The discovery of oxygen.

Contributors

Scheele, Carl Wilhelm, 1742-1786.
University College, London. Library Services

Publication/Creation

Edinburgh : W. F. Clay ; London : Simpkin, Marshall, Hamilton, Kent & Co., 1894.

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THE

DISCOVERY OF OXYGEN,

PART 2.

BY

CARL WILHELM SCHEELE.

(1777.)

Alembic Club Reprints,
No. 8.

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EXPERIMENTS BY

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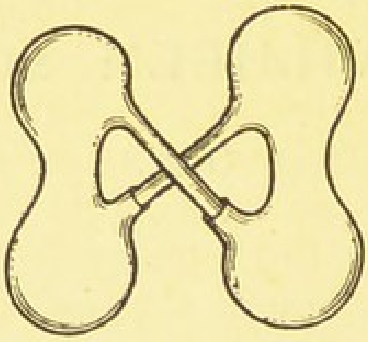

PREFACE.

THE portions of Scheele's "Chemical Treatise on Air and Fire" here reproduced in English are intended to form a companion volume to No. 7 of the Club Reprints, which contains Priestley's account of his discovery of oxygen. Not only have the claims of Scheele to the independent discovery of this gas never been disputed, but the valuable volume of "Letters and Memoranda" of Scheele, edited by Nordenskjöld, which was published in 1892, places it beyond doubt that Scheele had actual priority in the discovery by about two years, although his printed account of it appeared subsequent to that of Priestley by about the same length of time. Scheele's laboratory notes, which are still preserved in the Royal Academy of Science in Stockholm, shew that oxygen had been obtained by him prior to 1773, not only by heating red oxide of mercury, but also by several other methods. Priestley first obtained oxygen from red oxide of mercury in 1774.

In his "Chemical Treatise" Scheele endeavours, at considerable length, to prove by experiments his views as to the compound character of heat and of light. These portions of the work have been entirely omitted from what is reproduced here. All the places where omissions have been made are indicated.

Every care has been taken in the endeavour to make the translation a faithful reproduction of the meaning of the original, whilst literal accuracy has been aimed at rather than literary elegance.

L. D.

CHEMICAL TREATISE ON AIR AND FIRE.*

1. IT is the object and chief business of chemistry to skilfully separate substances into their constituents, to discover their properties, and to compound them in different ways.

How difficult it is, however, to carry out such operations with the greatest accuracy, can only be unknown to one who either has never undertaken this occupation, or at least has not done so with sufficient attention.

2. Hitherto chemical investigators are not agreed as to how many elements or fundamental materials compose all substances. In fact this is one of the most difficult problems; some indeed hold that there remains no further hope of searching out the elements of substances. Poor comfort for those who feel their greatest pleasure in the investigation of natural things! Far is he mistaken, who endeavours to confine chemistry, this noble science, within such narrow bounds! Others believe that earth and phlogiston are the things from which all material nature has derived its origin. The majority seem completely attached to the peripatetic elements.

3. I must admit that I have bestowed no little trouble upon this matter in order to obtain a clear conception of it. One may reasonably be amazed at the numerous ideas and conjectures which authors have recorded on the subject, especially when they give a decision respecting the fiery phenomenon; and this very matter was

* Carl Wilhelm Scheele's Chemische Abhandlung von der Luft und dem Feuer. Upsala und Leipzig, 1777.

of the greatest importance to me. I perceived the necessity of a knowledge of fire, because without this it is not possible to make any experiment; and without fire and heat it is not possible to make use of the action of any solvent. I began accordingly to put aside all explanations of fire; I undertook a multitude of experiments in order to fathom this beautiful phenomenon as fully as possible. I soon found, however, that one could not form any true judgment regarding the phenomena which fire presents, without a knowledge of the air. I saw, after carrying out a series of experiments, that air really enters into the mixture of fire, and with it forms a constituent of flame and of sparks. I learned accordingly that a treatise like this, on fire, could not be drawn up with proper completeness without taking the air also into consideration.

4. Air is that fluid invisible substance which we continually breathe, which surrounds the whole surface of the earth, is very elastic, and possesses weight. It is always filled with an astonishing quantity of all kinds of exhalations, which are so finely subdivided in it that they are scarcely visible even in the sun's rays. Water vapours always have the preponderance amongst these foreign particles. The air, however, is also mixed with another elastic substance resembling air, which differs from it in numerous properties, and is, with good reason, called aerial acid by Professor Bergman. It owes its presence to organised bodies, destroyed by putrefaction or combustion.

5. Nothing has given philosophers more trouble for some years than just this delicate acid or so-called fixed air. Indeed it is not surprising that the conclusions which one draws from the properties of this elastic acid are not favourable to all who are prejudiced by previously conceived opinions. These defenders of the Paracelsian

doctrine believe that the air is in itself unalterable; and, with Hales, that it really unites with substances thereby losing its elasticity; but that it regains its original nature as soon as it is driven out of these by fire or fermentation. But since they see that the air so produced is endowed with properties quite different from common air, they conclude, without experimental proofs, that this air has united with foreign materials, and that it must be purified from these admixed foreign particles by agitation and filtration with various liquids. I believe that there would be no hesitation in accepting this opinion, if one could only demonstrate clearly by experiments that a given quantity of air is capable of being completely converted into fixed or other kind of air by the admixture of foreign materials; but since this has not been done, I hope I do not err if I assume as many kinds of air as experiment reveals to me. For when I have collected an elastic fluid, and observe concerning it that its expansive power is increased by heat and diminished by cold, while it still uniformly retains its elastic fluidity, but also discover in it properties and behaviour different from those of common air, then I consider myself justified in believing that this is a peculiar kind of air. I say that air thus collected must retain its elasticity even in the greatest cold, because otherwise an innumerable multitude of varieties of air would have to be assumed, since it is very probable that all substances can be converted by excessive heat into a vapour resembling air.

6. Substances which are subjected to putrefaction or to destruction by means of fire diminish, and at the same time consume, a part of the air; sometimes it happens that they perceptibly increase the bulk of the air, and sometimes finally that they neither increase nor diminish a given quantity of air—phenomena which are certainly remarkable. Conjectures can here determine nothing

with certainty, at least they can only bring small satisfaction to a chemical philosopher, who must have his proofs in his hands. Who does not see the necessity of making experiments in this case, in order to obtain light concerning this secret of nature?

7. General properties of ordinary air.

(1.) Fire must burn for a certain time in a given quantity of air. (2.) If, so far as can be seen, this fire does not produce during combustion any fluid resembling air, then, after the fire has gone out of itself, the quantity of air must be diminished between a third and a fourth part. (3.) It must not unite with common water. (4.) All kinds of animals must live for a certain time in a confined quantity of air. (5.) Seeds, as for example peas, in a given quantity of similarly confined air, must strike roots and attain a certain height with the aid of some water and of a moderate heat.

Consequently, when I have a fluid resembling air in its external appearance, and find that it has not the properties mentioned, even when only one of them is wanting, I feel convinced that it is not ordinary air.

8. Air must be composed of elastic fluids of two kinds.

First Experiment.—I dissolved one ounce of alkaline liver of sulphur in eight ounces of water; I poured 4 ounces of this solution into an empty bottle capable of holding 24 ounces of water, and closed it most securely with a cork; I then inverted the bottle and placed the neck in a small vessel with water; in this position I allowed it to stand for 14 days. During this time the solution had lost a part of its red colour and had also deposited some sulphur: afterwards I took the bottle and held it in the same position in a larger vessel with water, so that the mouth was under and the bottom above the water-level, and withdrew the cork under the water;

immediately water rose with violence into the bottle. I closed the bottle again, removed it from the water, and weighed the fluid which it contained. There were 10 ounces. After subtracting from this the 4 ounces of solution of sulphur there remain 6 ounces, consequently it is apparent from this experiment that of 20 parts of air 6 parts have been lost in 14 days.

9. **Second Experiment.**—(*a.*) I repeated the preceding experiment with the same quantity of liver of sulphur, but with this difference that I only allowed the bottle to stand a week, tightly closed. I then found that of 20 parts of air only 4 had been lost. (*b.*) On another occasion I allowed the very same bottle to stand 4 months; the solution still possessed a somewhat dark yellow colour. But no more air had been lost than in the first experiment, that is to say 6 parts.

