived of air by the air-pump. Observing the same precautions as before, a portion of venous blood from a Sheep was received into each of them. When less than half of the water and of the solution, as well as could be guessed, was expelled, the vials were closed with the glass stoppers belonging to them, and instantly immersed in water, and as soon as possible subjected to the air-pump. The results of exhaustion in the two instances was perfectly distinct. From the blood mixed with water gas was disengaged; there was a continued ascension of bubbles. From the blood mixed with the alkaline solution no gas was liberated, excepting a bubble or two, which might fairly be considered as entangled air derived from contact of the blood with the stopper.

A similar comparative trial was instituted with arterial blood of the Sheep, and the results also were similar and equally well marked.

Considered as test experiments, the first inference from these results is, that carbonic acid gas, or an air absorbable by a solution of potash, is disengaged both by venous and arterial blood in vacuo; and next, that no other gas is disengaged from either of them, neither oxygen nor azote, each of which is unabsorbable by the solution in question. I have repeated the experiment twice on the venous blood of Man, and twice on the venous blood of the Sheep, and twice on the arterial blood of the latter, without variation of results; and they are more to be depended on, as the alkali has the effect of preserving the blood liquid.

I may mention another method which I have employed as a test experiment; and first in relation to carbonic acid. If the blood contain a notable portion of this gas capable of being extricated by the air-pump, it necessarily follows, that when subjected to the action of the air-pump, and deprived as far as possible of its fixed air by this means, it will be capable of absorbing a larger quantity of carbonic acid than previous to the exhaustion. The following Table contains the results of three comparative trials on the venous blood of the Sheep, its fibrin separated or detached in the usual manner.

Table showing the absorption of carbonic acid gas by

Venous blood subjected to the air-pump.			Venous blood not subjected to the air-pump.		
Volume of blood used.	Volume of carbonic acid introduced.	Volume of gas absorbed.	Volume of blood used.	Volume of gas introduced.	Volume of gas absorbed.
27	46	38	27	47	37
27	45	39	27	45	39
27	46	35	27	45	34

Although I have thought it right to notice these results, and although they are in accordance with the preceding, I do not attach value to them, excepting as tending to show that the quantity of carbonic acid gas extracted by the air-pump, when the blood affords it in vacuo, is small.

The same mode of reasoning suggested comparative trials of the absorbent power of arterial blood for oxygen, before and after exhaustion by the air-pump, as a further test experiment, whether arterial blood contain oxygen in a free state, that is, admitting of being extricated by the removal of atmospheric pressure. The result of this trial also has been negative; its power of absorbing oxygen has not appeared to be at all increased by exhaustion; this at least was the result of one experiment carefully conducted.

VI. *Is any oxygen contained in the blood not capable of being extricated by the air-pump?*

Before I was acquainted with the researches of M. MAGNUS I had instituted some experiments to endeavour to determine whether any oxygen in a free state, or in a condition approximating to that state, exists in the blood, and especially in the arterial, admitting of being detected by means of substances possessing a strong attraction for oxygen, or of being expelled by substances of greater solubility in blood than oxygen. Hydrogen, phosphuretted hydrogen, sulphuretted hydrogen, nitrous oxide and nitrous gas, it appeared advisable to try, as belonging to the first class of substances, and carbonic acid gas as belonging to the latter.

The results with hydrogen, sulphuretted hydrogen, nitrous oxide and carbonic acid gas, were of a negative kind. Neither using arterial blood, nor blood which had been agitated with oxygen, and which had absorbed or made to disappear a certain quantity of this gas, could I detect any indications of its presence either by combination or expulsion. In the instances of nitrous oxide and carbonic acid, however, it may be worthy of remark, that the blood which had been agitated with oxygen absorbed less of either of these gases than it did before it was so treated*.

The results with phosphuretted hydrogen, the spontaneously inflammable species, were of an ambiguous kind, not sufficiently clear to deduce from them any satisfactory conclusion. In one trial, serum of the venous blood of the Sheep absorbed nine per cent. of this gas; venous blood 11·3 per cent.; and arterial 5·8 per cent†.

The results with nitrous gas were of a different kind, and may be deserving of being specially noticed.

The blood used was that of the Sheep prepared in the usual manner. The experiments were made during the very cold weather which prevailed in the beginning of the year, and the difference of colour between the venous and arterial blood was very strongly marked.

* Nitrous oxide I find is absorbed in about the same proportion by venous and arterial blood, and by the serum of blood, and also in about the same proportion by water. Thus, at the temperature of 45° over mercury, using the blood and serum of the same animal (the Sheep), thirty-two measures of each absorbed twenty-two measures of this gas, and thirty-two of distilled water absorbed 21·5.

† Supposing the gas decomposed, the phosphorus uniting with the oxygen in the blood, the apparent smaller absorption by arterial than by venous blood is what might be expected, on the idea that the former kind of blood contains most oxygen.

1st. On Arterial Blood.

1. Fifty-three measures of this blood were agitated over mercury with forty-six measures of nitrous gas; there was a diminution of volume of seventeen measures.

