The "Statesman's Year-Book," edited by Dr. J. Scott Keltie, with the assistance of Mr. I. P. A. Renwick, annually improves in character and increases in usefulness. The volume just published by Messrs. Macmillan and Co. is the thirty-fifth; and it contains in the 1166 pages the latest statistical and other data referring to all the States of the world. The special features this year are maps showing, by means of different colours, the distribution of British commerce throughout the world, a map illustrating the Niger question, and a series of coloured diagrams exhibiting the course of trade in leading countries during the past twenty-five years. Trustworthy information upon all questions of political and commercial geography can be obtained from the volume, which keeps its place as the most handy and complete annual of geographical statistics in existence.

The additions to the Zoological Society's Gardens during the past week include a Molucca Deer (*Cervus moluccensis*, ♂) from the Molucca Islands, presented by H.G. the Duke of Bedford; a Great-billed Touracou (*Turacus macrorhynchus*) from West Africa, presented by Mr. R. J. Nicholas; two Cambayan Turtle Doves (*Turtur senegalenis*) from West Africa, presented by Sir Edward Burne-Jones; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Captain Francis W. Bate; two Arctic Foxes (*Canis lagopus*) from the Arctic Regions, four Oyster-catchers (*Hæmatopus ostralegus*), European, purchased; a Caucasian Wild Goat (*Capra caucasica*, ♂, juv.) from the Caucasus, received in exchange; a Burchell's Zebra (*Equus burchelli*, ♀), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

Spectrum Analysis of Meteorites.—A research of great interest has been undertaken by Messrs. W. N. Hartley and Hugh Ramage on the wide dissemination of the rarer elements and the mode of their association in the more common ores and minerals. The outcome of this work has led us to believe that the rarer metals are more widely distributed than was ever dreamt of, the authors showing that out of ninety-one iron ores obtained from the Dublin Royal College of Science, thirty-five contained the extremely rare metal gallium, while most of them contained constituents of an unusual character. Thus rubidium was commonly present: the magnetites invariably contained gallium, but no indium; the siderites all contained indium, but lacked gallium. In a more recent research they have investigated spectroscopically numerous meteoric ores, siderolites and meteorites (*Scientific Proc. of the R. Dublin Soc.*, vol. viii. (N.S.) Part vi., No. 68), the range of spectrum being between the wave-lengths 6000 and 3200, and the results they obtained in this case, arranged in tabular form, are of great interest. It is shown that the composition of different meteoric irons is very similar, though the proportions of constituents differ somewhat. Meteoric irons, different varieties of iron ores, and manufactured irons contain copper, lead, and silver. Gallium is a constituent of meteoric irons, but not of all meteorites, and occurs in varying proportions. Sodium potassium and rubidium are constituents of meteoric irons, but only in very small proportions. Meteoric stones, but not the irons, contain chromium and manganese. Nickel was found to be a principal constituent in all meteorites, meteoric irons, and siderolites, cobalt occurring in the two last varieties. The authors describe the chief points of difference between telluric and meteoric iron to be the absence of nickel and cobalt in any considerable proportion from the former, and the presence of manganese. Meteoric irons, on the other hand, contain nickel and cobalt as notable constituents, and, except in minute traces, manganese is absent. In referring to the photographic spectra of iron meteorites obtained by Sir Norman Lockyer from the Nejed and Obernkirchen meteorites, the authors point out that of the two lines, one described as "unknown," and the other as "doubtfully ascribed to iron," the former is certainly, and the latter probably, a gallium line. At the conclusion of their paper the authors give three plates, which reproduce the flame spectra of six metallic irons and three siderolites with comparison spectra.

Stellar Parallaxes.—Dr. Bruno Peter, during the years 1887 to 1892, made a series of parallax observations with the Leipzig heliometer. The results of this investigation have been published in vol. xxii. No. 4, and xxiv. No. 3, of the *Abhandlungen der Math.-Phys. Classe der K.S. Gesel. der Wissenschaften*; but Dr. Peter makes a short abstract in the *Astronomische Nachrichten*, No. 3483, which we briefly refer to here. In the following table, which brings together these results very clearly, ϵ represents the mean error of the parallax, and ϵ' that for one evening. In the three references to the star Lal 18115, (1) relates to the preceding component, and (2) to the following one, while (3) deals with the pair as a whole. The last column gives the comparison stars employed in each case.

Star.		Proper motion.	Parallax.	ϵ	No. of obs.	ϵ'	Comparison stars.	
	m.	"	"	"		"	°	°
η Cassiopeiæ ...	4	1.20	+0.18	0.030	45	0.15	+57.112	+57.172
μ " ...	5.5	3.74	.13	.037	23	.16	53.207	54.241
Lal 15290 ...	8.5	1.97	.02	.043	32	.16	31.1648	30.1620
Lal 18115 (1) ...	8.0		.18	.027	22	.11		
" (2) ...	8.0		.18	.032	21	.12	53.1309	53.1330
" (3) ...		1.69	.18	.020	43	.11		
δ Ursa Maj. ...	3	1.11	.09	.035	22	.14	52.1389	51.1536
A-Oe. 10603 ...	6.5	1.45	.17	.013	27	.12	50.1707	49.1946
β Comæ ...	4	1.20	.11	.042	42	.18	28.2207	28.2184
31 Aquilæ ...	5.5	0.96	.06	.015	40	.16	11.3802	12.3929
Bradley 3077 ...	6	2.08	.13	.012	39	.14	56.2956	56.2978

JAMES WATT, AND THE DISCOVERY OF THE COMPOSITION OF WATER.[1]

When your Secretary did me the honour to communicate the wish of the Committee that I should deliver this lecture, he was good enough to send me a list of the names of my predecessors in the position I was invited to occupy, together with a statement of the subjects on which they had addressed you. I confess I read his letter with very mingled feelings. To be asked to form one of such a distinguished company was in itself an honour which I deeply appreciated. On the other hand, it seemed well-nigh hopeless to find any theme associated with the life and work of the great man whose services to humanity we are this day called upon to commemorate, that had not been dealt with by one or other of those who preceded me. Naturally, and as befits the subject, the greater number of those who have spoken on these occasions have been distinguished engineers and mechanicians, and they have been able to speak with a fulness of knowledge, and a weight of authority, on the outcome of the great engineer's labours to which I, who know nothing of engineering or machinery, can have no pretensions.