10. **Third Experiment.**—I mixed 2 ounces of caustic ley, which was prepared from alkali of tartar and unslaked lime and did not precipitate lime water, with half an ounce of the preceding solution of sulphur which likewise did not precipitate lime water. This mixture had a yellow colour. I poured it into the same bottle, and after this had stood 14 days, well closed, I found the mixture entirely without colour and also without precipitate. I was enabled to conclude that the air in this bottle had likewise diminished, from the fact that air rushed into the bottle with a hissing sound after I had made a small hole in the cork.

11. **Fourth Experiment.**—(*a.*) I took 4 ounces of a solution of sulphur in lime water; I poured this solution into a bottle and closed it tightly. After 14 days the yellow colour had disappeared, and of 20 parts of air 4 parts had been lost. The solution contained no sulphur, but had allowed a precipitate to fall which was chiefly gypsum. (*b.*) Volatile liver of sulphur

likewise diminishes the bulk of air. (*c.*) Sulphur, however, and volatile spirit of sulphur, undergo no alteration in it.

12. **Fifth Experiment.**—I hung up over burning sulphur, linen rags which were dipped in a solution of alkali of tartar. After the alkali was saturated with the volatile acid, I placed the rags in a flask, and closed the mouth most carefully with a wet bladder. After 3 weeks had elapsed I found the bladder strongly pressed down; I inverted the flask, held its mouth in water, and made a hole in the bladder; thereupon water rose with violence into the flask and filled the fourth part.

13. **Sixth Experiment.**—I collected in a bladder the nitrous air which arises on the dissolution of the metals in nitrous acid, and after I had tied the bladder tightly I laid it in a flask and secured the mouth very carefully with a wet bladder. The nitrous air gradually lost its elasticity, the bladder collapsed, and became yellow as if corroded by *aqua fortis.* After 14 days I made a hole in the bladder tied over the flask, having previously held it, inverted, under water; the water rose rapidly into the flask, and it remained only $\frac{2}{3}$ empty.

14. **Seventh Experiment.**—(*a.*) I immersed the mouth of a flask in a vessel with oil of turpentine. The oil rose in the flask a few lines every day. After the lapse of 14 days the fourth part of the flask was filled with it; I allowed it to stand for 3 weeks longer, but the oil did not rise higher. All those oils which dry in the air, and become converted into resinous substances, possess this property. Oil of turpentine, however, and linseed oil rise up sooner if the flask is previously rinsed out with a concentrated sharp ley. (*b.*) I poured 2 ounces of colourless and transparent animal oil of Dippel into a bottle and closed it very tightly; after the expiry of two months the oil was thick and black. I then held

the bottle, inverted, under water and drew out the cork ; the bottle immediately became $\frac{1}{4}$ filled with water.

15. **Eighth Experiment.**—(*a.*) I dissolved 2 ounces of vitriol of iron in 32 ounces of water, and precipitated this solution with a caustic ley. After the precipitate had settled, I poured away the clear fluid and put the dark green precipitate of iron so obtained, together with the remaining water, into the before-mentioned bottle (§ 8), and closed it tightly. After 14 days (during which time I shook the bottle frequently), this green calx of iron had acquired the colour of crocus of iron, and of 40 parts of air 12 had been lost. (*b.*) When iron filings are moistened with some water and preserved for a few weeks in a well closed bottle, a portion of the air is likewise lost. (*c.*) The solution of iron in vinegar has the same effect upon air. In this case the vinegar permits the dissolved iron to fall out in the form of a yellow crocus, and becomes completely deprived of this metal. (*d.*) The solution of copper prepared in closed vessels with spirit of salt likewise diminishes air. In none of the foregoing kinds of air can either a candle burn or the smallest spark glow.

16. It is seen from these experiments that phlogiston, the simple inflammable principle, is present in each of them. It is known that the air strongly attracts to itself the inflammable part of substances and deprives them of it : not only this may be seen from the experiments cited, but it is at the same time evident that on the transference of the inflammable substance to the air a considerable part of the air is lost. But that the inflammable substance* alone is the cause of this action, is plain from this, that, according to the 10th paragraph, not the least trace of sulphur remains over, since, according to my experiments this colourless ley contains only some vit-

[* "Das Brennbare."]

riolated tartar. The 11th paragraph likewise shews this. But since sulphur alone, and also the volatile spirit of sulphur, have no effect upon the air (§ 11. *c*), it is clear that the decomposition of liver of sulphur takes place according to the laws of double affinity,—that is to say, that the alkalies and lime attract the vitriolic acid, and the air attracts the phlogiston.

It may also be seen from the above experiments, that a given quantity of air can only unite with, and at the same time saturate, a certain quantity of the inflammable substance: this is evident from the 9th paragraph, *letter b*. But whether the phlogiston which was lost by the substances was still present in the air left behind in the bottle, or whether the air which was lost had united and fixed itself with the materials such as liver of sulphur, oils, &c., are questions of importance.

From the first view, it would necessarily follow that the inflammable substance possessed the property of depriving the air of part of its elasticity, and that in consequence of this it becomes more closely compressed by the external air. In order now to help myself out of these uncertainties, I formed the opinion that any such air must be specifically heavier than ordinary air, both on account of its containing phlogiston and also of its greater condensation. But how perplexed was I when I saw that a very thin flask which was filled with this air, and most accurately weighed, not only did not counterpoise an equal quantity of ordinary air, but was even somewhat lighter. I then thought that he latter view might be admissible; but in that case it would necessarily follow also that the lost air could be separated again from the materials employed. None of the experiments cited seemed to me capable of shewing this more clearly than that according to the 10th paragraph, because this residuum, as already mentioned, consists of vitriolated tartar and alkali. In order therefore to see whether the lost air had been converted into fixed air, I tried whether the latter shewed itself when some of the caustic ley was poured into lime water; but in vain—no precipitation took place. Indeed, I tried in several ways to obtain the lost air from this alkaline mixture, but as the results were similar to the foregoing, in order to avoid prolixity I shall not cite these experiments. Thus much I see from the experiments mentioned, that the air consists of two fluids, differing from each other, the one of which does not manifest in the least the property of attracting phlogiston, while the other, which composes between the third and the fourth part of the whole mass of the air, is peculiarly disposed to such attraction. But where this latter kind of air has gone to after it has united with the inflammable substance, is a question which must be decided by further experiments, and not by conjectures.

We shall now see how the air behaves towards inflammable substances when they get into fiery motion. We shall first consider that kind of fire which does not give out during the combustion any fluid resembling air.

17. **First Experiment.**—I placed 9 grains of phosphorus from urine in a thin flask, which was capable of holding 30 ounces of water, and closed its mouth very tightly. I then heated, with a burning candle, the part of the flask where the phosphorus lay; the phosphorus began to melt, and immediately afterwards took fire; the flask became filled with a white cloud, which attached itself to the sides like white flowers; this was the dry acid of phosphorus. After the flask had become cold again, I held it, inverted, under water and opened it; scarcely had this been done when the external air pressed water into the flask; this water amounted to 9 ounces.

18. **Second Experiment.**—When I placed pieces of phosphorus in the same flask and allowed it to stand,

closed, for 6 weeks, or until it no longer glowed, I found that $\frac{1}{3}$ of the air had been lost.

19. **Third Experiment.**—I placed 3 teaspoonfuls of iron filings in a bottle capable of holding 2 ounces of water; to this I added an ounce of water, and gradually mixed with them half an ounce of oil of vitriol. A violent heating and fermentation took place. When the froth had somewhat subsided, I fixed into the bottle an accurately fitting cork, through which I had previously fixed a glass tube A (Fig. 1). I placed this bottle in a vessel filled with hot water, B B (cold water would greatly retard the solution). I then approached a burning candle to the orifice of the tube, whereupon the inflammable air took fire and burned with a small yellowish-green flame. As soon as this had taken place, I took a small flask C, which was capable of holding 20 ounces of water, and held it so deep in the water that the little flame stood in the middle of the flask. The water at once began to rise gradually into the flask, and when the level had reached the point D the flame went out. Immediately afterwards the water began to sink again, and was entirely driven out of the flask. The space in the flask up to D contained 4 ounces, therefore the fifth part of the air had been lost. I poured a few ounces of lime water into the flask in order to see whether any aerial acid had also been produced during the combustion, but I did not find any. I made the same experiment with zinc filings, and it proceeded in every way similarly to that just mentioned. I shall demonstrate the constituents of this inflammable air further on; for, although it seems to follow from these experiments that it is only phlogiston, still other experiments are contrary to this.

