THE BICENTENARY OF JOSEPH PRIESTLEY.

THE following papers were read by SIR PHILIP HARTOG, K.B.E., C.I.E., PROFESSOR A. N. MELDRUM, and SIR HAROLD HARTLEY, C.B.E., M.C., F.R.S., at the meeting held on April 6th, 1933, to commemorate the life and work of JOSEPH PRIESTLEY.

JOSEPH PRIESTLEY.

1733—1804.

WE are met here to celebrate the bicentenary of Joseph Priestley, who was born at Fieldhead, near Leeds, on March 13th, 1733 (O.S.). Priestley was a man of marked simplicity of character, but of an intelligence with so many facets that he has been somewhat bewildering to his biographers. No clear or final picture of Priestley, the scientific man, has yet been produced, nor is such a final picture perhaps possible. You have asked Professor Meldrum, Sir Harold Hartley, and me to draw for you, within the brief space of time available, lightning sketches. The sketches will all be different. The great French painter, Monet, is said to have painted thirty or forty different pictures of the Thames from his window in Chelsea; and if another painter had painted as many pictures of the changing river in the same period these would all have been different from any of Monet's. You may say that I am thinking in terms of art and not of science; certainly I am not using the terms of that most excellent and old-fashioned science which supposed that there was only one kind of mathematics, and one kind of physics, which imposed themselves forcibly on every intelligence. We recognise now the individuality of the great artists in the simplified pictures of the external world that we call science. Yet the different pictures must all have truth in them if they are to be of any value at all. I want to-night to throw some light, if I can, on Priestley's own peculiar personality and history as a scientific man and theorist, for it seems to me that he has been singularly misjudged in some ways. Even Monet does not make the Thames look like the Irk or the Mersey.

I have said that Priestley's mind was one of many facets. He had an amazing facility for mastering different subjects. In his seventy-one years of life he published some fifty works on theology, thirteen on education and history, about eighteen on political, social, and metaphysical subjects, and—what matters to ourselves most—twelve books and some fifty papers dealing with physics, chemistry, and physiology (animal and vegetable). He had a gift for languages. He knew Latin, Greek, French, German, Italian, Hebrew, Chaldee, Syriac, and began Arabic. At quite an early age he read S'Gravesande's "Natural Philosophy," and we have the record that when he was twenty-three he read Boerhaave's "Elements of Chemistry," a fact of importance in considering his views about gases. From the seed planted in this fertile soil an amazing crop developed. But it was not a jungle. Priestley had definite purposes before him, and was a man of extraordinary method. He knew, far better than most of us, "how to live on twenty-four hours a day." He worked to a plan, was never hurried, and always had leisure for his friends. The main purpose of his life was one of religion and piety—piety which translated itself no less in the desire for clearness and proof in his theological views than in the desire for the active promotion of the happiness of his fellow creatures. It was through no chance that Bentham derived his utilitarian formula—"the greatest happiness of the greatest number"—from Priestley's work. Heterodox in his theological views, according to the standards of the time, he became a unitarian minister. He was essentially a liberal, both in religion and in politics, desiring toleration for views the most different from his own.

To trace the history of his many occupations as a preacher, and as a teacher of languages, of history, of belles-lettres, would take far more time than I can afford. But I must point out that Priestley's love of science, which he regarded as a relaxation (though it was through his scientific work perhaps more than anything else that his memory has been kept alive), became obvious at a very early stage. While he was a minister at Nantwich, at the age of twenty-five, he made his first experiments in science to please himself and the thirty or forty pupils at his school, to whom he never gave a holiday. With the help of his school fees he bought a few scientific books, a small air-pump, and an electric machine; and he made his pupils perform experiments and lecture before an audience. He was the pioneer of the teaching of science in schools.

In 1761 his preference for science over literature bursts out in so strange a place as his English Grammar. In his " Chart of Biography " of 1765 he wrote, " Few are qualified to make new discoveries; . . . but when discoveries have been made, and the principles of science have been ascertained, persons of inferior abilities . . . are sufficient to digest those principles into a convenient method." Priestley, not usually diffident, was obviously thinking of himself as one of those " inferior persons." And when he met Benjamin Franklin, also an amateur of genius, he suggested to him that he might undertake his first scientific work, a " History of Electricity," if Franklin would lend him the necessary books. Franklin agreed; other friends also lent him books; and, in 1767, within a year after Priestley had given Franklin his plan, the book was in print. From the inexperience of the author and the speed with which he worked, we might have expected an amateurish performance. But there are few, if any, signs of the amateur either in the " History " or the original experiments into which Priestley was drawn while writing it, and of which he has included a description. The book must be at least glanced at by all those who wish to form a judgment of their own on Priestley as a theorist and as an experimenter. It went through five editions. It is still referred to by leading authorities on the Continent.

Let us first consider Priestley, the theorist. He was confronted, in electricity, with two theories, the single-fluid theory of Watson and Franklin, and the earlier two-fluid theory of Du Fay and his successors. Priestley tries to hold the balance even between them, although he prefers the single-fluid theory. He even suggests at one point that neither theory may be true and that electrification may be a modification of the body electrified. In the preface to his third edition he apologises for having been a little unfair to the views of Nollet. But from his first preface onwards, Priestley contrasts the use of speculation (which he later calls " a cheap commodity ") * with the discovery of facts, for which he regarded speculation as only a means. The object of science, he tells us in the " History," is " to comprehend things clearly, and to comprise as much knowledge as possible in the smallest compass." † Though Priestley obviously owes much to Bacon, and perhaps something to Locke, the formula is, I think, a new one, at any rate in the mouth of a scientific man. It comes singularly close to the famous formulæ of Kirchhoff and of Mach, who defines science in effect as a shorthand description of Nature, leaving no room for cause or effect.‡ Priestley seems to depart from that view in his own discussion of the question of cause and effect, on which all scientific metaphysicians, up to the most recent and one of the most brilliant, Meyerson, argue " about and about." § But I am not sure whether, in spite of his use of the terms, cause and effect, Priestley's view is really distinguishable from that of Mach. He has a clear vision of the value of hypothesis in scientific investigation. Every experiment, he tells us, in which there is a design, is made to ascertain some hypothesis, for an hypothesis is nothing more than a preconceived idea of an event. An hypothesis absolutely verified ceases to be termed such, and is considered as a fact. Hypotheses lead persons to try a variety of experiments in order to ascertain them, and in these experiments new facts generally arise which serve to correct the hypothesis which gave rise to them.

By this method of successive approximations we may hope to discover all the facts and to form a perfect theory of them.|| Thus science is to be reduced to a statement of all the

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* " Experiments and Observations on . . . Air," vol. iii (1777), preface, pp. xxix—xxx.

† " History of Electricity," 1st edition, p. 442.

‡ See E. Mach, " The Science of Mechanics " (Eng. trans., 1893, p. 483), " There is no cause nor effect in nature . . . nature simply *is*." See also Karl Pearson's " Grammar of Science " (1892), pp. 159, 227 and *passim*.

§ See E. Meyerson's " Identity and Reality " (3rd edition, translated from the French, 1930).

|| " History of Electricity," p. 445. He says: " by this perfect theory, I mean a system of propositions accurately defining all the circumstances of every appearance, the separate effects of each circumstance, and the manner of its operation."

facts in the smallest compass, and the hypotheses finally disappear. Yet Priestley warns us that " a philosopher who has been long attached to a favourite hypothesis, and especially if he have distinguished himself by his ingenuity in discovering or pursuing it, will not sometimes be convinced of its falsity by the plainest evidence of fact," and thus " both himself and all his followers are put upon false pursuits, and seem determined to warp the whole course of nature to suit their manner of conceiving of its operations."

In regarding speculation as a cheap commodity, Priestley was influenced, no doubt, by Franklin, whose indifference to his own theories and to those of others he quotes with approval. It is not, says Franklin, " of much importance to us to know the manner in which nature executes her laws. It is enough if we know the laws themselves. It is of real use to us to know that china left in the air, unsupported, will fall and break; but how it comes to fall, and why it breaks, are matters of speculation. It is a pleasure indeed to know them, but we can preserve our china without it." *

Franklin's attitude is very far from being that of the author of the theory of gravitation. The famous passage in which Newton says, " *hypotheses non fingo,*" " I frame no hypotheses," is too often quoted without its almost immediate context in the " *Principia,*" that paragraph in which Newton speaks as if with equal conviction of the ether " a certain most subtle spirit which pervades and lies hid in all gross bodies "; a spirit to whose action are to be attributed heat, light, and electricity, and the excitation and transmission of the nervous impulse. Newton admits that there are insufficient experiments to demonstrate the laws by which this " electric and elastic spirit operates." † And then, besides the ether, we have Newton's famous " solid, massy, hard, impenetrable, movable particles " created by God in the beginning,‡ the atoms of which bodies are composed, and on whose properties they depend : the atoms inherited by Newton through a long line of philosophers from the Greek atomists.

It has been pointed out in a recent book by Madame Hélène Metzger § that eighteenth-century chemistry owes more to Newton than has generally been recognised. Priestley, at any rate, refers to him again and again. I think it would be roughly true to say that in the electrical investigations we see mainly the influence of the first Newton, the Newton of economical thought, of the *hypotheses non fingo,* and of the almost positivist Franklin. When we come to the chemical work we find the influence of the second Newton, the Newton of those passages in the " *Principia* " to which I have referred, and of what Priestley himself calls the " bold, eccentric thoughts " of the Queries and the Opticks.||

Repeatedly in the course of his work he returns to his general views on the relation of speculation to facts. In the preface to the third volume of " Experiments and Observations on Air " (1777), he writes, " The ingenious Abbé Nollet is a standing example of a fond adherence to systems, and Dr. Franklin of the advantage arising from facility in forming or rejecting them as phenomena occur. ¶ As it was his example and encouragement that led me to attempt philosophical investigations, it is his example that I propose to form myself upon, and that I would recommend to others." We shall see a little later in what way Priestley reconciled these philosophical views with adherence to the phlogiston theory, generally regarded as the great blot on his scientific memory. Sir Oliver Lodge has told us that Priestley in theory had no instinct for guessing right. Let us test that statement. Priestley shows that besides the metals, charcoal, black-lead, and red-hot glass are conductors, and that flame makes the surrounding air a conductor. He hazards the suggestion that there is no absolute distinction between conductors and non-conductors, but that there is a scale of conductivity in which all bodies must be placed. I quote his words : " Independent of moisture, there is probably a gradation in all substances from the

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* " History of Electricity," p. 467.