It occurred to me, however, on reflection, that there was one incident in Watt's career, which, so far as I could learn, had not been handled by any one of those whom you have invited to appear here, and to which, as it comes within my own province, I thought I might venture, without presumption, to engage your attention. I was the more impelled to select it in that it illustrates one side of Watt's intellectual activity which those who regard him only as an inventor and a mechanician are apt to undervalue or lose sight of altogether. It serves, too, to throw additional light upon his mental character and moral worth, and thus enables us to form a fuller and more just appreciation of the attributes of the man we wish to honour. The incident, in a word, relates to Watt's share in the establishment of the true view of the chemical nature of water.

To the historian of science this is doubtless an old story, on which it would be difficult to say anything new. The literature concerned with it occupies many volumes, largely owing to the circumstance that it has given rise to a controversy which has engaged the active interest of some of the strongest and subtlest intellects of this century. Some of the disputants have been men like Brougham, Jeffrey and Muirhead, skilled in the arts of advocacy and in the faculty of eliciting and weighing evidence, who have stated their conclusions with all the "pomp and circumstance" of a judicial finding; others are men like Arago, Dumas, Harcourt, Whewell, Peacock, Kopp, George Wilson,

[1] The Watt Memorial Lecture, delivered in the Watt Memorial Hall, Greenock, on March 11, by Prof. T. E. Thorpe, LL.D., F.R.S.

eminent in science and literature. who have defended their convictions with great power, ample knowledge, much argumentative force, and occasional eloquence. At one time the contest was waged with no little fury and bitterness; it threatened, indeed, like the famous controversy on the proper form of a lightning-conductor during Sir John Pringle's presidency of the Royal Society, or like the equally famous controversy on the discovery of the planet Neptune, to attain the dignity of a national question, far more acute, I should imagine, than that which has just occasioned all right feeling Scotchmen to approach the Queen in Council on the subject of Scotland's proper place and designation in Imperial concerns.

But the acrimony and ill-feeling have happily long since passed away. There is no longer any need to discuss the question either as an advocate or as a partisan. What I shall attempt to-night is to treat it dispassionately, and, within the compass of an hour, to assess, as impartially as I am able, Watt's true place in regard to this discovery.

It was, indeed, an epoch-making event. The discovery of the composition of water was as momentous for science as the greatest of Watt's inventions was for social and economic progress. The very fact itself, apart from all that flowed from it, was of transcendent interest. But to those who had eyes to see, its supreme importance was in its fruitful and far-reaching consequences. It signified nothing less than the passing away of an old order of things, the downfall of a system of philosophy which had outlived its usefulness, in that it no longer served to interpret natural phenomena, but which was rather a hindrance and a stumbling-block to the perception of truth. The discovery at once led to the inception of a more rational and more truly comprehensive theory, which not only explained what was already known, in a fuller, clearer and more intelligible manner, but pointed the way to new facts hitherto undreamt of, which, in their turn, served to strengthen and extend the generalisation which led to their discovery. No wonder, then, that those who loved and revered Watt, and who were rightly jealous of his honour, should have sought to do all in their power to vindicate what they honestly conceived to be his just title to so signal and so fundamental a discovery.

No man has a juster claim to be regarded as a scientific man, in the truest and noblest sense of that term, than James Watt. The scientific spirit was manifest in him even in boyhood. The very circumstances of his condition, his weakly frame, the solitariness of his school-life, and the early habits of introspection thus induced in a mind forced to feed only on itself, served to strengthen and develop the instinct. Even his early struggles, and the jealousy of the Glasgow Guilds which forbade him to practise his trade in the burgh in which he had not served an apprenticeship, conduced to mould his character and to determine the bent of his mind. Hard and illiberal as it seemed at the time, the *Zunftgeist* which drove him to the shelter of the old College in the High Street, and secured for him the abiding friendship of Black and Robison, was in reality the most fortunate circumstance in his career. It brought him directly under the influence of one of the greatest natural philosophers of his age, and so stamped him permanently as a man of science. It would not be difficult to trace how this influence reacted upon all that Watt subsequently did—from the time of his earliest speculations on the loss of energy in Newcomen's engine down to the very last of his mechanical pursuits in the dignified retirement of Heathfield Hall. He approached the question of the improvement of the steam-engine as a scientific problem, and under the direct inspiration of the doctrine of the great discoverer of the principle of latent heat. It was this same mental attitude towards scientific truth, the same receptivity for scientific doctrine, the same love of pondering over and speculating upon the true inwardness of things that brought him the friendship of Priestley, Withering, Wedgwood and De Luc, and that ultimately made him a cherished member of the foremost scientific academies of the world. It will occasion little surprise to one who has formed a true perception of his character to learn that Watt was wont, even at periods of great mental depression, and of physical suffering, amidst all the toil and anxious worry of a business surrounded with difficulties, to find peace in the contemplation of natural phenomena, and to spend time in philosophical speculation. The shrinking, diffident man, in thus communing with himself and with nature, followed a true and constant impulse to withdraw from the strife and turmoil of the world, and to seek his pleasure and his rest in the silent contemplation of natural truth. No one can look upon that contemplative face without being struck with its expression of philosophic calm. What deep, genuine pleasure these communings brought to the harassed man may be gleaned from his correspondence. In truth, nature intended Watt to be a philosopher of the pattern of Boyle, or Newton, or Dalton; it was destiny that drove him into the world of affairs where, as he said, he was out of his sphere. It is necessary to dwell for a moment on this aspect of Watt, in order to form a just appreciation both of his position and of his merits in regard to the great chemical truth with which his name is associated.