We shall now see the behaviour of air towards that kind of fire which gives off, during the combustion, a fluid resembling air.

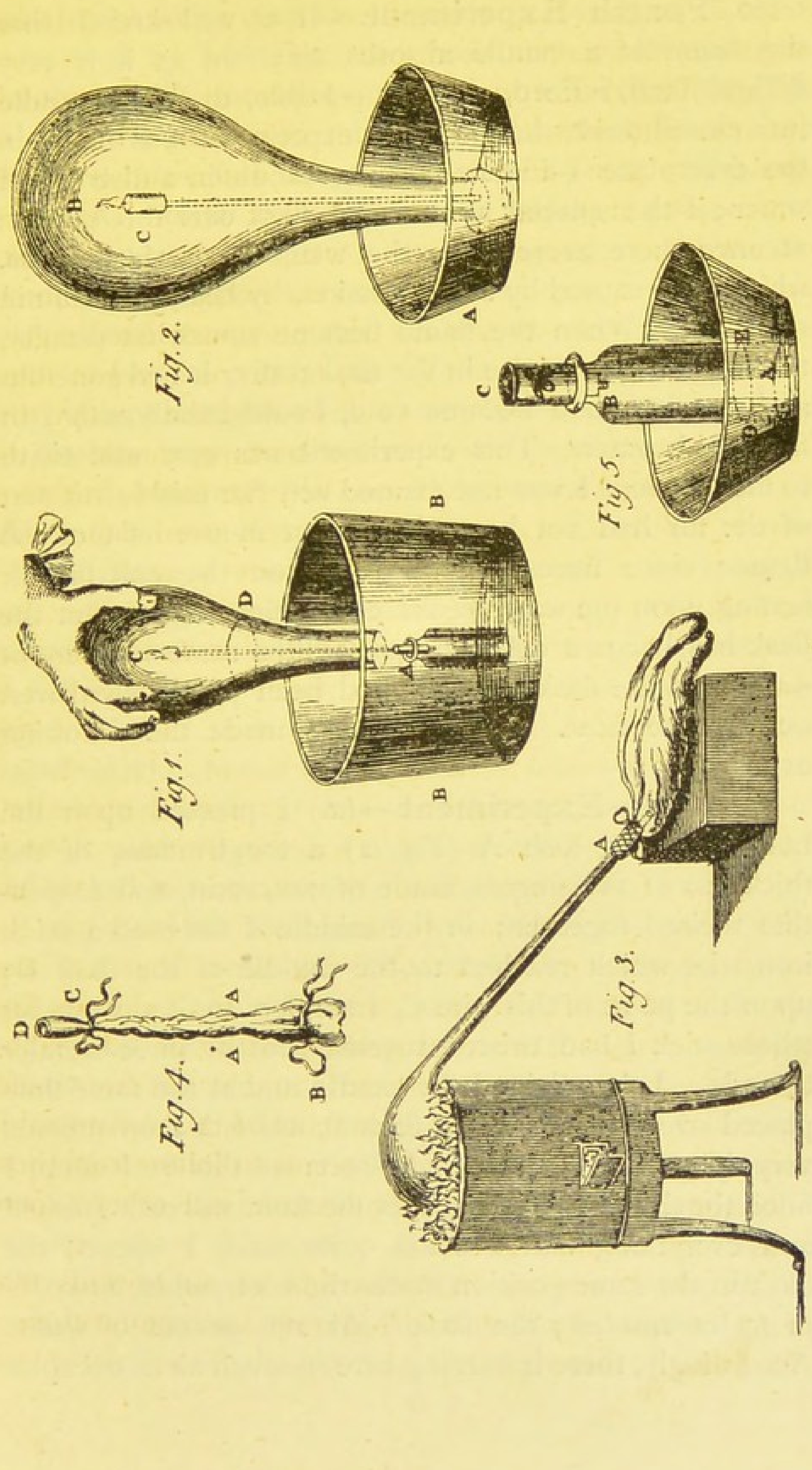
Fig. 1.
Fig. 2.
Fig. 3.
Fig. 4.
Fig. 5.

20. **Fourth Experiment.**—It is well known that the flame of a candle absorbs air; but as it is very difficult, and, indeed, scarcely possible, to light a candle in a closed flask, the following experiment was made in the first place:—I set a burning candle in a dish full of water; I then placed an inverted flask over this candle; at once there arose from the water large air bubbles, which were caused by the expansion, by heat, of the air in the flask. When the flame became somewhat smaller, the water began to rise in the flask; after it had gone out and the flask had become cold, I found the fourth part filled with water. This experiment was very undecisive to me, because I was not assured whether this fourth part of the air had not been driven out by the heat of the flame; since necessarily in that case the external air resting upon the water seeks equilibrium again after the flask has become cold, and presses the same measure of water into the flask as of air had been previously driven out by the heat. Accordingly, I made the following experiment:—

21. **Fifth Experiment.**—(*a.*) I pressed upon the bottom of the dish A (Fig. 2) a tough mass, of the thickness of two fingers, made of wax, resin, and turpentine melted together; in the middle I fastened a thick iron wire which reached to the middle of the flask B; upon the point of this wire C, I stuck a small wax candle, whose wick I had twisted together out of three slender threads. I then lighted the candle, and at the same time placed over it the inverted flask B, which I then pressed very deep into the mass. As soon as this was done, I filled the dish with water. After the flame was extinguished and everything had become quite cold, I opened the flask in the same position under the water, when 2 ounces of water entered; the flask held 160 ounces of water. Accordingly, there is wanting here so much air as occupies

the space of 2 ounces of water. Has this air been absorbed by the inflammable substance, or has the heat of the small flame driven it out even before I could press the flask into the tough mass? The latter seems to have taken place in this case, as I conclude from the following:—I took a small flask capable of holding 20 ounces of water; in this I caused a candle to burn as in the preceding; after everything had become cold, I opened this flask likewise under water, whereupon similarly nearly 2 ounces entered. Had the former 2 ounces measure of air been absorbed, then there should have been only 2 drachms measure absorbed in this experiment.

(*b.*) I repeated the preceding experiment with the large flask in exactly the same way, except that I employed spirit of wine in place of the candle. I fastened three iron wires, which were of equal length and reached up to the middle of the flask, into the tough mass which was firmly pressed on to the bottom of the dish. Upon these wires I laid a four-cornered plate of metal, and upon this I placed a small vessel into which spirit of wine was poured. I set fire to this and placed the flask over it. After cooling, I observed that 3 ounces measure of air had been driven out by the heat of the flame.

(*c.*) Upon the same stand I placed a few small glowing coals, and allowed them to go out in the same way under the flask. I found after cooling that the heat of the coals had driven out three and a half ounces measure of air.

These experiments seem to prove that the transference of phlogiston to the air does not always diminish its bulk, which, however, the experiments mentioned in §§ 8-16 shew distinctly. But the following will shew that that portion of the air which unites with the inflammable substance, and is at the same time absorbed by it, is replaced by the newly formed aerial acid.

22. Sixth Experiment.—After the fire had gone out and everything had become cold in the experiments mentioned above (§ 21. *a. b. c.*), I poured into each flask 6 ounces of milk of lime (lime water which has in it more unslaked lime than the water can dissolve); I then placed my hand firmly on the mouth of the flask and swung it several times up and down; then I held the flask inverted under water and drew my hand a little to one side, so that a small orifice might be made. Water immediately rose into the flask. Then I shut the mouth again very tightly with my hand under water, and afterwards shook it several times up and down. I opened it again under water; this operation I repeated twice more until no more water would rise into the flask, or until no more aerial acid was present in it. I then perceived that in each experiment between 7 and 8 ounces of water rose into the flasks, consequently the nineteenth part of the air has been lost. This was indeed something, but since in the combustion of phosphorus (§ 17) nearly the third part of the air was lost, there must be another reason besides, why as much is not absorbed in this case also. It is known that one part of aerial acid mixed with 10 parts of ordinary air extinguishes fire; and there are here in addition, expanded by the heat of the flame and surrounding the latter, the watery vapours produced by the destruction of these oily substances. It is these two elastic fluids, separating themselves from such a flame, which present no small hindrance to the fire which would otherwise certainly burn much longer, especially since there is here no current of air by means of which they can be driven away from the flame. When the aerial acid is separated from this air by milk of lime, then a candle can burn in it again, although only for a very short time.