† " The Mathematical Principles of Natural Philosophy," translated by A. Motte (1729), vol. ii, pp. 392—393.

‡ Newton's " Opticks," 3rd edition, 1721, *Queries*, p. 375.

§ " Newton, Stahl, Boerhaave et la doctrine chimique," by Hélène Metzger (F. Alcan, 1930).

|| The reference is no doubt especially to *Queries* 30 and 31 following the " Opticks." See Priestley, " Experiments and Observations on . . . Air," vol. i (1774), p. 259.

¶ *Op. cit.*, p. xvii.

most perfect conductors to the most perfect non-conductors of electricity." * He not only had a general conception of conductivity but he tried to measure it by experiments which are only invalid owing to the fact that he was using variable currents and had no knowledge of what we now call impedance. I should put alongside this general idea of conductivity the general idea, which has been hitherto overlooked, that any body may be converted by sufficient heat into a gas. In Volume III of the "Experiments and Observations" (1777), p. 329, he writes: "I by no means believe that I have discovered *all* the kinds of air that may exist in nature; or, in other words, all the kinds of substances, simple or compound, that are capable of being reduced to a dry or permanently elastic vapour. For I believe there is no substance in nature but what is capable of assuming that form in a *certain degree of heat*." But perhaps an even more striking instance of Priestley's power of generalisation is this. From an experiment of Franklin's, which he amplifies, he shows that two pith balls introduced into an electrified metal cup showed no signs of electrification. He then suggests that the attraction of electricity is subject to the same laws as that of gravitation, that is, that it follows the law of the inverse square of the distance, and he says: "It is easily demonstrated that, were the earth in the form of a shell, a body in the inside of it would not be attracted to one side more than another." His was the first announcement of Coulomb's fundamental law of electrostatics. I have no time to dwell on his important work on the "lateral explosion" (re-discovered by Lodge as the "side-flash") and the oscillating spark, which I have discussed elsewhere.†

I pass from Priestley's electrical work to his chemical. From 1770 onwards his scientific work was almost entirely related to chemistry. As Sir Harold Hartley pointed out a few nights ago at the Royal Society, the contrast between Priestley's method of approach to electricity and his method of approach to chemistry is striking. He makes his electrical experiments after a systematic study of all that is known on the subject. He maintains, as I have pointed out, a balance between the two main theories and even suggests a third. But he slips into chemistry, as it were, by a side-door, and finding only one theory of any generality, the phlogiston theory, he adopts it without question. As you are all aware, the main hypothesis of the phlogiston theory is that when a body burns it gives up a principle called phlogiston.

It was Priestley's reflexion that charcoal, like metals, was a conductor and, like metals, contained phlogiston, that led him to make one of his first chemical experiments, if not the first. Priestley knows that charcoal will not consume by heating except in the open air, and he wishes to find out if metals behave in the same way. So he melts lead in a crucible after covering it with pipeclay and sand to keep it from contact with the air, and finds that it is only slightly calcined.

But I do not propose to make any complete survey of Priestley's chemical work. By mutual agreement (and not I confess without a personal pang) I have left it entirely to my very competent friends, Professor Meldrum and Sir Harold Hartley, to point out the amazing services rendered to chemistry by Priestley's discovery of a large number of new gases.‡ It is Priestley the theorist whom I wish to describe and I now turn to Priestley in his relation to that theory of Lavoisier which effected, as Priestley himself says, a revolution in chemistry. I am speaking here to an expert audience, to whom Lavoisier's discoveries

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* "History of Electricity," 1st edition, p. 435, and 2nd edition, p. 411. He inserts the word "probably" in the second edition.

† In a discourse on "Joseph Priestley and his place in the History of Science," given at the Roya Institution, of which I have made free use in the present address (see *Proceedings of the Royal Institution of Great Britain*, vol. xxvi, pp. 395—430).

‡ I am permitted to add here a reference to a letter published subsequently to the delivery of this address on the date and place of Priestley's discovery of oxygen, in *Nature* for July 1st, 1933 (vol. **132**, pp. 25—26). In this letter I have shown that for Priestley himself the place of the discovery was London (at Shelburne House, now Lansdowne House) and the date, March 1st, 1775, and not the usually accepted date, August 1st, 1774, when Priestley made his famous experiment on *mercurius calcinatus per se*, but, as he himself says, did not recognise the "real nature" of the "air" produced by heating that substance. Prof. R. M. Caven has pointed out, in a letter which gave rise to my own but was published simultaneously, that the experiment of August 1st, 1774, was made at Calne, in Wiltshire.—P. J. H.

and views are familiar at any rate in their main outlines. I would only remind you that in the establishment of Lavoisier's views the centre point in Priestley's eyes is Lavoisier's theory of the composition of water; the crucial experiment, the decomposition of steam by red-hot iron, with the liberation of hydrogen or " inflammable air " as it was then called.

Roscoe and Schorlemmer (famous teachers, to whom I owe a personal debt of gratitude) tell us that Priestley was " unable to grasp " the new ideas of the composition of water, and that " he remained to the end of his days a firm believer in the truth of the phlogistic theory, which he had done more than any one else to destroy "; and Roscoe and Schorlemmer's views represent those still held by what I may call the chemical " man in the street." Yet does it not seem a little absurd to suggest that a man of Priestley's intelligence should be unable to grasp the Lavoisierian theory? So far from this being true, he tells us himself that he was at one time a convert to the new ideas. He tells us in 1783 that he found Lavoisier's theory so specious that he was tempted to adopt it. But certain experiments which he made on the absorption by calces (metallic oxides) of inflammable air, without noticing the formation of water, led him to reject it. Lavoisier pointed out his mistake.

In 1785 he wavered again, on the strength of beautiful quantitative experiments obviously made to meet Lavoisier's criticism. First of all, by means of a burning-glass, he calcines iron in a vessel containing oxygen and standing over mercury, and shows that the weight gained by the iron is equal to the weight lost by the oxygen. He then reduces the iron oxide to iron by means of hydrogen, and shows that the two processes can be repeated indefinitely. He shows again that the volumes of hydrogen and oxygen involved are as 2 to 1, just those which combine without residue to form water by means of the electric spark. In one of his experiments the figures show that the ratio of the loss of weight of the iron oxide to the weight of water formed is as 15 to 17. Our ratio of to-day is 16 to 18. The inaccuracy is trifling for the period.

He interrupts the account of his experiments to say that it appears to him very evident that water with or without fixed air (that is, carbon dioxide, due to carbon as an impurity in the iron) was the product of the inflammable air (hydrogen) and the pure air (oxygen) let loose from the iron in this mode of operation. But, he adds (alas for the tragedy of it) —" I was taught by Mr. Watt to correct this hypothesis, and to account for this result in a different manner "—by that great engineer James Watt of all men, about whom a book has been written to show that he discovered the composition of water. It seems all so simple to us; yet Cavendish, the other reputed discoverer of the composition of water, whose experiments are so convincing to us, believed that the hydrogen of which water was partly composed itself contained water. Cavendish was a very able man, but even his friend Priestley found it difficult to reconcile that strange explanation with the principles of logic.

Up to 1803, the year before his death, Priestley never ceased to worry about Lavoisier's experiment and his ideas, to make fresh experiments and to write fresh memoirs on water. He says, repeatedly, that he is willing to adopt the new ideas if new and stronger evidence can be produced for them, but he still thinks that the facts admit of an easier explanation on the old system. The title of his last book, " The Doctrine of Phlogiston Established, and that of the Composition of Water Refuted," is, in part, misleading. In spite of the title he professes himself faithful to the principles of that scientific theory set forth in the " History of Electricity," and he is willing to abandon the phlogiston theory if the other can be shown to fit the facts better. " In forming any general theory (he writes) we must content ourselves with the fewest difficulties." * And again, " Though the title of this work expresses perfect confidence in the principles for which I contend, I shall still be ready publicly to adopt those of my opponents, if it appears to me that they are able to support them. Nay, the more satisfied I am with the doctrine of phlogiston, the more honourable shall I think it to give it up upon conviction of its fallacy "; and he then quotes " the noble example of Mr. Kirwan," who had given it up.† In his final Note on Humphry

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* " Doctrine of Phlogiston Established," 2nd edition (1803), p. 43.

† *Op. cit.*, p. 104.

Davy's essays he speaks of " phlogiston, *if there be such a thing as phlogiston.*" * It is his last reference to the subject.

I admit that Priestley's protean interpretations of the properties of phlogiston (into which I have no time to enter) are trying and difficult to follow. But some of his criticisms of Lavoisier's experiments had a very real foundation and value. He pointed out that the product of combustion of the " inflammable air " generated by passing steam over iron contains two bodies which the Lavoisierian theory could not account for, nitrous (our nitric) acid, and fixed air (our carbon dioxide). The nitric acid was due to nitrogen, an undetected impurity in the steam. The fixed air was due to carbon, an undetected impurity in the iron; I say undetected, not unsuspected, for in reply to his critics Priestley tries to prove that neither of these impurities was present.†

Priestley had, oddly enough, himself shown in 1791 that no fixed air is produced if malleable iron is used instead of cast-iron,‡ but he was no expert in iron analysis. Even if he had known that there was carbon in cast-iron he could have had no idea that it could be converted into an inflammable gas. Neither he nor anyone else had been able to explain on the Lavoisierian theory the repeated experiments on the " heavy inflammable air " (our water-gas) produced by passing steam over red-hot charcoal. The mystery was solved by the discovery of carbon monoxide by William Cruickshank in 1801. Priestley tells us that he had heard how the French advocates of the new theory were for long " kept in torture by these (*i.e.*, his) objections, but that they were relieved from that torture by Mr. Cruickshank." But he himself could not admit the possibility of an unsaturated oxide of carbon, an oxide that was both inflammable and not an acid.§

Nor were his objections at an end. If a metal was dissolved in acid, and hydrogen was evolved from the water, what became of the oxygen? And I will quote, in his own words, one final and not unreasonable objection, which proves, if further proof were needed, how clearly Priestley understood the theory he found himself unable to accept. The passage is from the " Doctrine of Phlogiston Established," 2nd edition, p. 91. " I shall conclude this section with observing that in order to complete their proof of the decomposition of water, the antiphlogistians should produce some substance which by uniting with hydrogen in water should let go the oxygen in the form of dephlogisticated air, or some acid; and surely some such substance may be found, if their theory is true."

