

XXXVIII. *An Account of further Discoveries in Air.* By
the Rev. Joseph Priestley, LL.D. F. R. S. in Letters to
Sir John Pringle, Bart. P. R. S. and the Rev. Dr.
Price, F. R. S.

L E T T E R I.

TO SIR JOHN PRINGLE, BART. P. R. S.

DEAR SIR,

March 15, 1775.

Redde, May 25,
1775. **H**AVING been pretty fortunate in the
profecution of my experiments on dif-
ferent kinds of air, since the publication of my treatise
on that subject, I think it due to the attention with which
you have from the first honoured them, to give you some
account of what I have lately done. I know that every
new discovery, in any branch of natural knowledge,
gives you pleasure; and it is peculiarly flattering to me,
that you consider some of those, which I have been happy
enough to make, in a light of some importance. As I
have materials enough for another separate publication, I
shall not trouble the Society with a particular account of
my observations; but if you think proper to communicate
to them the following very general account, as a mark
of my respect for the Society, as well as for yourself, you
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will add to the many obligations you have conferred upon me.

To the marine acid air, which I had discovered at the time of my former publication, I have now added three more; *viz.* the vitriolic, the nitrous, and the vegetable. The vitriolic acid air is produced by boiling in oil of vitriol any inflammable matter, or almost any thing that contains *phlogiston*; as oil, camphor, spirit of wine, charcoal, and most of the metals. For though this acid seems to have no affinity with some of these substances when it is cold, it affects them considerably, and particularly takes *phlogiston* from them, when it is hot; and by means of the *phlogiston*, of which it deprives them, it is early made volatile, so as to assume the form of a transparent air, like that of the marine acid; being as readily imbibed by water, and as readily forming a white cloud upon the admission of alkaline air. But the affinities of the vitriolic acid air with various substances, and many of the phenomena attending it, are strikingly different from those of the marine acid air. I thought it a little singular, that the solution of iron, zinc, and tin, in a diluted vitriolic acid should yield inflammable air; and that when boiled in the same acid concentrated, they should chiefly yield acid air, which is not at all inflammable, and cannot be confined by water. This, however, is in fact the produce of the process; and the very same as when copper, silver, or quicksilver is boiled in the same acid. From gold, platina, or lead, I was not able to procure any air at all by this means.

The vegetable acid air is as easily procured from the concentrated vegetable acid, as the marine acid air is from spirit of salt; and, I think, in greater quantity. This air also is perfectly transparent, is instantly imbibed by water, and makes a white cloud upon the admission of alkaline air; though several of its properties are exceedingly different from those of the marine or vitriolic acid airs.

The nitrous acid I have exhibited in the form of air, though only, as it were, for a moment; since no fluid, that I am acquainted with, is capable of confining it. The more I consider the nitrous acid, the more wonderful and inexhaustible the subject appears. The kinds of air which it forms, according to its various combinations with *phlogiston*, are, I believe, more numerous than all the kinds that can be formed by the other acids. Many of the phenomena which have lately occurred to my observation relating to it are, to me, altogether inexplicable; though I perceive certain analogies among some of them. Upon this subject I shall have a pretty long chapter. But, to avoid being tedious at present, I shall only observe, that by boiling various hard substances containing *phlogiston*, and especially charcoal, in the nitrous acid, I get genuine nitrous air, the very same that I get from the solution of various metals in that acid. At the time of my last publication I had not a large burning lens; and as the focus of the mirror cannot be thrown upon any thing in the form of a powder, or that requires a solid support, my experiments with the solar rays were exceedingly incomplete. I have now procured one of
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twelve inches in diameter; and the use of it has more than answered my highest expectations. The manner in which I have used it, has been to throw the focus upon the several substances I wished to examine, either *in vacuo*, or when confined by quicksilver, in vessels filled with that fluid, and standing with their mouths immersed in it. I presently found that different substances yield very different kinds of air by this treatment; and though the reasons, or analogies, of the different products, in many of the cases, be sufficiently obvious, and such as I had conjectured *à priori*, yet in other cases I am not a little puzzled and surprized. Various metals yield inflammable air by this process; several saline substances yield fixed air; many metallic *calces* yield the same, and some of them a *phlogisticated* common air; and some of the precipitates, in which the nitrous acid was employed, yield nitrous air, in one or other of its forms. But the most remarkable of all the kinds of air that I have produced by this process is, one that is five or six times better than common air, for the purpose of respiration, inflammation, and, I believe, every other use of common atmospherical air. As I think I have sufficiently proved, that the fitness of air for respiration depends upon its capacity to receive the *phlogiston* exhaled from the lungs, this species may not improperly be called, *dephlogisticated air*. This species of air I first produced from *mercurius calcinatus per se*, then from the red precipitate of mercury, and now from red lead. The two former of the substances yield it pure;

