# MATHEMATICS AT THE INTERNATIONAL CONGRESS OF PHILOSOPHY, PARIS, 1900.

THE first international congress of philosophy was held at Paris, August 1–5, 1900, under the presidency of M. E. BOUTROUX, professor at the Sorbonne. The proceedings of the third section. devoted to logic and history of the sciences, are of especial interest to mathematicians. The deliberations of this section were directed by Professor JULES TANNERY. In his inaugural address as president of the section, after the usual felicitations and customary acknowledgments, Professor Tannery emphasized the union between science and philosophy, a union which, he said, is better designated by the term unity. Any separation between scientists and philosophers is only apparent. The savants of the present century have set most important results before philosophers for speculation. Count the powerful geniuses who from the beginning of the century to its end have attacked the notion of space ; measure how much the critique of this notion has done for the problem of knowledge. What illumination has been thrown upon the notion of function and, ascending farther, on the notion of number, by the revision of the principles of analysis ! Will not the theory of ensembles show somewhat how we ought to think of infinity? Will not mathematical logic furnish us a sure instrument for the discernment and transformation of the data of intuition? Will not the algebra of logic enable us to penetrate deeper into the mechanism of deduction? Every hope seems legitimate, after what has been accomplished in the foundations of geometry. What surprises may be in store for us in the study of the principles of mechanics ! Before turning aside from the mathematical sciences, Professor Tannery called attention to the import for philosophy of those works which had been devoted to the evolution and history of these sciences.

Among the papers which were presented to this section the following deal more or less directly with questions mathematical. They are given in the order of presentation. The résumés of the papers and the discussions occasioned by them are drawn from the number of the *Revue de Métaphysique et de Morale*, for September, 1900, which is specially devoted to the congress of philosophy.

G. VAILATI, "The classification of the sciences."

G. MILHAUD, "On the origins of the infinitesimal calculus."

S. GÜNTHER, "On the history of the origins of the newtonian law of gravitation."

M. CANTOR, "On the origins of the infinitesimal calculus."

H. POINCARÉ, "The principles of mechanics."

B. RUSSELL, "The idea of order and absolute position in space and time."

H. MACCOLL, "Symbolic logic."

G. PEANO, "Mathematical definitions."

C. BURALI-FORTI, "The different logical methods for the definition of the real number."

A. PADOA, "Essay at an algebraic theory of integral numbers, with a logical introduction to any deductive theory whatever."

M. PIERI, "On geometry considered as a pure by logical system."

P. PORETSKY, "The theory of logical equalities with three terms."

E. SCHRÖDER, "An extension of the idea of order."

W. E. JOHNSON, "The theory of logical equations."

A. MACFARLANE, "The ideas and principles of the geometric calculus."

A. CALINON, "The rôle of number in geometry."

G. LECHALAS, "The comparability of various spaces."

J. HADAMARD, "On induction in mathematics."

R. BLONDLOT, "Exposition of the principles of mechanics."

M. LE VERRIER, "On the genesis and import of the principles of thermodynamics."

A. VASSILIEF, "Principles of the calculus of probabilities."

In his paper on the origins of the infinitesimal calculus, M. G. MILHAUD commenced by seeking these origins in antiquity; in the discovery of the incommensurable magnitudes, which destroyed the atomism of the pythagoreans; in the theory of ratios of Euclid, applied by Hippocrates to the quadrature of his lunes; finally, in the method of exhaustion of Eudoxus employed by Archimedes for the quadrature of the parabola. Passing to modern times, he discussed the method of indivisibles Cavalieri, as compared with the method of exhaustion of the ancients; then the problem of tangents and the methods proposed by Descartes and Roberval for solving it; finally, the problem of maxima and minima solved by Fermat. The paper concludes with an account of the contributions of Huygens and Barrow, and their respective relations with Leibniz and Newton.

M. GÜNTHER began his memoir on the origins of the newtonian law of gravitation by reviewing the early conceptions of weight, notably the nominalistic conception of Aristotle, which he opposed to the animistic conception and to the atomistic conception. It was against the peripatetic theory of heaviness and lightness that all the modern reformers struggled : Copernicus was not altogether free from it ; it was Galileo who finally destroyed it. In the seventeenth century weight was conceived as a universal property of matter. Kepler conceived of gravitation as a magnetic attraction, and Roberval likened it to heat. On the other hand, Gassendi, Berigard, Digby, Hobbes, and Huygens attempted to explain it by corpuscular hypotheses. Up to this point only the law of masses was considered. The law of distances was formulated by Boulliau, but his explanation is bad ; Borelli, on the contrary, correctly understood centripetal acceleration, but he did not find the formula. The author finds, in Leibniz's theory of verticity a precursor of Faraday's theory of lines of force. He comes finally to Hooke, the immediate predecessor of Newton, to whom the latter showed himself rather unfair. He concludes by declaring that the success of the newtonian law resulted in the triumph of the theory of action at a distance, in which Leibniz saw the restauration of the doctrine of occult qualities, and eclipsed for a time the corpuscular and kinetic theories, which are however returning to favor in our days.

M. Couturat added a historical note to M. Günther's memoir. The latter credited the Dutch physicist Deusing with having anticipated modern theories by distinguishing a potential effort from an actual force. M. Couturat remarked that this distinction between potential and actual is fundamental in the physics and metaphysics of Aristotle from whom Deusing and modern physicists have appropriated these terms.

The memoir of Professor M. CANTOR on the origins of the infinitesimal calculus was read at a general session presided over by Professor H. Poincaré. The author recalled that the ancients had anticipated the integral calculus by their methods of quadrature and cubature. In the middle ages, Petrus de Dacia designated the continuous generation of magnitudes by the term "fluere" ; Bradwardine distinguished two kinds of infinity ; Nicole Oresme, from the consideration of latitudes and longitudes, discovered that the variation is zero at the points of maxima and minima.