Priestley has been largely misjudged, because, in spite of the simplicity of his style, he is not easy for us to read. His nomenclature, except in his latest years, is entirely different from ours. Though individual sentences are clear, he writes with little idea of a plan, and seems irritated by the admirable planning which makes Lavoisier's memoirs so easy to follow. The very copiousness and accuracy of the descriptions of his experiments, often vitiated by impurities of which Priestley was not aware, and at which we may find it difficult to guess, sometimes make him seem obscure, even when he is thinking with perfect lucidity.

Again, with a pang, I keep within my prescribed limits, and refrain from trying to conjure up a full-length portrait of Priestley the physicist and chemist.

But I cannot conclude without saying one word more of the man himself; of the transparently honest man of whom Augustus Toplady, a theological opponent, wrote: " I love a man whom I can hold up as a piece of crystal and look through him "; of the lovable man who, in Huxley's words, " charmed away the bitterest of prejudices in personal

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* *Op. cit.*, p. 119. The italics are mine.—P. J. H.

† With regard to nitrogen, for instance, see the " Doctrine of Phlogiston Established," 2nd edition, p. 63; with regard to carbon Priestley tells us that " MM. Berthollet and Adet and all my opponents say that this fixed air comes from the plumbago contained in the iron " (*op. cit.*, p. 85).

‡ *Phil. Trans.*, 1791, **81**, pp. 221—222.

§ " It therefore appears to me an absolute abandonment of the most fundamental principles of the new theory to call the air from finery cinder (magnetic iron oxide) and charcoal an *oxide*. If substances be combustible in proportion to their affinity with oxygen, and their consequent readiness to unite with it, this air, which is inflammable, must be of this class, and therefore the very reverse of the oxides, which are saturated with oxygen, and incapable of receiving more " (" Doctrine of Phlogiston Established," 2nd edition, p. 31).

intercourse "; of the ardent advocate of civil and religious liberty for all, the challenger of Edmund Burke and the defender in 1791 of the principles of the French Revolution, though not of its crimes; of the man whose house, on account of that defence, was burnt down by a mob at Birmingham and who barely escaped with his life; of the man who remained cheerful despite the chilly indifference of his colleagues at the Royal Society, and the threat of being burnt alive by more violent opponents. Priestley remained serene and happy in his religious belief that all was pre-ordained by divine providence. He died, in exile in America, working at his beloved theology, and intrepid to the end. He was, said Frederic Harrison, though not the greatest mind, the hero of our eighteenth century.

We do well at this bicentenary to illumine that great figure of the past with the flame of our memory. Priestley was a glory not only of British science, but of British manhood.

P. J. HARTOG.

THIS paper is divided into two parts. The aim, in the first part, is to show that Priestley, by his work on gases, made a contribution to science that is unique. The aim, in the second part, is to exhibit his work on nitrogen peroxide.

PART I. PRIESTLEY'S UNIQUE CONTRIBUTION TO SCIENCE.

" . . . some sulphureous Steams, at all times when the Earth is dry, ascending into the Air, ferment there with nitrous Acids, and sometimes taking fire cause Lightning and Thunder, and fiery Meteors. For the Air abounds with acid Vapours fit to promote Fermentations, as appears by the rusting of Iron and Copper in it, the kindling of Fire by blowing, and the beating of the Heart by means of Respiration " (Newton, " Opticks," Qu. 31).

Priestley's Knowledge of Chemistry.

Priestley, like Boyle, Dalton, Davy, Faraday, was not a university man; the education that he received was of the literary and philosophical kind; he was self-trained in experimental science. He never had a wide knowledge of chemistry: he was not " a practical chemist." He made that remarkable admission in scientific publications, in the memoirs of his life and in a letter that he wrote to Humphry Davy. He made it in a self-complacent mood, perhaps, glancing at trained men whose contributions to chemistry were smaller than his own. It is true, though it may seem incredible, that he made a great contribution to a science of which his knowledge was poor. I observe, as the clue to this mystery, that chemistry, up to his day, had been concerned so much with solids and liquids that its learning and processes, accumulated during centuries, were of little avail in work upon strange gases. Hence he could pursue the study of gases with the minimum of reference to the orthodox chemistry: he picked up a little of it, as he went along and as he felt the need, from books and from chemists who were his friends.

The Background of Priestley's Work.

It is necessary to pay attention to the background of Priestley's work. What had been done on gases up to the time that he began to study them? That can be shown in connection with two problems for chemists that arose in the 17th century and that were pursued in the 18th by Hales, by Black and by Cavendish.

The Study of the Air in the 17th Century.

Men of science, in the 17th century, felt a great impulse towards the study of the air. That impulse resulted, on the physical side, in the invention of the barometer and the air-pump and in the discovery of Boyle's law. On the chemical side, efforts were made to ascertain the part that the air takes in the respiration of animals and the part that it takes in combustion. It was perceived that respiration and combustion are analogous processes: it was found that air is absorbed in each. Again, certain gases were observed that appeared to be different from ordinary air, notably hydrogen, carbon dioxide, nitric oxide and nitrogen peroxide. Van Helmont was positive that gases exist which are distinct from air and from one another: in token he used the names *gas sylvestre* and *gas pingue*. Mayow, who tried to test this doctrine, was in two minds about it (*Alembic Club Reprints*, No. **17**, pp. 100, 113, 116–118). The elder Bernoulli rejected it and protested against Mayow for having taken it into consideration (see Harcourt, *Phil. Mag.*, 1846, **28**, 505–507).

Thus two problems were left for the 18th century to make what it could of them:—(i) the absorption and production of air in general and (ii) the existence of gases distinct from air and from one another.

Stephen Hales.

Hales made valuable contributions to the knowledge of vegetable and animal physiology and of chemistry. They are to be found in two books that he published: the "Vegetable Staticks," etc. in the year 1727 and the "Statical Essays," etc. in the year 1738. Arising out of his work on plants, he made copious experiments on the absorption and the production of air: he used materials of all kinds, solid and liquid, inorganic and organic, dead and living. From these experiments he drew the conclusion that air, in an inelastic state, is fixed in numerous solids and liquids ("Vegetable Staticks," pp. 313—315; "Statical Essays," p. 278). That conclusion, consolidating what the 17th century had thought of, was accepted in Holland by Boerhaave, in Italy by Boscovich, in Scotland by Black and in France by Rouelle and by Lavoisier.

The observations that Hales had accumulated remained, in other respects, as raw material for the chemist of the future. He obtained various gases and studied them without making any advance on earlier chemists. We may think that in carbon dioxide he should have seen an example, and in nitric oxide an outstanding example, of a gas that is distinct from ordinary air. But our ways are not as his ways.

Though we find Hales difficult to follow, Mayow and Newton could have understood him easily. They all belonged to the same school of thought in chemistry. Hales is an interpreter of chemistry in the 17th century. He recognised sulphur as a chemical principle and proposed that air should be recognised as another ("Vegetable Staticks," p. 316). Each of these principles was distinguished from the vulgar material, bearing the same name, just as elementary fire, in another school of thought, was distinguished from the fire that is used in the kitchen (Pott, "Lithogéognosie," etc., 1753, **1**, p. 346).

Hales thought that carbon dioxide has the nature of air. Let us see what he relied upon in comparing the one with the other. It was easy to show that air has weight; it was difficult to detect a difference in weight between one gas and another: Hales, as well as Hawksbee, tried to detect it and failed ("Vegetable Staticks," pp. 184—185). He did find that a gas, produced by fermentation, expanded and contracted under change of pressure and temperature like ordinary air (*op. cit.*, pp. 203—208). Dwelling on this agreement, he paid far too little attention to differences in other respects.

Hales repeated the experiment, that Mayow had made (*Alembic Club Reprints*, No. **17**, pp. 94—96), in which nitric oxide and air are mixed with one another in the presence of water ("Statical Essays," pp. 280—283). Each of them, looking at that experiment, considered the water as a means of watching the change in volume of the gases and did not perceive that the water acts as a solvent. Hales used sometimes colourless nitric oxide and sometimes the coloured mixture, of nitric oxide and nitrogen peroxide, as it came direct from iron and nitric acid. He paid no attention to the difference between them, unless in using the term "sulphureous, tho' clear air" (*op. cit.*, p. 283) for the colourless material. He regarded both as being "sulphureous."

The term "sulphureous" was applied to all substances that act vigorously with ordinary air. Hales classed those gaseous materials with sulphur and with coal. Accordingly, far from comparing nitric oxide with ordinary air, he went off in a direction which, though it seems a false direction to us, had been taken by Mayow and by Newton. He supposed that "sulphureous mineral vapours"—material, that is, comparable with nitric oxide—are "exhaled from the earth." Next he suggested that these vapours, setting up an "intestine motion" in the air, might cause the uncomfortable feeling that is put down to sultry weather and, under certain conditions, might give rise to thunder and lightning (*op. cit.*, pp. 284—288; compare Mayow, *Alembic Club Reprints*, No. **17**, pp. 147—150, and Newton, " Opticks," Qu. **31**).

Joseph Black.

Black made known his work on the mild and caustic alkalis first by a *Thesis* in the year 1754 and next by a memoir—" Experiments upon magnesia alba, quicklime and some other alcaline substances." That memoir was read at a scientific society in Edinburgh in the year 1755 and was published by the society (*Essays and Observations*, etc., 1756, **2**, pp. 157—225). He showed that chalk, when converted into quicklime, loses a particular gas that he called " fixed air " and that quicklime, on taking up fixed air, becomes chalk. He showed also that other substances are coupled in the same way :—magnesia and calcined magnesia, fixed alkali and caustic alkali, mild volatile alkali and caustic volatile alkali.

Thus Black, improving on Hales, commenced a revolution in science. The air and the gases had been regarded together as a kind of matter that is elastic and that is produced and absorbed in chemical change. The revolution in science required, first, that gases should be distinguished from one another and, next, that each gas should be referred to the substances that produce and absorb it. Black, in effect, defined fixed air as the difference between chalk and quicklime. He knew, then, very little more about it. He judged that it is an acid. He knew it to be soluble in cold and insoluble in boiling water. For the rest I quote his words :

" Quick-lime . . . does not attract air when in its most ordinary form, but is capable of being joined to one particular species only, which is dispersed thro' the atmosphere, either in the shape of an exceedingly subtle powder, or more probably in that of an elastic fluid. To this I have given the name of fixed air, and perhaps very improperly; but I thought it better to use a word already familiar in philosophy, than to invent a new name, before we be more fully acquainted with the nature and properties of this substance, which will probably be the subject of my further inquiry " (*Alembic Club Reprints*, No. **1**, pp. 30—31).

