The "Statesman's Year-Book," edited by Dr. J. Scott Kelbie, with the assistance of Mr. I. P. A. Renwick, annually improves in character and increased point. 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 Molucce Deer (Cerbus moluccensis, z) from the Molucca Islands, presented by H.G. the Duke of Bedford; a Great-billed Turtaco (Turtaco madagascariensis) from West Africa, presented by Mr. R. J. Nicholas; two Cambayan Turtle Doves (Turtur senegalensis) 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 (Haematopus ostralegus), European, purchased; a Caucasian Wild Goat (Capra caucasica, δ, juv.) from the Caucasus, received in exchange; a Burchell's Zebra (Equus burchelli, q.), 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 contain galium, but no indium; the siderites all contained indium, but lacked gallium. In a 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 iron, different varieties of iron ores, and manufactured iron contain copper, lead, and silver. Gallium is a constituent of meteoric iron, but not of all meteorites, and occurs in varying proportions. Sodium potassum and rubidium are constituents of meteoric iron, 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 iron, and siderolites, cobalt occurring in the two last varieties. The authors describe the chief points of difference between the iron and meteoric iron to be the absence of nickel and cobalt in any considerable proportion from the former, and the presence of manganese. Meteoric iron, on the other hand, contain nickel and cobalt as 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 Obergimbi meteorites, the authors point out that of the two lines, one described as "unknown," and the other as "double." In 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's conclusions are identical with those of the Astronomische Nachrichten, No. 348, 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 L 1845 (1), relates to the preceding component, and (3) to the following one, while (3) deals with the pair as a whole. The last column gives the comparison stars employed in each case.

<table>
<thead>
<tr>
<th>Star</th>
<th>Proper</th>
<th>Parallax</th>
<th>ε</th>
<th>No.</th>
<th>ε'</th>
<th>Comparison stars</th>
</tr>
</thead>
<tbody>
<tr>
<td>Casiopeia</td>
<td>4</td>
<td>17</td>
<td>0.018</td>
<td>0.009</td>
<td>0.018</td>
<td>0.018</td>
</tr>
<tr>
<td>L 1845</td>
<td>8</td>
<td>37</td>
<td>0.027</td>
<td>0.012</td>
<td>0.027</td>
<td>0.027</td>
</tr>
<tr>
<td>L 1845</td>
<td>8</td>
<td>37</td>
<td>0.027</td>
<td>0.018</td>
<td>0.027</td>
<td>0.027</td>
</tr>
<tr>
<td>6 Uta Mj.</td>
<td>6</td>
<td>22</td>
<td>0.025</td>
<td>0.017</td>
<td>0.025</td>
<td>0.025</td>
</tr>
<tr>
<td>Al-De. 1003</td>
<td>3</td>
<td>51</td>
<td>0.029</td>
<td>0.017</td>
<td>0.029</td>
<td>0.029</td>
</tr>
<tr>
<td>3 Aquila</td>
<td>5</td>
<td>20</td>
<td>0.016</td>
<td>0.017</td>
<td>0.016</td>
<td>0.016</td>
</tr>
<tr>
<td>Endeley 2077</td>
<td>5</td>
<td>20</td>
<td>0.012</td>
<td>0.015</td>
<td>0.012</td>
<td>0.012</td>
</tr>
</tbody>
</table>

**JAMES WATT, AND THE DISCOVERY OF THE COMPOSITION OF WATER.**

When your Secretary did me the honour to communicate the wish of the Committee that I should deliver, and having been, as I am, 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 fullness of knowledge, and a width of perception and research, which 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 mine 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, Wheatley, Peacock, Kopp, George Wilson.