Kepler rediscovered this theorem; he generalized the idea of Vieta that the circle is a polygon having an infinite number of sides infinitely small, and applied it to the quadrature of the circle and the cubature of the sphere and solids of revolution. Cavalieri conceived the method of indivisibles, which permitted him to effect veritable integrations. Descartes found a general method, purely algebraic, for calculating the normal to a curve, and consequently its tangent. But it is Fermat, the greatest French mathematician of the seventeenth century, who really anticipated the infinitesimal calculus by his method of maxima and minima; he also invented a method of tangents superior to that of Descartes. Roberval found another solution of the problem of tangents by the composition of movements; but if his method is ingenious, it is of little practical value, since at each application it demands a new exercise of ingenuity, and a good method ought to dispense with such demand once for all. After having spoken of Pascal's Traité des sinus du quart de cercle, of which a figure suggested to Leibniz his differential calculus, the author arrived at Barrow, the tutor of Newton, from whom the latter extorted the last of his Lectiones geometricae (1669). Leibniz invented his calculus October 29, 1675, a year before he saw the manuscript of Newton's Analysis per aequationes. While the priority belongs to Newton, the originality of Leibniz is incontestable. Moreover, the latter developed his method logically, while Newton was changeable in his conceptions, using limiting ratios to mask his infinitesimals. Finally, Newton did wrong in holding secret that which Leibniz threw to the winds.

In the discussion which followed the reading of M. Cantor's memoir, M. Milhaud remarked that M. Cantor had given the facility and generality of a method as the criterion of its value, and asked if Descartes, as the inventor of analytical geometry, ought not to be ranked above Fermat, whom Cantor had proclaimed to be the greatest French mathematician of the seventeenth century. M. Cantor replied that Descartes, in his Geometry, had not so much founded analytical geometry as the general theory of equations. Descartes is so little the inventor of analytical geometry that we do not find in his work the equation of the straight line, while Fermat was familiar with it. Analytical geometry is much older than either of these two savants; if we ask who wrote the first treatise on analytical geometry, it is De Witt. The superiority of Fermat's genius shines in the theory of numbers, which Descartes cultivated but little, but where Fermat discovered and demonstrated theorems of which we are still seeking demonstrations to-day. M. Ackermann asked whether Descartes had not at least invented the general method for translating geometry into algebra and algebra into geometry. M. Cantor replied that in this there was nothing new since the method was known to Vieta.

In reading extracts from his memoir Professor POINCARÉ said that mechanics is an experimental science. But have not its principles an empirical and approximate truth only? That is the question. The principle of inertia is not an a priori truth; nor is it an experimental law, since we can never verify it. Similarly for the law of acceleration, which is simply the definition of force. M. Poincaré refuted the mécanique anthropomorphique which pretends to have a psychological notion of force; he connected it with l'école du fil of M. Andrade who represents all forces by elastic strings more or less tense. The principle of reaction breaks up into an axiom (the uniform rectilinear motion of the center of gravity of an isolated system when constant coefficients are attributed to its elements) and a definition (that of mass), but we cannot verify the axiom in question be cause we are not in possession of an isolated system. It is approximately true for systems approximately isolated, but the question of knowing whether it is rigorously true for systems rigorously isolated is devoid of meaning.

The principle of relative motion seems to impose itself upon the mind and to be confirmed by experience; as a matter of fact, however, we can demonstrate it neither a priori nor a posteriori. M. Poincaré discussed in this connection Newton's argument in support of absolute motion. Finally the principle of the conservation of energy can be neither verified nor disproved by experience, since it reduces at bottom to this: "There is something which remains constant," which is the very formula of determinism.

M. Poincaré concludes that the principles of mechanics are from one point of view truths founded on experience and from another a priori and universal postulates. In a a word they are conventions, not absolutely arbitrary, but convenient, that is to say appropriate to experience. Thus is explained the fact that experience can construct or suggest the principles of mechanics, but can never overthrow them.

The discussion of M. Poincaré's memoir was opened by M. Painlevé who insisted upon the arbitrary character assumed by the principles of mechanics in M. Poincaré's exposition. They are conventions which experience can never bring to default: because as soon as any fact should contradict them we would always find, nolens volens, a means of adapting them to the new fact. For example, if in any case the principle of Kepler did not seem to be verified, we would explain the divergence by the existence of unsuspected facts, such as electrical and magnetic phenomena, etc., which would be manifested and measured by this divergence, and which would be the object of a new science. Without disputing the justness of M. Poincaré's conclusions, the speaker found them possessed of an excessive scepticism. The principles of mechanics are imposed by experience, they are the quintessence of innumerable experiences, crude or precise; and when they seem to become defective, the new facts that we are obliged to introduce to cover the deficiency assume of themselves a scientific character, that is to say, submit themselves to the principle of causation; in a word they appear as true phenomena, and not as phantasms or fictions. On the contrary, to replace one of the principles by a different one is to be submerged by innumerable complications in the study of the most simple facts. For example, the law of gravitation is verified by a multitude of observations, but in other cases it appears at fault; we explain this divergence by saying that the bodies in question are electrified, or magnetic, etc., and we measure these new phenomena precisely by the discrepancy between the true attraction or repulsion and the newtonian attraction. It would seem then that the law of Newton is only a convention that the facts never contradict, because when they seem to contradict it, we invent new facts to justify it. Still, who would dream of replacing Newton's law by the following convention: "Two bodies repel each other proportionally to their distance and inversely as their masses," correcting the divergence between this and experience by means of supplementary hypotheses? We feel that the law of Newton is a convention preferable to all others, because it is clearly imposed by the facts. Now the principles of mechanics are imposed by facts still more manifestly than the law of Newton. To sum up, M. Painlevé conceives physical science as a method of successive approximations, oriented initially by empiricism and guided by certain principles of experimental origin. The "convergence" of this method is not assured a priori, but well justified by its success, i. e., by the more and more natural and perfect accord between theory and reality. In seeking the laws of nature, it is the divergence and increasing complications which give warning that we have lost our way.

M. Poincaré replied that there was really no lack of accord between M. Painlevé and himself. He himself recognized that science has always proceeded and will always proceed by successive approximations. But he was anxious to point out the series of artifices, more or less conscious, by which the founders of mechanics had succeeded in transforming the first approximation, not into a provisional truth susceptible of correction, but into a definitive and rigorous truth; and this to a great improvement in clearness of statement, and consequently to the benefit of science itself.

M. Hadamard observed that if, with Kirchhoff, we assign as the object of mechanics, not the explanation of the phenomena of motion but merely their description in the simplest and most exact manner, the principles of this science, as we state them, are sufficiently justified. When we find facts in apparent contradiction with these principles we are perfectly justified in making a new force intervene, which is always found to account very simply for the phenomena, in place of changing the general principles and thus involving ourselves in contradictions with the aggregate of other known facts. Besides, according to a remark of M. Duhem, it is not a single determinate hypothesis, but the ensemble of the hypotheses of mechanics, that we can attempt to verify experimentally. As to the definition of force, M. Hadamard thought that we ought not to be satisfied with defining force as the product of mass and acceleration, because we do not thereby recognize one of the essential characteristics of force, namely, that it should represent the action of one body on another. To take account of this characteristic it is absolutely necessary to adjoin the principle of the independence of forces to that of inertia; the former is formulated thus: When a body is in the presence of several others, the acceleration which it experiences is the geometric sum of several segments of which each depends only on the state of the body influenced and that of one of the influencing bodies. The notion of "force exercised by one body on another," as introduced by this principle, is, moreover, necessary to the enunciation of the principle of the equality of action and reaction in all its generality; for by virtue of the definition of the internal forces of a system it can be applied to a non-isolated system.