Henry Cavendish.

Cavendish, about ten years later, produced a memoir that is entitled " Three papers, containing experiments upon factitious air " (*Phil. Trans.*, 1766, 141). He had worked with fixed air, taking guidance from Black's teaching, and with inflammable air produced from metal and acid : had ascertained the specific gravity of each relative to ordinary air and also the solubility of fixed air in water and in alcohol. He found very nearly the same properties in specimens of each gas, prepared in different ways. This memoir, taken as a whole, constitutes Cavendish the founder of physical chemistry. I have taken only what is necessary for the present purpose. Cavendish proved at last and once for all, what van Helmont had surmised, that carbon dioxide and hydrogen are distinct from air and from one another.

Joseph Priestley.

The year after Cavendish produced his memoir upon " factitious air," Priestley produced a history of electricity, carried out original experiments in that subject and turned his attention to chemistry. He settled in Leeds in the year 1767. Happening to live in the neighbourhood of a brewery, he was curious to examine the fixed air that is formed in the brewer's vat. He made many experiments with this gas, used it for making aërated water and, when he removed to a distance from the brewery, learnt to make the gas for himself. His views widening, he studied the absorption of air by many agents, studied inflammable air and discovered the effect of vegetation upon air that had been breathed by animals, etc.

At length, in the year 1772, he made his work known. He published a pamphlet that described his method of making aërated water. At the Royal Society, early in the year, he read a memoir that reported his work upon gases. What was then read is little in comparison with what is printed. He greatly extended his memoir, working up to the time when it must go to the printer. He made additions to the sections that were read : he added four sections. In one of these he described hydrochloric acid gas. In another, which was read late in the year, he described nitric oxide and a method of using that gas for analysing atmospheric air.

The pamphlet and the memoir aroused great interest. Each was translated into French and published in Rozier's Journal with the minimum of delay. The Council of the Royal Society awarded the Copley Medal to Priestley on account of the memoir. Lavoisier gave a *résumé* of it and described it, in generous terms, as being " the most laborious and the most interesting that had appeared since the time of Hales "; and as being " suitable, more than any other recent memoir, for showing that physics and chemistry still offer new directions for effort " (" Œuvres de Lavoisier," 1864, **1**, p. 512).

This comment is generous and also appropriate. The study of gases had been making slow progress : men of the first rank in science avoided the subject or pursued it in a constrained way. Black never published any memoir devoted to a gas. Cavendish, witness the memoir on " factitious air " and another, on the " hardness " of water (" Experiments on Rathbone-place water," *Phil. Trans.*, 1767, 92), was restricted to the two gases that had been known longest. He produced his next memoir on gases after an interval of 16 years.

" Philosophers refute one another and men of science improve on one another." Priestley owed much to Cavendish. He took a lesson from the memoir on " factitious air " and improved on it. Working with fixed air, he taught himself how to handle a gas. Next he followed up observations that had been put on record and left unexplained. Exploiting Hales, he studied the many ways in which air is absorbed, observed the effect of vegetation in restoring vitiated air and discovered nitric oxide. Exploiting Cavendish, he discovered hydrochloric acid gas. Again, he could go outside the limits of pure science. Cavendish had measured the solubility of fixed air in water. Priestley, exploiting fixed air for making aërated water, gave rise to an industry.

By dwelling on the difference that Priestley made to chemistry only, we come far short of what is due to him. We can estimate the difference that he made to knowledge if we contrast science when it was limited to the study of solids and liquids—the science of the dark ages—with modern science that studies the three states of matter, severally and together. Priestley is unique in being the first to study gases freely and to handle them boldly, easily and quickly. All those results, above mentioned, and in addition the discovery of five more gases—nitrous oxide, ammonia, sulphur dioxide, oxygen and nitrogen peroxide—he produced in less than 10 years, having begun as a tyro. Each gas, as he could show, has properties in abundance. Moreover, in accordance with his handling of gases, he wrote about them in a familiar way that brought them within the range and the grasp of scientific men.

PART II. PRIESTLEY'S WORK ON NITROGEN PEROXIDE.

> " We observe in the distilling of pure salt-peter, that at a certain season of the operation, the body, although it seem either crystalline, or white, affords very red fumes " " though . . . by skill and care a reddish liquor may be obtained from nitre, yet the common spirit of it, in the making even of which store of these red fumes are wont to pass over into the receiver, appears not to be at all red " (Boyle, " Experiments and Considerations touching Colours ").

What is meant by the discovery of a substance ? Chemistry affords curious instances of that question. There is, for example, the discovery of fluorine, a substance that was perceived and named many years before it was isolated. That is one extreme. At the other extreme there are substances that were isolated long before they were discovered : for example, nitric oxide.

Mayow and Hales, though they worked with nitric oxide, did not point to it as a gas distinct from ordinary air. Priestley is the discoverer of nitric oxide in the sense that he prepared it, worked with it, discovered many of its properties and recognised it as a distinct substance : he gave it, in token, the name of " nitrous air."

Nitrogen peroxide is formed, as a by-product, in making nitric acid. One can believe that it was seen when nitric acid was first prepared from nitre, centuries ago. Boyle commented on it in his " Experiments and Considerations touching Colours." Mayow and Hales saw it, the red gas that is formed when nitric oxide and the air come together. Priestley discovered it in the sense that he discovered nitric oxide : he prepared and isolated it, studied it intensively and established many facts concerning it that are common knowledge in chemistry. He is named in books as having observed two or three of these facts, one in this book and another in that. Yet, where he is credited with the discovery of nitric oxide, that is, in the text-books and dictionaries and histories of chemistry, even in the biography of him written by Thorpe (T. E. Thorpe, " Joseph Priestley," 1906), he gets no credit for the discovery of nitrogen peroxide. Davy and also Dalton gave him credit for it when they described their work on the oxides of nitrogen. Chemists, previously, paid almost no attention to nitrogen peroxide and Berzelius and Gay-Lussac, subsequently, named Davy and Dalton in connection with it. And so Priestley's name slipped out of notice.

Priestley carried out many investigations in which the starting point was nitric acid. From it he obtained nitric oxide, oxygen and nitrogen peroxide. Some time after having obtained nitric oxide he remarked on the importance of nitric acid : or, as he called it, " nitrous acid " :

" I do not know any inquiry more promising that the investigation of the properties of *nitre*, the *nitrous acid*, and *nitrous air* " (1774, **1**,* p. 273).

That amounts to a decision to study nitric acid and everything that he could get from it. Before long he got oxygen from it and commenced the work that led him to nitrogen peroxide.

The main parts of the present subject are : (i) a study of nitric acid; (ii) certain processes in which nitrogen peroxide was produced; (iii) the isolation and intensive study of nitrogen peroxide. In treating the subject I have departed here and there from chronological order. Hence I give the following conspectus where the salient topics are grouped under the volumes in which they are to be found.

Conspectus.

1774, **1**.

The decision to study nitric acid.

1775, **2**.

Nitric acid vapour and nitric oxide.

Efforts to get nitric acid " in the form of air."

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* Priestley published his chemical discoveries in books, including memoirs that had appeared in the *Philosophical Transactions*. There are three series, each consisting of three books, as is shown in the following table :—

			indicated by
Experiments and Observations on different kinds of Air, 1774—7;	volume	**1**	" 1774, **1** "
	,,	**2**	" 1775, **2** "
	,,	**3**	" 1777, **3** "
Experiments and Observations relating to various branches of Natural Philosophy with a continuation of the Observations on Air, 1779—86;	,,	**1**	" 1779, **4** "
	,,	**2**	" 1781, **5** "
	,,	**3**	" 1786, **6** "
Experiments and Observations on different kinds of Air and other branches of Natural Philosophy connected with the subject, 1790.	,,	**1**	" 1790, **7** "
	,,	**2**	" 1790, **8** "
	,,	**3**	" 1790, **9** "

Some of these volumes appeared in more than one edition. Except in respect of printer's errors, there is no difference between one edition and another—even the pagination is the same throughout. The third series affords the best means of making an approach to Priestley's work in chemistry. The first and second contain much that was " hastily put down just as the things themselves happened to turn up" and some of it wrong. In preparing the third, Priestley revised and pruned the old material, added new material, classified everything and made an effort to bring his opinions up to date.

A study of nitric acid.	**1777, 3.**	Nitrogen peroxide— The preparation from (i) nitric acid and bismuth; (ii) lead nitrate. The effect of heat. The action with (i) water; (ii) organic substances; (iii) red lead.
Nitric acid solution : the effect of heat.	**1779, 4.**	Nitrogen peroxide : the preparation from various nitrates. Nitrososulphuric acid.
	1781, 5.	Nitrogen peroxide from nitrates : the process reversible.
Nitric acid vapour : the effect of light.	**1786, 6.**	

A Study of Nitric Acid.

Let me remark that what is labelled " nitric acid " is not always the same thing : it must contain water and nitric acid; it may contain nitrous acid and oxides of nitrogen. Moreover, these substances act together so as to produce others that are not well understood even at this stage of the 20th century. Thus Priestley was engaged on complex material. He learnt, sooner or later, that nitric acid varies in quality : becoming partly aware of the difference between nitric and nitrous acids, he used the term " nitrous acid " for the ordinary material and " phlogisticated nitrous acid " for material that was rich in oxides of nitrogen.

Priestley had been buying the nitric acid that he used. At last, towards the end of the year 1775, he decided that he must make it for himself. His aims were, he said :—

" partly to save expence; but principally to examine with my own eyes everything relating to it, and make what variations I should think proper in the process, in order to get the acid in the several *different states* in which I might have occasion to use it, without depending upon the report of any practical chymist " (1777, **3**, p. 234).

The friends to whom Priestley appealed for guidance responded at once. Mr. Woulfe furnished " a most commodious apparatus for the purpose."

" Mr. Winch jun. . . . both gave me instructions how to use it, and also assisted me in conducting a process or two at the outset.

" From this time I have made so many distillations of this acid, and have varied the circumstances of it so much, that I now think myself qualified to teach others; and there are probably few persons who have had more experience in this particular process than myself " (*op. cit.*, pp. 234—235).

