Professor Auwers one of the secretaries of the Berlin Academy, occupied the chair, and the success of the meeting was largely due to the extreme ability and tact, combined with judicious firmness, with which he conducted the proceedings. Besides showing himself a master of the three languages—German, French and English—used in the debates, he was thoroughly informed on every point which came up for discussion. Fortunately, all the delegates appeared to be actuated by the desire to coöperate, and there was little difficulty in framing statutes which all were prepared to accept.

The immediate outcome of the conference has been that it is resolved to found an international union of the principal scientific and literary bodies of the world, the object of which will be to initiate or promote scientific enterprises of general interest recommended by one or more of the associated bodies, and to facilitate scientific intercourse between different countries. It is to be known as the International Association of Academies. A number of important bodies besides those represented at Wiesbaden are to be invited to join. General meetings of delegates from the various constituent academies are to take place, as a rule, at intervals of three years, but the interval may be varied and special meetings held, if necessary. The Royal Society had proposed, prior to the conference, that the first general meeting should be held in Paris next year. At the general meetings two sections will be constituted, one dealing with mathematics and the natural sciences, the other with arts and philosophy.

A council is to be appointed which will carry on the business in the intervals between meetings. The formation of committees of experts to initiate and promote scientific investigations of international importance is also contemplated.

It remains to be mentioned that the Berlin Academy had also arranged for the entertainment of the delegates at the close of the debates. On the Monday evening they were invited to attend a performance of Lortzing's opera *Undine*, and on the Tuesday they were entertained at dinner in the Kurhaus. On the latter occasion Professor Virchow occupied the chair, and opened the proceedings by toasting the delegates generally; he was followed by Professor Darboux, of Paris, who proposed the health of the Berlin Academy, and in the course of the evening numerous other toasts were proposed by the delegates.

SCIENTIFIC BOOKS.

The Kinetic Theory of Gases. By S. H. BURBURY. Cambridge University Press. 1899. Pp. 157.

Mr. Burbury has long been known as an occasional contributor to the Kinetic Theory of Gases. The first edition of Watson's treatise on this subject, published in 1876, acknowledged the indebtedness of its author to him; and in that very interesting discussion of the Kinetic Theory which was begun at the Oxford meeting of the British Association in 1894 and continued for months afterwards in *Nature*, Mr. Burbury took a conspicuous part, appearing as the expounder and defender of Boltzmann's H-theorem in answer to the question which so many have asked in secret, and which Mr. Culverwell asked in print, '*What is the H-theorem and what does it prove*'? Thanks to this discussion, to the more recent publication of Boltzmann's *Vorlesungen über Gas-theorie*, and finally to this treatise by Burbury, this question is not so difficult to answer as it was a few years ago; but it is probable that some readers of SCIENCE, even to this day, know less about the H-theorem than is contained in the following sketch of its history, which will serve to bring out one of the most interesting features of the book before us:

In 1860 Maxwell deduced from the laws of probability an expression for the final distribution of components of velocity among the particles of a gas consisting of very small elastic spheres having no action upon each other except at the instants of collision. Many ingenious minds have since occupied themselves with this problem; and many discussions of it have been published with the purpose of improving upon the work of Maxwell, though none, so far as the reviewer is aware, has reached, for the case of a much rarefied gas, a different result. The especial defect of Maxwell's argument is his failure to show that the condition which he arrives at as the final condition of the gas is a necessary state, although he has shown it to be a possible state. Boltzmann especially has undertaken to supply what was lacking in the demonstration of Maxwell. Starting with a gas which has not yet reached its condition of 'stationary motion,' and in which the particles influence each other only at impact, he made a very particular study of the possibilities and results of collisions, with the purpose of showing that these results would as a whole tend to bring about the state of Maxwellian distribution of velocities, which would therefore be a necessary and final state. As an indispensable part of his argument he framed and used the so-called *H-theorem.* To attempt here a definite statement of this theorem would be folly. Let it suffice that H is a function based upon the laws of probability and that, according to Boltzmann, it necessarily decreases, through collisions, with lapse of time and by its diminution marks the progress of the gas towards the Maxwellian state, which is attained when H becomes a minimum. But critics have objected, Why must the H function diminish? If we imagine the velocity of every particle of the gas reversed at any instant, the H function ought to increase. Are not the reverse velocities as probable as those you imagine? And should not the net effect of all collisions be to leave H unchanged? To this Boltzmann replied that reverse velocities would indeed cause H to increase; but he urged that it was not allowable to imagine every velocity reversed. For example, in a case where a partial mixture of gases has come about by interdiffusion, a reversal of all velocities would cause the gases to separate from each other. This was an admirable and enlightening reply to the doubt raised, but the discussion is so beset with difficulties and possible obscurities that Mr. Burbury has done students good service in examining with much care a fundamental assumption upon which the argument of Boltzmann is based. Burbury's statement of this 'assumption A' is as follows:

"The chance of any molecule having velocity in x between u and $u + du$ is independent, not only of its position in space, but also of the v, w, which it has in directions y and z, and further except in the case mentioned below, it is independent of the positions and velocities at the instant of all the other molecules of the system. The excepted case is when the two molecules are so placed that they are, or very recently have been, within one another's sphere of action. The force of this exception, and the necessity for it, will appear in the consideration of the H-theorem."

Some of the most salient facts of the situation are these:

1. Boltzmann in preparing his H-theorem treats the number of pairs of particles which are *on the point of colliding*, at given velocities and angles, as a function of these velocities and angles and of these alone; but he treats the number of pairs which are *just parting from each other* at the same velocities and angles as a function of the pre-collision velocities and angles of the now separating pairs, on the ground that their number is determined by the number of pairs which an instant before were on the point of colliding with each other at certain velocities and angles alone capable of producing the post-collision velocities and angles mentioned. This is a matter of principle, not merely of convenience; for if particles just about to collide and particles just parting were numbered by like functions of velocities and angles, the number of particles leaving any class would be exactly equal to the number entering it, and there would be no H-theorem.

2. The function which expresses the number of particles having velocities lying within certain limits becomes the Maxwellian function when H has reached a minimum; and when this state is attained the exception noted in assumption A disappears.

The close scrutiny of assumptions is characteristic of Burbury's book. The fact that he has named the statement above *assumption A* shows that he has in mind an assumption *B*. This latter, however, he does not attribute to Boltzmann. It is his own, and is of a character to show that he is entirely undismayed by the difficulties of the Kinetic Theory in its ordinary form. Assumption *B* is proposed as a substitute for assumption *A*, and it runs as follows:

"The chance of a given molecule having at any instant assigned velocities is *not* independent of the positions and velocities of all the other molecules at the instant. On this assumption *B*, instead of deducing the chance of the members of a group of n molecules having respectively at any instant the velocities $u_1 \cdots u_1 + du_1$, etc., from the assumed chances for individual molecules, we must reverse the process." According to this assumption "the chance that the x velocity of the first molecule shall lie between u_1 and $u_1 + du_1$, whatever be the positions and velocities of the other $n - 1$ molecules, is

$$\int\int\int_{-\infty}^{+\infty} (dx_1 dx_2 \cdots dz_n\, dv_1 dw_1 du_2 \cdots dw_n) \times F(x_1 y_1 \cdots u_1 \cdots w_n)."$$

He does not introduce this complication out of pure wantonness, nor is he in this particular case making an effort to get, in his own phrase, 'as near an approach to chaos as is possible in an imperfect world.' It is his hope by means of assumption *B* so to generalize the Kinetic Theory as to make it fit the case of a vapor approaching liquefaction. A few quotations will indicate some of the aims and results of his discussion. Thus on p. 46 under the heading *Finite Forces*, by which phrase he means to exclude the case of 'rigid elastic bodies,' which exert infinite force upon each other at collision, he writes, "I propose to prove in this and the next chapter that in a system consisting of molecules of finite dimensions in stationary motion, it is not true for molecules very near to one another, that the chances of their having velocities between assigned limits are independent, as condition *A* assumes; but, on the contrary, if the forces be repulsive, they tend to move on the average in the same direction," etc.

In § 99, under the heading 'Concerning the Maxwell-Boltzmann Law $m_1\bar{a}_1^2 = m_2\bar{a}_2^2 =$ etc.,' that is, the law which asserts that the mean kinetic energy of the molecules of one species of particles is equal to that of the molecules of any other species at the same temperature, we have, "It seems therefore to follow that the law $m_1\bar{a}_1^2 = m_2\bar{a}_2^2$, etc., cannot hold universally. It can be accepted only on the authority of the great physicists by whose name it is known."

In § 107, "It follows from this result that" * * * "the system would tend more and more, with increasing number of molecules in a given space, to assume the form of a number of denser aggregates, say clouds, moving through a comparatively rare medium." On p. 112, after a passage similar to that just quoted, but containing other particulars, we have "Such is the process which our analysis leads us to expect. Physicists may consider how far it corresponds with what is known to take place in gases under condensation, or on what (if any) farther hypothesis it may be made to correspond with it." This last quotation is especially significant as to the point of view from which the whole book is written.