23. Seventh Experiment.—I placed upon the stand (§ 21. *b*) a small crucible which was filled with sulphur; I set fire to it and placed the flask over it. After the sulphur was extinguished and everything had become cold, I found that out of 160 parts of air, 2 parts were driven out of the flask by the heat of the flame. I next poured 6 ounces of clear lime water into the flask and dealt with it by shaking, as already explained, and observed that the sixth part of all the air had been lost in consequence of the combustion. The lime water was not in the least precipitated in this case, an indication that sulphur gives out no aerial acid during its combustion, but another substance somewhat resembling air; this is the volatile acid of sulphur, which occupies again the empty space produced by the union of the inflammable substance with air. It is not, as may be seen, a trifling circumstance that phlogiston, whether it separates itself from substances and enters into union with air, with or without a fiery motion, still in every case diminishes the air so considerably in its external bulk.

24. Experiments which prove that ordinary air, consisting of two kinds of elastic fluids, can be compounded again after these have been separated from each other by means of phlogiston.

I have already stated in § 16 that I was not able to find again the lost air. One might indeed object, that the lost air still remains in the residual air which can no more unite with phlogiston; for, since I have found that it is lighter than ordinary air, it might be believed that the phlogiston united with this air makes it lighter, as appears to be known already from other experiments. But since phlogiston is a substance, which always presupposes some weight, I much doubt whether such hypothesis has any foundation. . . .

25. How often must not chemists have distilled the

fuming acid of nitre from oil of vitriol and nitre, when it is impossible that they should not have observed how this acid went over red in the beginning, white and colourless in the middle of the distillation, but at the end red again; and indeed so dark-red that one could not see through the receiver? It is to be noticed here that if the heat is permitted to increase too much at the end of the distillation, the whole mixture enters into such frothing that everything goes over into the receiver; and, what is of the greatest importance, a kind of air goes over during this frothing which deserves no small attention. If one takes for such distillation a very black oil of vitriol, not only does the acid go over at the beginning of a far darker red than when one takes a white oil of vitriol, but further, when one introduces a burning candle into the receiver after about an ounce has gone over, this goes out immediately. On the other hand, when one places a burning candle in the receiver filled with blood-red vapours, towards the end of the distillation when, as has been said, the mixture froths strongly, not only will it continue to burn, but this will take place with a much brighter light than in ordinary air. The same thing occurs when one attaches, at the close of the distillation, a receiver which is filled with an air in which fire will not burn, for, when this has been attached for half an hour, a candle will likewise continue to burn in the air.

In this case there now arises in the first place the question: Are the vapours of the acid of nitre naturally red? I beg leave to raise this question here because I believe there are people who advance the redness of this acid as a distinguishing characteristic. The colours of the acid of nitre are accidental. When a few ounces of fuming acid of nitre are distilled by a very gentle heat, the yellow separates itself from it and goes into the receiver, and the residuum in the retort becomes white and colourless like water. This acid has all the chief properties of acid of nitre, except that the yellow colour is wanting. This I call the pure acid of nitre; as soon, however, as it comes into contact with an inflammable substance, it becomes more or less red. This red acid is more volatile than the pure, hence heat alone can separate them from one another; and, for exactly the same reason, the volatile spirit must go over first in the distillation of Glauber's spirit of nitre. When this has gone over, the colourless acid follows; but why does the acid make its appearance again so blood-red at the end of the distillation? Why has not this redness already been driven over at the beginning? Where does it now obtain its phlogiston? This is the difficulty.

26. I intimated in the preceding paragraph that the candle went out in the receiver at the beginning of the distillation. The reason is to be found in the experiment which I have cited in § 13. In this case the acid of nitre, passing over in vapours, takes to itself the inflammable substance, whose presence is indicated by the black colour of the oil of vitriol; as soon as this has taken place it meets with the air, which again robs the now phlogisticated acid of its inflammable substance; by this means a part of the air contained in the receiver becomes lost, hence the fire introduced into it must go out (§ 15).

27. The acid of nitre can attract phlogiston in varying quantity, when it likewise receives other properties with each proportion. (*a.*) When it becomes, as it were, saturated with it, a true fire arises, and it is then completely destroyed. (*b.*) When the inflammable principle is present in smaller quantity, this acid is converted into a kind of air which will not unite either with the alkalies or with the absorbent earths, and with water only in very small quantity. When this acid of nitre, resembling air, meets with the air, the latter takes the inflammable substance from it again, it loses its elasticity (§ 13), the vapours acquire redness, and the air undergoes at the same time this no less remarkable than natural alteration, that it is not only diminished, but also becomes warm. (*c.*) When the acid of nitre receives still somewhat less phlogiston, it is likewise converted into a kind of air, which, like the air, is also invisible, but unites with the alkalies and earths, and along with them can bring forth real intermediate salts. This phlogisticated acid is, however, so loosely united with these absorbing substances, that even the simple mixture with the vegetable acids can drive it out. It is present in this condition in nitre which has been made red-hot, and also in *Nitrum Antimoniatum.* When this acid of nitre meets the air it also loses its elasticity and is converted into red vapours. When it is mixed in a certain quantity with water, this acquires a blue, green, or yellow colour. (*d.*) When the pure acid of nitre receives but very little of the inflammable substance, the vapours only acquire a red colour, and are wanting in expansive power; it is, however, more volatile than the pure acid. This acid holds this small quantity of phlogiston so firmly that even the air, which so strongly attracts the inflammable substance, is not able to separate this from it.

* * * *

29. I took a glass retort which was capable of holding 8 ounces of water, and distilled fuming acid of nitre according to the usual method. In the beginning the acid went over red, then it became colourless, and finally all became red again; as soon as I perceived the latter, I took away the receiver and tied on a bladder, emptied of air, into which I poured some thick milk of lime (§ 22) in order to prevent the corrosion of the bladder. I then proceeded with the distillation. The bladder began

to expand gradually. After this I permitted everything to cool, and tied up the bladder. Lastly I removed it from the neck of the retort. I filled a bottle, which contained 10 ounces of water, with this gas (§ 30, *e.*), I then placed a small lighted candle in it; scarcely had this been done when the candle began to burn with a large flame, whereby it gave out such a bright light that it was sufficient to dazzle the eyes. I mixed one part of this air with three parts of that kind of air in which fire would not burn; I had here an air which was like the ordinary air in every respect. Since this air is necessarily required for the origination of fire, and makes up about the third part of our common air, I shall call it after this, for the sake of shortness, Fire-air; but the other air which is not in the least serviceable for the fiery phenomenon, and makes up about two-thirds of our air, I shall designate after this with the name already known, of Vitiated Air.

30. Anyone might ask me in what way I bring air from one vessel into another. I find it necessary therefore to describe this in the first place. My arrangements and vessels are the very simplest that one can possibly have: flasks, retorts, bottles, glasses, and ox bladders are the things which I employ. The bladders, while they are still fresh, are rubbed, and blown up very fully, then tightly tied and hung up to dry. When I wish to use such a bladder and find it blown up just as fully as at first, I am thereby assured that it is tight.

(*a.*) When I wish to collect any kind of air in a bladder, for example the phlogisticated acid of nitre (§ 13), I take a soft bladder smeared inside with a few drops of oil, and place in it some filings of a metal, as iron, zinc, or tin; I then press the air as completely as possible out of the bladder and tie it very tightly over a small bottle into which some *aqua fortis* has been poured; I then partly unfold the bladder so that a few iron filings may fall into the *aqua fortis*, according as this dissolves the bladder becomes expanded. When I have collected enough of the air so produced, I tightly tie up the bladder with a thread close above the mouth of the bottle, and then detach it from the bottle. (*b.*) If this phlogisticated acid of nitre is mixed with aerial acid, which is the case when the acid of the nitre is extracted over sugar, I tie a bladder, softened with some water, to the extreme end of the neck of the retort A (Fig. 3); in order, however, that I may properly prevent the escape of the air it is necessary to scratch the neck of the retort somewhat at this place with a flint. (Retorts which I employ for investigations of this kind I have blown not larger than to be capable of holding only from one half to three ounces of water, but which have at the same time a neck which is about half an ell long, and that for this reason that the attached bladder may not be destroyed during the operation by the heat of the furnace or by the hot vapours.) Into this bladder I pour some milk of lime (§ 22), and press the air out as fully as possible. This lime will absorb the aerial acid during the distillation, and leave the phlogisticated acid of nitre untouched. (*c.*) In exactly the same way as is described in *a* I also collect aerial acid and the inflammable air of sulphur (of which I shall speak further on). But if the bladders are moist, or even if only the air surrounding them is so, both these kinds of air penetrate completely through the bladders in a few days; if the bladders and air are dry, however, this does not take place. I obtain inflammable air from the metals, as iron or zinc, in exactly the same way, except that I place the bottle in warm sand. This air is still more subtle than the preceding; it penetrates through the fine pores of the bladder in a few days, although air and bladder are dry.