The man of action is apt to regard the contemplative mind with something akin to contempt. I once heard a bustling, busy man, the head of a large engineering establishment, who had enjoyed the good fortune to be a pupil of Thomas Graham, say of that distinguished philosopher that he was the laziest man he had ever met. He did not say he "ever knew"—for how little he really knew of Graham was evident from the fact that at the period to which he referred Graham's thoughts were deeply occupied with some of the most memorable of his investigations.

It was in one of these contemplative moods—in what he himself styled his periods of excessive indolence—and as it happened at the very time that the Soho firm was struggling to protect itself against the unprincipled horde that was seeking to infringe Watt's fundamental patent, that he occupied himself with turning over in his mind the outcome of one of his friend Priestley's multitudinous experiments. Watt had long held the view that air was a modification of water, or, as he expressed it in a letter to his friend Black, under date December 13, 1782, that, "as steam parts with its latent heat as it acquires sensible heat, when it arrives at a certain point it will have no latent heat, and may, under proper compression, be an elastic fluid nearly as specifically heavy as water": at which point he conceived it would again change its state and become air. As he then relates, he sees a confirmation of this opinion in an experiment of Priestley's made, as he says, "in his usual way of groping about." "As he [Priestley] had succeeded in turning the acids into air by heat only, he wanted to try what water would become in like circumstances. He undersaturated some very caustic lime with an ounce of water, and subjected it to a white heat in an earthen retort. . . . No water or moisture came over, but a quantity of air, equal in weight to the water . . . a very small part of which was fixed air, and the rest of the nature of atmospheric air. . . . He has repeated the experiment with the same result."

About a fortnight later Priestley wrote that he was able to convert water into air "without combining it with lime or anything else, with less than a boiling heat, in the greatest quantity, and with the least possible trouble or expense." He added that "the method will surprise more than the effect," but that he would defer "the communication of the hocus pocus of it" until such time as Watt should give him the pleasure of his company in return for the pleasure he was to give Watt in speculating on the subject.

These experiments, as we shall see in due course, were wholly fallacious; in following them up with his wonted ardour, Priestley quickly found himself in a maze of contradictions, and ultimately discovered that this seeming conversion was absolutely mythical.

It may be useful, however, to make one or two comments on these passages at the present juncture. In the first place Watt's opinion as to the relation of water and air, although founded, as he thought, upon a more philosophical basis, simply embodied the teaching of the schoolmen. The notion that the so-called four elements were mutually convertible, or were in essence identical, ran through the doctrine of twenty centuries of teachers. Despite the onslaughts of the Spagyrists, and the author of the "Sceptical Chymist," it permeated the literature of natural philosophy down to the very beginning of this epoch. Watt was insensibly swayed by a belief which had descended to him, like the undying germ, through the ages, and he could no more shake himself free of it than he could get rid of the influence of heredity. The very mode in which he, in common with men of his time, uses the term "air," is an indication of the manner in which the ancient creed limited and cramped his thought. He knew that there were various "airs," but it is very doubtful if he realised that they were essentially different substances. There is abundant evidence in the few chemical papers that he published, and especially in his letters to Black, Priestley, De Luc, Kirwan and others, that he regarded them all as constituted of the same matter, affected by attributes more or less fortuitous and accidental. Thus, all the varieties of inflammable air were at bottom identical, with properties modified by their origin or their varying content of the hypothetical principle phlogiston—that is the principle that was assumed to make them burn.

From Watt's published correspondence we are able to judge how he regarded Priestley's further work on this so-called conversion of water into air. He admits that the facts are "in some degree contradictory to each other." The apparent conversion would seem to depend upon the material of the vessel in which it was made. In a glass vessel no air was produced, nor was any found in a gun-barrel when the distillation was done slowly; but when confined by a cock, "and let out by puffs, it produces much air; which," says Watt, "agrees with my theory, and also coincides with what I have observed in steam-engines. In some cases I have seen the tenth of the bulk of the water, of air extricated or made from it." Davy once said "the human mind is governed not by what it knows, but by what it believes; not by what it is capable of attaining, but by what it desires." However willing to catch at anything in support of his belief, it is possible that Watt might have been led to doubt the soundness of Priestley's experiment, if an apparent and wholly unlooked for confirmation of it had not arisen.

To make the account exact, and in view of what is to follow, it is necessary to go back a little, in point of time. In the spring of 1781, Priestley performed what he styled "a mere random experiment made to entertain a few philosophical friends." It was practically a repetition of Volta's experiment of firing a mixture of the inflammable air from metals, that is, hydrogen, with common air in a closed glass vessel by means of the electric spark. After the deflagration the vessel was found to be hot, and on cooling its sides were observed to be bedewed. Neither Priestley nor any of his philosophical friends seem to have paid particular attention to the deposit of moisture, or, at all events, if they did they failed to perceive its significance. One of them, however, Mr. John Warltire, a lecturer in natural philosophy in Birmingham, imagined that the experiment might afford the means of showing whether heat was ponderable or not; and accordingly he repeated it, using for greater safety a copper globe, weighed before and after the passage of the spark. A minute loss of weight was always noticed, "but not constantly the same; upon the average it was about 2 grains."[1]

Priestley, who, with Withering, was present when the experiments were made, confirmed the apparent loss of weight; but he added, with a caution that was not characteristic, that he did not think "that so very bold an opinion as that of the latent heat of bodies contributing to their weight should be received without more experiments, and made upon a still larger scale."