The last chapter (X.) is devoted to *Thermodynamical Relations*. It contains, with considerable matter descriptive of the speculations of others, the author's kinetic 'proof of the second law' of thermodynamics in accordance with 'assumption *B*.' His proof with assumption *A* was published in 1876, and Mr. G. H. Bryan,* who has made an exhaustive study of such efforts, declares it to be the simplest proof based on the 'Boltzmann-Maxwell law of distribution of speed.' But the wayfaring physicist who is seeking an excuse for avoiding an encounter with the new and more general proof offered by Mr. Burbury will find it in another remark made by Mr. Bryan in the conclusion of his report.† "Although many of the researches mentioned in this report are not infrequently called dynamical proofs of the Second Law, yet to prove the Second Law, about which we know something, by means of molecules, about which we know much less, would not be in consonance with the sentiments [judge the unknown from the known] expressed at the end of the last paragraph. The most conclusive evidence for regarding Carnot's principle

* B. A. Report, 1891, p. 85.

† Idem, p. 121.

as a theorem in molecular dynamics lies in the remarkable agreement between the results obtained by the methods described in the three different sections of this report, all of which are based on different fundamental hypotheses.''

EDWIN H. HALL.

CAMBRIDGE, October 28, 1899.

Elementi di calcolo infinitesimale con numerose applicazione geometriche. Per ERNESTO CESÀRO, professore ordinario della R. Universita di Napoli. Naples, Lorenzo Alvano. 1899. 8vo. Pp. 400.

The absence of a text-book on the calculus from a too well-known series of American mathematical text-books was recently remarked. The omission was excused by the observation that the author of the series knew nothing about the calculus. It might have been well for the cause of secondary and superior mathematical education in this country had the same modest confession been called into execution earlier and prevented the construction of the patch-work, fragmentary, stereotyped algebra of the same series. Contrast the confession of the razor-maker with the refusal made lately by a mathematician who declined to prepare an elementary treatise on the infinitesimal calculus on the ground that he knew too little arithmetic and algebra.

Cesàro had the courage to learn and make his mathematics before he began to publish any of his courses. His treatise* on algebraical analysis appeared five years ago and was most favorably received, although published against the advice of his friends. This work naturally contained an introduction to the infinitesimal calculus which gave full promise of the superb treatise which comes from the press this year. The former, which is by no means so finished a work of art as the latter, is a collection of sixty lectures on substitutions and determinants, linear forms, quadratic forms, irrational numbers, limits, series, functions, developments in series, complex numbers, quaternions, elimination, symmetric functions, enumeration of roots, numeric and algebraic resolution of equations, differences and interpolation, and factorial developments.

*Cesàro, Corso di Analisi algebrica con introduzione al Calcolo infinitesimale, Turin, Bocca, 1894.

Cesàro's course in the calculus is designed after the following plan the style of whose exposition is a most fortunate combination of mathematical rigor and poetic expression. There are three grand divisions occupied in order with fundamental theories, the differential calculus, and the integral calculus. The first of these consists of four chapters devoted to functions, derivatives, developments in series, and functions of several variables; the second part also contains four chapters presenting the theory of differentiation and its applications to the theories of plane curves, space curves and surfaces; the last division comprises five chapters on integration, applications to the evaluation of certain remarkable classes of integrals, applications to geometrical mensuration, differential equations and variations.

The reviewer has space to analyze but few of the chapters of this valuable work. The first chapter exhibits the principal properties of functions in all their modern refinement by the evolution of the following theorems: 1° If a function is finite throughout an interval it always admits of an inferior limit and a superior limit; 2° If a function is finite for all the numbers of an interval it is finite throughout the interval; 3° The first theorem of Weierstrass, if a function is finite in a finite interval, the latter contains at least one number for which the function has the same limits, inferior and superior, as the interval itself; 4° For the existence of a finite limit of $f(x)$ to the right of a it is sufficient that, given ε positive and as small as we wish, there can always be found a positive number h, such that, for every pair of values x' and x'' taken within the interval $(a, a+h)$, excluding the inferior limit, the absolute value of $f(x') - f(x'')$ is less than ε; 5° If $f(x)$ is continuous and different from zero for $x = a$, it possesses at a the sign of $f(a)$; 6° If a function is continuous in an interval it is also finite in the interval; 7° A function continuous in an interval at the extremities of which it takes opposite signs must vanish at least once in the interval; 8° A continuous function cannot pass from one value to another without passing through all the intermediate values; 9° Second theorem of Weierstrass, every function continuous in a finite interval takes the maximum and minimum value