2. Fifty-three measures of the same blood (another portion) were agitated with nine of oxygen; two measures were absorbed.

3. Fifty measures of this blood, so treated with oxygen, were agitated with forty-seven of nitrous gas; there was a diminution of twenty-two measures.

2nd. On Venous Blood.

1. Fifty-three measures of this blood were agitated with fifty of nitrous gas; there was a diminution of ten measures.

2. Fifty-three measures (another portion) were agitated with ten of oxygen; five measures were absorbed.

3. Fifty-one measures of blood so treated were agitated with forty-nine of nitrous gas; there was a diminution of seventeen measures.

The residual air in each instance was examined and was found to be a mixture of nitrous gas and azote without carbonic acid gas. The azote, it may be presumed, was introduced with the nitrous gas; it was in the same proportion as that which adulterated it, viz. about four per cent. That the residual air was free from carbonic acid was inferred from the circumstance, that in comparative experiments with and without addition of a portion of solution of caustic alkali, there was no difference in the proportion of nitrous gas absorbed: and it was corroborated by another circumstance, viz. that after the absorption of the nitrous gas, the blood was capable of absorbing seventy-five per cent of carbonic acid gas*; and further by the result that when nitrous gas is added not to saturation, the whole of it is absorbed†.

As regards the blood itself, the colour of both venous and arterial was altered; both were rendered darker and browner, as if a minute quantity of nitric acid had been added to them, a change long known to be occasioned by nitrous gas. In the degree of change there was however a difference; in the instance of arterial and of oxygenated arterial blood, it was more strongly marked than in that of the venous.

In conjunction with the unsuccessful attempts with the other gases already mentioned, do not the results just described indicate that a portion of oxygen exists in

* To some venous blood of a Sheep which absorbed 182 per cent. of carbonic acid gas, so much of a solution of pure hydrate of potash was added, that it absorbed 218 per cent. of the acid gas; seventeen measures of this blood with excess of alkali, agitated with fifty-one of nitrous gas, absorbed 5·5 measures; sixteen of the blood without the excess of alkali absorbed five measures: thirty-four measures of carbonic acid gas were added to the latter, the excess of nitrous gas being left in the tube; on agitation twelve measures of the carbonic acid were absorbed.

† In one experiment eighty-four measures of the venous blood of the Sheep were agitated with ten of nitrous gas over mercury; the whole of the gas was absorbed: twelve of oxygen were added; on agitation again there was no further absorption.

the blood, not capable of being extracted by the air-pump, and yet capable of entering into combination with nitrous gas, and which exists in largest proportion in arterial blood? Unless this conclusion is adopted, it must be supposed either that the nitrous gas which disappears is decomposed, or that it combines directly with the red particles; neither of which suppositions is well supported by facts. The circumstance that no azote is disengaged, is not favourable to the idea that the nitrous gas is decomposed; and the difference of effect in the instances of venous and arterial blood, and, after and before agitation with oxygen, is not in accordance with the notion of direct combination. I may mention another fact which seems to have the same bearing: serum, which does not absorb oxygen, I find also does not absorb nitrous gas, excepting in about the proportion in which water absorbs it: and, further, in corroboration, I may mention, that as blood putrefies, whether arterial or venous, its power of absorbing nitrous gas diminishes; and that it is also diminished by being agitated with phosphuretted hydrogen. Thus the arterial blood of a Sheep, which before agitation with phosphuretted hydrogen absorbed 45·3 per cent. of nitrous gas, after agitation with it absorbed 7·4 per cent. less; and, after it had become putrid, it absorbed twenty per cent. less. According to my observations, arterial blood does not lose its peculiar florid hue under the action of the air-pump. Is not this also in favour of the above inference, that a portion of oxygen is retained by the blood resisting extraction by the air-pump? I find also that when venous blood is agitated with oxygen and subjected to the air-pump, it, in like manner, retains its acquired florid vermilion hue, and likewise a power of absorbing an additional quantity of nitrous gas.

VII. *When oxygen is absorbed by the blood is there any production of heat?*

To endeavour to determine this point, of so much interest in connexion with the theory of animal heat, a very thin vial, of the capacity of eight liquid ounces, was selected and carefully enveloped in bad conducting substances, viz. several folds of flannel, of fine oiled paper, and of oiled cloth. Thus prepared, and a perforated cork being provided, holding a delicate thermometer, two cubic inches of mercury were introduced, and immediately after it was filled with venous blood, kept liquid as before described. The vial was now corked and shaken; the thermometer included was stationary at 45°. After five minutes that it was so stationary, the thermometer was withdrawn, the vial closed by another cork was transferred inverted to a mercurial bath, and $1\frac{1}{2}$ cubic inch of oxygen was introduced. The common cork was returned, and the vial was well agitated for about a minute; the thermometer was now introduced, it rose immediately to 46°, and continuing the agitation it rose further to 46·5, very nearly to 47°. This experiment was made on the 12th of last February on the blood of the Sheep.