Priestley's volume—the sixth in the series—was published in 1781, and was certainly known to Watt; indeed, in the Appendix are printed a number of observations made by him apparently as the work was passing through the press. Although, therefore, he must have had his attention drawn about this time to the formation of the dew in Priestley and Warltire's experiment, there is nothing to show that he attached any importance to the circumstance, or that, if he did, he dissented from Warltire's conclusion that common air deposits its moisture when it is phlogisticated.

For some time previous to the publication of Priestley's book, Mr. Cavendish was engaged upon an inquiry "to find out the cause of the diminution which common air is well known to suffer by all the various ways in which it is phlogisticated, and to discover what becomes of the air thus lost or condensed." In other words, it was an investigation to determine the changes experienced by air when bodies were made to burn in confined portions of it. On the appearance of Priestley's book he repeated Warltire's experiment, thinking "it worth while to examine more closely, as it seemed likely to throw great light on the subject I had in view." He confirmed the observation on the formation of dew; but although he made the experiment on a larger scale, and with varying proportions of the two airs, he was unable to satisfy himself as to the loss of weight after the explosion. As the result of a number of trials, made both with the inflammable air from zinc and from iron—that is, hydrogen—and mixed with common air in the proportion of 423 measures of the inflammable air to 1000 of common air, he says, "we may safely conclude that when they are mixed in this proportion, and exploded, almost all the inflammable air and about one-fifth part of the common air lose their elasticity, and are condensed into the dew which lines the glass." In order to examine the nature of this dew, large quantities of the hydrogen were burnt with two and a half times its volume of common air, and the product of the combustion was caused to pass through a long glass tube whereby it was condensed. "By this means 135 grains of water were condensed in the cylinder [*i.e.* the tube], which had no taste nor smell, and which left no sensible sediment when evaporated to dryness; neither did it leave any pungent smell during the evaporation; in short, it seemed pure water. . . . By the experiments with the globe, it appeared that when the inflammable and common air are exploded in a proper proportion, almost all the inflammable air and nearly one-fifth of the common air, lose their elasticity, and are condensed into dew. And by this experiment it appears that this dew is plain water, and consequently that almost all the inflammable air and about one-fifth of the common air are turned into pure water."

The idea that common air was for the most part a mixture of two gases—oxygen or the dephlogisticated air of Scheele and Priestley, and nitrogen or the mephitic air of Rutherford, the azote of Lavoisier—was familiar to chemists at this period as the result of the teaching of Scheele and Lavoisier, and there is reason to suppose that this opinion was shared by Cavendish. He had been engaged for some time past in an elaborate inquiry into the constitution of atmospheric air, the results of which admitted of no other interpretation than that common air was composed of two different gases, mixed or combined in constant relative proportions. It is true that in the memoir containing the results of his inquiry he nowhere directly gives his estimate of these relative quantities, but, from the data he affords, it is easy to deduce the amount and the constancy of the proportion. Cavendish's papers are characterised by remarkable conciseness and brevity; an experiment which must have involved the putting together of elaborate and complicated apparatus, and which must have occupied considerable time in its performance, is described in a few lines, and hence it is not always possible to gather with certainty the precise disposition of the arrangements. He never sets out his reasons or his conclusions with any great amount of detail, and his published words occasionally give little indication of his line of thought. But that he clearly recognised that only one portion of common air was concerned in the formation of water, and that this portion was the dephlogisticated air, or oxygen, is obvious from the next series of experiments in which he fired a mixture of about two measures of hydrogen and one measure of oxygen in a previously exhausted glass globe furnished with an apparatus for firing air by electricity. When the included air was fired, almost all of it lost its elasticity, so that fresh quantities of the explosive mixture could be introduced and the process repeated until a sufficient quantity of the moisture was obtained for examination. In these experiments Cavendish clearly and definitely demonstrated that the weight of the water was practically equal to the weight of the mixed gases which had combined to form it. In some cases the water was perfectly neutral in its reaction; in others it was slightly acid, and the cause of this acidity caused Cavendish much experimenting, but he is never in any doubt as to the main result; he says distinctly, "if those airs could be obtained perfectly pure, the whole would be condensed." Now if Cavendish had published this main result at the time he obtained it, namely in the summer of 1781, or even if he had formally communicated it to one of the meetings of the Royal Society during the ensuing session, there would have been no Water Controversy. But even if he were ready, it was characteristic of him to delay, not from inertia or indolence, but from a morbid shyness, an unconquerable reticence, which constantly led him to postpone any public announcement of his work. He had the additional, and to him all-sufficient, reason that he had not yet worked out the cause of the occasional acidity of the water. What he did, however, was to communicate the facts of his experiments to Priestley, as Priestley himself states in a subsequent paper published in the *Philosophical Transactions* for 1783. When or how he communicated them to Priestley does not appear, nor have we any means of knowing precisely what was said. Something, however, on this point may be inferred from what

[1] The account of these experiments is given in a letter to Priestley, and constitutes No. v. of the "Appendix to Priestley's Experiments and Observations relating to various branches of Natural Philosophy, &c.," vol. ii. (Birmingham, 1781)