1 The Watt Memorial Lecture, delivered in the Watt Memorial Hall, Greenock, on March 11, by Prof. T. E. Thorpe, L.L.D., F.R.S.
eminent in science and literature, who have defended their con-
victions with great power, ample knowledge, much argument-
ative 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-stroke by Sir Humphrey Davy, President of the
Royal Society, or like the equally famous controversy on the dis-
covery of the planet Neptune, to attain the dignity of a na-
tional question, far more acute, I should imagine, than that which
has just occasioned all right feeling Scotchmen to approach the
Quoight to do their duty in 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
a political or a parochial question. We are all applicants before
the bar of Time, and the only sentence that the Law intends to pass
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 trans-
cendent interest. But to those who had eyes to see, its supreme
importance, as an object of sense and of feeling, and of thought,
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. The discovery of what was called the Property of
Air was rather a 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
scoured the world over to safeguard what they honestly
coeeded 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 true 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 soli-
tatness 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, conducted to mould his character and to de-
terminate the line of his mind. At the time of the great flood
of facts at the end of the eighteenth century, Watt, in his work,
at the time, the Zanzibar 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 some of the greatest men 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
inwardness towards scientific truth, the same receptivity for scientific
discipline, the same love of pondering over and speculating upon
the true inwardness of things that brought him the friendship of
Priestley, Black, Wedgwood and De Luce, and that attitude
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 would shake himself free of all personal and material
sacrifice, of physical suffering, amidst all the toil and anxious worry of a
business surrounded with difficulties, to find peace in the con-
templation of natural phenomena, and to spend time in philo-
sophical speculation. The shrinking, diffident man, in thus
communing with himself and with nature, followed a train of
constant impulse to withdraw from the strife and turmoil of
the world, and to seek his pleasure and his rest in the silent con-
templation of natural truth. No one can look upon that con-
temptative face without being struck with its expression of
philosophic calm. What deep, genuine pleasure these com-
munings 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 because of the weight of affliction that he had cast
off, 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 appreci-
ation 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 another philosopher that he had never seen a man
who had accomplished more than the one with whom 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
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 indulgence—and as it
happened at the very time that the Soho firm was struggling 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
assumption that the air was more nearly as a static fluid
inert as water . . . as a fixed air, and a property of air not
acquired as one of its essentials, and he deduced from this
in a letter to his friend Black, under date December 13,
1782, that, "as steam parts with its latent heat as it ac-
quires sensible heat, when it arrives at a certain point it
will have no latent heat, and may, under proper compaction,
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 growing over one day's work. As he [Priestley]
succeeded in turning the acids into air by heat only, he wanted to
try what water would become in like circumstances. He under-
saturated some very caustic lime with an ounce of water, and
subjected it to a white heat in an earthen retort. No water or mois-
ture 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 he had tried more than one experiment of this kind, and
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
speaking on the subject.

As the results of these experiments, which shall see in due course, were wholly
classsical and, in following up them with his wonted ardour,
Priestley quickly found himself in a maze of contradictions,
and ultimately discovered that this seeming conversion was absolutily mystical.

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 consisted in the
assumption that the acid gases were various states of water, or that, as has been
already stated, 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 Luce, Kirwan and others, that he regarded them

NO. 1484, VOL. 57]
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 closely condensed into his forerunner in this SOCl\textsubscript{2} 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 the vessel was in a glass vessel, the vessel for water-proof, 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 to doubt the soundness of Priestley's experiment, if an apparent and wholly unlocked for confirmation of it had not appeared.

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 in one place particularly 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 on natural philosophy 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 had had his attention directed to 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 phlogistinated.

For some time previous to the publication of Priestley's book, Mr. Cavendish was engaged upon an inquiry "to find out the causes of the diminution which common air is well known to suffer by all the various ways in which it is phlogistinated, 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 combustion or in the oxydation of other bodies. On the burden inerta and indestructibility of the hypothesis 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 of Priestley and Warltire; but although he continued to increase 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 of metal to 1 of common air, he concludes that, "we may safely conclude that when they are mixed in this proportion, and exploded, almost all the inflammable air and about one-fifth of the common air lose their elasticity, and the other, which loses the least, 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 large glass condenser. After this process, the 35 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—logically or not—was entertained by Cavendish and Priestley, and nitrogen or the mephitic air of Rutherford, the aote 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. In a later time past, the experiment of Volta's experiment had been introduced 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 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 of expression; an experiment is called a fact, if the result is not immediately made known, and of which, together with 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 infer 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 globe furnished with a ready-made apparatus for the experiment. 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 was not 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, and the delay enabled but for the unanswerable reticence of the body, 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 he informed the Cavendish that he had communicated to Priestley the results of his experiments, he have 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, 1782)
Priestley proceeded to do. It appears from a letter to Wedgwood that he repeated Cavendish’s experiment during the March of that year and that he found the same result. He then 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 suppose that the water became aeriform, which he laments was not the effect in opening Priestley’s eyes to the origin of his mistake. 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 Warfline’s experiment might throw light upon this question, so did Priestley. ‘Are we not, then, authorized to conclude from this experiment that the change of water into air 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 as evidence as to the true and first discovery 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. ‘Behold!’ is the exclamation. ‘The Water, and with the indication 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 inter-change of steam and air, effected by diffusion through the porous surface of the retort. It may here be mentioned that a letter from Dr. Laws to Dr. Lee, ‘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 to Lavosier, 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 well as to present our principles, and I can assure you that I 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 which is, I 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, the same, he endeavoured to prove the cause of it; which he did in a 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 an experiment was made to prove that the so-called ‘ugly experiment’ was 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 and air] entirely, and both explanations of it, nor any other that I own 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 keeping a glass stopper 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 12, 1784.