Priestley was an uncomfortable pupil. He had obtained oxygen from nitric acid. But it was believed, he said, that air is absorbed during the preparation of the acid; he ascribed that belief, quite wrongly, to Woulfe. Consequently he proved in a course of experiments that air is produced in quantity during the preparation of the acid. In his eagerness he commenced on that subject by making an addition to Woulfe's apparatus the first time that Winch used it to conduct a process for him (*op. cit.*, p. 237).

Again, Priestley ascertained the effect of distillation upon the strength of the acid. His method of judging the strength was to measure the nitric oxide that the acid gave on acting with copper under certain conditions. This method of nitrimetry, which is the first on record and may arouse misgiving now, gave results in his hands. He found, for instance, that " the acid which comes over first in distillation is the strongest " (*op. cit.*, p. 246); also that on distilling a very weak acid " the first produce was pretty strong . . . the middle part . . . hardly differed from mere water . . . and that which remained in the retort had . . . a great deal of acidity " (*op. cit.*, p. 254).

Priestley paid great attention to the colour of nitric acid and nitrogen peroxide or, as he called them, " nitrous acid " and " nitrous acid air." He observed that the acid is sometimes colourless and sometimes yellow, brown, blue and green; that its vapour is sometimes colourless and sometimes red; and that " nitrous acid air " is always coloured. He wanted to understand these things. His mature conclusions were that the acid has naturally no more colour " than the vitriolic acid, or than water itself " (1779, **4**, p. 2) and " that with the less heat the acid is made, the lighter the colour of it will be " (*op. cit.*, p. 453). He found that it gains colour in various ways: (i) on communicating phlogiston to it; (ii) by the agency of heat; (iii) by the agency of light.

Nitric Acid and Nitric Oxide.

Various substances were used for imparting phlogiston to nitric acid. Amongst these Priestley paid great attention to nitric oxide; he observed the action between that gas and the acid in the state of liquid and of vapour. I proceed to consider the action between the gas and the vapour, that between the gas and the liquid being not quite relevant to the present subject although it is interesting and important.

The nitric oxide was contained in a bottle the mouth of which was immersed in water. The solution of the acid was boiled and the vapour was led into the gas by a tube that passed well within the bottle. A red vapour was formed and was absorbed by the water. At various stages in this experiment, Priestley withdrew nitric oxide in order to ascertain its condition. Thus he followed the process carefully and must have repeated it time and again. What he saw he put on record without theory or explanation. He noted two periods of induction, to use the modern term. The gas and the vapour being mixed, the red vapour was produced after a time; it persisted for a time before beginning to dissolve in the water.

" the whole progress of this experiment is not a little remarkable. The moment that the phial of nitrous air was exposed to this vapour, it became white, then transparent, then red: and, lastly, transparent again. I took one quantity of this air, when the whiteness had just gone off; and found that it was but little different from pure nitrous air, diminishing common air almost as much. Taking another phial when it was quite red, one-third of the quantity had disappeared, and its power of diminishing common air was about one-half of what it had been. I then let another phial remain exposed to this vapour, till I perceived that the diminution would go no further; when only 1/20 of the original quantity remained, and this did not affect common air at all.

" When this process is quick, that is, when the nitrous vapour comes very fast, the whiteness preceding the redness . . . can hardly be perceived. . . . I observed that the vessel containing nitrous air continued exceedingly red for about a minute, without any visible change in the dimensions of the air; after which it was suddenly diminished to about one-fourth of its original quantity " (1775, **2**, pp. 170—171).

Nitric Acid: the Effect of Heat.

Priestley observed, on heating coloured nitric acid, that the coloured material volatilises first and that the residue is colourless. He was surprised to find that this colourless acid, which he had expected would " always continue colourless " (1779, **4**, p. 14), became coloured when heated again. Suspecting that phlogiston had somehow got access to the acid, he took means to obviate that: he heated the acid in sealed tubes. He called this " a new mode of examining a variety of fluid substances " (*op. cit.*, p. 6). Years later, he put sealed tubes " in gun-barrels, closed with metal screws " (1790, **9**, p. 32). His purpose was, not to avoid danger if and when a tube should burst, but to ascertain the effect of heat on nitric acid in the absence of light.

The early experiments in sealed tubes, to which I return, led to observations that are surprising in their variety and that cannot all be readily explained. Priestley heated nitric acid of different qualities in tubes that were 3 to 4 feet long and described the results with great satisfaction. In a certain experiment he used " the strongest and palest sort " of acid. The first effect of heat was to give it an orange colour.

" After this, a red, or deep orange coloured vapour, appeared above the surface of the acid, and gradually ascended higher into the tube, at the same time that the acid itself grew paler, and at length became quite colourless, like water. . . .

" This red vapour kept rising higher and higher in the tube, leaving a considerable space, some times of ten or twelve inches, between it and the acid, all which space was quite transparent. This was a very pleasing appearance etc.

" I observed, however, that by the continued application of heat the quantity of red vapour increased, and the colour grew manifestly deeper; "

Fearing that the tube might burst, he allowed it to cool

" and presently saw the red vapour descend lower and lower, till it reached the colourless acid at the bottom of the tube, and, entering into it, communicated to it its own orange colour " (1779, **4**, pp. 7—9).

Nitric Acid: the Effect of Light.

Priestley made two contributions to photochemistry: he showed (i) as is well known, that green plants produce oxygen under the influence of light and (ii) that light decomposes the vapour of nitric acid.

The suggestion was made to him " by a philosophical friend "—Kirwan, it would seem (1790, **9**, p. 33)—

" that the air incumbent upon the acid might possibly affect its colour " (1786, **6**, p. 343). Priestley therefore commenced experiments by which he proved that " air *as such* had no influence in the case " except " as affording space for the vapour of the acid ": and that light does not act upon the acid in the state of liquid and does act upon it in the state of vapour.

Priestley prepared " colourless spirit of nitre " and let " it cool again in the dark."

" I put different portions of it into several phials, some of them quite full, and others only half full, with every different species of air incumbent upon them, except the nitrous; . . . Then leaving the phials exposed to the light of the sun, in a few days I found the acid in all of them that were only *half full* considerably coloured; whereas, the acid in the phials that were quite full remained as colourless as water " (*op. cit.*, pp. 343—344).

He observed next the effect in the absence of air:

" I contrived to have a *vacuum* above the acid; but still, when it was exposed to the light, it became coloured. . ."

He next observed the effect in the absence of light:

" I then took some of the phials that were only *half full*, and covering them from the light exposed them for several days to a considerable degree of *heat*. But in that situation they never acquired any colour."

He now felt himself master of the subject.

" Being now satisfied that it was the action of *light* upon the *vapour* of spirit of nitre that gave it colour, I amused myself with throwing a strong light, by means of a lens, into the upper part of a phial, the lower part of which contained colourless spirit of nitre. And in this manner I found that I could soon give a strong orange colour to the vapour of the acid; and that, being imbibed by the liquid acid with which it was in contact, *this* also became coloured, first at the top, and then quite through its substance " (*op. cit.*, pp. 344—345).

Nitric Acid " in the form of air."

The discovery of hydrochloric acid gas was a stimulus that led to the discovery of other gases: not only ammonia and sulphur dioxide but also nitrogen peroxide. Priestley hoped to obtain nitric acid " in the form of air." Thus he said, in the year 1777:

" My readers will easily recollect, from my former publications on the subject of air, that my greatest *desideratum* has been to exhibit the *nitrous acid* in the form of air, after having exhibited some other acids, and the alkaline principle, in that manner. . . . Since the idea first occurred to me, I have never once lost sight of it; being well aware of the unspeakable importance of it in such investigations as I have been engaged in " (1777, **3**, p. 174).

Priestley tried to treat nitric acid as he had treated hydrochloric acid. When he boiled the solution he found that the vapour dissolved in water and corroded mercury (1774, **1**, p. 273); next he tried to collect it over " melted hog's lard " and " got nothing " (1775, **2**, pp. 330—331). Such were his efforts up to the end of the year 1775. At last he made a pregnant reflection. During the collection of nitric oxide, prepared from a metal and strong nitric acid, he had often seen in the pneumatic trough,

" very large bubbles to issue from the end of the tube that transmitted the air, but exceedingly small ones rising to the top of the jar, I caught the hint of getting nitrous vapour by this means. For the large bubbles, I was well satisfied, must have been the *nitrous acid itself* in the form of air, but presently absorbed by water; while the small bubbles were the *nitrous air* " (1777, **3**, p. 177).

The Apparatus.

Consequently he made a fresh start. By the interaction of bismuth and strong nitric acid he obtained a gas that was nitrogen peroxide. An apparatus was ready to hand in which to prepare the gas. It was made solely of glass—a flask with a leading tube fitted to it by grinding : as Priestley said, " a phial . . . fitted with a ground stopple, perforated, and drawn out into a tube " (1775, **2**, p. xl). He descanted upon the advantages of this arrangement :

" Till I hit upon this contrivance, which was executed for me by the direction of Mr. Parker, I had a great deal of trouble in perforating common corks, bending and fitting tubes to them; and, after all the corks themselves, or the cement, with which I generally found it convenient to cover the ends of the tubes, were apt to give way, and to be the occasion of very disagreeable accidents. Besides, if any hot acid was used, the vapour would corrode the cork, and an allowance was to be made for the effect of that circumstance on the air : whereas, with this apparatus, which is exceedingly convenient and elegant, the operator may be sure that nothing but glass is contiguous to the materials he works upon, as he can perfectly exclude every other foreign influence; and, while it remains unbroken, it is never out of repair, or unfit for use " (*op. cit.*, pp. xl—xli).

This contrivance did not originate with Priestley. Cavendish, years earlier, used something similar and described it with the eloquence that lets things speak for themselves : he referred to a diagram

" where A represents the bottle, in which the materials for producing air are placed; having a bent glass tube C ground into it, in the manner of a stopper; E represents a vessel of water; D the bottle to receive the air " etc. (*Phil. Trans.*, 1766, p. 141).

Priestley and Cavendish : a Contrast.

What Priestley did was to substitute a flask for the bottle A in Cavendish's arrangement; the flask, long and narrow, was a better shape than the bottle and in other ways handier. The difference between the two arrangements is of less importance than the use to which each was put. That depended upon the temperament of each man, temperament being all-important in science. Priestley exploited his apparatus to the full. Cavendish made use of his with excellent results and stopped as if he could do nothing more. Between these two men, who were friends for many years, I can see nothing in common beyond a passion for science. Cavendish would publish only what satisfied his conscience and he withheld much from publication; in carrying out work he was laborious and artistic to an extreme degree. This aristocrat had none of the airs and graces that will be of no avail at the Day of Judgment and that are effective in society; he was troubled when he must meet a stranger. He was a social failure and Priestley was a social success. Priestley, who came of plain English stock and was a Nonconformist—one still comes across people who sneer at this Nonconformist—had the *joie de vivre* : he loved to meet his fellow-creatures, made innumerable friends, wrote books on many subjects, was always ready for a controversy, made experiments in profusion, discussed them with his acquaintances and published them eagerly.