I frequently experienced this to my vexation. (*d.*) I not infrequently catch air in bladders, without any bottles. I place in a soft bladder (AA, Fig. 4) the material from which I intend to collect the air, for example, chalk; above this chalk I draw the bladder together with twine BB; I then pour above it the acid diluted with water and press out the air as completely as possible; I finally tie up the bladder above at CC. I then untie the twine B, when the acid runs upon the chalk; it immediately drives out the aerial acid, whereupon the bladder must expand. (*e.*) When I require to get an air out of the bladder into a flask, glass, retort, or bottle, I fill such apparatus with water and place in it a tightly fitting cork; I then tie the bladder which contains the air, that is, the opening from C to D (Fig. 4), very firmly over such bottle; I then invert the bottle so that the bladder comes below and the bottle above, whereupon I hold the bottle with the left hand and with the right I withdraw the cork; I hold this cork firmly between both fingers inside the bladder until the water has flowed out of the bottle into the bladder, and the air has mounted out of the bladder into the bottle; I then put in the cork and detach the bladder from the bottle. When I wish to preserve the air for a long time I place the neck of the bottle in a vessel with water. (*f.*) When there is aerial acid in the bladder, or another air which can unite with water, and I wish to unite it with water neatly, I fill a bottle with cold water, and, after it has been attached to the bladder, I permit about the fourth part to run into the bladder; I then push the cork, which, as previously, was firmly held within the bladder, into the bottle again; I then shake the bottle gently, when the air will dissolve in the water. Thereupon I make a small opening by means of the cork, when air passes out of the bladder into the bottle in order to fill up again the space which has become empty, without any water running into the bladder; I then push the cork again into the bottle and shake the water contained in it. I repeat this operation two or three times more, when the water is saturated with this air. (*g.*) When I wish to mix together two kinds of air in a flask or bottle, I permit in the first place just as much water, by measure, to run from the bottle filled with water, into the bladder, as I wish to have of air. I then tie the bottle over with a bladder filled with another kind of air and permit the remaining water to run into the bladder, whereupon I immediately replace the cork in the bottle, as soon as the last of the water has run out. (*h.*) When I wish to have in a bladder an air collected in a bottle, I reverse the operation. That is to say, I fill the bladder with as much water as I wish to have in it of air and tie it up at the top; I then tie this bladder tightly over the top of the bottle and untie the ligature of the bladder, draw the cork out of the bottle and so permit the water to run out of the bladder into the bottle. I then tie up the bladder, which now contains the air out of the bottle, and detach it from the bottle. (*i.*) When I have in a bottle an air mixed with another kind of air which can be absorbed by water or lime, but wish to know how much of each kind is present in the bottle, I tie over it a bladder into which so much milk of lime has been poured that the bottle can be filled with it; I then withdraw the cork and permit the water or milk of lime to run into the bottle. I afterwards invert the bottle and permit the milk of lime to flow again into the bladder; I repeat this running out and in several times. So much air by measure has been absorbed as there now remains behind of milk of lime in the bottle.

These are the methods which I employed in my investigations of air. I admit that they will not particularly please some, because they do not decide with great exactness. They afforded me satisfaction, however, in all my investigations; and people will often split a hair where it is not in the least necessary.

31. Continuation of the Experiment mentioned in § 29 . . .

Anyone might object and say that the air obtained according to § 29 is perhaps nothing else than a dry acid of nitre converted into elastic vapours. But if this opinion had any foundation, this air should not only be corrosive, but should also produce nitre anew with alkalies. This, however, does not occur. Nevertheless, this objection would possess considerable weight were I not able to prove that several substances produce the same air as the acid of nitre does during distillation. But proof of this is not wanting.

I have proved in a treatise on manganese, which is to be found in the Transactions of the Royal Swedish Academy of Sciences for the year 1774, that this mineral is not soluble in any acid unless an inflammable substance be added, which communicates its phlogiston to the manganese, and by this means effects an entrance of the latter into the acids. I have shown in the same place that vitriolic acid, nevertheless, during a strong distillation with powdered manganese, unites with it and makes it soluble in water; and if this manganese is separated again from the vitriolic acid by means of precipitating agents, there are found in it the most distinct traces of the inflammable substance. . . . I had already observed a few years ago, that if in the calcination of manganese with oil of vitriol in an open crucible, some coal dust was driven by the current of air over the surface of this mixture, these fine coals took fire in the same instant with very great brilliancy. I accordingly made the following experiments.

32. First Experiment.—I mixed so much concentrated oil of vitriol with finely powdered manganese that it became a stiff magma. I distilled this mixture from a small retort on the open fire. In place of a receiver I made use of a bladder, empty of air, and, in order that the vapours which might pass over should not attack the bladder, I poured into it some milk of lime (§ 30, letter *b*). As soon as the bottom of the retort became red hot, an air passed over which gradually expanded the bladder. This air had all the properties of a pure fire-air.

33. **Second Experiment.**—When I distilled two parts of finely pulverised manganese with one part of the phosphorous acid of urine in the same way as is indicated in the preceding paragraph, I likewise obtained fire-air.

34. **Third Experiment.**—(*a.*) I dissolved in *aqua fortis* the white magnesia employed in medicine; I evaporated this solution to dryness. I then placed the salt in a small retort for distillation, as is described in § 32. Even before the retort was red hot the acid of nitre separated from the magnesia, and that in blood-red vapours; and at the same moment the bladder began to expand. The air thus obtained was my fire-air.

It is thus seen constantly that the acid of nitre goes off again blood-red when separated by means of heat from the metals which had been dissolved in this menstruum.

(*b.*) I distilled mercurial nitre in the foregoing manner until the acid of nitre had separated from the residual red precipitate. In this case also I obtained our fire-air. . . . Whence comes the boiling of nitre, fused in a crucible and obscurely red-hot? Neither smoke nor vapours are seen to rise from it, and yet coal dust flying above the open crucible takes fire, burning brilliantly. Whence comes it that such nitre maintained in red-hot fusion in a glass retort for half an hour, becomes moist in open air and deliquesces after cooling, and still does not show any trace of alkali? (§ 27, letter *c*.) What is the reason that this liquefied nitre permits its volatile acid to escape immediately, when rubbed or mixed with the vegetable acids? . . . If the chemists of the preceding century had thought worthy of a more particular examination, the elastic fluids resembling air which manifest themselves in so many operations, how advanced should we now be! They desired to see everything in corporeal form, and to collect everything as drops in the receiver. This is now for the first time better inquired into, and the air has begun to be carefully examined; and who is there who does not perceive the advantage which the results of such experiments carry with them?

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35. **Fourth Experiment.**—I put an ounce of purified nitre into a glass retort for distillation and made use of a bladder, moistened and emptied of air, in place of a receiver (Fig. 3). As soon as the nitre began to glow it also began to boil, and at the same time the bladder was expanded by the air that passed over. I proceeded with the distillation until the boiling in the retort ceased, and the nitre was about to force its way through the softened retort. I obtained in the bladder the pure fire-air which occupied the space of 50 ounces of water. This is the cheapest and best method of obtaining fire-air.

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38. **Fifth Experiment.**—I took a silver solution prepared with acid of nitre, and precipitated it with alkali of tartar; I washed the precipitate thus obtained and dried it. I then placed this calx of silver in a small glass retort on the open fire for reduction, and fastened an empty bladder to the neck. The bladder was immediately expanded by the air which passed over. After the end of the distillation I found the calx of silver half melted together in the retort, with its metallic lustre; however,

as I had effected the precipitation with alkali of tartar, and this is always united with a quantity of aerial acid which attaches itself to the calx of silver in the precipitation, so this acid was necessarily present also in the bladder. This acid was removed from it by milk of lime (§ 30, letter *i*), and there remained behind one-half of pure fire-air.

39. **Sixth Experiment.**—I precipitated with alkali of tartar a solution of gold which was made with *aqua regia;* I reduced in the foregoing manner the washed and dried calx of gold. I obtained in this case the same fire-air, except that no aerial acid accompanied it. This is not to be wondered at, because the saturated solution of gold effervesces with the alkali, which does not take place with the solution of silver.