In reply, M. Poincaré agreed that the experimental sciences can never verify anything but an ensemble of hypotheses. Every experiment furnishes us, so to speak, one equation in a very great number of unknowns. Our science, still imperfect, does not give us a sufficient number of equations; we have fewer than there are unknowns. We can count on new experiments to give us continually new equations, which will diminish the indeterminacy of the problem. But as regards the unknowns introduced by geometry (curvature of space) or by rational mechanics (its most general principles), there is something more. Not only does experience not give us enough equations to determine them, but it is absurd and contradictory to suppose that it can ever give them; for the reason that these unknowns enter into the experimental problems as auxiliary supererogatory variables. This explains why the hypotheses which one might make relative to these unknowns are neither true nor false. As to the principle of the independence of the effects of forces, M. Poincaré declared that it is not true. If a piece of iron be simultaneously subjected to the action of a magnet and another piece of iron, the effect experienced by it is not the geometric sum of those exerted upon it by the magnet and the second bit of iron separately. The principle can only be saved by a coup de pouce: we say that the second piece of iron is modified by the presence of the magnet.

M. Padoa said that the distinction between axioms and definitions has only a logical and subjective value; in the real world there are only facts all given on the same plane. There are no ideas more simple or more evident than other ideas; there are merely ideas not defined and propositions not demonstrated with respect to the logical system adopted. And this logical system can or cannot be verified by the facts, according to the interpretation given to the ideas not defined. In a word, M. Padoa maintains the mutual independence of the logical and the real.

M. Aars replied to M. Padoa to the effect that we encounter facts in the subjective and psychic world as well as in the real world. He held, contrary to M. Poincaré, that the axioms of mechanics ought to aim at the existence of mechanics, and consequently should be capable of being true or false, just as any other proposition relative to any existence whatever.

M. Poincaré replied that questions of existence of this nature seemed to him as devoid of meaning as those of the truth or objectivity of the principles of mechanics.

M. Ribert also protested against M. Poincaré's scepticism. He held that the laws of mechanics have an objective value, and that they are not creations of the human intellect. The world existed before humanity, and the world will exist after it. It already obeyed, and it will continue to obey, the laws of mechanics. Hence science is true in the sense that it deals with real existences.

M. Poincaré remarked that we raise here the question of the reality of the external world, which would be more in place in the first section (metaphysics).

Mr. Russell read extracts from his memoir on the idea of order and absolute position in space and time. After having distinguished the absolute series, that is to say those whose elements *are* positions, from relative series, whose elements *have* positions by correlation with those of an absolute series, the author defines the absolute theory of time according to which an event is dated by its relation to the instant when it exists, and the relative theory according to which it is dated only by relations of simultaneity or succession with other events. In order that the latter be tenable it is necessary that between two events, considered simply as qualities, there should exist a constant and determinate temporal relation, which is not the case. Similarly the simultaneity of several events can reduce to no property common to these events, unless it is the fact that they occupy the same instant. It is necessary then to admit temporal positions as absolute. Similarly it is necessary to admit spatial positions as absolute and for the same reasons. The author discusses the arguments invoked by Leibniz and Lotze in support of the relative theory of space. This theory contends that the position of a point is only the ensemble of its distances to other points, which supposes that the only relation which can exist between two points is their distance. But this is false; they have another relation which is the direction of the straight line (projective) which joins them. Without the latter there is no means of conceiving angle, which is a relation between two directions and not between two distances. The definition of a plane postulates a new fundamental relation (between three points). The author shows that the relative theory presents all the difficulties of the absolute theory, and reduces the theory of relations of Lotze to an absurdity. He finds the absolute theory to possess the advantages of logic, clearness and simplicity over the relative theory.

M. Tarde opened the discussion of the paper by contesting the analogy of space and time with respect to their relativity. It seems that space must be purely relative, while time has something of the absolute. In space the origin is arbitrary, in time it is not; the present instant differs qualitatively from the past, and still more from the future. Hence to say that two bodies occupy (successively) the same place has no meaning; while there is real meaning in saying that two events take place in the same time.

Mr. Russell replied that M. Tarde's distinction has only a psychologic interest but no logical value; to the consciousness time appears more real and more absolute than space; but for the theory of knowledge, time and space are altogether analogous.

M. Aars suggested that the question whether space and time are absolute or relative reduces to the question whether they are subjective or objective. Relation and relativity are purely subjective functions; if space and time are relative they will be subjective, if absolute they will be objective.

Mr. Russell declared in reply that he was concerned merely with a question of logic or epistemology that had nothing to do with the metaphysical question of the objectivity of space or the psychological question of the origin of knowledge, a question anterior and superior to each of these.

In a résumé of his memoir on symbolic logic and its applications Mr. MacColl stated that the essential element of pure logic is the proposition. Propositions arrange themselves into two classes: the true ($\tau$) and the false ($\iota$); or in fact into three classes: the variable ($\theta$), which can be true or false; the certain ($\varepsilon$), which are always and necessarily true (probability 1), and the impossible ($\eta$), which are always and necessarily false (probability 0). The symbol $A^x$ signifies that the proposition $A$ appertains to the class $x$; the symbol $A^{xy}$, equivalent to $(A^x)^y$, signifies that the proposition $A^x$ belongs to the class $y$. For example $A^{\theta\epsilon}$ states: "it is certain that $A$ is variable." The product $A^xB^y$ is the simultaneous affirmation of the two propositions $A^x$ and $B^y$; the sum $A^x + B^y$ is their alternative affirmation, namely, that one or the other is true. In general, the exponent $\tau$ is suppressed and the exponent $\iota$ is replaced by an accent (sign of negation). We have the formulæ:

$$(AB)' = A' + B', \quad (A + B)' = A'B', \quad A(B + C) = AB + AC,$$

$$(AA')^\eta, \quad (A + A')^\epsilon, \quad (A^\epsilon + A^\theta + A^\eta)^\epsilon,$$

$$A^\theta = A^{\epsilon\iota}A^{\eta\iota}, \quad A^{\iota\epsilon} = A^\eta, \quad A^{\iota\eta} = A^\epsilon.$$

The notation $(A : B)$ affirms that the proposition $A$ implies the proposition $B$. It is equivalent to each of the assertions

$$(AB')^{\eta}, \quad (A' + B)^{\epsilon}.$$

The equality $(A = B)$ is equivalent to the two inverse implications $(A : B)$, $(B : A)$. For simplicity, the implication $(A : B)$ is often written $A_B$.