The Collection of Nitrogen Peroxide.

A change in technique, even a trifling change, may make a big difference in result. Priestley's methods in handling gases, which were new and effective, were in the main improvements, some of them small, on earlier methods. He made a great success, following Cavendish, by using mercury in his work : that enabled him to collect hydrochloric acid, ammonia, sulphur dioxide, silicon fluoride. He collected all his gases over water or over mercury, one gas excepted.

Progress in technique sometimes depends on seeing what is obvious. Priestley had to find the right method of collecting nitrogen peroxide. Mercury, water and whale's oil, all which he tried, being useless, he thought of the obvious and simple method of passing the gas into a bottle :—

> " it occurred to me, that . . . it might not be wholly without its use, if I should shut up this vapour in dry glass phials, with ground stopples " (1777, **3**, p. 184).
>
> " And as I could not well produce this acid vapour at all without generating enough to fill a great number of phials, I generally placed six, eight, or ten of them in a row, filling them with the vapour one after another, and sometimes supplying them all several times in the course of one process " (*op. cit.*, p. 195).

Thus Priestley, it would seem, invented the method of collecting a gas by displacement of air, using a leading tube. He did not draw special attention to it or to the advantage that it has, over previous methods, of avoiding the presence of water or mercury along with the gas. He pointed to the use of a stream of gas, as being a " new mode of operating." He had thus three ways of trying the effect of the gas upon a substance :—

> " One was, to put the substance into a clean phial, and then to throw a stream of the vapour upon it. Another was, first to fill the phial with the vapour, by which method the quantity of it might be, in some measure, ascertained, and then to introduce the substance to it at the mouth of the phial. Lastly, if the substance was fluid, I could plunge the tube, through which the vapour was transmitted, as deep as I pleased into it, and thereby diffuse the vapour through the whole body of it " (*op. cit.*, pp. 194—195).

The Relation between Nitric Acid and Nitrogen Peroxide.

Before I proceed to the experiments that were made, let me give a brief account of the opinions that Priestley held regarding nitric acid and nitrogen peroxide in relation to one another. As a believer in the theory of phlogiston, he held that nitric acid becomes coloured when it gains phlogiston. In the end he came to believe that gain of phlogiston means loss of oxygen. In particular, he accepted Lavoisier's doctrine on acids. He said, in the year 1790 :—

> " the doctrine of dephlogisticated air being, or containing, the principle of *universal acidity*, had been advanced by Mr. Lavoisier, and admitted by myself, and others " (1790, **9**, p. 44).

Consequently he connected the production of colour in nitric acid with loss of oxygen :—

> " heat gives colour to the nitrous acid by expelling the pure air, which leaves the rest phlogisticated " (*op. cit.*, p. 17).

It is noteworthy that Priestley had not got all that he wanted when he obtained nitrogen peroxide. He knew it to be " loaded with phlogiston " and he wanted to " separate the phlogiston from it " (1777, **3**, p. 176). Thus he hoped, I infer, somehow to get from nitric acid a substance containing less phlogiston—more oxygen, that is—than nitrogen peroxide and he expected that it should be a colourless gas.

Priestley knew that nitrogen peroxide, as he made and collected it, contained some nitric oxide and some air. He was confident, when he set to work on it, of getting results.

> " I saw opened to me an intire new field for experiments, towards which I looked with pleasing expectation, even while the prospect which it afforded was very indistinct; being satisfied, from the nature of the acid, and the important part it acts in the system of nature, that it could not fail amply to reward whatever labour I should bestow upon it " (*op. cit.*, p. 194).

The Effect of Heat.

Priestley noticed the effect of heat upon the gas in deepening its colour. He reports that

> " In order to make some experiments of this kind to proper advantage, I procured a glass tube, three feet long, and about an inch wide, closed at one end, and fitted with a ground stopple at the other. This tube I easily filled with red vapour, in consequence of its being much heavier than common air; and closing the open end with the stopple, observed, that that part of the tube which I held in my hand was manifestly of a deeper colour than any other part of the tube. On this I held one end of it to the fire, and found that that end grew most intensely red, three or four times more so than the rest of the tube." [He also saw the whole tube assume] " the same deep red colour, when the whole length of it was made equally hot : . . . it became alternately of a deeper or lighter colour, according as it was kept hot or cold " (*op. cit.*, pp. 186—188).

Nitrogen Peroxide and Water.

Priestley found, as he expected, that this gas gives " nitrous acid " on dissolving in water. First he prepared the solution on a small scale. Next, in order to impregnate the water completely, he made ready an apparatus consisting of the flask in which to make the gas and of three wash-bottles placed in series : he " had all the glass tubes fitted to their respective holes by grinding " (*op. cit.*, p. 3). He observed, as the gas reached the water in the first bottle, that the water began to sparkle and become first blue, next green and finally yellowish.

> " The water, after becoming warm, began . . . to sparkle, and emit air; after which it became *blue*, still continuing to give air in much greater plenty than before. After this the water became *green*, about which time the emission of air ceased; and lastly, after the green colour had deepened very much, so as to appear almost black, when viewed in the same direction with the light that fell upon it, a *yellowish tinge* was perceived to be diffused through the green colour; and this was the last state to which I could bring the water by this impregnation " (*op. cit.*, p. 198).

He watched this process go on in one bottle after another.

> " I also observed that, about the time that the water in the first of these vessels became blue, that in the next began to sparkle; and when the water in the first turned green . . the water in the next vessel became blue, and that in the following to sparkle " (*op. cit.*, pp. 198—199).

The gas that came off during the sparkling of the water Priestley found to be nitric oxide. He was impressed by the amount of it. Taking a certain volume of the water

> " I got at one time more than ten times the bulk of the water, all pure nitrous air " (*op. cit.*, p. 200).

He considered what could be the source of this gas. All he could be sure of was that the gas, in such abundance, had not been produced along with the nitrogen peroxide. " The production of it in this case is quite *another thing* and must have a different *cause* " (*ibid.*).

Priestley observed that the green solution he had obtained " in this manner contains more phlogiston than common spirit of nitre " (*op. cit.*, p. 205). He put some of it in an open glass and blew upon it. He saw that a copious red vapour came off and that the colour changed to yellow.

> " When the green colour is blown out of this impregnated water, it is not to be distinguished, in any respect, from the strongest yellow spirit of nitre " (*op. cit.*, p. 204).

Nitrogen Peroxide and Organic Substances.

Numerous other substances were tried along with nitrogen peroxide. Spirit of wine dissolved the gas and at length formed two layers, the upper of which Priestley recognised as " nitrous ether " (*op. cit.*, p. 213). Whale oil, essential oil of mint, oil of turpentine, absorbed the gas, became hot, and changed colour; the liquid, on cooling, changed colour again and coagulated in course of time. The modern organic chemist, who uses nitrogen peroxide as a means of studying terpenes, can understand that his own crisp results were not obtained in the beginning. Priestley found that the solution of the gas in oil of turpentine, when kept for some time, " yielded air . . . copiously . . . this . . . was mere phlogisticated air [nitrogen], without any mixture of fixed air " (*op. cit.*, pp. 211—212).

Nitrogen Peroxide and Inorganic Substances.

Priestley found nitrogen peroxide to be very soluble in caustic alkali solution. Not being skilled in working with salts, he did not think of evaporating the liquid and getting crystals :—" seeing nothing remarkable in the appearance of the liquor, I did not prosecute the experiment " (*op. cit.*, p. 228). With hydrochloric acid solution he obtained " an aqua regia of incomparably more power," he said, than " common aqua regia " as a solvent for gold (*op. cit.*, pp. 219—220). He observed the gas to be very soluble in sulphur dioxide solution and failed to detect the production of sulphuric acid (*op. cit.*, pp. 221—222).

Nitrososulphuric Acid.

Quite the most important result was the production of nitrososulphuric acid. Nitrogen peroxide was absorbed by sulphuric acid: some of the resulting liquid, on addition of water, became hot and gave off a great quantity of " dense red vapour " (*op. cit.*, p. 218).

This liquid was put into phials. Priestley looked at one of these phials, after a year, and saw only liquid in it. He saw it, the next day

> " almost filled with the most beautiful crystallisations imaginable.
>
> " Their form, as nearly as I can describe it, was that of a feather. They were about twenty in number, some of them as large as the phial could contain, and many of them parallel to each other, but others lying in different directions. The two parts, as it were, of the feather made an angle with each other of about 160 degrees, and each of the single fibres that composed the feather, but which were connected, like the toes of a duck's foot, by the same substance (but thinner, and more transparent than the rest) made an angle with the stem from which they arose of about 45 degrees. A more beautiful appearance can hardly be imagined, and I am afraid I shall never see the like again " (1779, **4**, pp. 29—30).

The liquid, in another phial, was almost all crystallised after six months.

> " The crystals looked exactly like ice . . . on dropping a piece of this ice into pure water, it became green, and effervesced with great violence; and, what made a beautiful and striking phenomenon, all the water in which the ice was dissolved began instantly to sparkle with the spontaneous and copious production of air. With the help of a little heat, this production of air was so great, that the quantity was more than a hundred times the bulk of the ice that had been dissolved. It was the purest nitrous air " (*op. cit.*, p. 27).

These crystals had been formed in course of time. Priestley learnt how to produce them at will. He passed nitrogen peroxide into a little sulphuric acid, contained in a large bottle, until the acid was almost saturated.

> " Then immediately throw in a very copious nitrous vapour, so that the whole phial shall be intensely red, and running over; after which put in the stopper, and let it remain quite still . . . all the sides of the phial, and especially the parts towards the bottom will soon be quite covered with those crystals " (*op. cit.*, pp. 450—451).