40. **Seventh Experiment.**—It is likewise known that the red precipitate of mercury regains its flowing condition without the addition of an inflammable substance. Since mercury, however, really loses its phlogiston as well by means of vitriolic acid as of the acid of nitre, it must necessarily assume this again as soon as it recovers its metallic property.

(*a.*) I added a solution of alkali of tartar, drop by drop, to a solution of corrosive sublimate. I washed the brown-red precipitate obtained, and dried it; then I placed it, for reduction, upon the open fire in a small retort, which was provided with a bladder empty of air. As soon as the calx began to glow, the bladder became expanded, and quicksilver rose into the neck. The fire-air obtained had some aerial acid mixed with it.

(*b.*) Mercury converted into calx by the acid of nitre, or red precipitate, treated in the same way behaved similarly. In this case I obtained a pure fire-air, without any aerial acid in it.

41. **Eighth Experiment.**—I have proved, in a

treatise on arsenic communicated to the Royal Swedish Academy of Sciences, that this poisonous substance is compounded of a peculiar acid and an inflammable substance. I also shewed in the same treatise how this acid can be sublimed into ordinary arsenic simply by continued heat; and although I clearly perceived the reason for this, even at that time, still I was unwilling to mention it there in order to avoid prolixity. I placed some of this fixed acid of arsenic in a small retort with a bladder attached, for distillation. When the acid had gone into fusion, and glowed brightly, it began to boil; during this ebullition arsenic rose into the neck and the bladder became expanded; I continued with this heat as long as the retort would hold out. The air collected was likewise fire-air. In the same treatise I made mention of a peculiar explosion which took place in the distillation of zinc with the acid of arsenic. How clear, how manifest does the explanation of this phenomenon not become when one is satisfied that in this case fire-air is present in the retort in its greatest purity, and the zinc is in red hot fusion? What more is necessary for its ignition?

I have very often regarded with pleasure the brightly glowing sparks which are produced in a retort by heat alone, during the reduction of metallic calces, when only a very little coal dust is mixed along with it.

We shall now see whether this fire-air is not the same air which had been lost without fire (§§ 8-15), and with fire (§§ 17-23).

42. **First Experiment.**—I filled a bottle which was capable of holding 16 ounces of water with pure fire-air according to the method which is described in § 30, letter *e*. I placed the bottle, inverted, in a glass which was filled with a solution of liver of sulphur. The solution rose a little into the bottle hour by hour, and after the lapse of 2 days the bottle was filled with it.

43. **Second Experiment.**—I mixed in a bottle 14 parts of that air from which the fire-air had been removed by liver of sulphur (§ 8), and which I have called vitiated air (§ 29), with 4 parts of our fire-air, and placed the bottle, inverted and open, in a vessel which was also filled with a solution of liver of sulphur. After 14 days the 4 parts of fire-air were lost, and the solution had risen into their place.

44. **Third Experiment.**—After I had filled a bottle with our air, I poured some colourless animal oil into it and closed it tightly. After a few hours it had already become brown, and by the next day black. It is no small inconvenience to preserve this oil white in apothecaries' shops. It is found necessary to pour this oil into small phials, and to preserve it most carefully from the access of air. When such a colourless oil is mixed with any acid, the acid, as well as the oil, becomes black even in an hour, although it has been diluted with water. Even vinegar has the same effect. There is no other reason, therefore, why the oil becomes at once black in the air, than that the fire-air present in the air deprives it of its phlogiston, and thereby develops a subtle acid, previously united with this phlogiston, which produces the blackness.

45. **Fourth Experiment.**—(*a.*) Into a bottle of 7 ounces, which was filled with fire-air, I put a piece of phosphorus from urine and closed it with a cork. I then heated, by means of a burning candle, the place where the phosphorus lay; the phosphorus took fire with very great brilliancy. As soon as the flame had gone out, the bottle broke into fragments.

(*b.*) As the bottle in the foregoing experiment was very thin, I repeated it with a somewhat thicker bottle, and after everything had become cold I wanted to take the cork out of the bottle under water. It was not possible for me to do this, however, so tightly did the external air press the cork into the bottle. Accordingly I forced it inside the bottle; thereupon water entered the bottle and filled it almost completely. Since the first bottle was only very thin, the reason that it was crushed must be ascribed to the external air.

(*c.*) When I mixed vitiated air with one third of fire-air, and burned a piece of phosphorus in the mixture, only $\frac{1}{3}$ of it was absorbed.

46. **Fifth Experiment.**—I also repeated the same experiment which is described in § 19, only with this difference that I took the tube longer, and filled the flask with my fire-air. It was pleasing to observe how the water rose gradually into the flask; and how the flame went out when $\frac{7}{8}$ of the flask were full of water.

47. **Sixth Experiment.**—I laid some glowing coals upon the stand (§ 21, letter *c*), and placed over them a flask which was filled with fire-air. The coals had not even reached the air in the flask before they began to burn very brilliantly.

After everything had become cold, I made an aperture under the flask, whereupon the fourth part became filled with water. But when I removed, by means of milk of lime, the aerial acid which was present in the residual air (§ 22) there remained in the flask only the fourth part. In this air a candle could still burn.

48. **Seventh Experiment.**—I also examined the behaviour of fire-air with sulphur (§ 23). As soon as the burning sulphur came into contact with the fire-air contained in the flask, the flame became much larger and brighter. When this fire had gone out, the water in the dish had found a way to come through the mass into the flask, which became $\frac{3}{4}$ filled with it. As I employed for these last 3 experiments a flask which was only of 30 ounces measure, I was obliged to arrange the stand (§ 21) to suit.

49. I have mentioned (§ 16) that I found vitiated air lighter than ordinary air. Must it not follow from this that the fire-air is heavier than our air? As a matter of fact, I actually found, when I accurately weighed as much fire-air as occupied the space of 20 ounces of water, that this was almost 2 grains heavier than the same bulk of common air.

50. These experiments shew, therefore, that this fire-air is just that air by means of which fire burns in common air; only it is there mixed with a kind of air which seems to possess no attraction at all for the inflammable substance, and this it is which places some hindrance in the way of the otherwise rapid and violent inflammation. And in fact, if air consisted of nothing but fire-air, water would surely render small service in extinguishing outbreaks of fire. Aerial acid mixed with this fire-air, has the same effect as vitiated air. I mixed one part of fire-air with 4 parts of aerial acid; in this mixture a candle still burned moderately well. The heat which lurks in the small interstices of the inflammable substance cannot possibly make up so much heat as is felt in fire; and I think I am not mistaken when I conclude from my experiments that the heat is really brought forth and produced in the first place from fire-air and the phlogiston of the inflammable substance. . . .

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80. I had long wished to have some of the precipitate of mercury *per se*, in order to see whether it also would yield fire-air during reduction by means of heat alone. At length I obtained some from my much esteemed friend Doctor Gahn. This so-called precipitate had the appearance of small dark-red crystals resembling cinnabar. Now, as I know that mercury cannot be dissolved in muriatic acid unless it has lost its phlogiston, which takes place during its solution in acid of nitre or in vitriolic

acid; and as this is also the reason why nitre must be present in a mixture of calcined vitriol, common salt, and quicksilver, I therefore poured muriatic acid upon a part of this red precipitate; the solution was soon formed and was somewhat hot; I evaporated it to dryness and increased the heat. Everything sublimed, and a true corrosive sublimate was formed. Hence this precipitate, produced by heat alone, is a calcined mercury. I then placed the other part of this precipitate over the fire in a small glass retort to which I had fastened an empty bladder. As soon as the retort became red-hot the bladder became expanded, and at the same time the reduced mercury rose into the neck. In this case no red sublimate arose as customarily takes place with that calx which is prepared by the acid of nitre. The air obtained was a pure fire-air. This is a remarkable circumstance, that the fire-air which had previously removed from the mercury its phlogiston in a slow calcination, gives this same phlogiston up to it again when the calx is simply made red-hot. Still we have several such phenomena, where heat similarly alters the attractive forces between substances.

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83. Air is a Dulcified Elastic Acid.

In the foregoing experiments I have demonstrated the two proximate constituents of common air, because it was not necessary to know anything more about it for a clear knowledge of fire. I shall now go further, and see whether a still deeper decompounding of air is possible.