On the following day a similar experiment was made on the venous blood of Man. The vial was filled with eleven cubic inches of this blood, its fibrin broken up in the usual manner, and with three cubic inches of mercury; the temperature of the blood and mercury was 42·5, and the temperature was the same after the introduction of three cubic inches of oxygen. The temperature of the room being 47°, a fire having shortly before been lit, the vial was taken to an adjoining passage where the temperature of the air was 39°. Here the vial was well agitated, held in the hand with thick gloves on as an additional protection. After about three quarters of a minute the thermometer in the vial had risen a degree, viz. to 43·5.

On the 14th of the same month a third experiment was made on venous blood from the jugular vein of a Sheep. The vial was filled with 3·5 cubic inches of mercury and eleven cubic inches of blood. The thermometer in the bottle, left five minutes, was stationary at 49°; the temperature of the mercurial bath was 49°; the air of the room was 52°; a thermometer with its bulb moistened was 45°, which I mention because the outer covering of the vial was moistened with some blood which had overflowed. After three cubic inches and a half of oxygen had been introduced, before agitation, the thermometer was still 49°. The bottle was briskly shaken for about half a minute; now, on observing the thermometer, it was found at 50°; the vial was again agitated; there was no further increase of temperature: after ten minutes it had fallen to 49°.

I shall relate one experiment more, and that on arterial blood. It was made on the 14th of February, and in the same manner as those on the venous blood. Before and after the introduction of the oxygen, the blood, which was from the carotid artery of the Sheep, was 45°; after agitation with oxygen it rose to 45·5: this was done when the temperature of the air was 39°.

In a former part of this paper I proposed to recur to the question, Is the fixation of oxygen in the blood attended with the formation of carbonic acid gas? The change of colour accompanying the fixation of oxygen by the blood, so different from that produced by carbonic acid, and the effect of nitrous gas before and after, seem to be most in favour of the idea, that the oxygen, in the first instance, is simply absorbed, and that the heat evolved is merely the effect of its condensation; or, that if any of it enters into immediate union with the carbon, it is only a small part of the whole.

VIII. *Conclusion.*

Should the results detailed in the preceding pages be confirmed on repetition, they can hardly fail having some effect on the theory of respiration and animal heat.

As regards the former, they appear to me to tend to show that the lungs are absorbing and secreting, and perhaps exhaling organs, and that their peculiar function is to introduce oxygen into the blood and separate carbonic acid from the blood.

As regards animal heat, they appear to favour the idea, that it is owing, first, to the fixation or condensation of oxygen in the blood in the lungs in conversion from venous to arterial; and secondly, to the combinations into which it enters in the circulation in connexion with the different secretions and changes essential to animal life.

In illustration of what I imagine the secreting power of the lungs, I may mention the difference of effects in an instance of death by strangulation, and another by exhaustion of air from the lungs by the air-pump. A full-grown Guinea Pig was the subject of experiment in each. The one killed by strangulation died in about a minute after a cord had been drawn tightly round its neck; the other, placed on the plate of the air-pump and confined by a receiver just large enough to hold it, lived about five minutes after the exhaustion had been commenced, the pump the whole time having been worked rapidly. The bodies were immediately examined. The heart of the strangled animal was motionless; it was distended with dark blood; twelve measures of the blood, broken up and agitated with twenty-nine of carbonic acid gas, absorbed eighteen measures, or 150 per cent. The heart of the other Guinea Pig was also distended with blood, but of a less dark hue. Its auricles were feebly acting; the lungs were paler than in the former and more collapsed: ten measures of blood from the heart, broken up and agitated with fifty of carbonic acid gas, absorbed thirty-seven measures, or 370 per cent.!

Further, in illustration of this supposed secreting power of the lungs, I might adduce the condition of the blood in disease, and in instances in which I have examined it after death from disease, in the majority of which I have found the blood loaded with carbonic acid, as indicated both by the disengagement of this gas, when the blood was agitated with another gas, and by the comparatively small proportion of carbonic acid which the blood was capable of absorbing. This condition of the blood, in relation to carbonic acid, I believe to be one of great interest and importance, and capable, when further investigated, of throwing light on many obscure parts of pathology, and especially on the immediate cause of death, and that happy absence of pain in dying which is commonly witnessed.

As regards an exhaling power, which I suppose the lungs may possess, I conceive it may be exercised occasionally under peculiar circumstances—circumstances, in the first instance, favouring an accumulation of carbonic acid gas in the blood, as undue pressure of any kind, and, in the second instance, circumstances of a different nature, connected with the removal of undue pressure, admitting thereby the excess to pass off.

The view which I have alluded to relative to the production of animal heat, is, I believe, capable of explaining very many particulars of animal temperature in different classes of animals, and both during life, in health, and disease, and in a state of hybernation and after death. If correct, this it must necessarily do, theory being merely an expression of facts and truths in nature being perfectly consistent.

Fort Pitt, Chatham,
May 30th, 1838.

ERRATUM.

Page 288, line 28, *for* $\frac{114}{100}$ *read* $\frac{14}{100}$.