Priestley proceeded to do. It appears from a letter to Wedgwood that he repeated Cavendish's experiment during the March of 1783. It will be remembered that he was at this period engaged on his experiments on the seeming conversion of water into air. He had obtained a number of contradictory results which had led Wedgwood, as far back as the previous January, to put certain sagacious queries, which doubtless in the end had their effect in opening Priestley's eyes to the origin of his mistake. But at the time both he and Watt were seeking for fresh evidence to substantiate the possibility of this conversion. Now just as Cavendish thought that Warltire's experiment might throw light upon the particular matter on which he was engaged, so Priestley considered that Cavendish's work might afford evidence, indirect it is true, but still evidence, of the intimate connection between water and air. Cavendish had, he thought, established the converse of the proposition which he and Watt were seeking to prove in showing that "air," or rather certain kinds of "air," could be converted into water weight for weight. It was no longer the original Warltire experiment of exploding common air and hydrogen. Cavendish had indicated the particular kinds which were really concerned in the phenomena, and it was the Cavendish experiment, pure and simple, which he proceeded to repeat. This is obvious from what he says: "Still hearing of many objections to the conversion of water into air, I now gave particular attention to an experiment of Mr. Cavendish's concerning the *reconversion* of air into water by *decomposing* it in conjunction with inflammable air." Priestley here used the word "decomposing" in a sense contrary to that which the context implies; but that he is consistent in so using it is evident from what follows, and also from similar expressions to be found in his correspondence. But although he professed to repeat Cavendish's experiment, he neglected to do so in Cavendish's manner. He says: "In order to be sure that the water I might *find in the air was really a constituent part of it*, and not what it might have imbibed after its formation [*i.e.* by contact with the water of the pneumatic trough], I made a quantity of both dephlogisticated and inflammable air, in such a manner as that neither of them should ever come into contact with water, receiving them as they were produced in mercury; the former from nitre, and in the middle of the process (long after the water of crystallisation was come over), and the latter from perfectly made charcoal. The two kinds of air thus produced I decomposed by firing them together by the electric explosion, and found a manifest deposition of water, and to appearance in the same quantity as if both the kinds of air had been previously confined by water.

"In order to judge more accurately of the quantity of water so deposited, and to compare it with the weight of the air decomposed, I carefully weighed a piece of filtering-paper, and then having wiped with it all the inside of the glass vessel in which the air had been decomposed, weighed it again, and I always found, as nearly as I could judge, the weight of the decomposed air in the moisture acquired by the paper. . . . I wished, however, to have had a nicer balance for the purpose: the result was such as to afford a strong presumption that the air was reconverted into water, *and therefore that the origin of it had been water.*"

These passages, when compared with the accounts given of his own work by Cavendish, strikingly exemplify the difference in the character of the two experimentalists. It would be difficult to pack a greater number of errors into a couple of paragraphs than are contained in these sentences. The expressions in italics show that Priestley wholly failed to comprehend the true origin of the water. In his laudable anxiety to free the two gases from extraneous moisture, he committed blunder after blunder. His method of obtaining the oxygen was bad; that of procuring the inflammable air was worse. Both the gases must have been highly impure, and it was a physical impossibility that they should have given their aggregate weight in water, even after making every allowance for Priestley's crude and imperfect method of determining it.

Bad, however, as the experimental work was, what it appeared to teach was not lost on Watt: it clearly proved to him that water and air were mutually convertible. How the theory took shape in his mind is evident from the terms in which the two series of Priestley's experiments are coupled together in his letters to Gilbert Hamilton, to De Luc and to Black. Each set is regarded as complementary to the other, and, both taken together, are held to prove that air and water are mutually convertible, and are therefore essentially the same. Under date April 21, 1783, he tells Black that "Dr. Priestley has made more experiments on the conversion of water into air, and I believe I have found out the cause of it; which I have put in the form of a letter to him, which will be read at the Royal Society with his paper on the subject." He then proceeds to give Black a summary of the three sets of facts, or supposed facts, on which he bases his generalisation; and he makes use of these significant words: "In the deflagration of the inflammable and dephlogisticated airs, the airs unite with violence—become red-hot—and on cooling, totally disappear. The only fixed matter which remains is *water*; and *water*, *light* and *heat* are all the products. 'Are we not, then, authorised to conclude that water is composed of dephlogisticated and inflammable air, or phlogiston, deprived of part of their latent heat, and that dephlogisticated, or pure air, is composed of air deprived of its phlogiston, and united to heat and light; and if light be only a modification of heat or a component part of phlogiston, then pure air consists of water deprived of its phlogiston and its latent heat." Very similar turns of expression and trains of reasoning are to be met with in other letters to his friends, written at about the same period. In all it is abundantly clear that, whatever may have been his surmises as to the real nature of water, it was the conception of the mutual convertibility of air and water that was uppermost in his mind. These passages, however, constitute Watt's claim to be regarded as the true and first discoverer of the compound nature of water.

Three days after the letter to the Royal Society was written, or rather dated, there came a bolt from the blue in the form of a letter from Priestley to Watt. "Behold," it said, "with surprise and with indignation the figure of an apparatus that has utterly ruined your beautiful hypothesis, and has rendered some weeks of my labour in working, thinking, and writing almost useless." The doubts of Wedgwood, certainly no mean authority on the properties of baked clay, had, in fact, led Priestley to devise an experiment by which it was proved beyond all doubt that this seeming conversion of water into air was really due to an interchange of steam and air, effected by diffusion through the porous material of the retort. Well might Priestley cry to De Luc, "We are undone!" Watt's faith in the "beautiful hypothesis" was no doubt rudely shaken, but it was not shattered. In his answer to Priestley he denied that it was ruined: "It is not founded," said he, "on so brittle a basis as an earthen retort." Priestley, however, would have none of it: theories with him—always excepting the all-comprehensive one of phlogiston, which was the head and front of his creed, as, indeed, of his subsequent offending—had at no time much value, for, as Marat said of Lavoisier, he abandoned them as readily as he adopted them, changing his systems as he did his shoes. Indeed, he rather prided himself on his capacity for quick change. "We are, at all ages," he once said, "but too much in haste to *understand*, as we think, the appearances that present themselves to us. If we could content ourselves with the bare knowledge of new facts, and suspend our judgment with respect to their causes, till by their analogy we were led to the discovery of more facts, of a similar nature, we should be in a much surer way to the attainment of real knowledge." With a candour all his own, he immediately added: "I do not pretend to be perfectly innocent in this respect myself; but I think I have as little to reproach myself with on this head as most of my brethren; and whenever I have drawn general conclusions too soon, I have been very ready to abandon them. . . . I have also repeatedly cautioned my readers, and I cannot too much inculcate the caution, that they are to consider new *facts* only as discoveries, and mere *deductions* from these facts, as of no kind of authority; but to draw all conclusions, and form all hypotheses, for themselves."