If we multiply the two certitudes

$$(A + A')(A^{\epsilon} + A^{\theta} + A^{\eta}),$$

we find the following alternative (certain)

$$A^{\epsilon} + AA^{\theta} + A'A^{\theta} + A^{\eta} = A^{\epsilon} + AA^{\epsilon\iota} + A'A^{\eta\iota} + A^{\eta},$$

whose four terms signify respectively: 1° that $A$ is always true; 2° that $A$ is sometimes true, but not always; 3° that $A$ is sometimes false, but not always; 4° that $A$ is always false. The author thus comes upon the four classic forms of propositions ($A$, $I$, $O$, $E$).

The author defines and calculates on one hand the weakest premise from which we can deduce a given proposition; and on the other hand the strongest conclusion that can be drawn from a given proposition.

He applies his symbolic logic to the calculus of probabilities. The symbol $\frac{A}{B}$ represents the probability that $A$ be true when $B$ is true, that is to say the relative probability of $A$ with respect to $B$. The symbol $\frac{A}{\varepsilon}$ represents the probability of $A$ with respect to the data of the problem (regarded as certain), that is the absolute probability of $A$.

If $\frac{A}{B} = \frac{A}{\varepsilon}$, we say that the probability of $A$ is independent of $B$, since it does not change when we add $B$ to the data of the problem. Consequently we take as measure of the dependence of $A$ relative to $B$ the difference

$$\delta \frac{A}{B} = \frac{A}{B} - \frac{A}{\varepsilon}.$$

It is proved that if $A$ is independent of $B$, $B$ is also independent of $A$.

Compound probabilities are evaluated as functions of simple probabilities by means of the following formulæ:

$$\frac{AB}{\varepsilon}=\frac{A}{\varepsilon}\cdot\frac{B}{A}=\frac{B}{\varepsilon}\cdot\frac{A}{B},$$

$$\frac{A+B}{\varepsilon}=\frac{A}{\varepsilon}+\frac{B}{\varepsilon}-\frac{AB}{\varepsilon},$$

$$\frac{A'}{\varepsilon}=1-\frac{A}{\varepsilon},\quad \delta\frac{A'}{B}=-\delta\frac{A}{B}.$$

If we put

$$\frac{A}{\varepsilon}=a,\quad \frac{B}{\varepsilon}=b,\text{ etc.},$$

we have

$$\frac{A}{B}=\frac{a}{b}\cdot\frac{B}{A},\quad \frac{A}{B'}=\frac{a}{b'}-\frac{b}{b'}\cdot\frac{A}{B},$$

$$\delta\frac{A}{B}=\frac{a}{b}\delta\frac{B}{A},\quad \delta\frac{A}{B'}=-\frac{b}{b'}\delta\frac{A}{B},\quad \delta\frac{A'}{B'}=-\delta\frac{A}{B'}=\frac{b}{b'}\delta\frac{A}{B}.$$

By means of these formulæ it is easy to pass from a problem in logic to one in probabilities.

In the discussion occasioned by Mr. MacColl's paper, M. Couturat remarked that the calculus of probabilities had inspired both the system of MacColl and that of Boole; but the former, by his definition of the independence of two propositions, had been able to correct an error committed by Boole in a problem of probabilities. But the origin of Mr. MacColl's logic is found in a purely mathematical problem, namely, the calculation of the limits of a multiple integral when the order of the integrations is changed, a problem to which the last part of Mr. MacColl's memoir is devoted.

M. Peano read a memoir on mathematical definitions, in which he said that a definition is an equality whose first member contains the expression to be defined and the second member is composed of well-known terms. It supposes, then, a certain number of known terms which ought to be tabulated; the value of a definition is essentially relative to this table. M. Peano analyzed, by way of example, the first definitions of Legendre's geometry and some of Euclid's definitions. He shows that they define relatively simple and clear terms (line, point, straight line, surface) by means of terms which are less so (length, breadth, thickness, extremity, path, shortest), or a term (number) by a synonymous term (ensemble). He calls equalities of the form specified possible definitions. An idea will be definable or undefinable according to the order assigned to the ideas. It is convenient to arrange the ideas of a science in an order such that the number of primitive ideas relative to this order shall be as small as possible.

A definition should be complete, that is, intelligible by itself; and homogeneous, that is, the two members should contain the same variable terms. Thus, the formula

$$0 = a - a$$

is not a definition because we do not know what $a$ is. The complete proposition: "Let $a$ be any number; then $0 = a - a$," is not a definition because it is not homogeneous (the variable $a$ enters into the second member but not into the first). It is necessary to write: "$0 =$ the constant value of the expression $(a - a)$, whatever be the number $a$." In this proposition the letter $a$ is an apparent variable, since the second member does not depend on its value.

M. Peano criticised other non-homogeneous definitions by showing that we can draw false conclusions from them (for example, the definition of operations on fractions).

M. J. Tannery remarked that if every definition is an equality, it is necessary to place the idea of equality in the table of undefined ideas.

M. Schröder thought that it is unnecessary to impose too restrictive conditions on definitions. For example, the zero (nothing) of logic can be defined by the formula

$$0 = a\, a_1$$

whatever be $a$, where $a_1$ is the negation of $a$, or not-$a$.

M. J. Tannery took exception to the definition of the zero of arithmetic by the formula

$$0 = a - a,$$

whatever be $a$. In this formula there enters the sign of subtraction. Now we can define subtraction only in the case of unequal numbers. In other words, we define 0 as the number which when added to $a$ gives $a$ as sum. But, by hypothesis, we know no such number: every number added to another number gives a sum different from each of them. Then this does not give the idea of zero, which seems to be a primitive and indefinable idea.

M. Peano replied that the formula

$$0 = a - a$$

does not constitute a definition, since it is not complete, it being necessary to add what $a$ is. Nor is it homogeneous, the second member containing a term which does not enter the first. It is necessary then to say: "$0 =$ the constant value of $(a - a)$ whatever be $a$." The same observations apply, mutatis mutandis, to the analogous definition of the zero of logic. As to the equality definition, it is undoubtedly among the primitive ideas; but it differs from special equalities, which can consequently be defined.