First Experiment.—I placed a rat in a flask capable of holding 4 quarts of water; I gave it some bread softened in milk and closed the flask with a wet bladder. It died 31 hours afterwards. I then held the flask, inverted, under water and made a hole in the bladder, when two ounces of water rose into it. This small diminution of the air was probably caused by the heat which the rat took with it, which had previously driven the air out.

84. **Second Experiment.**—I took a large soft bladder and fastened a tube into its opening; then I filled it with the air out of my lungs, and held the tube and bladder with my right hand and closed my nostrils with the left. I respired the air as long as I could, and was able to make 24 inspirations (regarding which it is to be observed that at the last I was obliged to draw the whole bladder full of air into my lungs at once, while at the beginning only the half of it was necessary). I then closed the tube with my finger, and tied up the bladder. This air had properties similar to the preceding in which the rat died. That is to say, it contained one-thirtieth part of aerial acid, which I separated from it by milk of lime; and a burning candle at once went out in it.

85. **Third Experiment.**—I placed a few flies in a bottle into which I had put some honey smeared upon paper. After a few days they had died. They likewise had not absorbed any air; milk of lime, however, diminished this air about one fourth part, and the remainder extinguished fire.

I then took a bottle of 20 ounces measure and bored a hole in the bottom of it with the corner of a broken file (Fig. 5, A). Into this bottle I put a small piece of unslaked lime, and closed the mouth with a cork through which I had previously fixed a tube B. Round about this cork I placed a ring of pitch, and placed over it an inverted glass C, into which I had previously put a large bee and had given it some honey which was smeared upon paper; but in order that no air could penetrate within the ring of pitch, I pressed the glass firmly in; I afterwards placed the bottle in the dish D, into which I poured so much water that it was half immersed in it; as soon as I observed that the bottle was raised by the water, I put a small weight upon the glass. The water rose a little into the bottle every day through the opening A; and I also shook the bottle a little sometimes in order that the skin which formed over the milk of lime might break. After the lapse of seven days the water had risen to E, and the bee was dead. Occasionally I put 2 bees into the glass C, when just as much air was converted into aerial acid in half the time. Caterpillars and butterflies behaved in exactly the same way.

86. **Fourth Experiment.**—I placed some peas in a small flask, which was capable of holding 24 ounces of water, and poured so much water upon them that they were half covered with it; I then closed the flask. The peas began to strike roots, and grew up. As I found after 14 days that they would not increase further, I opened the flask, inverted, under water, and found the air neither increased nor diminished. The fourth part, however, was absorbed by milk of lime, and the remaining air extinguished flame. I kept fresh roots, fruits, herbs, flowers, and leaves, each by itself, in the flask, and after a few days I likewise observed the fourth part of the air converted into aerial acid. If flies are placed in such air they die immediately.

87. These are accordingly strange circumstances, that the air is not noticeably absorbed by animals endowed with lungs, contains in it very little aerial acid, and yet extinguishes fire. On the other hand insects and plants alter the air in exactly the same way, but still they convert the fourth part of it into aerial acid. Accordingly I was curious to know whether the fire-air was not that which was here converted into aerial acid, because in these latter experiments just as much of the air was converted into aerial acid as there was of fire-air present in it.

88. **Fifth Experiment.**—In a bottle of 20 ounces capacity, I mixed one part of fire-air with 3 parts of the preceding air in which peas would not any longer grow, and from which the aerial acid was separated. (That is to say, I filled the bottle with water, and placed 4 peas in it; I then allowed one fourth of the water to run into the bladder in which fire-air was contained, and the remainder into another bladder in which this vitiated air was contained (§ 30, *g*), while I took care that the peas did not fall into the bladder. I also left so much water behind, that the peas were half covered with it.) Here also I observed the peas growing up, and after they would not increase any more I found this air likewise not absorbed, but almost the fourth part was absorbed by milk of lime. Hence it is the fire-air which is here converted into aerial acid. In 3 parts of aerial acid and one part of fire-air peas do not grow. I mixed vitiated air (§ 29) with fire-air, which behaved in just the same way; that is to say the fire-air was converted into aerial acid.

89. **Sixth Experiment.**—I mixed, in the same proportions, fire-air and air vitiated by peas, and filled a bladder with it. Then when I had completely exhaled the air present in my lungs, I respired this newly compounded air as many times as possible. I then found that it contained very little aerial acid in it, and when this was separated from it, it extinguished fire. I believe that one must ascribe to the blood present in the pulmonary veins, the effect which animals endowed with lungs have upon the air. The following experiment gives me cause for this.

It is known that freshly drawn blood, when it stands in the open air, assumes a fine red on the surface, and that the under portions likewise become red when they come into contact with the air. Does the air in this case undergo any alteration? I filled a flask one third part with freshly drawn ox blood, closed it tightly with a bladder, and shook up the blood frequently. Eight hours afterwards I neither found aerial acid in this air, nor that its bulk was diminished; but the flame of a candle was immediately extinguished in it. I made this experiment in winter time, from which may be gathered that the effect cannot be ascribed to any putrefaction, for this blood was found still fresh 6 days afterwards, and besides, all putrefactions produce aerial acid. I was now curious to know how fire-air by itself would behave with animals and plants.

90. **Seventh Experiment.**—(*a.*) I put 2 ounces of nitre into a small glass retort upon glowing coals, and attached a large bladder softened with water (§ 35), and allowed the nitre to boil until I had received $\frac{3}{4}$ of a quart of fire-air in the bladder. I then tied up the bladder and separated it from the retort; I then placed a tube in its opening, and after I had completely emptied my lungs, I began to respire air from this bladder (§ 84). This proceeded very well, and I was able to make 40 inspirations before it became difficult for me; eventually I expelled the air again from my lungs as completely as possible. It did not seem to have diminished particularly, and when I filled a bottle with it and introduced a burning candle, this still burned. I then began to respire this air anew, and was able to make 16 more inspirations. It now extinguished the flame, but I found only some traces of aerial acid in it. (*b.*) I was surprised that I was not able the first time to take away from this air the property of allowing fire to burn in it; I thought that perhaps the great humidity prevented me from drawing this air into my lungs so often as was really possible. Accordingly I repeated the same experiment, only with this difference, that I put a handful of potashes into the bladder before the fire-air was driven into it. I then

began to draw this air into my lungs, and counted 65 inspirations before I was compelled to desist. But when I lowered a burning candle into this air, it still burned well, although only for a few seconds.

91. **Eighth Experiment.**—I closed the hole in the bottle at A (Fig. 5) with a cork, as also the tube B, and then filled the bottle with fire-air (§ 30, *e*). Then I had at hand the glass C, in which I had placed 2 large bees, and had provided some honey for their stay. I opened the stopped-up tube, placed this glass over it as quickly as possible, and pressed it into the ring of pitch. I afterwards placed the whole in the dish D, which I had filled with milk of lime, and withdrew the cork at A. In this case I observed the milk of lime to rise a little into the bottle every day, and after 8 days had elapsed the bottle was almost completely filled with it, and the bees were dead.

92. **Ninth Experiment.**—Plants, however, will not grow noticeably in pure fire-air. I filled with this air a bottle capable of holding 16 ounces of water, and which contained 4 peas (§ 88). They got roots, but did not grow up at all; with milk of lime the twelfth part was absorbed. I then filled this air into another bottle which also contained 4 peas. After 14 days they had got roots, but also did not grow up, and with milk of lime likewise only the twelfth part was absorbed. I repeated this experiment 3 times more with the same air, and it was observed that the fourth and fifth times the peas had grown upwards a little. There still remained one-half of the whole air, and in this fire could still burn. There is no doubt that the whole quantity of fire-air could have been converted into aerial acid if I had continued the operation longer. It may also be observed that the peas act more strongly upon the fire-air when they send out roots than subsequently.