Watt's mind was of a very different cast. He did not lightly adopt opinions; his convictions were slowly and deliberately formed, and were retained with a corresponding tenacity. But, all the same, he eventually thought it prudent to withdraw his letter; and three days prior to the reading of Priestley's paper, which accompanied it, Priestley informed Sir Joseph Banks of Watt's desire that the letter should not be publicly read. That it was withdrawn on account of what Watt calls Priestley's "ugly experiment," is stated by him in a letter to Black, on the ground that this experiment rendered "the theory useless in so far as relates to the change of water into air. . . . I have not given up my theory [that is, as to the mutual convertibility of water into air], though neither it nor any other known one will account for this experiment."

In the meantime Cavendish had been pursuing his inquiries, and towards the end of this year (1783) he was prepared to give the explanation of the cause of the disturbing factor in his proof of the real nature of water—that is, the origin of the occasional and apparently haphazard presence of small quantities of nitric acid. This he demonstrated to be due to the difficulty of excluding a greater or less quantity of atmospheric nitrogen from the gases employed; and he determined the conditions under which this nitrogen led to the formation of the acid, the true nature of which he thus for the first time established. The account of his labours was read to the Royal Society on January 15, 1784.

In the previous autumn, however, disquieting rumours reached this country that the French philosophers, and chief among them Lavoisier, were poaching upon the English preserves. The circumstance is alluded to in a letter from Watt to De Luc, dated November 30, 1783. "I was at Dr. Priestley's last night. He thinks, as I do, that Mr. Lavoisier, having heard some imperfect account of the paper I wrote in the spring, has run away with the idea and made up a memoir hastily, without any satisfactory proofs. . . . I, therefore, put the query to you of the propriety of sending my letter to pass through their hands to be printed; for even if this theory is Mr. Lavoisier's own, I am vain enough to think that he may get some hints from my letter, which may enable him to make experiments, and to improve his theory, and produce a memoir to the Academy before my letter can be printed, which may be so much superior as to eclipse my poor performance and sink it into utter oblivion; nay, worse, I may be condemned as a plagiary, for I certainly cannot be heard in opposition to an Academician and a financier. . . . But, after all, I may be doing Mr. Lavoisier injustice."

That Lavoisier did get some hints, and possibly even through the medium of Watt's letter, is beyond all question. The fact that he was informed of Cavendish's work is specifically stated in Cavendish's memoir in a passage interpolated by Blagden, the Secretary of the Royal Society and Cavendish's assistant and amanuensis, who himself told Lavoisier. The whole of the circumstances are set out in detail in a subsequent letter which Blagden addressed to the editor of the *Chemische Annalen* in 1786. That it was known to be Cavendish's experiment that was being thus repeated, is confirmed by a letter from La Place to De Luc, dated June 28, 1783, in which we read: "Nous avons répété, ces jours derniers Mr. Lavoisier et moi, devant Mr. Blagden, et plusieurs autres personnes, l'expérience de Mr. Cavendish sur la conversion en eau des airs dephlogistiqués et inflammables, par leur combustion. . . . Nous avons obtenu de cette manière plus de 2½ gros d'eau pure, ou au monis qui n'avoit aucun caractère d'acidité, et qui étoit insipide au goût; mais nous ne savons pas encore si cette quantité d'eau représente le poids des airs consumes; c'est une expérience à recommencer avec toutes l'attention possible et qui me paroit de la plus grande importance." The phrase "qui n'avoit aucun caractère d'acidité" is of special significance. The French philosophers, and Lavoisier in particular, could with difficulty, as Blagden relates, be brought to credit the statement that when inflammable air was burnt, water only was formed; their preconceptions concerning the part played by oxygen in such a case, led them to suppose that an acid would be produced. Cavendish was familiar with Lavoisier's doctrine, which is connoted in the very word oxygen, which we owe to the French chemists; and it may be that this circumstance was, amongst others, one cause of the pains he took to understand the origin of the acid he occasionally met with. Lavoisier was led to repeat Cavendish's experiment on June 24, 1783; and on the following day he announced to the Academy that by the combustion of inflammable air with oxygen "very pure water" was formed. It is this statement that has been said to constitute Lavoisier's claim to be considered as the true and first discoverer of the composition of water. That he has no valid claim has been implicitly admitted by Lavoisier himself. The eminent Perpetual Secretary of the French Academy, M. Berthelot, is no doubt accurate in regarding June 25, 1783, as the first certain date of publication of the discovery that can be established by authentic, *i.e.* official, documents; but, as I have elsewhere attempted to show, the circumstances under which that priority of publication was secured give Lavoisier no moral right to the title of the discoverer.[1]

Shortly after the reading of Cavendish's memoir to the Royal Society (January 15, 1784), De Luc wrote to Watt, giving an account of its contents, and insinuating that its conclusions had been formed in the light of knowledge obtained from Watt's letter to the Royal Society, which although, as we have seen, not publicly read, had, there is no doubt, been perused by others than Priestley, to whom it was originally addressed. De Luc was, no doubt, a zealous friend, but in this letter his zeal outran his discretion. The letter was, indeed, unworthy of him. He hastens to exculpate Lavoisier and La Place, but makes a charge against the honour and integrity of Cavendish, for which there was absolutely no justification. He stirs up Watt's suspicions, and then seeks to appease them; he rouses his anger, and then counsels him to silence by an argument which shows how wholly he misunderstood Watt. Watt's reply was characteristic: "On the slight glance I have been able to give your extract of the paper, I think his theory very different from mine; which of the two is the right I cannot say: his is more likely to be so, as he has made many more experiments, and consequently has more facts to argue upon. . . .