M. Padoa emphasized a distinction between the formula "$0 = a - a$" and the formula "$0 =$ the constant value of $(a - a)$ whatever be $a$." The first supposes a mathematical fact known, namely, that $(a - a)$ is constant independently of $a$. The second, on the contrary, implies this fact and expresses no more than a purely logical fact: the equality of two constant values.

M. Couturat analyzed the memoir of M. Burali-Forti upon the different logical methods for the definition of a real number. The author distinguishes three kinds of definitions: 1° the nominal definition, which consists of equating the new sign (to be defined) to an expression composed of known signs; 2° the definition by postulates, which consists in enumerating the fundamental relations verified by a group of primitive ideas; 3° the definition by abstraction, which consists in defining a function $f$ by saying in what cases we have $fx = fy$.

These distinctions made, all the definitions given of an integral number enter into one of the three classes. The nominal definition of an integral number rests upon that of homogeneous magnitude. "If $+$ is a binary similar operation, we say that $u$ is a *class* homogeneous with respect to $+$ if, $x$ and $y$ being any elements of $u$, $x + y$ is a determinate element of $u$." "Let $u$ be a homogeneous class with respect to $+$. We call zero the element $x$ of $u$ such that, whatever element of $uy$ may be, we have always

$$y + x = y."$$

We define then the ensemble $N_0$ of positive integers and zero as a class of operations. Let $x$ be any magnitude of the class $u$. We define the class of magnitudes $N_0x$ (multiples of $x$) by the following properties:

1° $N_0x$ is homogeneous with respect to $+$;

2° 0 and $x$ are elements of $N_0x$;

3° Every $N_0x$ which is not equal to zero is of the form $y + x$, $y$ being an $N_0x$. We can then define nominally the zero of arithmetic and the notion of successive integers, whence are deduced the five characteristic properties of integral numbers.

The definition by postulates (Peano-Dedekind) consists in putting these five properties as postulates. The definition by abstraction (G. Cantor) consists in defining the cardinal number by the conditions of equality between two cardinal numbers.

The memoir terminates with nominal definitions of rational numbers and real numbers.

M. PADOA presented the principal ideas of his introduction to any deductive theory. Every deductive theory rests upon a system of undefined symbols and a system of improved principles. The logical value of the deduction is independent of the meaning of these symbols; the system of ideas that we make correspond to them is only an interpretation. The logical value of a theory, then, is absolutely independent of empirical and psychological data; and, though the concrete interpretation is useful, it is sometimes dangerous in that it covers up gaps in the reasoning. The undefined symbols may have several interpretations which verify the undemonstrated principles; these interpretations are logically equivalent (e. g., the principle of duality in projective geometry).

The author defines, and gives the ordinary criterion for, an irreducible system of undemonstrated principles. He shows, moreover (and this is the personal contribution of the author to this theory), that the system of undefined symbols is irreducible with respect to the system of undemonstrated propositions when it is not possible to deduce from the latter the symbolic definition of any of the undefined symbols. To show this irreducibility it is sufficient to find an interpretation of the system of undefined symbols, such that they continue to verify the system of undemonstrated propositions when we change the sense of any one of them, and this for every one of the undefined symbols taken separately. These are the principles which M. Padoa applies to the theory of integral numbers (positive, negative and zero) founded upon three undefined symbols *integer*, *successive to*, *symmetric of*, and upon seven undemonstrated propositions, including the principle of complete induction understood in both senses.

The memoir of M. PIERI on geometry considered as a purely logical system was read in abstract by M. Couturat.

Geometry, stripped of every intuitive element, is an ideal and deductive science, which has as its object a certain order of logical relations. Like arithmetic it is becoming more and more abstract; the idea of point or spatial element is generalized, as well as that of space. Notwithstanding its empirical origin and the pedagogical and practical value of its applications, geometry is being little by little shorn of all geometric meaning in order to become a hypothetico-deductive system. Such a system ought to rest upon an aggregate of primitive propositions and primitive concepts (the latter have been much less studied than the former). M. Pasch has reduced the primitive concepts of geometry to four: point, plane, segment, and congruence. M. Peano has reduced them to three: point, segment, and movement. M. Pieri proposes to reduce them to two only: point and movement. He also attempts to restrict the rôle of movement, which he reduces to a relation between four points (congruence of the two couples $AB$, $CD$) and further to a relation between three (congruence of the two couples $AB$, $AC$).

The choice of primitive ideas (which cannot be guided only by vague considerations of simplicity or of evidence) ought to be regulated by the following criterion: every science being characterized by a maximum group of transformations which do not alter the properties that it studies, the choice ought to be so made that the primitive ideas should be invariants with respect to this group. As examples of trespassers against this rule of logic the author cites the idea of length in analysis situs, that of a half-ray in projective geometry, and even the general ideas of line, surface, solid, and space in elementary geometry. The memoir terminates with the enumeration of twenty postulates which serve as a logical basis for geometry and which define successively, by means of the idea of movement, the straight line, plane, perpendicularity, and finally segment.

As an introduction to M. PORETSKY's memoir on the theory of logical equalities having three terms M. Couturat gave an outline of the principles of Schröder's logical calculus.

The fundamental relation of the algebra of logic is the relation of inclusion. We reduce the concepts to their extension, that is, to classes or aggregates of corresponding objects; $a$ is said to be in $b$ and we write

$$a < b$$

if the ensemble $a$ is contained in the ensemble $b$. It is the translation of the universal affirmative proposition "all $a$ is $b$."

Similarly, propositions are reduced to their extension or to their domain of validity, that is, to the ensemble of instances or cases where each of them is true. If $a$ and $b$ represent propositions, $(a < b)$ signifies "every time that $a$ is true, $b$ is true," or "if $a$ is true, $b$ is true," or finally "$a$ implies $b$."

Thus every formula of the algebra of logic is susceptible of two interpretations, one in concepts and the other in propositions. At bottom this algebra is simply the calculus of ensembles, what Leibniz would call the theory of the containing and the contained. To this fundamental relation of inclusion we join that of equality, which is defined formally thus

$$(a = b) = (a < b)\ (b < a),$$

that is, every equality is equivalent to two inverse inclusions having the same members.