pure; but the red lead I have generally met with yields a greater proportion of fixed air along with it. Another quantity, however, gave this air and hardly any thing else. On what this difference depends I cannot tell; but hope to be able to investigate. That this air is of that exalted nature, I first found by means of nitrous air, which I constantly apply as a test of the fitness of any kind of air for respiration, and which I believe to be a most accurate and infallible test for that purpose. Applying this test, I found, to my great surprize, that a quantity of this air required about five times as much nitrous air to saturate it, as common air requires. Common air is diminished about one-fifth, by a mixture of one-half nitrous air; but one quantity of this air was diminished one-half, and another two-thirds, by the addition of twice as much nitrous air; and three times the quantity, left it little more than it was at the first. A candle burned in this air with an amazing strength of flame; and a bit of red hot wood crackled and burned with a prodigious rapidity, exhibiting an appearance something like that of iron glowing with a white heat, and throwing out sparks in all directions. But to complete the proof of the superior quality of this air, I introduced a mouse into it; and in a quantity in which, had it been in common air, it would have died in about a quarter of an hour, it lived, at two different times, a whole hour, and was taken out quite vigorous; and the remaining air appeared to be still, by the test of nitrous air, as good as common air. This experiment I also repeated,

peated, and with nearly the same success, with another mouse, and another quantity of this air, the virtue of which had been impaired. Examining all the degrees of the calcination of lead, I found nothing but fixed air, or a little *phlogisticated* common air, till I came to *masticot*, which is a state that precedes the red lead. This gave air about twice as good as common air, and the litharge, which follows the red lead, gave fixed again. Roman vitriol and sedative salt yielded air which was, as nearly as possible, of the same degree of purity with common air. My conjectures concerning the cause of these appearances are as yet too crude to lay before the Society. My present ideas of the last mentioned facts are, that, together with other observations which I shall lay before the publick, they afford some foundation for supposing, that the nitrous acid is the basis of common air, and that nitre is formed by a decomposition of the atmosphere. But it is possible I may think otherwise to-morrow. It is happy, when with a fertility of invention sufficient to raise *hypotheses*, a person is not apt to acquire too great attachment to them. By this means they lead to the discovery of new facts, and from a sufficient number of these the true theory of nature will easily result.

I have made many other original experiments of a miscellaneous nature; but I shall not take up your time with the mention of them in this place. If this imperfect communication gives you, or the Society, any satisfaction, I shall be very happy, and shall be encouraged to prosecute

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profecute these inquiries as much as my leifure from other purfuits will admit. I am, &c.

L E T T E R II.

TO THE REV. DR. PRICE, F. R. S.

DEAR SIR,

April 1, 1775.

AS you are pleased to interest yourself in my experiments, I hope it will give you some pleasure to be informed, that, I think, I never was more successful than I have been in the few days that I have been able to attend to these matters, since my return into the country. By the heat of the flame of a candle, and catching the air that arises in the manner described in fig. VIII. pl. 2. in my late Treatise, I get the pure air I discovered in London in great plenty, from a variety of cheap materials; not only from red lead, but many earthy substances moistened with spirit of nitre and dried, as chalk and quick-lime; which demonstrates that red lead, *mercurius calcinatus per se*, &c. extract the nitrous acid from the air; and that this acid is the most essential among the various ingredients which compose the atmosphere. From tobacco-pipe-clay, and some other things, moistened with spirit of nitre, I get fixed air; which seems to prove that this species of air (which is a kind of acid) is a modification of the nitrous acid, and in some measure accounts for the
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existence of so much fixed air in the atmosphere. I believe this experiment is the first instance of the proper generation of fixed air from other principles. What we have got of it hitherto has been by dislodging it from substances, that were supposed to contain it. Notwithstanding red lead yields so pure an air, paint made with it diminishes common air, and makes it noxious, as much as white paint; which seems to prove, that it is the oil, that yields the *phlogiston*, which injures the air to which it is exposed.