"As to what you say of making myself *des jaloux*, that idea would weigh little; for were I convinced I had had foul play, if I did not assert my right, it would either be from a contempt of the modicum of reputation which could result from such a theory: from a conviction in my own mind that I was their superior: or from an indolence, that makes it easier for me to bear wrongs than to seek redress. In point of interest, in so far as connected with money, that would be no bar; for though I am dependent on the favour of the public, I am not on Mr. C. and his friends; and could despise the united power of *the illustrious house of Cavendish*, as Mr. Fox calls them.

"You may, perhaps, be surprised to find so much pride in my character. It does not seem very compatible with the diffidence that attends my conduct in general. I am diffident, because I am seldom certain that I am in the right, and because I pay respect to the opinion of others, where I think they may merit it. At present *je me seus un peu blessé*; it seems hard that in the first attempt I have made to lay anything before the public, I should be thus anticipated."

There was no desire on the part of anybody connected with the management of the Royal Society to withhold from Watt his just due; and it was eventually arranged that his letter to Priestley, together with one he subsequently addressed to De Luc, should be publicly read to the Fellows, and they were subsequently ordered to be printed in the *Philosophical Transactions* in such manner as their author might desire. By his directions the two letters were merged together, and they appear as having been read on April 29, 1784, under the title, "Thoughts on the constituent parts of water, and of Dephlogisticated air: with an account of some experiments on that subject. In a letter from Mr. James Watt, Engineer, to Mr. De Luc, F.R.S." The greater part of the "thoughts" are concerned with the dephlogisticated air. What relate to water have already been given in the extracts from his correspondence. The terms in the letter to De Luc, as printed in the *Philosophical Transactions*, are substantially identical with those of the letters to Black, Hamilton, Smeaton and Fry.

I have now given all the essential facts which led to the recognition of the true chemical nature of water, and I have stated, as accurately and as impartially as I could, the relative share of Watt, Cavendish and Lavoisier in their discovery and interpretation. As regards Lavoisier, it cannot be claimed that he was the first to obtain the facts. To Cavendish belongs the merit of having supplied the true experimental basis upon which accurate knowledge could alone be founded. Watt, on the other hand, although reasoning from imperfect and, indeed, altogether erroneous data, was the first, so far as we can prove from documentary evidence, to state distinctly that water is not an element, but is composed, weight for weight, of two other substances, one of which he regarded as phlogiston and the other as dephlogisticated air. It would be a mistake, however, to suppose that Watt taught precisely the same doctrine of the true nature of water that we hold to-day. Nor did Cavendish utter a more certain sound. What we regard to-day as the expression of the truth we owe to Lavoisier, who stated it with a directness and a precision that ultimately swept all doubt and hesitation aside—except to the mind of Priestley, whose "random experiment" gave the first glimmer of the truth.

In this respect the conclusion of Lord Brougham is most just. It was a reluctance to give up the doctrine of phlogiston, a kind of timidity on the score of that long-established and deeply-

[1] Priestley, Cavendish, Lavoisier, and "La Révolution Chimique": the Presidential Address to the Chemical Section of the British Association, 1890; see also "Essays in Historical Chemistry" (Macmillan, 1891).

rooted opinion that prevented Watt and Cavendish from doing full justice to their own theory; while Lavoisier, who had entirely shaken off these trammels, first presented the new doctrine in its entire perfection and consistency.

We thus see that each of these eminent men played an independent and, we may say, an equally important share in the establishment of one of the greatest scientific truths that the eighteenth century brought to light.

As regards Watt, the history of this incident serves to bring out only more clearly what we know to be the true character of the man. It illustrates the vigour of his intellectual grasp, the keenness of his mental vision. At the same time it exhibits his love of truth for truth's sake; his unaffected modesty, and the sense of humility that was not the less real because accompanied by a sense of what his inherent love of rectitude taught was due also to himself. The voice of envy and detraction has not been unheard amongst the strife of partisans in the Water Controversy, but throughout it no syllable has been breathed that reflected even remotely upon his honour and integrity.

SCIENTIFIC SERIALS.