Two undemonstrated principles are admitted: 1° the principle of identity

$$a < a\,;$$

2° the principle of the syllogism

$$(a < b)(b < c) < (a < c).$$

Logical addition and multiplication are formally defined by the following expressions:

$$(a < c)(b < c) = (a + b < c),$$
$$(c < a)(c < b) = (c < ab),$$

whence we conclude that $(a + b)$ is the ensemble of elements which contains the two classes $a$ and $b$ united, and that $ab$ is the ensemble of elements common to two classes $a$ and $b$. We define two special classes, the class 1, which contains all the others; the class 0, which is contained in all the others, and consequently contains itself no element. Finally negation is defined by the formulae

$$aa_1 = 0, \quad a + a_1 = 1,$$

of which the first translates the principle of contradiction, and the second the principle of excluded middle; which proves, in passing, that these two principles are independent of the principle of identity $(a < a)$.

The algebra of logic tends to transform the inclusions (which transcribe the verbal propositions directly) into equalities (more manageable for the calculus). To this end serve the two formulæ (known to Leibniz)

$$(a < b) = (a = ab),$$
$$(a < b) = (ab_1 = 0).$$

The second permits also of transforming any equality into an equality whose second member is zero

$$(a = b) = (ab_1 + a_1b = 0).$$

Every system of premises can be reduced to a single equality with second member zero, which will be the equation of the problem. This equation can be solved with respect to one of the variables, it matters not which. With regard to the unknown $x$ it has the form

$$(ax + bx_1 = 0) = (ax = 0)(bx_1 = 0).$$

But

$$(ax = 0) = (x < a_1),$$
$$(bx_1 = 0) = (b < x).$$

Therefore

$$(ax + bx_1 = 0) = (b < x < a_1).$$

The equation is equivalent then to a double inclusion : $x$ contains $b$ and is contained in $a_1$. Such is the solution of the problem with respect to $x$.

In his memoir on the theory of logical equalities having three terms, M. Poretsky employs an original method of which the following are the principles.

The double inclusion

$$b < x < a$$

he expresses by the equality

$$x = ax + bx_1,$$

which is in fact equivalent to the two equalities

$$(x = ax) = (x < a),$$
$$(bx_1 = 0) = (b < x).$$

The system of M Poretsky rests upon another equivalence

$$(a = ab_1 + a_1b) = (b = 0),$$

paradoxical enough because the first member contains a letter $a$ which the second does not. Hence $a$ is absolutely indeterminate in this formula. Any equality

$$A = B$$

is equivalent to each of the following equalities:

$$(AB_1 + A_1B = 0) = (N = 0),$$
$$(AB + A_1B_1 = 1) = (N_1 = 1).$$

M. Poretsky calls $N$ the complete logical zero and $N_1$ the complete logical whole of the problem represented by the equality $(A = B)$. Consequently being given any class $U$, the equality in question becomes

$$(A = B) = (U = UN_1 + U_1N) = (N = 0).$$

This is the law of forms of logical equalities, that is to say, the rule which shows how to find all the equalities equivalent to a given equality.

M. Poretsky employs for this purpose an exhaustive method, which consists in forming all the possible classes of discourse of $n$ letters. Being given $n$ simple classes $a$, $b$, $c$, $d$, $\cdots$, we can form with them $2^n$ elements (constituents of Boole) which the author calls the minima of discourse. To obtain all possible classes it is sufficient to form the additive combinations of the $2^n$ elements of which the number is $2^{2^n}$, including 0 and 1. If we take for $U$ each of these classes, we obtain $2^{2^n}$ different forms for each equality.

Any equality whatever consists in equating to zero the sum of $m$ elements, that is, in annulling them separately. It is equivalent then to $m$ elementary equalities, and to all their combinations $2^m$ in number, including the equality in the $m$ elements themselves and the identity $0 = 0$. This is the law of consequences.

On the other hand, any equality in $m$ elements can be considered as the consequence of an equality containing at least these $m$ elements (with others). Hence all the possible causes of the given equality are obtained by joining to it all the combinations of the $2^n - m$ other elements, of which the number is $2^{2n-m}$. This is the number of different causes of the equality, including the equality itself and the absurdity $1 = 0$. This is the law of causes.

M. Poretsky sums up the law of forms, the law of consequences, and the law of causes in a single table which includes the $2^{2^n}$ classes of discourse, from which may be written, by simple inspection without calculation, all the forms, consequences, and causes of the equality to which the table refers.

M. Couturat remarked in concluding his abstract that M. Peano was the first to determine the number of propositions that can be affirmed relative to $n$ simple classes. As we have seen, the number of independent propositions is $2^{2^n}-1$, excluding the absurdity $1 = 0$. But we can affirm the alternative of any number of these propositions; there are as many alternatives as there are additive combinations possible among these propositions, that is,

$$2^{2^{2^n}-1}-1,$$

excluding again the absurdity $0 = 1$.

For $n = 2$, we find the number 32,767, or 32,766 if with M. Peano we exclude the insignificant identity $1 = 1$. Thus, with two terms $a$ and $b$ and their negations, we can construct 32,766 different judgments. We thus see the richness and complexity of the combinations permitted by the algebra of logic.

M. Schröder gave a rapid summary of the principles of the algebra of relations before presenting his memoir on an extension of the idea of number. The algebra of relations is the logic of relative terms, while the classic logic treats only of absolute terms. The proposed generalization of order he calls gradation. It consists in admitting several elements of the same rank. A gradation will be expressed by two relations; the one $\rho$ responding to the double question "What element precedes what element??" the other, $\gamma$, responding to the double question "What element is of the same rank as what element??"

The essential characteristics of a gradation are the following:

1° Given any two elements $i, j$, either $i$ is inferior to $j$, or $i$ is superior to $j$, or $i$ is of the same rank as $j$;

2° If $i$ is inferior to $j$, it is not of the same rank;

3° On the same hypothesis, $j$ is not inferior to $i$;

4° If $i$ is of the same rank as $h$ and $h$ inferior to $j$, then $i$ is inferior to $j$;

5° If $i$ is inferior to $h$ and $h$ inferior to $j$, then $i$ is inferior to $j$.