This subject was not new. Guyton de Morveau, reviewing it afterwards in the " Encyclopédie méthodique, Chimie," named various chemists—Bernhardt, Lavoisier and Bucquet, Cornette, Dehne—who had obtained these crystals before Priestley or about the same time as he. Bernhardt (1765) obtained them when making nitric acid by distilling a mixture of nitre and " calcined vitriol." Lavoisier and Bucquet (1777) used what is Bernhardt's method in essentials. Cornette (1777) heated a mixture of charcoal and a nitric acid which, as he ascertained, contained sulphuric acid. Dehne used nitre and fuming sulphuric acid. Guyton de Morveau perceived that the method of Priestley threw light on all the other methods: he called it " le plus simple, le plus direct, le plus facile " (" Encyclopédie méthodique, Chimie," **1**, p. 187).

Priestley arrived at a theory regarding the crystals and of the process in which they were formed. He found that the liquid in contact with the crystals, when they were well formed, consisted mainly of nitric acid. Therefore he concluded that the crystals consist of sulphuric and nitric acids and, also, that the process depends on a strong affinity between nitric acid and water.

> " Here then is a case in which the nitrous acid appears to have a stronger affinity with water than the vitriolic: for in a course of time, it intirely expells the vitriolic acid from it, and unites with it itself; all the vitriolic acid being precipitated in the crystals that consist of both the acids " (*op. cit.*, p. 34).

The Action with Red-lead.

The effect of nitric acid upon red-lead, as Priestley knew, is to give an almost black substance. When he passed nitrogen peroxide over red-lead, he obtained, to his surprise, " a perfectly *white and brittle* " substance (1777, **3**, p. 231). He had to find a good method of carrying out this change.

> " After many trials . . . I succeeded best by first slightly moistening the inside of a glass jar, and, by applying the red-lead to every part of it, giving the jar as thick a coating as I could; and after this throwing the vapour into it, by inserting the tube through which it issued very deep into the jar. . . . It is remarkable that, in this experiment, the red-lead that is nearest to the glass becomes white first " (*ibid.*).

The action between a solid and a gas is not so simple as it may seem. Priestley, trying a plausible method that did not work, passed nitrogen peroxide through bottles containing red-lead and connected in series. He found that

> " the red-lead in the first phial became very slightly white, just at the bottom where the vapour entered it, and also in a circle close to the glass at the top; and that near the top of the second phial there was a similar circle, but not near so white, while the rest of the lead was of a darker colour. But the whole quantity was considerably increased in weight by this means " (*op. cit.*, pp. 231—232).

Priestley's want of familiarity with ordinary chemistry is seen in his tardy identification of this white substance. When first reporting it, he called it a " new kind of white-lead " (*ibid.*). Two years later he called it " white minium " (1779, **4**, p. 35). At last, after another two years, he reported having obtained it from lead and nitric acid and called it " nitre of lead " and " nitrated calx of lead " (1781, **5**, pp. 239—241).

Finally, Priestley arrived at a method for making nitrogen peroxide which came to be, and for long remained, the standard method: that is, by heating lead nitrate. He noticed what appears to be inevitable in it, namely, the production of a small amount of water along with the peroxide. He heated the nitrate in a glass tube that was closed at one end. When the red vapour filled the tube and was escaping, he sealed the open end. Next he removed the other end together with the residue from the nitrate (1779, **4**, pp. 35—36).

Extending this method, he prepared the same gas from other nitrates. He used in all, he said, " white nitrated calces of lead, zinc, copper and tin " (1781, **5**, pp. 240—241). Also, he made the important observation that the residue from the nitrate could absorb the gas.

"If the white calx from which the red vapour was expelled be suffered to remain long in the tube, it will reimbibe the whole of it. But then the vapour may be expelled again by heat, and will continue to fill the tube a considerable time" (*op. cit.*, p. 242). "The red vapour was reabsorbed by all the calces, but less slowly by the calx of lead than by those of tin and copper, and most quickly by that of zinc " (*op. cit.*, p. 241).

This work on nitrogen peroxide shows the well-known Priestley: his ardour in discovery, his clever hands and eyes that nothing could escape, his resource in difficulty. It shows him making contributions to the knowledge, not only of gases, but also of liquids and solids. Again, certain of his discoveries came to him easily—ammonia gas, for example—and were not greatly extended when he recurred to them. This work is remarkable because it shows effort made in a particular field renewed time and again and rewarded by success after success. The work is abundant in material for the teacher and the investigator. Some of the material remains, ready for exploitation, just as Priestley left it.

A. N. MELDRUM.

AFTER listening to Sir Philip Hartog and Professor Meldrum, you may wonder if there remains anything for me to add, but Priestley's life was so full of accomplishment and interest that even without their friendly connivance I think there must have been some new aspects left to explore.

Sir Philip has given us a picture of Priestley's personality, his varied activities and interests, his versatility, his quickness of perception and his rapid grasp of a new subject, which explains to some extent his vast output of literary and scientific writings. I was particularly glad that he spoke of Priestley's " History of Electricity," his first scientific work, and an astonishing accomplishment for a novice in less than a year. He described for us Priestley's attitude towards scientific theories and his reasons for adhering deliberately to the theory of phlogiston.

Professor Meldrum described the historical background of Priestley's discoveries and sketched for us the great tradition of pneumatic chemistry in this country. For a century chemistry made but slow progress owing to the neglect of gases by chemists, and Priestley's position is unique as he first taught chemists how to handle gases and convinced them of their chemical individuality.

May I first add a little to what Professor Meldrum has said of Priestley's debt to his forerunners as regards technique. Boyle and Hooke first devised methods of handling gases, and these were added to by Mayow, whose " Essays " published in 1674 contain the first illustrations of pneumatic apparatus. Mayow anticipated the principle of the McLeod gauge in a method he devised for comparing the compressibility of gases with that of air in order to ascertain whether they are true gases or not. Mayow's book was not known directly to Black, Cavendish, Priestley, or Lavoisier, but luckily it came into the hands of Stephen Hales, a most ingenious experimenter, who developed Mayow's methods and described them in his " Vegetable Staticks " published in 1727. Hales played an important part in the history of pneumatic chemistry. Not only did he hand on his knowledge of Mayow's methods to Black, Lavoisier and Priestley, but his writings had a most stimulating effect in directing their thoughts towards the study of gases. Black, speaking in his lectures of his discovery that magnesium carbonate loses a large proportion of its weight on heating, said "This appeared at first an unaccountable fact; but it made me recollect some of Dr. Hale's experiments, described in an essay to which he gave the title 'The Analysis of the Air.' . . . I began to suspect that the loss of weight was occasioned by the loss of a quantity of elastic aerial matter or air."

Lavoisier in the historic note, dated November 1st, 1772, but not published until 1805, in which he explained the increase in weight of sulphur, phosphorus and metals during combustion as due to the fixation of air, says that he used an apparatus described by Hales for reducing litharge by heating it with charcoal.

The first mention of Priestley's researches on gases is in a letter dated 25th February, 1770, when he says "I am now taking up some of Dr. Hales' enquiries concerning air."

Professor Meldrum has spoken of Priestley's discoveries of new gases and has described the thoroughness with which he investigated nitrogen peroxide. We think of Priestley as a discoverer—as he said to Davy "I was a discoverer before I was a chemist"—but I wonder how many realise the extensiveness of the study Priestley made of the gases he discovered, in how many different ways he prepared them and how many facts he established about them. I have in my hand the fifth volume of his experiments, in which he gives "a summary view of all the most remarkable facts in this and the four preceding volumes." He deals with ten different gases and the summary is a remarkable record of his discoveries concerning them. I will take three examples—oxygen, nitrous oxide and ammonia.

Dephlogisticated air or oxygen, Priestley prepared from a large number of different substances, by heating oxides, nitrates and sulphates, and he observed correctly that he could never obtain it by heating a chloride. "It is found in the bladders of sea weed, in water and in sea water. It is produced by a green vegetable matter in water, but not without the influence of light. . . . It is heavier than common air."

Dephlogisticated nitrous air or nitrous oxide he made from nitric oxide by long exposure to iron, or to a mixture of sulphur and iron, and also by dissolving metals such as tin and zinc in dilute nitric acid. He purified the gas by dissolving it in water and then expelling it by heat from the solution. "When absorbed by water it imparts no acidity to it."

Alkaline air or ammonia he prepared by heating sal-ammoniac with slaked lime. He found it heavier than inflammable air but lighter than marine acid air (hydrogen chloride). "The electric spark taken in alkaline air produces inflammable air, and the quantity of inflammable air is three times that of the alkaline air."

Priestley's great service to chemistry was the extensiveness of his work which revealed to chemists the variety of substances which could exist in the gaseous state, their individuality and the importance of the part they played in chemical reactions. No longer could chemists believe that "there was only one kind of air loaded with different vapours."

Davy has told us what his work meant to his contemporaries:—"It is scarcely possible," he said, "to advance a step, or to perform a process in pneumatic chemistry, without having recourse to his methods, and making use of substances he first exhibited. His experiments, though neither accurate nor minute, were almost always on subjects of importance. He prepared the way for more accomplished chemists and furnished them with methods of enquiry."

It is the more remarkable that Priestley made so many discoveries without understanding their true meaning, with a very limited knowledge of chemistry, and with the very simple tests available to him—the burning candle, the mouse, the nitric oxide test, solubility, colour and smell. He was quick to recognise gases he had obtained previously and he had a flair for finding out the simplest methods of preparing them—nitric oxide he made from copper and nitric acid, and nitrogen peroxide by heating lead nitrate.

Priestley often ascribes his discoveries to chance, but in this he is unfair to himself, as he had a real genius for taking advantage of the opportunities that fortune offered him, and many of his researches showed that he could follow up a clue with a fine logical piece of investigation.

There remains the fascinating contrast between Priestley and his contemporaries. Why was it that Priestley was so much more prolific in actual discoveries than Black, Cavendish and Lavoisier, with their greater knowledge and experience and their experimental resources? There are two aspects of his work which, I think, help to answer this question.

Sir Philip has already referred to Priestley's power of generalisation. He had, too, in a most unusual degree the gift of seeing the wider issues raised by his experiments and the possibilities of extending those experiments and applying their results to practical purposes. His work on carbon dioxide suggested to him a new industry—the making of aerated waters. His experiments on combustion and respiration in confined volumes of air led him at once to the conclusion that there must be in nature some means for maintaining the purity of the atmosphere. To quote his own words :—" The quantity of air which even a small flame requires to keep it burning is prodigious. It is generally said that an ordinary candle consumes, as it is called, about a gallon a minute. Considering this amazing consumption of air by fires of all kinds, volcanoes, etc., it becomes a great object of philosophical enquiry, to ascertain what change is made in the constitution of air by flames and to discover what provision there is in nature for remedying the injury which the atmosphere receives by this means."