In the previous autumn, however, disquieting rumours reached this country that the French philosophers, and chief among them, Lavoisier, were poisoning upon the English preserves. The cir- cumstances are set out in a letter to De Lu offer 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 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 improve his hypothesis, and produce a memoir to the Academy before my letter can be printed, which may be as much superior as to eclipse my poor performance and sink it into utter oblivion; may, worse, I may be accused of plagiarism..."

In a subsequent letter which Blagden addressed to the editor of the Chemische Anzeiger in 1786, that it was known to Cavendish's experiment that was being thus repeated, is confirmed by a letter from La Place to De Lu offer dated June 28, 1784, in which we read: "Nous avons répété, ces jours derniers, la loi du doc. de Lavoisier et moi, devant Mr. Blagden, et plusieurs autres personnes, l'expérience de Mr. Cavendish sur la conversion en eau des aires dephlogistiques et inflammables, par leur combustion... Nous avons obtenu de cette manière plus de 234 gr. d'eau pure, ou au moins qui n'avait aucun caractère d'acide, et qui était insensible au goût; mais nous ne savons pas encore si cette quantité d'eau représente le poids des aires consommées; c'est une expériences que nous ferons avec toutes les précautions possibles et qui me parait de la plus grande importance." The phrase "qui n'avait aucun caractère d'acide" is of special significance. The French philosophers, and Lavoisier in particular, could with difficulty, as Blagden relates, be brought to credit the statement that inflammable air was converted into water by 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 connected in the very word oxygen, which we owe to the French chemists; and it may be that this circumstance was, among 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 the combustion of inflammable air produced "very pure water." It is this statement that has been said to constitute Lavoisier's claim to be considered as the first discovery of the true principle of combustion. He claimed it was "very pure water.

In his memoir on the chemical composition of water, he states that he was able to secure pure water free from all impurities, that he had no valid claim has been implicitly admitted by Lavoisier himself. The eminent Perpetual Secretary of the French Academy, M. Berthelot, is not doubt accurate in regarding June 25, 1783, as the first certain demonstration of the discovery. In his "La Révolution Chimique": "the Presidential Address to the Chemical Section of the British Association, 1850; see also "Essays in Historical Chemistry" (Macmillan, 1859).

---

1 Priestley, Cavendish, Lavoisier, and "La Révolution Chimique": the Presidential Address to the Chemical Section of the British Association, 1850; see also "Essays in Historical Chemistry" (Macmillan, 1859).
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 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 engendered in him because accomplished by a sense of the 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 charge has been brought 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 with a padded coat of mail. The figure is life-sized, and its workmanship is peculiar to Mexican antiquities.

—a description of the temple-pyramid of Tepoztlan, by Dr. E. Seier, 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 room 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 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 a 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, 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 be used in the ground, and many others 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 hutsmen, 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 Mophals of the coast of Mahbrau, by Dr. Emil Schacht, 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 predominates. 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, Samoytians 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-loreists who have followed Mr. Mac Ritchie'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 $T(n_2 - n_1)$, 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 $kr + my + nz = t$ is a tangent plane to the wave-surface. Writing the roots of the biquadratic as series proceeding by powers of sine $i$, 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 $$a_0 + a_1 \sin i + a_2 \sin^2 i + a_3 \sin^3 i + \ldots,$$

and $$m_0 + m_1 \sin i + m_2 \sin^2 i + m_3 \sin^3 i + \ldots,$$

while the relative retardation is

$$T(a_0^2 - a_2^2).$$

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

$$n^2 + (a_0 + a_2 \sin^2 i) + b_2 \sin^2 i + a_2 \sin^2 i + a_1 \sin^2 i = 0. $$

Neglecting the coefficient of $n,$ the roots are

$$a_0 \pm c \sqrt{n},$$

while the series proceeding by even and odd powers of $n$ respectively. Assuming that the actual roots are

$$\pi \pm n \pi \pm \alpha,$$

the successive terms of the series $a, b, \gamma, \delta$ are determined as in the former method, and, as for terms of the fourth order, have the form

$$a = -\gamma = a_2 \sin^2 i + a_3 \sin^3 i + \ldots,$$

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

so that

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

**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 gravel 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, none agreeing with the author's views, and some wishing to extend them.—An old citizen of Cenomarian and Turonian near Honiton, with a note on Holaster attus, Ag., by A. J. Jukes-Browne. Although an outlying patch of chalk in the parish of Widworthy was mentioned by Fitton and marked on Dr. La Bèche'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 localities, by W. S. G rapper. 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

NO. 1484, VOL. 57]