These five properties of any gradation can be expressed by the following formulæ:

$$1 = \gamma + \rho + \breve{\rho}, \quad \rho(\gamma + \breve{\rho}) = 0, \quad (\gamma + \rho) ; \ \rho < \rho,$$

where $\breve{}$ signifies the converse of a relation, and $x ; \ x < x$ signifies that the precedent of a precedent is a precedent; the first formula transcribes the property 1°; the second the properties 2° and 3°

$$\rho\gamma = 0, \qquad \rho\breve{\rho} = 0;$$

and the third the properties 4° and 5°

$$\gamma ; \ \rho < \rho, \qquad \rho ; \ \rho < \rho.$$

From these formulæ we can deduce all the properties of two relations $\gamma$ and $\rho$: they are transitive, $\gamma$ is symmetric ($\gamma = \breve{\gamma}$), and copulative ($\gamma ; \ \gamma = \gamma$). Every gradation furnishes thus a root of the equation

$$x ; \ x = x.$$

To exhibit the logical importance of his researches M. Schröder remarked that we do not know whether we can affirm that every ensemble is capable of being arranged in a simple order, and that the solution of this fundamental question of order depends upon the algebra of relations.

Mr. Macfarlane remarked relative to M. Schröder's memoir the difference existing between the algebra of logic, where the relations $=$ and $<$ are transitive, and the algebra of relations in which transitive relations constitute only a particular case. The algebra of relations serves as a connecting link between the symbolic logic and various branches of mathematics such as the calculus of operations and the geometrical calculus.

M. Couturat added that the theory of substitutions is a special branch of the algebra of relations. The product of two substitutions is their relative product; the inverse ($s^{-1}$) of a substitution is the converse ($\breve{s}$); the identical substitution (1) is the identical relation 1.

The memoir of Mr. Johnson on the theory of logical equations was presented in abstract by Mr. Russell. The author proposes to solve symmetrically the general logical equation in $n$ unknowns by means of $n$ indeterminates. To this end he solves not with respect to the unknowns ($x$, $y$, $z$) themselves, but with respect to their constituents $xyz$, $xy\bar{z}$, $\bar{x}yz$, $\cdots$. But the latter can be replaced by any divisors possessing the same properties, namely those of being mutually exclusive and collectively exhaustive, that is, verifying the conditions

$$\Sigma\rho = 1, \quad \Sigma\rho\rho' = 0.$$

The equations of logic are solved with respect to these divisors $\rho$. In passing from this solution to the solution relative to the unknowns $x$, $y$, $z$, $\cdots$, a dissymmetry is introduced which the author has attempted to remove. He expresses in conclusion his acknowledgments to the works of Schröder and Whitehead on the algebra of logic.

The paper of Mr. MACFARLANE on the ideas and principles of the geometric calculus is in abstract as follows: By geometric calculus he means that branch of algebra which is founded upon the properties of space of three dimensions. The researches of Hamilton ending in the invention of quaternions have their origin in Kant's theory of knowledge; proceeding from the idea that algebra is the science of pure time, he was finally led to the true spatial generalization of algebra. The algebra of quaternions bears the same logical relation to the algebra of complex quantities as does the latter to ordinary algebra. The fundamental rules of quaternions and those of vector analysis can be harmonized by modifying those of the latter so as to render them associative; the rules thus obtained form the special rules of reduction of the geometric calculus. The geometric calculus has been extended in such a manner as to include the trigonometry of surfaces of the second order. The fundamental laws of algebra have no need of modification to be applicable to plane algebra, that is to say, to the algebra of complex quantities; but modification is necessary in order to be applicable to the algebra of space, since the axis of the plane is then variable. In the geometric calculus the principal theorems, such as the binomial theorem, exponential theorem, and Taylor's theorem, remain true, provided the relative order of the vector symbols be preserved. The philosophical conclusion of the memoir is that the process by which the science of algebra has been extended is not an arbitrary convention, but a patient logical generalization tending to a determinate end and appropriate to the object to be represented.

M. CALINON read an account of his memoir on the rôle of number in geometry. Whatever may have been historically the geometric origin of this or that extension of number, the generalization of number ought and can be made independently of any intuitive consideration. As soon as the numerical continuum is constituted, namely the ensemble of real numbers, it can serve to define the geometric continuum. The line (continuous) is defined by establishing a one to one correspondence between the ensemble of real numbers and a series of points; the surface by setting up such a correspondence between the ensemble of real numbers and a series of lines; the solid, by establishing an analogous correspondence between the ensemble of real numbers and a series of surfaces. Thus it is the numerical continuum, simpler and clearer, which generates the geometric continuum, and number is essential to the definition of all geometric magnitudes. Projective geometry is independent only in appearance, for it implies a continuity which reduces in the final analysis to the numerical continuum.

The following is a brief résumé of the memoir of M. LECHALAS on the comparability of various spaces. There are those who deny to general geometry the right to consider of one or several spaces as contained in a space of four dimensions, and to conceive of several coexistent spaces. Nor do they allow the identification of euclidean spheres and Riemannian planes or euclidean pseudo-spheres and Lobachevskian planes. M. Lechalas first set aside the purely verbal objections founded on the use of the word space to designate varieties included the one in the other. If, in a plane four-dimensional space, we consider a spherical space of three dimensions and a euclidean plane space of three dimensions passing through the center of the former, their intersection is a surface (two-dimensional variety) which possesses, in the euclidean space, all the properties of the euclidean sphere and in the spherical space, all the properties of the Riemannian plane. Why refuse to admit the identity of this euclidian sphere and this Riemannian plane, since they are the same figure? It is maintained that it possesses different properties in the two spaces. Unquestionably, as the circle (intersection of a plane and sphere) possesses different properties on the two surfaces: in the plane it has but one center; on the sphere it has two; if the plane passes through the center of the sphere the circle will be a geodesic on the sphere without being a geodesic of the plane. But this does not prevent it from being the same figure in both cases. Besides the partisans of the incomparability of spaces are logically driven to maintain the indiscernibility of Riemannian spaces among themselves and of Lobachevskian spaces among themselves, and consequently to admit only three types of spaces qualitatively heterogeneous and foreign to one another (euclidean, Riemannian, Lobachevskian). They thus suppress the continuity which binds these three spaces together, and which makes the euclidean geometry a limiting case of the other two ; they thus destroy the unity of general geometry and its philosophical character.

In discussing the paper of M. Lechalas, M. Peano remarked that we can establish for four-dimensional space propositions which are not valid for space of three dimensions. In fact they rest upon an additional postulate. To the three following postulates

1°. If $a$ is a point, there are other points than $a$;

2°. If $l$ is a straight line, there are points outside of $l$;

3°. If $\pi$ is a plane, there are points not in $\pi$;

we ought to add the postulate

4°. If $\Sigma$ is a space of three dimensions, there are points not in $\Sigma$.

It is easily seen that there are propositions which result from these four postulates taken together and which do not result from the first three. Conversely such a theorem as the property of homological triangles is true for the plane and three dimensional space but does not hold for space of four dimensions, because it depends on the number of dimensions of the space.