This he set out to investigate, his guide being " generally to consider the influence to which the atmosphere is exposed." He tried the effect of washing foul air with water, of exposing it to light, of heating it and cooling it, and of subjecting it to the action of those " effluvia which are continually exhaling into the air." He tried the fumes of burning sulphur and of the smoking spirit of nitre. There he let the discovery of oxygen slip through his fingers for the first time.

After many experiments he discovered the effect of vegetation on the composition of the atmosphere. " One might have imagined," he said, " that since common air is necessary to vegetable, as well as to animal life, both plants and animals had affected it in the same manner; and I own I had that expectation, when I first put a sprig of mint into a glass jar inverted over a vessel of water : but when it had continued growing there for some months, I found that the air would neither extinguish a candle, nor was it at all inconvenient to a mouse which I put into it." This unexpected result led him to try the effect of vegetation on different kinds of foul air and he found that in some cases the sprigs of mint he used died quickly " but if they do not die presently, they thrive in a most surprising manner. In no other circumstances have I ever seen vegetation so vigorous as in this kind of air which is immediately fatal to animal life. . . . This observation led me to conclude that plants, instead of affecting the air in the same way as animal respiration, reverse the effects of breathing and tend to keep the atmosphere sweet and wholesome, when it is become noxious in consequence of animals either living and breathing or dying and putrefying in it." The critical experiment was made in the beginning of August, 1771. " I took a quantity of air made thoroughly noxious by mice breathing and dying in it, and divided it into two parts; one of which I put into a phial immersed in water, and to the other (contained in a glass jar standing in water) I put a sprig of mint. . . . After eight or nine days I found that a mouse lived perfectly well in that part of the air, in which the sprig of mint had grown, but died the moment it was put into the other part of the same original quantity of air, and which I had kept in the very same exposure, but without any plant growing in it." A little later in the same month he showed that a growing plant has also the same power of restoring air in which a candle had burnt out. Thus, within a year, Priestley had solved his problem in spite of the handicap that he had no idea of the nature of the chemical changes involved.

The discovery of nitric oxide and of its reaction with common air to give red fumes which are absorbed by water with a loss of volume amounting to about one fifth of the common air, led him to the conclusion that " the diminution, occasioned by the mixture of nitrous air, is peculiar to common air or *air fit for respiration*, and is at least very nearly, if not exactly, in proportion to its fitness for that purpose; so that by this means the goodness of air may be distinguished much more accurately than it can be done by putting mice, or any other animals, to breathe in it. . . . This was a most agreeable discovery to me, especially as from this time I had no occasion for so large a stock of mice." From this experiment of Priestley's has grown the science of gas-analysis, and the word " eudiometer," literally the measurer of good weather, reminds us of the purpose for which it was first used.

Finally, there is the discovery, on 1st August, 1774, that mercuric oxide when heated evolves a gas. Priestley was slow to realise its true nature. At first he thought it was nitrous oxide as it enlarged the flame of a candle and he failed to notice that it was insoluble in water; and he was of this opinion when he told Lavoisier of his discovery in October of 1774. Later, he found that it was barely soluble in water and tests with nitric oxide and mice gradually made him recognise in March, 1775, that he had discovered a new gas, which he announced to the President of the Royal Society in the following words:—

> " But the most remarkable of all the kinds of air that I have produced by this process is one that is 5 or 6 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."

Lavoisier was quick to realise that the new gas was the final clue he needed to explain the problem of combustion. He named it oxygen and made it the central element of his new system, which marks the birth of modern chemistry. But while Priestley failed completely to grasp the inner significance of his discovery, he saw at once its practical applications. " Nothing," he writes, " would be easier than to augment the force of fire to a prodigious degree, by blowing it with dephlogisticated air instead of common air. . . . Possibly much greater things might be effected by chemists, in a variety of respects, with the prodigious heat which this air may be the means of affording them." And again: " From the greater strength and vivacity of the flame of a candle, in this pure air it may be conjectured, that it might be peculiarly salutary to the lungs in certain morbid cases, when the common air would not be sufficient to carry off the phlogistic putrid effluvium fast enough. But perhaps we may also infer from these experiments that though pure dephlogisticated air ought to be very useful as a *medicine*, it might not be so proper for us in the usual healthy state of the body: for, as a candle burns out much faster in dephlogisticated than in common air, so we might, as may be said, *live out too fast*, and the animal power be too soon exhausted in this pure kind of air. A moralist, at least, may say, that the air which nature has provided for us is as good as we deserve. My reader will not wonder that after having ascertained the superior goodness of dephlogisticated air . . . I should have the curiosity to taste it myself. I have gratified that curiosity by breathing it. . . . The feeling of it to my lungs was not sensibly different from that of common air, but I fancied that my breast felt peculiarly light and easy for some time afterwards. Who can tell but that, in time, this pure air may become a fashionable article in luxury. Hitherto only two mice and myself have had the privilege of breathing it."

Never, I venture to say, was an inventor's vision better justified; each year in Europe and America some 5,000,000,000 cubic feet of oxygen are used, as Priestley had foreseen, in industry and medicine.

You will see from those examples how active was Priestley's imagination, ever stimulating him to widen the range and application of his experiments.

Another aspect of Priestley's work reveals him as one of the earliest physical chemists.

Following Cavendish, he determined roughly the relative densities of gases by weighing balloons filled with them. He also examined the volume ratios in which a number of pairs of gases combine, but although these were nearly always in the neighbourhood of simple numbers, he did not speculate as to the significance of this. He observed the relative solubilities of gases in water and used these as a means of identifying them.

Priestley measured the expansion of gases by heat in a crude apparatus, but the coefficients he found were not, alas, identical for different gases. However, he noticed that ammonia appeared to have a much greater coefficient than the rest, and he ascribed this rightly to the presence of water in his apparatus and the smaller solubility of ammonia at higher temperatures. He also compared the conductivity of sound in different gases by means of a clockwork bell in a receiver which he filled with them. His conclusion was that " the intensity of sound depends solely on the density of the air in which it is made, and not at all upon any chemical principle in its constitution."

He then compared the conductivity of heat in various gases by means of a thermometer in a glass bulb which he put into hot and cold water and observed the rate of change of temperature when the bulb contained different gases. He found that inflammable air (hydrogen) conducted heat better than any other gas and that fixed air is a worse conductor than common air.

Priestley was the first photo-chemist, as he discovered that sunlight was necessary for the purification of air by a growing plant and he showed the effect of light in decomposing nitric acid. " Light," he said, " besides serving the important purpose of vision is likewise a chemical principle, the effects of which are as yet but little known."

He made use of the electric spark both for making gases enter into combination, *e.g.*, hydrogen and oxygen, and for decomposing them, *e.g.*, ammonia. He tried to compare the electrical conductivity of different substances, and he was one of the first to describe the phenomena accompanying the electrical discharge through gases at low pressures. " The application of electricity to chymistry," he wrote, " is as yet in its infancy. I have made some use of it in the doctrine of air, and Mr. Cavendish has made a most capital one. This should encourage us to apply it with more assiduity."

Having determined the relative densities of different gases Priestley began to speculate as to their behaviour when mixed, and this led him to the first investigation of the problem of gaseous diffusion. " Considering the very different specific gravities, and other remarkedly different properties of different kinds of air," he says, " it might naturally enough be taken for granted that those which differ very much in specific gravity at least, would separate from such other after they were mixed." This conclusion he proceeded to examine by mixing fixed and common air, inflammable and nitrous air, nitrous and common air, nitrous and fixed air, and leaving them to stand in a cylinder. He then expelled portions of the mixtures very carefully by admitting water at the bottom of the cylinder, and tested the composition of the early and later fractions. " The result of my trials has been this general conclusion ; that when two kinds of air have been mixed, it is not possible to separate them by any method of decanting them or pouring them off. . . . They may not properly incorporate, so as to form a new species of air possessed of new properties ; but they will remain equally diffused through the mass of each other." Thus Priestley established the irreversibility of gaseous diffusion.

In spite of the crude methods by which these physical measurements were made, and the inaccuracy of the results, taken as a whole they are a remarkable achievement, especially when we consider how limited was Priestley's general scientific experience. They illustrate once again his courage and initiative as an experimenter. Undoubtedly, too, the evidence that the various gases possessed definite but different physical properties must have helped to establish their individuality in the minds of chemists.

What was the secret of Priestley's genius as an investigator ? His enthusiastic energy and curiosity, his fertility of mind, and the ingenuity and enterprise with which he devised new experiments with the simplest means, his keen observation, his exceptional visual memory and the rapidity with which he could develop a new investigation. And behind everything there was the impelling force of Priestley's love for science, his devotion to experiment, and his boundless faith in the possibilities of new scientific discoveries. Listen to this passage from a letter to Lord Shelburne in 1778 :—

> " My view in advising your Lordship to establish and furnish a laboratory for philosophical purposes was double : First, to accustom Lord Fitzmaurice, at an early age, to the use of philosophical instruments, and the sight of philosophical experiments and processes, in order to do for him, if he should happen to acquire a taste for natural science, what all his fortune will not otherwise be able to do, viz., to make him happy in active and pleasing pursuits at home ; and I know of nothing in the range of human life that can answer this invaluable purpose so well. Mere literary pursuits are generally unfavourable

to health or cheerfulness, though they may contribute to amuse and tranquillise the mind.

"My other view was to prosecute original inquiries under your Lordship's auspices, to indulge my own inclination and ardour in these pursuits, and at the same time to make myself really useful to your Lordship's general fame and character."

After a reference to his new volume of experiments in which he wishes to acknowledge his debt to Lord Shelburne, he concludes :—

"I am the more desirous of doing this, as by some of my other publications I may involve your Lordship in some part of the odium I bring upon myself with the ignorant and narrow-minded. But wherever I go, I must be taken for better and for worse; and I think it the first of duties to that Being who has given me whatever faculties I am possessed of, to pursue and propagate at any risk, all important truth."

This is one of the most revealing passages in all Priestley's writings: "I must be taken for better and for worse . . . to pursue and propagate at any risk, all important truth." We see him here with his frank simplicity and directness of purpose, incapable of compromise, and regardless of personal considerations when principles were at stake.

Forgive me if I quote once again a phrase from Dante, for I know no other that describes so well Priestley's eager restless spirit, fearless and untiring in the defence of liberty and in the quest of truth :—

"con l'ali snelle . . . del gran disio"
Borne on the swiftly beating wings of great desire

H. HARTLEY.