SEVERAL contributions of anthropological interest appear in the January and February issues of *Globus*.—An old Mexican terra-cotta figure in the American Museum of Natural History is described and figured. It was discovered near Texcoco, and represents a warrior in a padded coat of mail. The figure is of life-size, and its workmanship is peculiar to Mexican antiquities.—A description of the temple-pyramid of Tepoztlan, by Dr. E. Seler, contains not only interesting details, but several very good illustrations of the plan and construction of the temple. Tepoztlan is the place where the Mexican kings had their famous pleasure gardens, and the inhabitants have preserved their ancient language and many of their old customs in their mountain home. The temple lies 2000 feet above the town on a cliff. The ruins consist of several buildings of all kinds and sizes, which are suggested to have been the dwellings of the priests. The temple itself has massive walls built of black and red volcanic stone. The inner space is divided into two rooms by a door let in a thick wall. In the inner room was found a rectangular cavity containing coal and two pieces of copal, showing probably that here was the place where the holy fire was burnt. The door leading to the inner room is flanked by two pillars, richly carved, but the most interesting feature of the room is its benches of sculptured stone. In this room stands an idol, and there were found two pieces of sculpture: one a bas-relief painted in dark red, the other a relief of a Mexican king's crown. Altogether, this is a notable discovery; and if it is really the fact that these people have preserved their ancient culture, it is greatly to be hoped that a scientific exploration will be undertaken before it is too late.—Another people of South America is noted in a paper by Dr. Ehrenreich on the Guayaki in Paraguay. Their territory is bounded on the east and south by Parana, on the north by the rivers Acaray and Monday, and on the west by well-wooded hills. Very little is known about them, and only few ethnographical specimens have found their way into museums. The personal possessions of the people consist of a conical-shaped cap made out of a jaguar skin, chains made of pierced teeth and bones of animals, stone axes, bows and arrows, lances made out of the bark of the palm, and a sharp instrument made out of animal bones. Their vessels are particularly remarkable. Some are egg-shaped, and obviously intended to fix in the ground, and most of them belong to the so-called basket pottery. Several illustrations accompany the paper, including three photographs of a Guayaki man. He is very short, with strikingly short legs, long arms, broad shoulders, short neck and large head. They live entirely as huntsmen, without any tillage, and the very primitive character of the race suggests that they, and possibly other tribes on the boundary line of Brazil, would reveal much information of value to the anthropologist.—An account of the Moplahs of the coast of Malabar, by Dr. Emil Schmidt, is exceedingly useful. They are partly of Hindoo and partly of Arabian origin, and the mixture is shown in their customs. In the north the young husband settles in his wife's house, and the woman's right of succession is admitted; in the south, male succession is the rule. A careful study of these mixed peoples is much needed.—Dr. Nehring gives an account of the worship of the ringed snake among the old Lithuanians, Samoyitians and Prussians.—A paper by Mr. C. G. Hoffman, on the Niggers of Washington, contains some notes on the curious superstitious practices of the Voodoo, said to be a survival of the old religion.—Mr. Christian Jensen's paper on the grave mounds and giants' graves in the islands of North Friesland, contains information of special interest to English folk-lorists who have followed Mr. MacRitchie's ingenious explanation of some fairy beliefs.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, March 10.—"On the Relative Retardation between the components of a Stream of Light produced by the passage of the Stream through a Crystalline Plate cut in any direction with respect to the Faces of the Crystal." By James Walker.

If the surface of the plate be the plane of xy, the positive axis of z being directed inwards, the relative retardation is $T(n_1 - n_2)$, where the velocity of light in air is unity, T is the thickness of the plate, and n_1, n_2 are the positive roots of a biquadratic in n obtained by expressing that $lx + my + nz = 1$ is a tangent plane to the wave-surface. Writing the roots of the biquadratic as series proceeding by powers of sin i, and expressing the coefficients (which are linear functions of sin i) as symmetrical functions of the roots, the terms of the series may in general be determined in succession by means of linear equations, and have the form $\pm\alpha' + \gamma$, $\pm\alpha'' - \gamma$, where

$$\alpha = a_0 + a_1 \sin i + a_2 \sin^2 i + a_4 \sin^4 i + \ldots,$$

and

$$\gamma = \gamma_3 \sin^3 i + \gamma_5 \sin^5 i + \ldots,$$

while the relative retardation is

$$T(\alpha' - \alpha'' + 2\gamma).$$

This method fails when the plate is perpendicular to an optic axis, in which case the biquadratic may be written

$$n^4 + (c_0 + c_2 \sin^2 i)n^2 + b_3 \sin^3 i\, n + a_0 + a_2 \sin^2 i + a_4 \sin^4 i = 0.$$

Neglecting the coefficient of n, the roots are

$$\pm(\pi + \rho),\ \pm(\pi - \rho),$$

π and ρ being series proceeding by even and odd powers of sin i respectively. Assuming that the actual roots are

$$\pi + \rho + \alpha,\ -\pi - \rho + \beta,\ \ldots$$

the successive terms of the series α, β, γ, δ are determined as in the former method, and, as for terms of the fourth order, have the form

$$\alpha = -\gamma = a_2 \sin^2 i + a_3 \sin^3 i + a_4 \sin^4 i,$$

$$\beta = -\delta = a_2 \sin^2 i - a_3 \sin^3 i + a_4 \sin^4 i,$$

so that

$$\Delta = 2T(\rho + \alpha).$$

Geological Society, March 23.—W. Whitaker, F.R.S., President, in the chair.—The Eocene deposits of Devon, by Clement Reid. A re-examination of the area around Bovey has led the author to think that Mr. Starkie Gardner is probably right in referring the supposed Miocene strata to the Bagshot period. Lithologically as well as botanically the deposits in Devon and Dorset agree closely. The gravelly deposits beneath the Bovey pipeclays are also shown to belong to the same period, and not to be of Cretaceous date. This correction has already been applied by Mr. H. B. Woodward to a large part of the area. The plateau gravels capping Haldon are also considered to belong to the Bagshot period, for they correspond closely with the Bagshot gravels of Dorset to the east, and of the Bovey Basin to the west, and possess peculiarities which distinguish them from any Pleistocene Drift. Several speakers took part in a discussion upon the paper, some agreeing with the author's views, and some were opposed to them.—On an outlier of Cenomanian and Turonian near Honiton, with a note on *Holaster altus*, Ag., by A. J. Jukes-Browne. Although an outlying patch of chalk in the parish of Widworthy was mentioned by Fitton and marked on De La Beche's map, it has not hitherto been described. The tract is about 4½ miles south-west of Membury, 3½ miles east of Honiton, and about 7 miles from the coast at Beer Head.—Cone-in-cone: additional facts from various countries, by W. S. Gresley. Examples of flinty stone in the "fire-clay series" of the Ashby coalfield exhibit "areas of conic structure lying unconformably." In the same stratum of shale are large masses of the same flinty rock, more or less coated with