93. Hence it is the fire-air by means of which the circulation of the blood and of the juices in animals and plants is so fully maintained. Still it is a peculiar circumstance that blood and the lungs have not such action upon fire-air as insects and plants have, for the latter convert it into aerial acid, and the former into vitiated air (§§ 29, 89, 90). It is not so easy to furnish the reason for this, yet I will risk it. It is known that the acids lose those properties by which they reveal themselves as acids, by the addition of the inflammable substance, as sulphur, the elastic acid of nitre, regulus of arsenic, sugar, and the like, plainly shew. I am inclined to believe that fire-air consists of a subtle acid substance united with phlogiston, and it is probable that all acids derive their origin from fire-air. Now, if this air penetrates into plants, these must attract the phlogiston, and consequently the acid, which manifests itself as aerial acid, must be produced. This they again give up. The objection that so great a quantity of aerial acid is nevertheless obtained in the destruction of plants, and that, consequently, these must attract the aerial acid, has no weight, since otherwise the air in my vessels in which the peas were contained must have become for the most part lost, which, however, did not take place. . . . If plants abstract the phlogiston from the air, the aerial acid must be lighter. But experiment shows me the opposite; I found it, after careful weighing, somewhat heavier, but this is not contrary to my opinion; as it is known that all acids retain water strongly, the aerial acid must possess the same property, and this may consequently cause the most of the weight. If all this is accurate, another question then arises: Why do not blood and the lungs likewise convert fire-air into such an aerial acid? I take the liberty here also of giving my opinion of this, for how would all these laboriously executed experiments help me if I had not

the hope of coming by means of them nearer to my ultimate object, the truth? Phlogiston, which makes most substances with which it unites liquid as well as mobile and elastic, must have the same effect upon blood. The globules of blood must attract it from the air through the small pores of the lungs. By this union they become separated from one another, and are consequently made more liquid. They then appear bright red (§ 89). They must, however, give this attracted phlogiston up again during the circulation, and in consequence, be placed in a condition to absorb the inflammable substance anew from the air at that place where they are in the most intimate contact with it, that is, in the lungs. Where this phlogiston has gone to during the circulation of the blood, I leave to others to ascertain. The attraction which the blood has for phlogiston cannot be so strong as that with which plants and insects attract it from the air, and then the blood cannot convert air into aerial acid; still it becomes converted into an air which lies midway between fire-air and aerial acid, that is, a vitiated air; for it unites neither with lime nor with water after the manner of fire-air, and it extinguishes fire, after that of aerial acid. But that the blood really attracts the inflammable substance I have an additional experiment to prove, since I have removed phlogiston by help of my lungs from inflammable air, and have converted this into vitiated air.

I filled a bladder with the air which one obtains from iron filings and vitriolic acid (§ 30, *c*), and respired it in the manner previously described (§ 84). I was only able to inhale it 20 times, and after I had somewhat recovered, I expelled the air once more from my lungs as completely as possible, and again inhaled this inflammable air; after 10 inhalations I was compelled to desist from it, and observed that it could no longer be kindled, and also would not unite with lime water. In one word it was a vitiated air.

I kept a piece of sulphur in continuous ebullition over the fire in a retort, capable of holding 12 ounces of water, with an empty bladder attached in place of a receiver, the retort also placed so that the sulphur which rose into the neck could run back again. After all had become cold, I found the air neither increased nor diminished; it smelt slightly hepatic, and extinguished a burning candle. I shall prove further on that sulphur can unite with more phlogiston; and it seems to me to follow from this experiment that something inflammable from the air had deposited itself upon the sulphur, and that the air had thereby acquired the property of a vitiated air. It is, however, also remarkable that other bodies which attract the inflammable substance more strongly, as for example, the fuming acid of nitre, do not abstract it from the air. It is likewise strange that I was able to inhale the inflammable air into my lungs only 20 times; and I observe here as something peculiar that, if I mistake not, I became very warm a quarter of an hour afterwards. It is also to be observed that fire-air, vitiated by the lungs, extinguishes fire; why does not the aerial acid attract the phlogiston again? why not also the vitiated air? Mr. Priestley indeed has accomplished this, but it did not succed with me however much I also wished it. He has converted aerial acid into wholesome air by means of a mixture of iron filings, sulphur, and some water. When I desired to repeat this experiment, the aerial acid was always absorbed by the iron filings. I likewise powdered finely some iron filings which had been fused together with excess of sulphur, moistened this with water, and preserved it in a bottle which was filled with aerial acid: but with the same result. After 2 two days the aerial acid was almost entirely absorbed. This philosopher also says that he has made vitiated air wholesome again by agitation with water. I must admit, however, that with me this likewise failed. I filled a flask one fourth part with vitiated air, and the remainder with fresh water; I closed the flask very tightly, and shook it up and down for almost a whole hour. Then when I collected this air in a bladder, and from this in a bottle, I found that the candle was extinguished afterwards as it was before. He mixed with water, by agitation, the inflammable air from metals; this also would not succeed with me, although I used only little inflammable air, and much water. He also observed that plants made vitiated air wholesome again. It follows from my experiments that they vitiate air. I kept plants, in the dark as well as exposed to sun-light, in a flask which was filled with vitiated air and carefully secured (which careful securing must really be attended to). I tested a little of this air every 2 days, and always found it vitiated.

94. Water has the peculiar property of separating the proximate constituents of air; of uniting with fire-air; and of entering into no kind of union with vitiated air. (1.) I filled a large bottle with boiled water which had been cooled shortly before, and permitted the tenth part to run out. I then placed the bottle, inverted and open, in a vessel with water. I observed the quantity of air to diminish a little every day, and when this diminution ceased, I collected the remaining air first in a bladder (§ 30, *h*), and from the bladder in a bottle (§ 30, *e*), and brought a burning candle into the bottle; it had scarcely reached the mouth when it went out. (2.) I then took the same kind of water freed from air, filled a bottle with it, and permitted the tenth part of it to run into a bladder filled with vitiated air. I next placed the bottle, inverted, in a vessel with water, and observed the space which the air occupied in it. I found, 14 days afterwards, that the

water had not absorbed the smallest quantity of it. (3.) I placed a large bottle, from which the bottom was knocked out, in a deep kettle with water, so that the water outside reached above the top of the bottle. I then tied a bladder, empty of air, over the top of the bottle, and made the water boil up once over the fire. The air which was in that portion of the water contained under the bottle rose into the bladder; and after I had tied up the bladder, and detached it from the bottle, I filled a phial with it, and put a small burning candle into it; it burned there more brightly than in ordinary air.

This fire-air, dissolved in water, must be as indispensable for aquatic animals as for those which live upon the earth. They must draw it into their bodies, and convert it either into aerial acid or into vitiated air. Into whichever kind it is, however, it must always become separated from the water again, for as aerial acid it does not remain with the water in the open air, and vitiated air cannot unite with water at all (No. 2), the water is then in a condition again to absorb fire-air anew, and to convey it to the animals. My experiments made with respect to this matter agree with this entirely. I allowed a few leeches to remain in a bottle, which was half filled with water and well closed, until they died. I then examined the air standing over this water. It had no smell, nor had the water; it appeared to have increased a little and it extinguished fire. It seems that these creatures live only upon the phlogiston in fire-air, perhaps also upon the heat. I have preserved them alive in water, and that the same water, for two years; the bottle was only tied over with gauze. I have a convenient method to ascertain whether fire-air is present in water or not. I take, for example, an ounce of it, and add to it about 4 drops of a solution of vitriol of iron, and 2 drops of a solution of alkali of tartar which has been somewhat diluted with water. A dark green precipitate is immediately formed, which, however, becomes yellow in a couple of minutes if the water contains fire-air; but if the water has been boiled, and has become cold without access of air, or if it is even a recently distilled water, the precipitate retains its green colour, and does not become yellow sooner than an hour afterwards, and not yellow at all if it is protected from access of air in full bottles. I have already shown (§ 15) that the green precipitate of iron owes its colour to phlogiston which still adheres to the earth, and it follows from this that fire-air, although not in the elastic condition, is able to attract phlogiston. The following experiment likewise shewed me that aquatic animals take fire-air from the water. I placed a leech in a bottle which was completely filled with water, and was protected from every kind of air. After two days it was almost dead. I then examined the water in the manner described above, and found that the earth of iron retained its green colour. The swelling up of peas in cold water is to be ascribed mainly to the fire-air present in the water. If a bottle is filled full of water and a few peas are placed in it, after 24 hours the water contains aerial acid it is true, but no fire-air. In water boiled and become cold, peas swell up only a little. I perceive in this the reason why the waters distilled from plants not only lose their smell, but why also a mucilaginous substance settles to the bottom, when the bottles are frequently opened, whereas the same waters, in perfectly full bottles, retain their smell and clearness unchanged. All plants communicate to water some mucilaginous material which is carried over along with it. Fire-air is the chief cause of this corruption; if this enters the water again, it attracts to itself the inflammable substance from the subtle oily and mucilaginous matter, and alters the whole of the water.

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