L E T T E R III.

TO SIR JOHN PRINGLE, BART. P. R. S.

DEAR SIR,

London, May 25, 1775.

AS I am desirous to present to the Royal Society a general review of my late observations on air, without troubling them with a detail of my experiments, I beg you would lay before them the following particulars, in addition to those contained in the letter which I took the liberty to write to you, dated March 15, and in the extract from that to Dr. PRICE, dated April 1, 1775; submitting the whole to the disposal of the Society.

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I have found, that the earths of all denominations, even the cryftalline and the talcky, which are thought to be infoluble in acids, yield a pure *dephlogifticated air*, when treated in the manner mentioned in my former letters; but that the calcareous earths, and fome of the earths of metals, as red lead and the flowers of zinc, yield it in the greateft plenty. Upon the whole, I think, it may fafely be concluded, that the pureft air is that which contains the leaft *phlogifton*: that air is impure (by which I mean that it is unfit for refpiration, and for the purpose of fupporting flame) in proportion as it contains more of that principle; and that there is a regular gradation from *dephlogifticated air*, through common air, and *phlogifticated air*, down to nitrous air; the laft fpecies of air containing the moft, and the firft-mentioned the leaft *phlogifton* poffible, the common basis of them all being the nitrous acid; fo that all thefe kinds of air differ chiefly in the quantity of *phlogifton* they contain; though with refpect to nitrous air, there feems to be a farther difference in the mode of combination. By attending to the quantity of *phlogifton* contained in the fubftances with which the fpirit of nitre is mixed, any of thefe kinds of air may be produced at pleafure, and fometimes all the kinds will be produced in the different ftages of the fame procefs. White wood-afhes yield an exceedingly pure air; but the leaft bit of charcoal in the afhes depraves the air; and if there be much charcoal in them, the whole produce will be ftrongly nitrous. The phænomena of detonation (which has been a very
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puzzling appearance in chemistry) admit of a very easy explication by the help of my late experiments. It is generally supposed, that in this case a sulphur is formed, by the union of the nitrous acid and the *phlogiston* of the body with which it is detonated; which sulphur is so inflammable, that it cannot exist a moment without decomposition: and it has been thought, that in the process of making the *chrysis* of nitre, the acid is intirely destroyed or changed. But, in both these cases, I have no doubt, that the acid enters into the composition of some of the kinds of air which are generated upon those occasions. I once mixed a quantity of the ore of lead with spirit of nitre, and when it was dry, put it into a gun-barrel, filled up to the mouth with sand, in order to collect the air that heat would expel from it, in the usual manner. The production of air was very great and rapid; and when the heat became considerable, all the contents of the gun-barrel were exploded with great violence, and a loud report, demolishing the vessel which I had placed to receive the air. The next time, putting the same materials into a glass vessel, and disposing the apparatus in such a manner as that the explosion could not affect the collected air, I found it to be very strongly nitrous. Such, therefore, I conclude to be the produce of the explosion of gun-powder, since charcoal with spirit of nitre yields this kind of air. In the detonation of nitre with substances that contain little *phlogiston*, the acid may form common air, or air much purer than that.

As I mean these letters to contain a general outline of what I have lately observed with respect to air, I shall add, that by the favour of that most intelligent and generous chemist Mr. WOLFE, I have lately procured some of that phosphoric spar, from which a new mineral acid, first discovered in Sweden, is procured. This acid, as well as the marine, vitriolic, and vegetable, I throw into the form of air, confined by quicksilver; and thus have an opportunity of examining its affinities with the greatest ease and certainty. I shall in this place only observe with respect to it, that this acid air decomposes nitre, but not near so rapidly as the marine acid air; and that the salt which is formed by its union with alkaline air is not sensibly soluble in water. I am, &c.

P. S. Upon second thoughts, I am not so well satisfied with my conjecture, hinted in the letter to Dr. PRICE, that fixed air may be a modification of the nitrous acid, though the experiment there mentioned seems to make it probable. But I lay no stress upon any opinions, farther than as they may lead to the discovery of new facts.