M. Hadamard expressed the opinion, in the discussion, that space of four dimensions is only an analytical ensemble; and that general geometry is only a geometrical interpretation, though a legitimate one, of analytical facts and formulæ.

Mr. Russell said that a plane space of two dimensions is not identical with a plane of a space of three dimensions ; the latter enjoys special properties which it possesses because situated in a space of higher dimensions. One of the proofs is that projective geometry even of the plane demands a space of three dimensions for its construction ; in fact, to prove that the construction of von Staudt's quadrilateral is a univocal operation (with a unique result) it is necessary to be able to effect it in two different planes. Besides, in order that the essential duality of projective geometry shall be complete, a space of at least three dimensions is necessary. There is then a specific difference (of nature, and not of degree) between spaces of two dimensions and those of three dimensions.

Mr. Russell objected that M. Lechalas passes from the identity of the analytical properties of two spaces to the conclusion of their real identity. This is to ignore the difference between analysis and geometry. From an analytical point of view we conceive whatever we wish : one only constructs algebra and has need to speak neither of points nor of figures. But to construct a geometry it is necessary to get a space to which we can apply the analytical formulæ, and in this space planes and straight lines which correspond actually to their projective definition (namely, of being determined by three or two points). We cannot apply the abstract projective geometry to any figure, for example, take for plane any surface whatever ; to take a sphere is to suppose the whole of the metrical geometry. To be sure, the geodesics on a sphere are determined by two points, but this presupposes that the sphere itself has been determined by its distance from an exterior center or by some other equivalent metrical property. The sphere then can never be likened to a plane, not even to a non-euclidean one.

M. Hadamard read a résumé of his memoir on induction in mathematics. The method of invention in mathematics is generalization. What is the value of this process? what is the probability of its success? The author thinks that it often fails ; not only is there no reason why generalization should succeed, but there are often reasons for it not to succeed. If a certain problem has been solved in a certain particular case, this is generally due to the fact that this particular case offers some special property that renders it simpler and easier to treat. We cannot extend this property to the general case to resolve it in an analogous manner. The recent progress of mathematics offers plenty of examples in support of this statement. The more science advances, the more the part of chance in the discovery of truth diminishes ; the more, consequently, should induction and analogy be found misleading. Being given two analogous problems, one of which has been solved and the other not, there is occasion to think that the results found in the solution of the first are very different from those to be obtained in the solution of the second.

The memoir of M. Blondlot on the principles of mechanics was read by M. Couturat. The author has followed Kirchhoff and Mach, and the object of his work is didactic rather than critical. He marks sharply the distinction between ideal and fictitious mechanics and real and positive mechanics. The former supposes an ideal system of data (refères) and an absolute clock. Admitting the notion of material point, the principles of mechanics may be formulated as follows:

1°. A material point, supposed alone, can assume no acceleration.

2°. Two material points determine on each other accelerations directed in opposite senses along the line joining them.

3°. The ratio of the numerical values of the accelerations produced by two given material points $A$ and $B$ is constant.

The latter principle permits of defining and measuring the mass of a material point by means of the following addition: "$X$ being any material point, the ratio of the mutual accelerations of $A$ and $B$ is equal to the ratio of the mutual accelerations of $A$ and $X$ divided by the ratio of the mutual accelerations of $B$ and $X$. Whence the ratio of the masses of two material particles will be, by definition, the inverse of the ratio of their mutual accelerations."

4°. The acceleration induced in any material point $M$ by an aggregate of several material particles $S$, $S'$, $S''$, $\cdots$ is obtained by compounding, by the rule for the composition of vectors, the accelerations determined separately by $S$, $S'$, $S''$, $\cdots$, when acting successively on $M$. The notion of force is useless, but, to abbreviate, we can define it as the product of the mass by the acceleration.

Theoretical mechanics is to positive mechanics as a model to an actual construction. To apply the former to the latter we choose a system of data and a time-piece in such a manner as to obtain the most exact description possible of actual movements. This is undoubtedly inexact; but if it is approximately exact, it is simply because theoretical mechanics, fictitious and conventional as it is, was invented to be applied to reality.

In the discussion M. Kozlowski was of the opinion that the idea of force should not be banished from mechanics. Du Bois-Reymond has shown that impact is inconceivable, and that action at contact can be explained only by action at a distance. If science substitutes acceleration for force it is because it tends to replace muscular symbols by visual symbols.

M. Bulliot would not agree to replace force by acceleration under the pretext that force is an obscure idea taken from our common experience. Why substitute for notions a little obscure but rich in content, others clear but barren? Modern science through an excessive love of logical clearness tends to lose all contact with reality to which it owes its origin.

M. J. Tannery replied that if science replaces experimental notions and the confused data of sense by abstractions, it is to satisfy a need of rigor and precision ; but it does not thereby lose all contact with reality, and applied science is benefited by the progress of pure science.

M. Foveau de Courmelles did not admit the distinction between statical force and accelerating force : both correspond to the same psycho-physiological impression of effort.

M. Vassilief confirmed the ideas of M. Kozlowski upon the difference between visual and muscular sensations. The privileged rôle of visual sensations in science is due to the fact that they are the only ones which are exactly measurable. The speaker recalled Lobachevsky's definition of time : time is a motion suitable to measure other motions.

In M. VASSILIEF's memoir on the principles of the theory of probabilities he distinguishes three kinds of probabilities : 1° subjective probability, simple sentiment of expectation or hope ; 2° mathematical probability, defined numerically as the ratio of the number of favorable cases to the number of possible cases ; 3° objective or physical probability, which results from the observation of a very great number of experimental cases. We know that the last approaches mathematical probability as the number of observed cases increases. M. Vassilief asks whether there is not an analogous relation between subjective and mathematical probability. They do not seem to him to be proportional ; he believes rather that they obey Fechner's law. In concluding he renders homage to the French philosopher Cournot, who, in his opinion, has expressed most profound views on the theory of probabilities.

In the discussion M. Couturat took exception to the value and sense of Fechner's law, and was of the opinion that subjective probability has no other measure than mathematical probability, and that, consequently, it is out of place to raise a question of proportionality between them.

M. Dickstein cited the works of Goziewski (analyzed in the *Jahrbuch über die Fortschritte der Mathematik*), and stated that M. Petzoldt (*Vierteljahrschrift für wissenschaftliche Philosophie*) had attempted to apply the theory of probabilities to the principles of mechanics.

E. O. LOVETT.

PRINCETON UNIVERSITY.